

2003 EMAILS
3664. 2003-01-02

cc: mann@virginia.edu
date: Thu, 02 Jan 2003 10:04:43 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Gil-Alana manuscript
to: k.briffa@uea.ac.uk

Dear Keith,
First off, happy new year!
Sorry I haven't gotten back to you sooner on this manuscript. It has been sitting in my inbox in Charlottesville while I was on sabbatical this fall, so I just found it the other day.
Hopefully not too late. Here is my quick review based on admittedly only skimming the paper. I hope it is still helpful!
best regards,
mike

Review of manuscript "A Global Warming in the Temperatures in the Northern Hemisphere Using Fractionally Integrated Techniques",
author: L.A. Gil-Alana
This manuscript describes some interesting statistical modeling experiments with the CRU instrumental 'Northern hemisphere mean temperature' series of 1854-1989, building on previous work by Bloomfield and others.
The primary problem with this, and other similar past papers of this kind, however, is that the wrong null hypothesis is assumed, creating somewhat of a 'straw man' for the argument in favor of a long-range dependent noise process. The null hypothesis invoked is that the observed NH mean temperature series is a realization of a stationary noise process, and that null hypothesis is subsequently rejected in favor of a non-stationary noise process (i.e., a fractionally-integrated noise process). The null hypothesis thusly assumed is inappropriate however, leading to false conclusions regarding the statistical character of the series. It is very likely that at least 50% of the low-frequency variability in the series in question is externally forced (by volcanic, solar, and in particular in the 20th century, anthropogenic radiative forcing). See e.g.: Crowley, T.J., Causes of Climate Change Over the Past 1000 Years, Science, 289 (14 July), 270-277, 2000.
The non-stationary (ie., the 20th century trends) in the series in large part arises from the linear response of the climate to these forcings, and much of the apparent 'non-stationarity' is simply a result of the non-stationary nature of the forcings, not the non-stationarity of the noise term. Moreover, this associated temporal dependence structure is almost certain to change over time, as the emerging anthropogenic forcing increases the relative importance of the forced vs. internal (noise) component of variance. See e.g.:

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Wigley, T.M.L., R.L. Smith, and B.D. Santer, Anthropogenic Influence on the Autocorrelation Structure of Hemispheric-Mean Temperatures, Science, 282, 1676-1680, 1998.

The appropriate null hypothesis (and a challenging one to beat, in my opinion) would be

that the observed temperature series is the sum of an externally-forced component as

modeled e.g. by Crowley (the data is available here:

<http://www.ngdc.noaa.gov/paleo/pubs/crowley.html>) plus a simple autocorrelated AR(1)

internal noise process. This is the most physically-plausible model for the observed NH

mean temperature variations, so the fractionally-integrated process must at the very least

do better (in a statistical sense) than this model...

There are a number of other minor problems:

1) No account is taken of the obvious change in variance (and presumably, the temporal dependence structure as well) back in time with increased sampling uncertainty (and

potentially, bias due to limited spatial representation in the underlying data network) in

the sparser early observations. For some purposes that isn't a problem. However, in this

study, where it is precisely the variance and temporal dependence structure of the series

that is being analyzed, I believe this is a problem.

2) It looks as if an unnecessarily outdated version of the CRU NH series has been used. A

revised, and updated version through 2001 is available online here:

The author should also reference more recent work:

[1]<http://www.cru.uea.ac.uk/cru/data/temperature/>

Jones, P.D., M. New, D.E. Parker, S. Martin, and J.G. Rigor, Surface Air Temperature and

its Changes over the Past 150 Years, Reviews of Geophysics, 37 (2), 173-199, 1999.

see also the additional references and information in the website indicated above.

3) It seems to me that a number of other papers on long-range dependence in surface

temperature series have been published over the past 5 years (e.g. Smith, Nychka, others),

and the author needs to do a far more thorough literature review. The reviewers literature

review looks, on the average, to be about 5 years or so out of date...

I would thus suggest that the authors resubmit the paper for consideration after appropriately dealing with the issues outlined above.

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

106. 2003-01-09

date: Thu Jan 9 12:52:31 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: DEFRA meeting - 17 January

to: "Jonathan Koehler" <J.Koehler@econ.cam.ac.uk>

Thanks Jonathan - do you intend to make the DEFRA meeting on stabilisation etc.

Page 2

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on Friday

17th January? I know Terry is going and Alex also and me. I have put your name down

anyway in case you can/do want to make it. Starts at 1030 at DEFRA I think.

Mike

At 10:41 09/01/03 +0000, you wrote:

Mike,

My view of Terry's concerns that he has expressed is that the funding so far does only

provide for a pilot (disequilibrium) model. I am happy with this, because we are

building

a fundamentally new type of model, which will require considerable work before it

functions properly, in particular the incorporation of non-linear technological change.

There are many issues, such as Foreign Direct Investment, arrangements for technology transfer through the Kyoto protocol, the extension of the model to include

other geographical regions besides the current US, EU, China (and now UK) split, for

which extra money would have to be found.

Jonathan

Date sent: Thu, 09 Jan 2003 10:21:13 +0000

To: "Terry Barker (DAE)" <Terry.Barker@econ.cam.ac.uk>

From: Mike Hulme <m.hulme@uea.ac.uk>

Subject: Re: DEFRA meeting - 17 January

Copies to: alex.haxeltine@uea,j.kohler@econ.cam.ac.uk

> Thanks Terry for your comments. I made the point about benefits explicitly

> since although economists are usually on the look-out for costs&benefits,

> environmental scientists who do some of the initial modelling work of

> climate change impacts are not always so even-handed.

>

> Your other points about the scale of the economic modelling work are

> interesting and I will bear them in mind, both in our FP6 meeting next week

> in Amsterdam and also in the DEFRA meeting and other Tyndall meetings.

>

> I presume Jonathan shares your concern, although he has not expressed it in
> quite such a direct way to me.

>

> See you next week,

>

> Mike

>

>

> At 16:55 06/01/03 +0000, you wrote:

> >Dear Mike

> >

> >The only comment on your note I have is on the 4th bullet point on

> >impacts/adaptation. I suggest that it read "assessing any benefits of

> >climate change", because costs and benefits are usually set off

> >against each other.

> >

> >On a related matter, I am concerned that we in the Tyndall Centre

> >do not have sufficient funding and resources for the construction of

> >the global dynamic economy-energy model being proposed. I am

> >hoping that the FP6 proposal will include something more to add to

> >our present projects, but the proposal appears (from Annex A) to be

> >relying on current economy models. However, the large models

> >used in the TAR post-SRES stabilisation exercise all adopt the

> >general equilibrium approach.

> >

> >I see an important role for Tyndall in developing a dynamic, non-

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> >equilibrium model, preferably with the capacity to simulate the
> >historical world economy/energy/GHG emissions, so that the effects
> >of e.g. oil prices shocks can be measured. This will add to the
> >credibility of such a model and provide a challenge to the general
> >equilibrium models.
> >
> >Our current projects are a start in this direction, but the actual global
> >model is being developed on a shoe string. If I compare the
> >resources that went into E3ME (a comparable model, but at an EU
> >level rather than the world and for 5-10 years into the future rather
> >than for the next 100 years) with those going into E3MG, there is an
> >order of magnitude difference for a much bigger task. I fear that we
> >may end up with a prototype model or a pilot study for a model.
> >
> >Ideally we would have a large E3 mitigation project here to
> >supplement the Tyndall projects, but we clearly need to do more
> >work on funding. Perhaps the DEFRA meeting will bring out this
> >problem and suggest a solution.
> >
> >best wishes for the New Year
> >
> >Terry
> >
> >
> >Date sent: Fri, 03 Jan 2003 15:17:51 +0000
> >To:
> >tsb1@econ.cam.ac.uk,f.berkhout@sussex.ac.uk,n.adger@uea.ac.uk,
> >"N.W.Arnell" <N.W.Arnell@soton.ac.uk>,alex.haxeltine@uea,
> >e.tompkins@uea.ac.uk,j.kohler@econ.cam.ac.uk
> >From: Mike Hulme <m.hulme@uea.ac.uk>
> >Subject: DEFRA meeting - 17 January
> >
> > > -----_21341716==_
> > > Content-Type: text/html; charset="us-ascii"
> > >
> > > <html>
> > > Dear All,

> > > As promised, please find this short document attached which I intend to
> > > submit to DEFRA ahead of the 17 January meeting. This summarises
> > > some thoughts and work relevant to the stabilisation/Article 2 Q David
> > > Warrilow/IPCC is keen to promote.

 Please let me have any
> > > comments/additions etc. by Tuesday 7th January, since I need to
> > > send it to DEFRA by next Wednesday.

 Many thanks,

> > > Mike

 </html>
> > >

3777. 2003-01-14

date: Tue, 14 Jan 2003 17:07:42 +0000 (GMT Standard Time)

from: Julie Burgess <J.Burgess@uea.ac.uk>

subject: Your Grant numbers

to: k.briffa@uea, c.goodess@uea, c.hanson@uea, t.holt@uea, p.jones@uea,
m.kelly@uea, t.osborn@uea, j.palutikof@uea, s.raper@uea, d.viner@uea

Jean asked me to circulate a list of grant numbers to P.I.s in CRU, so here it
is (attached for you to print off and within this email for those without
proper computers):

Julie

1RCRUI0----	1287	INTAS	10/98-9/2000	KRB		
1RCRUH0----	1297	The Holocene	11/98-12/02	KRB		
1RCRUA0----	1460	NERC/BAS	TSUNAMI	9/99-9/02	JPP	
1RCRUC0----	1728	Met Office	07/00-06/02	MH		
1RCRUH0----	1776	CEC Agence Nat.	Gestn DR BIOCLIM	10/00-9/03	JPP, CG	
1RCRUF0----	1832	NIREX	10/00-10/03	M Hoar		

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1RCRUH0----	1857	EC SWURVE Newcastle	12/00-11/03	PDJ	
1RCRUH0----	1858	EC CLIWOC Madrid	12/00-11/03	PDJ	
1RCRUH0----	1872	EC HOLSMEEER Bangor	1/01-12/03	PDJ, KRB	
1RCRUC0----	1901	LINK	12/00-2/03	DV	
1RCRUA0----	1937	NERC	8/01-2/02	PDJ	
1RCRUJ0----	1977	US DoE	5/01-4/04	PDJ	
1RCRUH0----	1898	CEC CLIMAG West Africa	3/01-8/03	JPP	
1RCRUA0----	2005	Tyndall "Scenario development"	4/01-7/02		MH, TO, CG, JPP
1RCRUA0----	2007	Tyndall "An integrated assessment"	STORMS		4/01-6/03
JPP, TDD, TO					
1RCRUA0----	2012	Tyndall BLUEPRINT	4/01-5/02	JPP	
1RCRUA0----	2032	UMIST/Tyndall	4/01-8/03	PDJ, JPP, TO	
1RCRUA0----	2077	NERC	4/01-3/03	TO, AW	
1RCRUA0----	2099	NERC/CLRC	1/01-9/03	TO, JPP	
1RCRUL0----	2125	ACCELERATES, CEC Oxford	1/02-12/03	DV	
1RCRUH0----	2130	PRUDENCE	11/01-10/04	JPP	
1RCRUH0----	2156	MICE	1/02-12/04	JPP	
1RCRUA0----	2177	NERC/SOUTHAMPTON	5/01-4/03	SR, AR	
1RCRUC0----	2196	Met O	12/01-11/02	PT, PDJ	
1RCRUC0----	2199	Met O	12/01-1/03	TO, KRB	
1RCRUH0----	2209	CEC STARDEX	2/02-7/05	CMG, PDJ	
1RCRUJ0----	2272	NAOO	11/01-10/02	PMK	
1OCRUL0----	2280	TIEMPO	4/01-3/03	PMK	
1RCRUB0----	2300	ODI	11/01-6/02	PMK, LEB	
1OCRUJ0----	2366	UN FCCC	3/02-9/02	SCBR	
1RCRUH0----	2459	EMULATE	11/02-10/05	PDJ	
1RCRUH0----	2471	Simulations observations...	11/02-10/05	KRB, TO	
1OCRUF0----	2494	SNIFFER	10/02-5/03	PDJ, JPP	
1RCRUH0----	2501	Marie Curie Visiting Fellowship	1/03-12/03	CR (Cyrille), KRB	
not known yet RGI CRANIUM 4/03-3/06 PDJ, CMG, DV					
1RCRUI0----	2553	ESF, Clim. change, env. & tourism	12/02-12/03	DV	
1RCRUA0----	2569	EPSRC, Constr. clim. scen...built env, transp. & utils.			
4/03-3/06 PDJ, JPP, CMG, DV					

Awarding Body Types (level 4)

A0	Research Council or British Academy
B0	UK based charities
C0	UK Central Government; British Council; Royal Society; Non-departmental
Public Bodies	
D0	Local Authorities
E0	UK Public Corporations
F0	UK Industry/Commerce
G0	UK Health/Hospital Authorities
H0	Governmental bodies operating
in EEC countries	
I0	Non-governmental bodies operating in EEC countries
J0	Other overseas (non-EEC) sources
K0	Teaching Company
L0	Other sources

Julie Burgess

Climatic Research Unit

University of East Anglia

Norwich NR4 7TJ

Tel. +44 (0)1603 592722

Fax. +44 (0) 1603 507784

CRU web site: <http://www.cru.uea.ac.uk/>

Attachment Converted: "c:\eudora\attach\grantnos.doc"

888. 2003-01-16

date: Thu, 16 Jan 2003 08:08:47 -0700

from: Tom Wigley <wigley@ucar.edu>

cg2003
subject: Re: simulations of past 500 years
to: Tim Osborn <t.osborn@uea.ac.uk>
The main person is Caspar Ammann ... ammann@ucar.edu.
Tom.

Tim Osborn wrote:

>
> Tom - we have our first project meeting for our SOAP project next week and
> one of the things on the agenda is to discuss whether we wish to establish
> collaborative links with other groups undertaking similar research (i.e.,
> simulation of the 500 or 1000 years and comparison with proxy data and
> associated reconstructions). You told a little about some runs being done
> by NCAR that are similar, though perhaps with improved forcing
> histories. Could you let me know who (and their email) is undertaking this
> project, so that I can find out some more about what is being
> done? Thanks. Our SOAP project now has a website with a small amount of
> information on it:
> <http://www.cru.uea.ac.uk/cru/projects/soap/>
>
> Best wishes
>
> Tim
>
> P.S. This email has reminded me about the temperature st. dev. fields you
> need - I'll sort them out soon.
>
> Dr Timothy J Osborn | phone: +44 1603 592089
> Senior Research Associate | fax: +44 1603 507784
> Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
> School of Environmental Sciences | web-site:
> University of East Anglia | <http://www.cru.uea.ac.uk/~timo/>
> Norwich NR4 7TJ | sunclock:
> UK | <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

5139. 2003-01-17

cc: Keith Briffa <k.briffa@uea.ac.uk>
date: Fri Jan 17 09:34:54 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Fwd: Re: simulations of past 500 years
to: ammann@ucar.edu

Dear Caspar,
together with Keith Briffa, I am coordinating a 3-yr EU project that has just begun, called "SOAP: Simulations, Observations & Palaeodata...". This brings together a number of European climate modelling and palaeoclimate groups with the purpose of using both models and proxy data to study the climate of the past 500 to 1000 years. There is a little more information on our project website: <http://www.cru.uea.ac.uk/cru/projects/soap/>
I was talking with Tom Wigley about this project and he mentioned that some similar model simulations were being undertaken at NCAR and that I should contact you to find out some more information about them.
Would you be willing to give me some brief details about your project? I would be interested to have an overview of the model simulation(s) that you are carrying out, and what your plans are for proxy data comparisons.
Best regards
Tim

88. 2003-01-18

date: Sat, 18 Jan 2003 21:05:49 +0000
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: Pattern scaling document for the TGCIA
to: Timothy Carter <tim.carter@ymparisto.fi>, t.mitchell@uea.ac.uk

<x-flowed>

Tim,

As promised some comments on the paper.

General: It is very good, just what is needed and puts the last 4 years of debate into the right context.

General: why consistently 'climate changes' rather than the more usual 'climate change'?

Abstract, line 10: why only quote as high as 0.99 and not the lowest correlation (which actually is more to the point - it is still very good after the 2020s, even for precip).

Abstract, lines 12-13: as worded this does not quite follow, although I see from later that the ellipses used are at 95% confidence. Just because they fall outside natural variability does not *in itself* prove they are stat. sig.

p.2, lines 17-19 (and also several places on p.4): impacts are mentioned, but nothing said about adaptation. It is really adaptation actions/decisions that are crucial, impacts are only one way to get there. Alter the focus.

p.2, line -10: add 'necessarily' between 'not' and 'be'. AOGCMs may actually do not so bad a job on occasions about climate change (relative changes for example), so don't completely dismiss this one.

p.5, section 2: general point: there is no list or table or statement about exactly what these 17 experiments are. The models are listed, but not the experiments. e.g. which SRES scenarios did which modelling group and how many ensembles? For the lay person this is not obvious.

p.7, top line: you should perhaps make the point that simple bias indices such as these may partly be explained by elevation offsets (model height vs. real height). It is to my mind a mitigating factor than can work in a model's favour (not always). It should be mentioned, because the biases may not be due to just dumb models, but due to simple resolution issues that can be adjusted easily. A similar point perhaps applies in the next para. about ocean/land boundaries. OK, you could say this just shows how bad models are, but it perhaps gives people a poorer view of the model physics and credibility than is truly needed. Another point to mention in this para about precip. is the obvious point about decadal natural variability. It's a tall order to expect the models to get the 1961-90 monthly mean precip. exactly right, owing to internal variability. Indeed, give such variability can be plus/minus 10-20% or more it would be astonishing if they matched. Be generous to models I say.

p.9, middle - interesting point about ECHAM4 and NCAR masks!!

p.15, para 2: didn't you have AIFI available from Hadley? Surely it could have been used to test this? Last sentence in this para: why 'evidently conform'?

p.16, last line: interesting point here: if you claim the pattern-scaling didn't work for the 2020s because of nat var (S/N ratios) then why actually should we go with the raw model results anyway - certainly if it is the signal we are interested in (and not the noise), it suggests the raw 2020s models results are misleading us! This is a rather circular argument I realise but the bottom line point again comes back to S/N ratios and the role of nat decadal variability, esp. for precip. Are we going to recommend adaptations to noise or to signals - and why?

p.17, middle para: what about mentioning climate sensitivity here? I know its out of vogue now, but PCM and NIES differences are explained by overall model sensitivity aren't they.

p.17, para 4: this point about where agreement occurs between models is important. Some people - I heard Wigley do it recently - write models off at regional scales re. precip changes because they all disagree. They do for some regions, but not all and where we think we have physical grounds to accept agreement as legit. (e.g. UK; cf. UKCIP02 scenario methodology) then we should be confident to say so.

p.17, line -7: why use 'forecasting' here? Could confuse some people. The old argument about terms I guess. And again top line on p.18 is dangerous - we can "predict" nat. variability in a stochastic sense using ensembles. Change the wording.

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p.18, line 9: not only are they difficult to foresee, they are simply unforeseeable to a significant extent because it is we who determine them; I prefer to make the distinction between different types of prediction problem more explicit.

p.18, lines 19-20: I don't like the use of 'truth' and 'precise' here. It implies a strong natural science view prediction and the competence of science (modellers!) which I think should be softened.

p.18, para 4: the inter-model differences bit being as large as the inter-scenario differences. Again at least mention the role of nat var here - some of these inter-model differences *must* be due to nat var, not simply models not able to agree with each other.

p.19, para 1: I think the stabilisation case should be mentioned here. What about pattern-scaling stab scenarios? As I hear it from DEFRA and Hadley here in UK this was a big issue at the TGCIA meeting. Make a comment at least; I think in principle p-scaling is probably OK (within some limits) even here. I think you should make reference to some of Tim Mitchell's work here (and/or elsewhere) since he has looked at some of these things too. His thesis or his CC paper perhaps.

And finally, w/o sounding as self-serving as Tom Wigley, it would be nice if you could reference (perhaps in section 3.3) the Hulme/Brown (1998) paper in CR which was the first time I published scatter plots in this form for GCMs results - and possible the first time this form of presentation had been used anywhere (but I stand corrected of course; maybe I simply picked it up from someone else).

So there it is: a great piece of work and a good write up. I don't know Kimmo but pass on my congratulations to him. I'll look out for it on the web site.

Best wishes,

Mike

At 13:42 13/01/03 +0200, Timothy Carter wrote:

>Dear Mike and Tim,

>

>I know that you are not now involved in the TGCIA, but there is still some old baggage from the days of Mike's tenure that you may have some interest to comment on concerning regional pattern-scaling work.

>

>I attach a paper that we have prepared and distributed at the latest TGCIA meeting for comment (last week). If you have any comments, I would be very appreciative. I need comments if possible by the end of this week.

>

>The 96 pages of scatter plots are currently enormous files, and I can't possibly attach these for you to see. I am working on a way to get these substantially reduced in size. I have attached one example so you can see what to expect.

>

>Any feedback would be much appreciated. We intend to post this document, or something like it, on the DDC.

>

>Tim - have you published any of your Ph.D. results yet?

>

>Best regards and Happy New Year,

>

>Tim

>

>

>

>

>*****

>Timothy Carter

>Research Professor

>Research Programme for Global Change

>Finnish Environment Institute (SYKE)

>Box 140, Mechelininkatu 13a, FIN-00251 Helsinki, FINLAND

>Tel: +358-9-40300-315; GSM +358-40-740-5403; Fax: +358-9-40300-390

cg2003

>Email: tim.carter@ymparisto.fi
>Web: <http://www.ymparisto.fi/eng/research/projects/finsken/welcome.html>
>*****
</x-flowed>

732. 2003-01-21

date: Tue Jan 21 11:18:25 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: follow-on request re. data
to: Rahmstorf_Stefan

Dear Stefan,

Many thanks for sending me your powerpoint slides re. Integration. In preparing my paper and presentation for the abrupt meeting in 2 weeks time (I am talking at the RAPID kick-off meeting tomorrow), one thing I would like to do is to re-plot your two THC scenarios from your 1999 CC paper in terms of a magnitude/rate plot and compare them with the IPCC standards (see my rough hand-drawn version attached as graph.jpg). Your two scenarios I mean are in the attached ppt slide.

Would you be willing to let me either have the raw data behind these two plots - so I can calculate the decadal rate of change vs. magnitude myself - or else perhaps ask one of your team to extract these parameters for me from the data so I can plot them up? I would need these data quite soon if possible, i.e., before Friday this week.

I also attach my latest draft of the paper for the Royal Society meeting - and if you have any comments on this I would very much welcome them in the next 2 weeks. I still have some more work to do on this of course before publication.

Many thanks for your help and best wishes,
Mike

1724. 2003-01-21

date: Tue, 21 Jan 2003 11:18:37 -0000
from: Dan Tapster <dan.tapster@bbc.co.uk>
subject: Draft script for climate change
to: "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>

Dear Dr Hulme,

Thanks for taking the time to talk to me this morning. As discussed, I'm sending you the

latest copy of the script for this programme. It would be great for me if you could read

it and let me know your thoughts.

My background is zoology, so I'm no climate change expert so there may be errors or

serious omissions in there which would be very useful to know about. I'd be grateful if

you could send me your hourly/daily rate as a script consultant so that I can budget your

time and also advise you how long we can afford for you to help!!

At this stage I'm guessing that you will certainly question the sea-level rising by 55

metres if Antarctica melts. Remember that this is about the future of Britain - not just

the short term but also the long term (hence the piece about the arrival of the next ice

age).

After getting your initial thoughts it would be great if I could visit the Centre and meet

various people whose advice is essential, but I can certainly arrange this through

Asher.

Please do keep this script confidential for the time being!

Dan

8. THE FUTURE OF BRITAIN

Last changed: 19 December 2002

Dan Tapster

SERIES TITLE

1. TEASE

Waves crash against the Scottish coast.

Aerials over coastline

Pick out Presenter walking across barren looking landscape

Crash into Presenter

Images from other episodes -

volcanoes spitting fire

Scotland's mountains being created

Tropical seas rising and falling graphic

Trees felled

Agriculture spreads over the UK

T/L of motorways and cities being built

Graphics: As Presenter walks and talks on Lewis, roads begin to appear and then houses

and sky-scrapers

I'm on the remote island of Lewis. 8 weeks ago, at the start of this series, this was

where I started my journey through Britain's history. I came here for these. Rocks.

Lewisian gneiss to be more precise. Why are they important? Because they are the oldest

part of the British Isles formed over 3 billion years ago. Even in geological terms,

these are the ultimate survivors. Since they've been deposited they have seen some amazing

things.....

They've felt the force of volcanoes erupting across Britain.

They've survived huge mountain ranges - bigger than the Himalayas being pushed up from the

Earth's mantle

They've been hundreds of feet underwater in tropical seas.

And in recent years, they've seen humans change their environment incredibly....

Forests were felled,

Land was turned to agriculture

And the growth of grey.

But what will these great surviving rocks see in the future? Will humans continue to

dominate the changes we see in our landscape? Will somewhere as remote as Lewis eventually

succumb to the growth of grey? In this programme I'm going to find out what the future

holds for our beloved Britain.

PROGRAMME TITLE

THE FUTURE OF BRITAIN

2. THE CLIMATE IS CHANGING

Lush spring flower meadow of Gilbert White's house

Locked off Time/lapse of spring flowers erupting into bloom (field has oak and ash trees in

it). Presenter walks into garden.

Brief details of church

Details of windows

NH cutaways.

Back to Presenter in garden

Time/lapse of poppies bursting into flower

Time/lapse of oak trees going to leaf.

Back to Presenter

Feeling of food-chain disruption

Back to Presenter standing by a pond looking at mating frogs and toads

NH footage of toads laying eggs.

Back to Presenter

Images of a sweltering Britain

Library archive of weather reports with suns across the whole of the UK

Flash frames of European stories?

Over 200 years ago, this was the home of one of Britain's most gardeners ever!

Gilbert

White. Gilbert was so important because he was pretty much our first ever naturalist. He

spent hours observing the plants and animals around his home. In the church up the road

where he was curate, there are stained glass windows commemorating his life.

Whilst others were polluting the countryside and destroying woodland, Gilbert was

fascinated by it. He took detailed records of all the bird life around, all the mammals

and even the amphibians. Yet he was more important than that. You see he was the first to

observe the timing of the natural world - he was the father of ecology. And since his

initial interest, this has grown into a great and vitally important topic.

But if Gilbert was alive today, he'd be very surprised at how nature's timings have

changed. In fact, it's amazing how things have altered even in my lifetime.

Forty years ago, when I was growing up, I used to pick flowers of dead nettles.

They're beautiful aren't they, so much so that I still enjoy picking them today. But there's a

difference and not just in my age! When I was a lad, I used to hunt for these flowers in

March. Yet here I am sitting amongst them and it's the end of January.

And they're not alone - Poppies are coming in to bloom three weeks earlier than they did

in the 1960s.

This trend is not just restricted to flowers. Trees also seem to be in a rush.

Oak trees are leafing 10 days earlier than they did when I was growing up.

Traditionally,

oak and ash have been fairly equal in their quest to leaf first. This led to an old

Lincolnshire saying which was used to predict summer rainfall:

It was said: "If the oak is before the ash, we're in for a splash. If the ash is before the

oak, we're in for a soak." But these days, not many people use the saying at all.

This shift in leafing date has had profound knock-on effects. Now that the oak is winning

the race to sprout leaves first and capture the largest share of sunlight, ash trees are

being out-competed. As a result, the biological make-up of Britain's woodlands is

changing. This in turn has severe implications for other wildlife that depends on the

ash. For instance, there are 68 species of insects that live on ash trees. So the early

flowering of oak will disrupt food chains, food webs and ultimately entire eco-systems.

But it's not just plants that are out of kilter. There's animal evidence too. Take these

frogs. They are their warty cousins, toads, are laying their eggs 10 days earlier than

30 years ago.

So what does this all mean? Britain is warming up. And if you don't believe the natural

evidence what do the scientists say?

Well meteorologists tell us that the 10 warmest years in the last 100 have all been since

1980 and the 3 warmest years in the past 600 have been post 1990. And this trend is not

restricted to Britain - across Europe, glaciers are retreating fast, cicadas are singing

in the Baltic and summers are getting hotter.

3. OUR HOT PAST

Presenter leaving Gilbert White's garden and walking towards the church (up the famous

zig-zag path)

Jib arm reveals church in background

Details of church

Cut to presenter in church grounds

Details of the church

Stained glass detail

Presenter walking through church grounds.

Graphics create an element of warmth before vineyards and olive trees and exotic fruit

trees appear. Presenter picks some.

Shots of wine factory with bottles with British labels (e.g. Birmingham Sauvignon Blanc)

on it

So, there is no doubt, global warming is happening. But is this a problem?

Britain's climate has changed incredibly in the recent past as our planet's orbit round the

sun has changed ever so slightly. So frequent have these climate shifts been that looking

back at Britain's climate over the last 2 million years, the only constant thing has been

change. We've been gripped by ice ages as harsh an environment as Antarctica today. Yet

we have also been warmer than today and there seems to have been no bad side-effects.

Around 1000 years ago Britain entered a period of extremely warm weather known as the

Climatic Optimum. This was a time of long summers, short winters and bumper harvests!

Some people became so rich through selling farm produce that they could build buildings

like this church - St Mary's - the very church that centuries later Gilbert White was to be

curate of.

The intricate details of the carvings show that this 'Climatic Optimum' church was a costly

piece of work.

And the bumper harvests were not always what you might expect. Nearby stained glass

windows show that vineyards were a common feature of the landscape.

So might the same happen again?

If all global warming is going to do is allow us to grow olives and peaches, harvest giant

root vegetables and make wine whilst sunbathing in a Mediterranean-like climate, then

shouldn't we all be celebrating? Just think, after lunch in the sun, we'd all be allowed

to siesta for a couple of hours before going back to work. Then we'd party into the night

sipping the local vino and whilst eating the local olives and sun-dried tomatoes.

Maybe British wine will become world famous

4. OUR WILDLIFE FEELS THE HEAT

Double doors to church are flung open and Presenter with parasol steps out into the sweltering sunshine. Graphics change the background to one where everything is dry, dusty and dying. Beech tree woodland. Graphics of beech, birch and spruce dying in the heat. Top of Ben Nevis with beech trees on Squirrels, deer and badgers eating beech tree nuts. Graphics fade out beech tree leaving animals looking forlorn! WHAT WILL REPLACE THEM Presenter in Caledonian pine forest Ptarmigans, snow buntings, arctic hares ghost out. Durham's bee-eaters Poole's egrets Presenter outside church - graphics unusual birds flying overhead - e.g. screeching macaws. Graphics of bee-eaters nesting on bank of Thames, parakeets the same and vultures nesting on St Paul's cathedral Ostriches Vulture feeding on heat-killed deer/badger? Unfortunately, things are not that simple. You see it looks as though temperatures are going to be higher even than the Climatic Optimum, and this could have very serious consequences. By 2080 temperatures could have risen by a further 3.5 degrees. Summer temperatures could peak at a whopping 40°C. Summers would become much, much drier whilst milder winters will be marked by extreme rainfall. Such changes will have massive effects on our wildlife. Beech trees, spruce and birch for instance, will be unable to cope with the warmer temperatures. They will steadily migrate further north to keep up with the cooler climates that are their preference. They might end up stuck on the high Cairngorm mountains with nowhere else to go. Plants are at the very base of the food chain - ultimately it is plants that fuel entire eco-systems. Without certain species, animals will suffer. Beech tree nuts are an important food source for many animals - a whole host of birds, and mammals too - squirrels, deer and even badgers depends on these seeds. Without them, they will really suffer. RESEARCH - WHAT WILL REPLACE THEM Today in Britain we have a number of species which are literally remnants of the ice-age. Like Caledonian pine. At one point around 12,000 years ago, this species covered great areas of Britain. Today it holds on in only a handful of locations. Despite intensive replanting programmes, global warming will cause this wonderful species to become extinct in our country. And are other ice-age remnants - the ptarmigans, snow bunting and Arctic hare may also

finally lose their grip.

As the ptarmigans and snow buntings disappear, a whole host of new birds will move in.

This is already happening in parts of Britain. In May 2002, bee-eaters were seen nesting

near Durham - the first time this has ever happened in Britain. And in Poole Harbour in

Dorset there have been resident little egrets for a number of years. These yellow-footed

herons used to be restricted to the tropics, but now they crop up all over parts of

southern England.

If temperatures continue to rise then more exotic birds will move in.

We'll have bee-eaters nesting on the banks of the Thames and more parakeets screeching

around. There'll be crowned cranes dancing and maybe even ostriches roaming our fields.

We could even get vultures circling overhead - certainly there would be a lot for them to

feed on as many of our mammals would over-heat during the summers.

5. OUR KNOTTED FOREST

Brief stylised montage of animal invaders which are today very successful - grey squirrels, mink, American signal crayfish, the Chinese Mitten crab, the sika deer and

the pigeon, etc.

Presenter crawling through extremely dense - almost impenetrable vegetation:

Graphics pull out to very high altitude to show that Presenter is in Wales.

Crash back in to aerials of 'knotweed forest'

Time/lapse (to music) of - both outdoors and through roads.

Presenter walks into shed

Possible flash frame of library footage of Presenter talking about ornamental knotweeds on

Gardener's World.

T/L of knotweed growing into shed culminating in shed explosion!

Graphics of knotweed growing even faster and spreading across the whole of Britain -

including over its roads and cities.

Cut aways of seeds blowing across UK

Graphics of famous landmarks becoming overrun with knotweed - e.g. Stone Henge, the Angel

of the North, Cheddar Gorge, St. Abbas Giant, Tintun Abbey.

Aerials of London as all green areas turn to knotweed.

Graphics as M1 turns to knotweed. And Heathrow too.

Knotweed office representation

Global warming, then, will provide the opportunity for new species to invade Britain,

replacing our natives. Invading species aren't new. There have been immigrant plants and

animals arriving on our shores since Neolithic times - grey squirrels and mink are two of

the most famous examples. But global warming could change an old invader in a very

dangerous way indeed.

With vegetation as dense as this you might think that I'm in the Amazon! It's so thick

that it is almost impossible for me to move. But I'm not deep in the heart of the jungle,

I'm just outside Swansea!

In this part of Wales, Japanese knotweed thrives. It spreads incredibly fast and soon

forms massive aggregations - some over 25,000 square metres - where it simply chokes all

other plants to death..

It was introduced from Asia to Europe Asia in the mid nineteenth century as an ornamental and fodder plant. But it spread and it did so because it has such an extraordinary growth rate.

And it can even grow up through roads and into buildings. It causes such a problem now

that you can actually be prosecuted for helping it spread.

Given the vast swathes of land that it covers, what is most amazing is that it is sterile

- it only spreads vegetatively.. You see, it is a dioecious plant which means that you

need male and female plants for sexual reproduction to occur, yet in Britain and Europe we

only have female plants. However, the real risk with knotweed is that it will somehow

hybridise with another plant and then reproduce sexually. In the past, I've encouraged

people to plant other exotic knotweeds in their gardens and these are exactly the

candidates that could hybridise. And global warming will increase that likelihood so I'm

going to dig mine up now!

Areas that are already desolate except for knotweed would begin to expand and expand and at

an alarming rate.

But the problems would not end there. Knotweed seeds would be blown up across the whole of

the British Isles and wherever they germinated, it would not be long before knotweed

thrived. In fact, knotweed would grow everywhere.

Even our cities would not be safe - all our parks would be dense thickets

It could even cause havoc to our transport system as it employs its ability to grow through

concrete.

At the moment, Swansea City Council employs the one and only 'knotweed officer' in the

country. Maybe - because of climate change he will need lead a team of millions!

6. THE LANDSCAPE CHANGING BEETLE

Presenter walking through the National Forest

Presenter fumbles in his pocket and brings out mounted beetle specimens.

NH footage of Asian long-horn beetle life-cycle with sync intercut

Graphics of familiar forests becoming stands of dead trees.

Street trees die out.

Famous trees die out too - like the Royal Mile near Buckingham Palace, Windsor Great

Park, trees at Lord Cricket Ground, churchyards, etc.

Mobile phone tree masts are all that remain!

Knotweed begins to creep up the mobile mast.

Global warming will also allow new invaders to colonise warmer Britain. This combined with

the massive increase in global travel and trade means that invading species will increase.

And there is an animal which is capable of changing the landscape as much as a fertile

Japanese knotweed could At the moment Britain is too cold for it, but if things change,

the National Forest where 30 million trees have been planted could be particularly

vulnerable. So what is the culprit? Well I have one just with me.

It's the Asian longhorn beetle. Originally from China it has smuggled its way around the

world bored into wooden crates. Once it arrives, it climbs out and has massive effects on

the landscape. How? It kills trees. Females will chew through tree bark with their massive jaws to lay 60 or so eggs.

Once these hatch the larvae do the damage by boring deep into the wood which will

eventually kill the tree. It is not fussy which trees it goes for. So far it has not

reached Britain, but if it along with other pests - like the 8-toothed spruce bark beetle

or diseases such as the mysterious sudden oak death do reach Britain, their effects could

be catastrophic.

Our trees would be lost.

But we would not become a treeless country. Those mobile phone tree masts would certainly

be impervious!!

And knotweed forests would grow so fast that they would not be threatened. Soon they'd be

clambering over the mobile phone masts!

7. LIFE FOR US WOULD BE TOUGH

Back to Presenter at Church:

Graphics of vineyards again. This time they wilt.

Time/lapse of wilting crops

Graphics of plagues of locusts going through British farms.

Presenter on church tower looking over bushfires

Details of fires

Graphics of forest fires - including material shot from satellites

Graphics of people going about their daily business as places nearby burn.

Presenter back on church tower - the ground begins to crack up and the church begins to

crumble. Presenter wobbles?

Famous buildings in London collapse.

So global warming will change our plants, it will change our animals and it could pave the

way for new invaders which could have devastating effects on our landscape. But the

problems would not end there. Life in the extra hot Britain would be tough.

Long summer droughts would mean that crops would fail repeatedly - even vineyards and

sunflowers! If vineyards can't cope than wheat and barley, potatoes and sugar beet will

have no chance!

We will develop drought resistant crops but they won't be able to cope with a new threat -

locusts!

Long summer droughts will also mean that, like today in Australia, bush fires are a common

occurrence.

Forest fires cause billions of pounds worth of damage every year in the States and in

Australia - we too could see fires burning over massive areas - so large that they can even

be seen from space.

Like residents of Sydney, we would have to get used to them....

Clay, the very material that London, Bristol and other great urban areas are built on

will begin to dry and crack. This could be so severe that houses could start to crumble.

8. THE HEAT BRINGS EXTREMES

Presenter in church grounds with everything normal again until off-camera rain machine

begins drowning him in water:
Shots of heavy rain, and archive of floods over Britain
Back to Presenter, rain stops but lo and behold an off camera wind machine
kicks in!
Archive of hurricanes.
Graphics of fallen trees all over Britain. Knotweed remains unaffected.
Violently rough seas
Bulldozers working shale beaches (Martin Smith).
Graphics has huge wave coming over the wall.
But it's not going to be one long summer for us. Outside of the summer
droughts, Britain
will experience greater extremes of weather. Winters could be very wet indeed -
prompting
great flooding over much of the country - worse than anything we have seen over
recent
years!
Moreover, it seems likely that we will also have much greater wind storms -
both tornadoes
and if Britain's seas get really warmer - hurricanes too could become more
common!
Trees will be blown down all across the UK
And our seas could experience some of the roughest weather around. We will have
to improve
our sea defences to cope with these great swells of the future. Bulldozing
shale will not
be good enough.
9. THE GREAT MELT TAKE 2
Presenter in the Cairngorms, skiing towards camera. He parallel stops in front
of camera
and pulls up his goggles.
Presenter skis off. Mix to grass skiing tournament in France.
Melting icicles leads torrents flowing which eventually leads to Graphics map of
sea-level
rise (mostly pinched from programme 3 - except the higher sea level rise).
Crash zoom into various locations - e.g. Water gushing down Channel Tunnel,
Thames Barrier
overwhelmed, etc (could use archive flood news footage).
Graphics of new coastline with submerged Britain
Presenter on top of Church tower again. As he talks water fills up everything
except the
last bit of the tower.
Graphics of new outline of Britain
Yorkshire Moors becomes metropolis
Graphics of University Boat Race as they row past towers only. Seals and whales
are
nearby. They row past a jetty with submarine tours. Follow underwater to see
tour of
submerged London.
Graphics of Canary wharf being a new bird rock.
Today millions of us enjoy skiing all over the world, and Scotland certainly
has some
great pistes to offer. But we better had make the most of it since pretty soon
there might
be no snow.
Global warming will certainly jeopardise the skiing industry since snow and
glaciers will
melt fast. Fortunately, for those of us who enjoy hurtling down mountains at
breakneck
speed, ski manufacturers are already designing grass skis for our enjoyment!
But it is not just ice on the mountain tops which will thaw, ice in the Arctic
may melt
too which could have devastating effects.
Trillions of gallons of water which had been locked up as ice will be released

into the world's oceans. The sea levels could rise by nearly 1 metre. This will be devastating for Britain. Once the Thames Barrier is overcome, Soho will be flooded within a matter of minutes. Water will pour down the Channel Tunnel. The new outline of Britain shows how many areas will be affected. But things could be even worse than this. There is evidence that the currently stable ice sheet in Antarctica has melted in the past. If this begins to melt then global sea levels could rise by a devastating 60 metres! This would change our country immensely. Britain's new outline would be unrecognisable. It would disrupt everything - there'd be no London, no Manchester, no Edinburgh, no Bristol. Instead there'd be thriving commercial centres on the uplands. Certainly where I grew up on Ilkley Moor, things would be very different. No doubt those hardy Oxbridge rowers would still try to keep up their traditions. And entrepreneurs would lead tours around a submerged London. The spires and towers which stuck out of the water would not be deserted, they would soon become home to a multitude of birds. Like Bass Rock, Canary Wharf could become a vast gannetry. And in the new hotter temperatures it might even be canaries which roost there!

10. THE FUTURE'S COLD

Double doors of the Church swing open again to reveal Presenter, dressed as before. It starts to snow. Graphics of Arctic melting and graphics of gulf stream stopping. Presenter back in snowy scene. Sync could be done this winter in Scotland? Images from Churchill Polar bears trundle past. Presenter in London, standing by the side of the Thames. Graphics of Thames freezing over. Presenter climbs down onto jetty and 'walks across frozen Thames'. Cut-aways of appropriate images. Snow-ploughs going along roads. Country houses become blanketed in snow. Graphics of trees and woods dying in the snow. Red fox scavenging in the snow. Presenter in Caledonian Pine forest in snow. Graphics cause it to grow to the horizon. But skiers and snowboarders needn't hang up their boots just yet and dive enthusiasts should not open shops in London. For there is another scenario associated with global warming that would prevent this wholesale warming and flooding, but would have just as pronounced effects on our landscape and natural history. Perversely, global warming could actually make Britain colder. As temperatures continue to increase the Arctic ice sheets will melt. But in doing so a massive amount of cold water will be released into the northern Atlantic. This cold water will completely disrupt the ocean currents, forcing the warm Gulf Stream current further

south or even halt its flow altogether. Ominously, this great current has decreased by

20% off northern Scotland since 1950, suggesting the driving force behind the Gulf Stream

flow is already in decline.

It is impossible to underestimate the value of the Gulf Stream- in fact it provides us with

as third as much energy as the sun! Without it we would have a climate similar to other

places on Britain's latitude like Churchill in Canada.

Churchill is very cold indeed. The average temperature in January is minus 28 degrees

Celsius, in June a mere 12. In fact the yearly average is an incredible minus 7! Clearly

it's a hit with polar bears.

But again, do we need to worry since we've been colder in the past too. And I'm not

talking ice-ages here, but colder periods that have lasted a couple of hundred years. In

fact when the climatic Optimum was over, it was soon replaced by the Little Ice Age.

Winters- such as that of 1739 were so severe that the Thames froze over.

Vast ice fairs were set up on the frozen river. Booths selling the likes of brandy balls,

ginger breads, black puddings, plum cakes, pancakes and glasses of hot ale with spices and

wine. Printing presses were popular too, selling certificates and poems commemorating the

novelty of printing on the Thames. But it wasn't all fun. IN February of that year it's

estimated that over 20,000 people in London were killed by the cold. (CHECK)

How would the rest of Britain fair if the Gulf Stream stopped?

Our roads would regularly be blocked by vast winter snow drifts - we would need to increase

the number of snow ploughs by ten fold at least.

More people would move to the cities for their artificially raised temperatures.

And what of our animals? Nearly all the plants and animals that we have today are not

adapted to the cold. Most would die off. The adaptable red fox would scavenge many of

those killed.

If Britain cooled through global warming than the Caledonian Pine Forests would in all

likelihood greatly expand their ranges once again. It would certainly be very different.

11. METEOR BRINGS DUST

Presenter back in church grounds at night..

Cut-aways of massive volcanic clouds of dust.

Presenter star gazing

Graphics of asteroid hurtling towards Earth.

Graphics of impact in Britain

Graphics of Dark Britain

Back to Presenter

Although temperatures would drop by up to 10 degrees, this would not be like an ice-age.

For that to happen, there would need to be some sort of trigger.

The coldest winter in the Little Ice Age was that of 1816. This was in fact the coldest

single year on record in many places in Europe and North America.

The reason being that the previous year a massive volcanic eruption in Indonesia had thrown

vast plumes of smoke and dust into the sky reducing the warming effect of the sun.

cg2003

Alarmingly this could happen again, not with a volcano, but a meteor.

On March 16, 2880 an asteroid 3,300 feet in diameter, weighing over one billion tonnes

could crash into our planet.

It is travelling at a speed around 9.5 miles per second. If it does hit our planet, the

energy released could be as much as 44,800 megatons of TNT. If it impacts on land it will

create a crater around 14 miles in diameter, with a blast radius of intense damage of

around 190 miles. Massive dust clouds would be thrown up and Britain would be plunged into

darkness for years.

This vast dust cloud and the dent in the world's orbit could be enough to swing the balance

and send us into a real ice-age.....

12. THE NEXT ICE-AGE

Graphics of Britain becoming frozen over. New Ice age then carves Britain - familiar

landmarks are obliterated - e.g. Angel of the North, Blackpool tower, Alton towers, The

London Eye, etc.

Graphics of the appropriate sea level changes (from Programme 3).

Polar bears, Arctic foxes, musk ox walking across ice sheet with famous landmarks

sticking out in the background.

Presenter with reindeer in snow

Graphics of mammoths walking through Britain.

If this occurred then Britain would be changed forever. The powerful ice sheets and

glaciers would completely recarve our landscape. Certainly nothing that we have built

would be immune.

And our coastline would change immeasurably since global sea levels would fall and fall.

Once again we would be joined to the continent and new animals would venture onto our

shores.

Reindeer would once again thrive in Britain. Those people brave enough to stay rather than

head south, would probably end up farming them.

And who knows, by then scientists may even have developed the technology to re-create

mammoths from their DNA trapped in frozen specimens. Maybe they too would grace our shores

once again.

13. THE LONG TERM OUTLOOK

Presenter walking along Ilkley Moor on a summer's day

Flash frames of asteroid, tidal wave, female Alan

Short section with some of the most dramatic graphics sequences of mountain building/volcanoes/floods from other programmes.

Short section on the most beautiful landscapes of modern Britain (pinched from other

programmes)

Back to Presenter:

Aerials away

Much of this programme has been speculation - no one can know for sure what the future

holds for Britain. But global warming is a fact, it is happening. What is debatable is

how much of it is down to us and what its effects will be. An asteroid will hit our planet

at some point and massive tidal waves could also strike Britain. Mystery

pollutants could
even turn us all female.
Ilkley Moor is where I grew up and it is also the place I started my journey of
discovery
of the history of the British landscape. In this series, I've learned a great
deal about
our past and the over-riding message is that an examination of our ancient past
tells us
that the planet and this small piece of land that comprises Great Britain always
has and
always will be in a state of constant change. Maybe global warming is simply
the next
change that has arisen sooner because of our activities. Whether we can do
anything about
it remains to be seen since from our incredible history we should remember that
there are
forces infinitely more powerful than ourselves operating on the planet - the
forces of
nature.
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2354. 2003-01-21

date: Tue Jan 21 17:34:22 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: a query ref. Andre D's proposal
to: "Elaine Jones" <E.L.Jones@uea.ac.uk>
Elaine,
OK, you're probably right, but if we can get at the heart of an appropriate idea
for
Tyndall we should be able to i.d. the right internal partner to be the
fundholder.
Could we talk about this and also a couple of other matters on Thursday this
week? (I away
tomorrow). Would 2pm be possible?
Other matters would be:
Plans for ECF conference - I have some ideas
SD3 sustainability in practise event
Cranfield business seminars
DTI annual report and strategy for contract renewal
Thanks,
Mike
At 18:51 17/01/03 +0000, you wrote:
Mike,
Been looking at this but, before speaking to Andrew, and before summarising
it all, I
suspect that he's not eligible for Tyn.funding himself and that the project
would need a
UK University person to lead it ? (I know he has a sort of loose affiliation
to CRU but
wouldn't think that is enough)
before I go any further, what do you think ?
The Carbon Disclosure Project - "we would like to work with the Tyn.Centre to
develop

cg2003

our requests beyond there current level"

could be a v.good collaborator

it has US charitable status.

it's a special project of the Philanthropic Collaborative of Rockefeller

Philanthropy

Advisors in New York

It's chaired by Tessa Tennant (ex-Head of ethical investment research team at

Henderson's; also 4 cottages on the family's Scottish home estate a Solar

Century

project and she won the British Environment and Media Award for Awareness in

2001 for

her work in social investment)

Co-ordinator is a Paul Dickinson (London address)

Elaine

4900. 2003-01-23

cc: <cvy@nerc.ac.uk>, <ppn@nerc.ac.uk>, <cg1@soc.soton.ac.uk>

date: Thu, 23 Jan 2003 14:17:00 +0000

from: "Eric W Wolff" <ewwo@bas.ac.uk>

subject: Re: RAPID modelling subgroup

to: <plemke@awi-bremerhaven.de>, <ewwo@bas.ac.uk>, <r.r.dickson@cefas.co.uk>, <maria.noguer@defra.gsi.gov.uk>, <mccave@esc.cam.ac.uk>, <haugan@gfi.uib.no>, <studhope@glg.ed.ac.uk>, <B.Turrell@marlab.ac.uk>, <rwood@meto.gov.uk>, <sfbtett@meto.gov.uk>, <j.m.slingo@reading.ac.uk>, <p.j.valdes@reading.ac.uk>, <j.lowe@rhubnc.ac.uk>, <jym@soc.soton.ac.uk>, <mas@soc.soton.ac.uk>, <pc@soc.soton.ac.uk>, <a.j.watson@uea.ac.uk>, <k.briffa@uea.ac.uk>, <m.hulme@uea.ac.uk>, <lkeigwin@whoi.edu>

Dear Meric,

Thank you for the message. i am very happy that progress is being made on this, and that some people have agreed to serve on it. However, my impression was that the £0.5 M was for something a bit more than that. I suspect that none (or few) of the people on the group were in the room when we decided about this, so it may be necessary for others to refresh their memories about it. The two tasks you have listed require a sub-group but not a lot of money.

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I think we need:

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- 2) To tell the subgroup whether we want them to commission the work, or (more correctly) recommend to us how the work should be commissioned

Best wishes

Eric

Eric Wolff
British Antarctic Survey
High Cross
Madingley Road
Cambridge CB3 0ET
United Kingdom
E-mail: ewwo@bas.ac.uk
Phone: +44 1223 221491
Fax: +44 1223 221279
Alternate fax: +44 1223 362616

>>> Meric Srokosz <mas@soc.soton.ac.uk> 23/01/03 12:21:49 >>>

cg2003

Dear Steering Committee

One of the decisions made at the last SC meeting was to put aside £0.5M for modelling activities, but there was no time to firm up what this money is to be used for. This e-mail is to try to do that "firming up" (apologies for not getting to this sooner, as a number of you have asked me what is happening).

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It would be helpful to have your response / views by Thursday 6th Feb (2 weeks today), so that I can press on with arranging a subgroup meeting soon and get this aspect of RAPID underway.

Regards, Meric

--

Dr. Meric Srokosz, Room 254/43, Southampton Oceanography Centre (SOC)

Empress Dock, Southampton, SO14 3ZH, UK

Tel: +44-(0)23-80596414 (direct line); Fax: +44-(0)23-80596400

e-mail: mas@soc.soton.ac.uk or M.Srokosz@soc.soton.ac.uk

<http://www.soc.soton.ac.uk/JRD/SAT/pers/mas.html>

Science Coordinator NERC Rapid Climate Change Programme

<http://rapid.nerc.ac.uk/>

1163. 2003-01-24

date: Fri Jan 24 09:28:51 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: Fw: IPCC Exploratory Meeting on Adaptation to Climate Change,

to: "Neil Adger" <N.Adger@uea.ac.uk>

Neil,

Thanks for this.

Of course it is worth noting, and perhaps the point can be made in the meeting itself, that

all of the scientists here - Arnell, Adger, Nicholls, Berkhout, Cannell and Kovats - are

either Tyndall scientists or working for Tyndall, i.e., Tyndall Centre is helping to fund

much of this research activity in the UK. It is certainly a point I will be making to the

research councils in our Annual Report.

The presumably means Martin is not attending our meeting the following day?

Mike

At 15:44 23/01/03 +0000, you wrote:

Mike

FYI.

Neil

----- Original Message -----

From: "Van Der Linden, Paul" <paul.vanderlinden@metoffice.com>

To: "Maskell, Kathy" <kathy.maskell@metoffice.com>; "Harrison, Mike" <mike.harrison@metoffice.com>; "DEFRA - Diana Wilkins"

cg2003

<Diana.Wilkins@defra.gsi.gov.uk>; <nwal@soton.ac.uk>;
 <r.nicholls@mdx.ac.uk>; <n.adger@uea.ac.uk>; <f.berkhout@Sussex.ac.uk>;
 "'lshrm.ac.uk'" <mgrc@ite.ac.uk>sari.kovats>; <chris.west@ukcip.org.uk>
 Cc: "'Parry, Martin'" <parryml@aol.com>
 Sent: Thursday, January 23, 2003 3:41 PM
 Subject: IPCC Exploratory Meeting on Adaptation to Climate Change,
 DEFRA, 29th Jan 14:00 to 17:00
 > Message from Martin Parry:
 >
 > Dear Colleagues:
 >
 > EXPLORATORY MEETING ON ADAPTATION, WEDNESDAY 29th January 2003 at DEFRA,
 > 1400-1700
 >
 > Thank you for agreeing to attend this meeting. The Venue is Room 3A, 3rd
 > Floor, 3/B4 Ashdown House, 123 Victoria Street, London SW1E 6DE. Entrance
 > passes can be collected at the reception area. Please visit
 > [1]<http://streetmap.co.uk> and type in SW1E 6DE for the exact location of
 > Ashdown House.
 >
 > The purpose of the meeting is to consider how best the IPCC, which is now
 > beginning to think about the structure of the Fourth Assessment (AR4),
 > should handle the issue of adaptation to climate change. In particular,
 > there is the need to evaluate adaptive capacity more effectively than was
 > achieved in the TAR, and to consider the limits and costs of adaptation.
 > Ultimately, AR4 would also wish to achieve a more integrated analysis of
 > the mutual roles that adaptation and mitigation can play (including some
 > analysis of their relative costs).
 >
 > This is not a formal IPCC meeting, but one of several informal discussions
 > on different topics, prior to more formal consultations planned for
 > mid-2003.
 >
 > I think we should treat this as a very informal discussion but, to give
 > you
 > an idea of what to think about beforehand, may I suggest the following as
 > a
 > rough agenda (there will be ppt and an overhead available):
 >
 > 1. Introduction. Martin Parry
 >
 > 2. How adaptation was treated in the IPCC Third assessment. Neil Adger
 > (10
 > mins)
 >
 > 3. Next steps (5 mins each participant): i) (in your field) what new
 > knowledge on adaptation can be expected for AR4, from current post-TAR
 > research? ii) what new areas of research should be fostered? iii)
 > measuring
 > adaptive capacity, and its limits (eg with respect to Article 2 of the
 > UNFCCC since, when adaptive limits are exceeded, then climate change
 > becomes
 > more 'dangerous'?) ; iv) measuring the costs of adaptation.
 >
 > 4. Treating adaptation as an issue in AR4. What are the alternative ways
 > of
 > tackling this? Who are the 'new names' in addition to IPCC-known
 > scientists
 > that AR4 should involve? (General discussion).
 >
 > The following will be attending the meeting:
 > Chris West
 > Nigel Arnell

cg2003

> Robert Nicholls
> Neil Adger
> Frans Berkhout
> Sari Kovats
> Melvin Cannell
> Martin Parry
> Mike Harrison
> Paul van der Linden
> Kathy Maskell
> Diana Wilkins
>
> I look forward to seeing you on the 29th.
>
> Yours,
> Martin Parry
>
>
>
> Professor Martin Parry,
> Co-Chair Working Group 2 (Impacts and Adaptation),
> Intergovernmental Panel on Climate Change,
> School of Environmental Sciences,
> University of East Anglia,
> Norwich NR4 7TJ,
> United Kingdom.
> Tel +44 1986 781437
> Fax +44 1986 781437
> e-mail: parrym1@aol.com
> or: martin.parry@uea.ac.uk
>
>
>
>
>
>

2160. 2003-01-24

cc: <cvy@nerc.ac.uk>, <ppn@nerc.ac.uk>, <cg1@soc.soton.ac.uk>
date: Fri, 24 Jan 2003 18:00:02 -0000
from: "Andrew Watson" <a.watson@uea.ac.uk>
subject: Re: RAPID modelling subgroup
to: "Eric W Wolff" <ewwo@bas.ac.uk>, <plemke@awi-bremerhaven.de>,
<r.r.dickson@cefas.co.uk>, <maria.noguer@defra.gsi.gov.uk>, <mccave@esc.cam.ac.uk>,
<haugan@gfi.uib.no>, <studhope@glg.ed.ac.uk>, <B.Turrell@marlab.ac.uk>,
<rwood@meto.gov.uk>, <sfbtett@meto.gov.uk>, <j.m.slingo@reading.ac.uk>,
<p.j.valdes@reading.ac.uk>, <j.lowe@rhbnc.ac.uk>, <jym@soc.soton.ac.uk>,
<mas@soc.soton.ac.uk>, <pc@soc.soton.ac.uk>, <a.j.watson@uea.ac.uk>,
<k.briffa@uea.ac.uk>, <m.hulme@uea.ac.uk>, <lkeigwin@whoi.edu>

Dear Meric

I'd like to support Eric's view. As I remember it, the reason for keeping back the sum of money, and for convening the modelling group, is to do something to rectify the following problem: the programme at the moment lacks any initiative that could help to unify the disparate elements, and get at the "core" of the rapid climate change problem. At the moment, it's not so much a programme, more some interesting research projects that have some loose relevance to one another. This is important to rectify and I don't think that we should wait until the next funding round to get something underway.

Do we want the subgroup to actually commission the work? I would say yes, subject to approval by the rest of the committee of course, but first we should make sure that we are not missing out people who may be able to contribute. I would recommend expanding this group to include members from

cg2003

U. Liverpool, possibly Proudman, UEA (but not me!) and any other centre (Imperial?) that is doing significant modelling work. Then we ask them to design the core modelling programme of RAPID, within the budgetary limit of £0.5m. It's important to have this done, either by representatives of all those groups who might materially contribute, or else by people who are entirely disinterested -- but short of getting them all from outside the UK, that is impossible to do.

Cheers,

Andy

Prof Andrew J. Watson
email: a.watson@uea.ac.uk
or : a.j.watson@uea.ac.uk
phone: (44) 1603 593761 direct
1603 456161 switchboard
1603 507719 fax
School of Environmental Sciences
University of East Anglia
NORWICH NR4 7TJ
U.K.

<http://www.uea.ac.uk/~ajw/ajw.htm>

----- Original Message -----

From: "Eric W Wolff" <ewwo@bas.ac.uk>
To: <plemke@awi-bremerhaven.de>; <ewwo@bas.ac.uk>;
<r.r.dickson@cefas.co.uk>; <maria.noguer@defra.gsi.gov.uk>;
<mccave@esc.cam.ac.uk>; <haugan@gfi.uib.no>; <studhope@glg.ed.ac.uk>;
<B.Turrell@marlab.ac.uk>; <rwood@meto.gov.uk>; <sfbtett@meto.gov.uk>;
<j.m.slingo@reading.ac.uk>; <p.j.valdes@reading.ac.uk>;
<j.lowe@rhbnc.ac.uk>; <jym@soc.soton.ac.uk>; <mas@soc.soton.ac.uk>;
<pc@soc.soton.ac.uk>; <a.j.watson@uea.ac.uk>; <k.briffa@uea.ac.uk>;
<m.hulme@uea.ac.uk>; <lkeigwin@whoi.edu>
Cc: <cvy@nerc.ac.uk>; <ppn@nerc.ac.uk>; <cg1@soc.soton.ac.uk>
Sent: Thursday, January 23, 2003 2:17 PM
Subject: Re: RAPID modelling subgroup

Dear Meric,

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- 1) other SC members to say if I am right, and to refine the area to be filled;
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Best wishes

Eric

Eric Wolff

cg2003

British Antarctic Survey
High Cross
Madingley Road
Cambridge CB3 0ET
United Kingdom

E-mail: ewwo@bas.ac.uk

Phone: +44 1223 221491

Fax: +44 1223 221279

Alternate fax: +44 1223 362616

>>> Meric Srokosz <mas@soc.soton.ac.uk> 23/01/03 12:21:49 >>>

Dear Steering Committee

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£0.5M for modelling activities, but there was no time to firm up what
this money is to be used for. This e-mail is to try to do that "firming
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that the proposed tasks (b above) are the correct ones for the group to
pursue on behalf of the SC and RAPID.

It would be helpful to have your response / views by Thursday 6th Feb
(2 weeks today), so that I can press on with arranging a subgroup meeting
soon and get this aspect of RAPID underway.

Regards, Meric

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Dr. Meric Srokosz, Room 254/43, Southampton Oceanography Centre (SOC)

Empress Dock, Southampton, SO14 3ZH, UK

Tel: +44-(0)23-80596414 (direct line); Fax: +44-(0)23-80596400

e-mail: mas@soc.soton.ac.uk or M.Srokosz@soc.soton.ac.uk

<http://www.soc.soton.ac.uk/JRD/SAT/pers/mas.html>

Science Coordinator NERC Rapid Climate Change Programme

<http://rapid.nerc.ac.uk/>

3606. 2003-01-24

cc: "Paul Wilkinson" <Paul.Wilkinson@lshtm.ac.uk>

date: Fri Jan 24 20:07:33 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: climate change and health; status report from Tyndall

to: "Sari Kovats" <Sari.Kovats@lshtm.ac.uk>

Thanks Sari. Here is amended version, with a few other alterations as suggested
by people.

Please share it with others at LSHTM if appropriate - I only copied it to you
and Paul.

Mike

At 17:41 24/01/03 +0000, Sari Kovats wrote:

Hi Mike

This is very interesting - I have one comment - perhaps you could put more on
the
success of the scientific collaboration between Tyndall and LSHTM -

cg2003

e.g. several inputs

to CCASHH . - and make clear that the joint programme is not going well because the funders do not support interdisciplinary research - to avoid any misinterpretation.

I think two proposals are going into the MRC on Jan 30th -

1) Armstrong et al. Statistical methods for estimating the effect of weather on health from time series data.

2) Wilkinson et al. DECISION-ANALYSIS FRAMEWORK FOR ADAPTING TO FUTURE BURDENS OF

THERMAL EXTREMES IN LOW- & MIDDLE-INCOME COUNTRIES.

best wishes

Sari

Sari Kovats

Centre on Global Change and Health

Dept of Epidemiology and Population Health

London School of Hygiene and Tropical Medicine

Keppel St, London WC1E 7HT

tel: +44 20 7927 2962

fax: +44 20 7580 6897

sari.kovats@lshtm.ac.uk

>>> Mike Hulme <m.hulme@uea.ac.uk> 01/24/03 09:40am >>>

Dear All,

Please read the attached 2-pager.

This was prepared in response to a question from DEFRA about climate change and health research and Tyndall Centre.

I hope I have mentioned the main items, but please correct me/inform me of any oversights. What I haven't included is a section on up-coming proposals arising out of the MRC-NERC Co-op. Perhaps I could add a section about this if I get the raw material.

Many thanks,

Mike

3619. 2003-01-24

cc: ppn@nerc.ac.uk, cvy@nerc.ac.uk, cg1@soc.soton.ac.uk

date: Fri, 24 Jan 2003 18:27:31 +0000

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: RAPID modelling subgroup

to: Meric Srokosz <mas@soc.soton.ac.uk>, lkeigwin@whoi.edu,

plemke@awi-bremerhaven.de, ewwo@bas.ac.uk, r.r.dickson@cefas.co.uk,

maria.noguer@defra.gsi.gov.uk, mccave@esc.cam.ac.uk, haugan@gfi.uib.no,

studhope@glg.ed.ac.uk, B.Turrell@marlab.ac.uk, rwood@meto.gov.uk,

sfbtett@meto.gov.uk, j.m.slingo@reading.ac.uk, p.j.valdes@reading.ac.uk,

j.lowe@rhnbc.ac.uk, jym@soc.soton.ac.uk, pc@soc.soton.ac.uk,

a.j.watson@uea.ac.uk, k.briffa@uea.ac.uk

Dear Meric,

Although I was not present during this earlier discussion, might I now feed in a comment to

this discussion from an "end-user" perspective (as we - Tyndall - have been called!), that

a focused (i.e., delivery-led) modelling activity that helped address one, two or all three

of the IPCC TAR cited deficiencies (as mentioned in my presentation on Wednesday to the

kick-off meeting), namely:

"whether an irreversible collapse in the THC is likely or not,

or at what threshold it might occur

and what the climate implications could be. "

would be a valuable contribution of RAPID to the policy/scenario community, both in the UK

cg2003

and internationally. A number of modelling strategies might help deliver this of course - from full complexity to low complexity models, or mixed-mode modelling - but I think it would be a very useful exercise.

Mike

At 12:21 23/01/03 +0000, Meric Srokosz wrote:

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Regards, Meric

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Empress Dock, Southampton, SO14 3ZH, UK

Tel: +44-(0)23-80596414 (direct line); Fax: +44-(0)23-80596400

e-mail: mas@soc.soton.ac.uk or M.Srokosz@soc.soton.ac.uk

[1]<http://www.soc.soton.ac.uk/JRD/SAT/pers/mas.html>

Science Coordinator NERC Rapid Climate Change Programme

[2]<http://rapid.nerc.ac.uk/>

517. 2003-01-27

cc: Laurent Labeyrie <Laurent.Labeyrie@lsce.cnrs-gif.fr>, Keith Alverson <keith.alverson@pages.unibe.ch>, didier.paillard@lsce.cnrs-gif.fr, Dominique Raynaud <domraynaud@glaciog.obs.ujf-grenoble.fr>, jean jouzel <jouzel@lsce.saclay.cea.fr>, Gerald Ganssen <gang@geo.vu.nl>, Jean Marc Barnola <barnola@glaciog.obs.ujf-grenoble.fr>, Ralph Schneider <rschneid@uni-bremen.de> John.Birks@bot.uib.no p.jones@uea.ac.uk, Reinhard.Boehm@zamg.ac.at, a.moberg@uea.ac.uk, brazdil@porthos.geogr.muni.cz, christian.pfister@hist.unibe.ch, wanner@giub.unibe.ch, camuffo@clima.ictr.pd.it, sigfus@gfy.ku.dk, jouzel@lsce.saclay.cea.fr, njs5@cam.ac.uk, mjeronen@mappi.helsinki.fi, esper@wsl.ch, Dirk.Verschuren@rug.ac.be, atte.korhola@helsinki.fi, uligraf@free.fr, zoli@gfz-potsdam.de, john.Birks@bot.uib.no, ingemar.renberg@eg.umu.se, a.lotter@bio.uu.nl, r.battarbee@ucl.ac.uk, matti.saarnisto@gsf.fi, fmcder@pop3.ucdi.ie, stein.lauritzen@geol.uib.no, karin.holmgren@natgeo.su.se, markku.makila@gsf.fi, vangeel@science.uva.nl, kebarber@pop3.soton.ac.uk, berger@astr.ucl.ac.be,

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goslar@zeus.polsl.gliwice.pl,beer@eawag.ch,
j.haigh@ic.ac.uk,hans.von.storch@gkss.de, renh@geo.vu.nl,stocker@climate.unibe.ch,
sfbtett@meto.gov.uk,p.j.valdes@reading.ac.uk,
pasb@lsce.saclay cea.fr,stephen.juggins@nc1.ac.uk,
guiot@cerege.fr,mdiepenbroek@awi-bremerhaven.de,
joussaume@cea.fr,sharris@bgc-jena.mpg.de,Christoph.Spoetl@uibk.ac.at,gasse@cerege.f
date: Mon Jan 27 15:08:16 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: WORKSHOP INVITATION+FP6
to: Eystein Jansen <Jansen@geol.uib.no>

Eystein

I am not able to come to this - it is during our annual family holiday.

Could someone else do it and I would be happy to help put the evidence
together with
them?

I go into hospital this week for an operation on my back (I have been virtually
immobile

because of a disc problem for months)

and I will also be out of action for a couple of months after.

OF MORE INTEREST TO ALL NOW _

I have talked at length with Hans Brelen last week at a start up meeting of our
EC project

SOAP , and I am now sure that there will only be one opportunity for a large
project that

is specifically Palaeo based . It will be in the next call but one (due for a
start of work

in 1995) and will be a new instrument (ie IP or NoE). Hence it becomes more
essential in

my mind to think of refining HOLCLIM and DOCC , to bring them together. I still
believe the

Holocene is a better focus than the last glacial cycle or longer , and certainly
that an IP

is by far preferable to a NoE. Of course the scale of the work is also vital ,
as 10-12

million Euro looks like a reasonable guess for possible budget , and an
integration of data

and modelling seems ideal. I am now far less convinced that I (or the Unit) is
an

appropriate place to co-ordinate such an initiative , so you and the rest should
also

consider whether Norway is the better candidate (or elsewhere if anyone is
keen).

At 06:59 AM 1/26/03 +0100, you wrote:

Dear Keith,

We would like to invite you to an Images workshop on Holocene climate
variability which

will be held August 27-30 this year in magnificent surroundings in a
spectacular fjord

and glacier landscape in Hafslo in Western Norway, just North of Bergen. The
workshop

is organised by Eystein Jansen and Peter deMenocal on behalf of the Images
Holocene

working group.

An outline of the workshop is given in the attached document. We believe this
workshop

will be a cornerstone in synthesizing the exciting reserach efforts on
Holocene climate

variability being perfomed globally. We hope to have many of the key
scientists of the

field participating in the workshop.

We would like to invite you to give a talk in the workshop session on:

Last 1,000 years +historical (instrumental) data; Little Ice Age, Medieval
Warm Period;

cg2003

trend for future?

We will be able to cover board and lodging and assist in your travel costs.

Please respond as fast as possible by checking the form at the bottom of the enclosed

circular and return it by e-mail before February 15 to:

Charla.Melander@bjerknes.uib.no

Looking forward to hearing from you,

Eystein Jansen

Peter deMenocal

--

Dr. Eystein Jansen
Professor / Director
Bjerknes Centre for Climate Research
Allégaten 55
N-5007 Bergen, Norway
Tel: +47-55583491
Secretary: +47-55589803
[1]<http://www.bjerknes.uib.no>

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

5172. 2003-01-27

cc: Keith Alverson <keith.alverson@pages.unibe.ch>, Ayako Abe-Ouchi <abeouchi@ccrs.u-tokyo.ac.jp>
date: Mon, 27 Jan 2003 10:21:46 -0500
from: Mike MacCracken <mmaccrac@comcast.net>
subject: IUGG Symposium MC10 Invitation
to: robert.mendelsohn@yale.edu, rprinn@mit.edu, socci.tony@epa.gov, sumi@ccsr.u-tokyo.ac.jp, tcrowley@moorcock.acpub.duke.edu, trenbert@ncar.ucar.edu, wcollins@ucar.edu, wmw@ncar.ucar.edu, Akio Kitoh <kitoh@mri-jma.go.jp>, Alan Robock <robock@envsci.rutgers.edu>, Albert Arking <arking@aa.gsfc.nasa.gov>, André Berger <berger@astr.ucl.ac.be>, Andrei Sokolov <sokolov@mit.edu>, Atul Jain <jain@uiatma.atmos.uiuc.edu>, Axel timmermann <atimmermann@ifm.uni-kiel.de>, Ben Santer <santer1@llnl.gov>, Bill Randel <randel@vortex.shm.monash.edu.au>, Bryant McAvaney <Bryant.McAvaney@lmd.jussieu.fr>, Byron Boville <boville@ucar.edu>, CDIAC <cdiac@ornl.gov>, Chris Bretherton <breth@whirlwind.atmos.washington.edu>, Curtis Covey <covey1@llnl.gov>, Dave Pollard <pollard@essc.psu.edu>, David Battisti <david@atmos.washington.edu>, David Karoly <djk@vortex.shm.monash.edu.au>, David Randall <randall@redfish.atmos.colostate.edu>, David Rind <cddhr@giss.nasa.gov>, David Viner <d.viner@uea.ac.uk>, Don Wuebbles <wuebbles@atmos.uiuc.edu>, Eric Barron <eric@essc.psu.edu>, Eugene Rozanov <e.rozanov@pmodwrc.ch>, gary yohe <gyohe@mail.wesleyan.edu>, George Boer <George.Boer@ec.gc.ca>, Gera Stenchikov <gera@envsci.rutgers.edu>, Granger Morgan <gm5d@andrew.cmu.edu>, Hadi Dowlatabadi <hadi@sdri.ubc.ca>, Herve LETREUT <Herve.Letreut@lmd.jussieu.fr>, James Hack <jhack@cgd.ucar.edu>, James Hansen <jhansen@giss.nasa.gov>, James Risbey <ris@alum.mit.edu>, Jean Jouzel <jouzel@obelix.saclay.cea.fr>, Jeong-Woo Kim <jwkim@atmos.yonsei.ac.kr>, Jerry Meehl <meehl@meeker.ucar.edu>, JOEL SCHERAGA <SCHERAGA.JOEL@epa.gov>, Joel Smith <JSmith@stratusconsulting.com>, John Katzenberger <johnk@agci.org>, Jonathan Gregory <jonathan.gregory@metoffice.com>, Jonathan Overpeck <jto@u.arizona.edu>, Joyce Penner <penner@umich.edu>, Juerg Beer <juerg.beer@eawag.ch>, Karol <karol@main.mgo.rssi.ru>, Keith Shine <K.P.Shine@reading.ac.uk>, Ken Caldeira <kcenc@llnl.gov>, Ken Sperber <sperber@space.llnl.gov>, Klaus Keller <kkeller@geosc.psu.edu>, kodera kunihiro <kodera@mri-jma.go.jp>, Konstantin Vinnikov <kostya@metosrv2.umd.edu>, Larry Gates <gates5@llnl.gov>, Laurie Geller <LGeller@nas.edu>, Marty Hoffert <mih1@bellatlantic.net>, Maurice Blackmon <blackmon@ncar.ucar.edu>, Michael Prather <mprather@uci.edu>, Michael Rampino <mrr1@nyu.edu>, Mike Hulme <m.hulme@uea.ac.uk>,

cg2003

Mike MacCracken <mmaccrac@comcast.net>

Dear Colleagues:

This message is to invite you to submit a paper for presentation at the upcoming IUGG Assembly in Sapporo, where there will be a number of very interesting sessions on climate and climate change (further information is available at: <http://www.jamstec.go.jp/jamstec-e/iugg/index.html>). Among these sessions is MC10, which is being convened to consider past examples and future possibilities for rapid or surprising changes--so nonlinearities, exceedances of thresholds, irreversibilities, etc. In convening this symposium, rapid is meant to imply changes that are unusual in their context (could well include an unusual rate of change in the frequency of some weather event as a result of a small change in climate), that result from terms of processes not generally included in GCMs (e.g., rapid glacial changes) , and large changes that seem to result from small climatic shifts (e.g., change of the thermohaline circulation). The prospectus for the symposium is as follows:

MC10 Prospects for and Past Examples of Unexpected Nonlinearities, Thresholds, and Surprises in the Climate System (ICCL)

Although the projections of future changes in climate tend to be gradual, experience has provided a number of examples of relatively large climate changes occurring over relatively short periods of time or that were quite unexpected. The purpose of this symposium is to explore the potential and likelihood for unexpected nonlinearities, thresholds, and surprises occurring as a result of changes in large- to global-scale physical or environmental systems. Examples could include changes in the global thermohaline circulation, shifts in the atmospheric circulation, thresholds that might initiate rapid change of ice sheets or ecosystems, sudden breaching of coastal Barriers, thresholds that might lead to large changes in atmospheric chemistry or biogeochemical cycles, unusual changes in natural forcings, or potential destabilization of methane clathrates. Papers are invited on both analyses of past events and prospects for future such events.

Conveners:

Michael C. MacCracken, 6308 Berkshire Drive, Bethesda MD 20814, United States, tel: +1-301-564-4255, fax: +1-301-564-4255, mmaccrac@comcast.net

Keith Alverson, PAGES International Project Office, Baerenplatz 2, 3011 Bern, Switzerland, tel: +41-31-312 31 33, fax: +41-31-312 31 68, keith.alverson@pages.unibe.ch

Ayako Abe-Ouchi, Center for Climate System Research, The University of Tokyo 4-6-1, Komaba, Meguro-ku, Tokyo, 153- 8904, Japan, tel: +81-3-5453-3955, fax. +81-3-5453-3964, email: abeouchi@ccsr.u-tokyo.ac.jp

The Symposium is to be held Wednesday and Thursday of the first week of the IUGG Congress (so July 2-3). Abstracts need to be submitted electronically by January 30. Please pass along this invitation to colleagues whom you think might be interested.

Mike MacCracken

3093. 2003-01-28

cc: Tim Osborne <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Irina Fast <f14@zedat.fu-berlin.de>, Scott Rutherford <srutherford@gso.uri.edu>, mann@virginia.edu

date: Tue, 28 Jan 2003 12:33:35 -0500

from: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

subject: Re: multiproxy

to: Ulrich Cubasch <cubasch@zedat.fu-berlin.de>

Dear Ulrich,

That's fine--you can go ahead and use it. But I have to issue a number of caveats first.

This is a version we gave Tim Osborne when he was visiting here, and since Tim hasn't used

it, and we haven't compared results from that code w/ our published results, I can't vouch

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for it--it may or may not be the exact same version we ultimately used, and it may or may

not run properly on platforms other than the one I was using (Sun running ultrix). Scott

Rutherford (whom I've cc'd on this email) has worked with the code more frequently.

The code is not very user friendly unfortunately. For example, the determination of the

optimal subset of PCs to retain is based on application of the criterion described in our

paper, which involves running the code many times w/ different choices. So the "iterative"

process has to be performed by brute force.

The method, as outlined, is quite straightforward and others have implemented it themselves. So you might prefer to code it yourself. That would be my

suggestion. But you are, of course, free to use our code.

That having been said, we have essentially abandoned that method now in favor of a

somewhat more sophisticated version of the approach, which makes use of the RegEM method

for imputing missing values of a field described by Schneider (J. Climate, 2000).

Some initial results are described here:

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', Geophysical

Research Letters, 29 (10), 1501, doi: 10.1029/2001GL014554

[1][ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.\[2\].pdf](ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.[2].pdf)

and in a paper in press in Journal of Climate.

Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., The Performance of Covariance-Based Methods of Climate Field Reconstruction Under Stationary and Nonstationary

Forcing, J. Climate, in press, 2003.

(I don't have the preprint--Scott Rutherford can provide you with one however).

In our view, this is a preferable approach on a number of levels, though the results

obtained are generally quite similar.

I will be in Nice, and looking forward to seeing you there,

Mike

At 04:59 PM 1/28/03 +0100, Ulrich Cubasch wrote:

Dear Michael,

as you might know we (Briffa, Wanner, v. Storch, Tett ...) have an

European project called SOAP,

which aims at combining multy proxy and model data.

more under [3]<http://www.cru.uea.ac.uk/cru/projects/soap>

In the workpackage I am coordinating we would like to use your

multi-proxy program for some

temperature reconstructions. The colleagues in Norwich have got your

program already, but I would like

to implement it here in Berlin. I therefore would like to ask you if you

can grant me the permission to use it.

I will probably copy it then from Keith and Tim directly.

I will keep you informed about the results we obtain with it.

regards

Ulrich Cubasch

P. S.

Are you coming to Nice?

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

cg2003

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[4]<http://www.evsc.virginia.edu/faculty/people/mann.>[5]shtml

4183. 2003-01-30

date: Thu, 30 Jan 2003 15:44 -0500
from: GRLOnline@agu.org
subject: 2002GL016772 Request to Review from Geophysical Research Letters
to: m.hulme@uea.ac.uk

Dear Dr. Hulme:

would you be willing and available to review "Vegetation index trends for the African Sahel 1982-1999" by Lars Eklundh, Lennart Olsson, submitted for possible publication in the Geophysical Research Letters.

The manuscript's abstract is:

Pathfinder AVHRR NDVI data have been analyzed for the African Sahel to study recent trends in vegetation greenness. A strong increase in seasonal NDVI was observed over large areas in the Sahel during the period 1982-1999. The increase is interpreted as a vegetation recovery from the drought periods of the 1980's. Although strong shifts in satellite overpass times have led to shifting solar zenith angles (SZA) over the time period, only minimal influence of SZA's on the Pathfinder NDVI was found in our data. A preliminary analysis of rainfall data indicates increasing rainfall during the period. The observed trends may have important implications to the Sahel including changes to the water cycle, energy exchange and carbon storage.

If you agree to review this manuscript, I would ask for your comments within 14 days from your acceptance.

To ACCEPT, click on the link below:

<<http://grl-submit.agu.org/cgi-bin/main.plex?el=A6K7VaD7A4mRc1D4A9oK3yLMUD0APPA3Qux7KnjgZ>>

If you are unable to review this manuscript at this time, I would appreciate any suggestions of other potential reviewers who would be qualified to examine this manuscript. (via reply e-mail.)

To DECLINE, click on the link below:

<<http://grl-submit.agu.org/cgi-bin/main.plex?el=A2K7VaD2A6mRc6E7A9oK3yLMUD0APPA3Qux7KnjgZ>>

If you have any questions or need more information feel free to reply to this e-mail.

Thank you for your consideration and support of Geophysical Research Letters.

Sincerely,

James Famiglietti

Associate Editor

Geophysical Research Letters

1490. 2003-01-31

date: Fri Jan 31 09:45:01 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Fwd: Re: Scenario data
to: j.turnpenny

John,

Here are the data for the plot. The format seems quite simple and I think he has even

calculated a rate of change.

Can you show me what you have before the end of today, because it has to be finished on

Monday for my talk on Tuesday.

I will need it eventually as a gif or jpeg for my powerpoint, but hard copy at the draft

stage please.

Thanks,

cg2003

Mike

Sender: andrey@pik-potsdam.de
Date: Thu, 30 Jan 2003 16:02:03 +0100
From: "Dr. A. Ganopolski" <andrey@pik-potsdam.de>
Organization: pik
X-Mailer: Mozilla 4.7 [en] (X11; I; AIX 4.3)
X-Accept-Language: en
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: Scenario data

Dear Mike,

At last I have the data which I promised you. They are in ascii format in attached file. If you have problem to use them, I also can prepare a graph. In this case please let me know in which format you would like to have the figure (postscript, tiff, etc.). The data are the result of simulations with the current version of CLIMBER-2, which evolved considerably compared to the version which was used in Rahmstorf and Ganopolski, 1999. Results, nevertheless, are very similar. The cooling starts almost at the same time, and amplitude is almost the same. The only difference is that with the old model version we have one strong kink immediately after beginning of cooling, and in new one there are several smaller kinks, thus the averaged cooling trend over 22th century is smoother. The reason is that in the old version latitudinal resolution of the ocean was 10 deg, while in the new one the resolution of the ocean is four times high (2.5 deg). The file (trate.dat) contains winter temperature changes (compared to equilibrium preindustrial values) and the rate of winter temperature changes over the Atlantic sector from 50N to 60 N (i.e. the same as in RG, 1999). The data is organized as following:

first column: year

second column: temperature anomaly in the standard experiment (exp. 0 in RG, 1999)

third column: temperature anomaly in the experiment with enhanced hydrological sensitivity (exp. 0.2 in RG, 1999)

forth column: the rate of temperature changes (deg per decade) in the standard experiment

fifth column: the rate of temperature changes (deg per decade) in the experiment with enhanced hydrological sensitivity

Because time derivative of the temperature is rather noisy, the rate of temperature changes was smoothed using 10-years moving window.

With best wishes,

Andrey.

> Dear Andrey,

>

> Thank you for looking out for these data.

>

> If you can readily re-run the model I would much appreciate that. The immediate purpose for the data is the paper I have prepared for the Royal Society meeting on 4-5 Feb. at which you also are speaking.

>

> Although I plan to submit my paper today, if you can forward me the relevant data then I at least can prepare the diagram I need for the meeting itself, and then also in the final published version of the paper.

>

> So any data received up until Friday 31 January would in fact be helpful, or even if later than that at least for the final version of the paper.

>

> Many thanks,

>

> Mike

>

> At 17:23 23/01/03 +0100, you wrote:

> >Dear Prof Hulme,

> >

> >Stefan Rahmstorf forwarded me your email with the request of our

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> >scenario data, published in CC, 1998. Unfortunately I failed to find a
> >row data from theses experiments. Probably I've deleted them a while
> >ago, since we have, although large, but still limited disk space. The
> >same happened with the model version and experimental set-up used for
> >this publication. Although it is not a big problem to repeat these
> >experiments with the new version of our model, I am afraid I will be
> >able to do that not early then next week, and as I understand from your
> >letter, you need these results not later than tomorrow. Nevertheless,
> >if you are still interesting to get these results later, I will be happy
> >to provide you with them as soon as possible.

> >

> >Best regards,

> >

> >Andrey Ganopolski.

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1846	.0025	.0056	.0057	.0047
1847	.0055	.0037	.0059	.0059
1848	.0037	.0068	.0059	.0052
1849	.0067	.0061	.0048	.0050

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1850	.0045	.0056	.0042	.0032
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1853	.0082	.0076	.0026	.0010
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1860	.0092	.0148	.0077	.0032
1861	.0171	.0074	.0094	.0037
1862	.0072	.0129	.0104	.0040
1863	.0150	.0086	.0103	.0039
1864	.0060	.0099	.0088	.0043
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1866	.0168	.0074	.0075	.0049
1867	.0197	.0101	.0073	.0048
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1870	.0161	.0098	.0083	.0063
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1875	.0303	.0153	.0087	.0092
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1877	.0246	.0179	.0097	.0105
1878	.0237	.0190	.0103	.0109
1879	.0260	.0203	.0097	.0116
1880	.0285	.0215	.0097	.0124
1881	.0272	.0227	.0089	.0129
1882	.0297	.0240	.0089	.0132
1883	.0319	.0253	.0099	.0134
1884	.0292	.0260	.0120	.0135
1885	.0322	.0279	.0133	.0136
1886	.0314	.0271	.0146	.0133
1887	.0359	.0294	.0153	.0134
1888	.0343	.0295	.0153	.0137
1889	.0381	.0341	.0154	.0142
1890	.0378	.0340	.0158	.0150
1891	.0405	.0373	.0167	.0165
1892	.0420	.0401	.0183	.0176
1893	.0469	.0416	.0200	.0190
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1901	.0574	.0524	.0238	.0190
1902	.0623	.0553	.0241	.0193
1903	.0663	.0608	.0246	.0199
1904	.0705	.0651	.0248	.0207
1905	.0761	.0684	.0267	.0212
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1910	.0845	.0732	.0375	.0211
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1912	.0906	.0762	.0381	.0187

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1913	.0925	.0803	.0364	.0175
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1915	.1010	.0847	.0340	.0172
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1919	.1227	.0926	.0380	.0195
1920	.1335	.0949	.0385	.0195
1921	.1351	.0966	.0392	.0193
1922	.1425	.0988	.0390	.0190
1923	.1431	.0918	.0391	.0187
1924	.1490	.1023	.0393	.0183
1925	.1439	.0999	.0391	.0184
1926	.1403	.1022	.0377	.0189
1927	.1430	.1044	.0368	.0196
1928	.1520	.1068	.0347	.0203
1929	.1585	.1094	.0321	.0209
1930	.1603	.1121	.0301	.0225
1931	.1661	.1139	.0288	.0230
1932	.1684	.1172	.0281	.0238
1933	.1698	.1165	.0291	.0245
1934	.1794	.1188	.0302	.0251
1935	.1754	.1207	.0309	.0246
1936	.1853	.1233	.0312	.0249
1937	.1863	.1260	.0314	.0246
1938	.1944	.1280	.0302	.0244
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1940	.1989	.1374	.0298	.0246
1941	.1939	.1400	.0299	.0251
1942	.1969	.1429	.0305	.0256
1943	.1977	.1455	.0306	.0260
1944	.2036	.1484	.0307	.0265
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1946	.2087	.1530	.0309	.0268
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1949	.2159	.1578	.0332	.0274
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1951	.2224	.1627	.0359	.0287
1952	.2283	.1657	.0381	.0298
1953	.2336	.1689	.0391	.0310
1954	.2376	.1722	.0422	.0324
1955	.2417	.1736	.0459	.0334
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1957	.2472	.1791	.0528	.0368
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1960	.2614	.1913	.0589	.0437
1961	.2665	.1953	.0600	.0460
1962	.2704	.1994	.0607	.0470
1963	.2726	.2053	.0619	.0475
1964	.2786	.2097	.0637	.0487
1965	.2872	.2156	.0659	.0491
1966	.2917	.2200	.0689	.0503
1967	.3131	.2247	.0721	.0535
1968	.3255	.2256	.0756	.0572
1969	.3274	.2349	.0793	.0601
1970	.3376	.2436	.0833	.0639
1971	.3425	.2517	.0866	.0658
1972	.3486	.2582	.0901	.0667
1973	.3539	.2640	.0935	.0673
1974	.3604	.2724	.0951	.0679
1975	.3658	.2670	.0958	.0688

				cg2003
1976	.3754	.2704	.0971	.0704
1977	.3856	.2833	.0981	.0721
1978	.3928	.2827	.0989	.0736
1979	.4103	.3001	.1012	.0752
1980	.4178	.3164	.1044	.0764
1981	.4331	.3256	.1072	.0774
1982	.4456	.3331	.1109	.0795
1983	.4546	.3421	.1146	.0824
1984	.4644	.3421	.1194	.0844
1985	.4783	.3493	.1277	.0869
1986	.4924	.3561	.1370	.0891
1987	.5045	.3643	.1470	.0898
1988	.5149	.3722	.1571	.0899
1989	.5260	.3831	.1659	.0904
1990	.5385	.3945	.1714	.0903
1991	.5509	.4021	.1761	.0921
1992	.5636	.4141	.1793	.0943
1993	.5748	.4230	.1820	.0974
1994	.5872	.4297	.1850	.1010
1995	.6042	.4395	.1879	.1049
1996	.6206	.4495	.1909	.1081
1997	.6485	.4593	.1943	.1108
1998	.6964	.4672	.1984	.1134
1999	.7304	.4792	.2027	.1156
2000	.7557	.4961	.2072	.1177
2001	.7794	.5051	.2111	.1191
2002	.7964	.5139	.2147	.1201
2003	.8137	.5264	.2180	.1207
2004	.8308	.5383	.2199	.1212
2005	.8483	.5517	.2182	.1218
2006	.8663	.5664	.2150	.1239
2007	.8848	.5811	.2106	.1258
2008	.9028	.5956	.2056	.1284
2009	.9224	.6107	.2014	.1318
2010	.9432	.6248	.2001	.1350
2011	.9656	.6382	.2002	.1367
2012	.9868	.6529	.2010	.1393
2013	1.0076	.6674	.2022	.1419
2014	1.0223	.6715	.2035	.1442
2015	1.0457	.6794	.2048	.1466
2016	1.0682	.6920	.2062	.1497
2017	1.0906	.7029	.2075	.1530
2018	1.1119	.7169	.2083	.1561
2019	1.1331	.7420	.2088	.1591
2020	1.1542	.7556	.2089	.1621
2021	1.1768	.7733	.2096	.1659
2022	1.1960	.7916	.2099	.1702
2023	1.2162	.8085	.2103	.1747
2024	1.2344	.8183	.2109	.1798
2025	1.2549	.8365	.2114	.1851
2026	1.2761	.8608	.2112	.1887
2027	1.2971	.8817	.2115	.1921
2028	1.3187	.9019	.2124	.1947
2029	1.3402	.9219	.2135	.1962
2030	1.3625	.9425	.2150	.1979
2031	1.3832	.9633	.2168	.2013
2032	1.4039	.9834	.2193	.2035
2033	1.4250	1.0037	.2214	.2054
2034	1.4469	1.0207	.2241	.2075
2035	1.4678	1.0405	.2272	.2083
2036	1.4905	1.0618	.2304	.2086
2037	1.5130	1.0848	.2332	.2100
2038	1.5341	1.1059	.2373	.2121

				cg2003
2039	1.5566	1.1277	.2406	.2137
2040	1.5786	1.1504	.2447	.2159
2041	1.6082	1.1690	.2489	.2181
2042	1.6298	1.1880	.2544	.2198
2043	1.6525	1.2147	.2593	.2212
2044	1.6768	1.2379	.2645	.2230
2045	1.7036	1.2538	.2689	.2239
2046	1.7291	1.2755	.2732	.2249
2047	1.7581	1.2986	.2766	.2256
2048	1.7861	1.3168	.2788	.2267
2049	1.8143	1.3441	.2814	.2281
2050	1.8391	1.3737	.2842	.2297
2051	1.8771	1.3988	.2870	.2307
2052	1.8983	1.4161	.2890	.2322
2053	1.9336	1.4413	.2917	.2336
2054	1.9626	1.4645	.2937	.2346
2055	1.9993	1.4886	.2950	.2360
2056	2.0330	1.5114	.2959	.2373
2057	2.0598	1.5360	.2971	.2372
2058	2.0868	1.5527	.2974	.2365
2059	2.1153	1.5810	.2983	.2363
2060	2.1443	1.6058	.2984	.2357
2061	2.1727	1.6296	.2983	.2357
2062	2.2026	1.6531	.2971	.2360
2063	2.2327	1.6777	.2957	.2367
2064	2.2633	1.7004	.2938	.2362
2065	2.2900	1.7244	.2929	.2365
2066	2.3204	1.7501	.2917	.2359
2067	2.3504	1.7742	.2913	.2350
2068	2.3798	1.7939	.2918	.2341
2069	2.4092	1.8191	.2924	.2325
2070	2.4383	1.8434	.2927	.2299
2071	2.4714	1.8660	.2938	.2273
2072	2.4993	1.8896	.2952	.2247
2073	2.5281	1.9128	.2965	.2219
2074	2.5566	1.9394	.2974	.2172
2075	2.5865	1.9618	.2985	.2129
2076	2.6130	1.9848	.2989	.2081
2077	2.6420	2.0047	.2991	.2031
2078	2.6727	2.0270	.2983	.1984
2079	2.6971	2.0498	.2978	.1963
2080	2.7271	2.0709	.2973	.1932
2081	2.7581	2.0936	.2971	.1895
2082	2.7910	2.1059	.2968	.1846
2083	2.8210	2.1255	.2984	.1799
2084	2.8561	2.1435	.2998	.1756
2085	2.8873	2.1639	.3012	.1725
2086	2.9181	2.1854	.3020	.1709
2087	2.9487	2.1808	.3027	.1717
2088	2.9783	2.1982	.3023	.1724
2089	3.0068	2.2145	.3012	.1737
2090	3.0371	2.2332	.3000	.1740
2091	3.0612	2.2538	.2987	.1746
2092	3.0923	2.2754	.2972	.1747
2093	3.1215	2.2860	.2961	.1747
2094	3.1522	2.2977	.2956	.1764
2095	3.1827	2.3028	.2953	.1790
2096	3.2185	2.3254	.2956	.1808
2097	3.2494	2.3474	.2958	.1824
2098	3.2805	2.3697	.2964	.1844
2099	3.3000	2.3923	.2969	.1838
2100	3.3307	2.4196	.2970	.1842
2101	3.3591	2.4411	.2966	.1860

				cg2003
2102	3.3875	2.4614	.2957	.1895
2103	3.4170	2.4709	.2932	.1925
2104	3.4456	2.4922	.2900	.1949
2105	3.4764	2.5140	.2862	.1962
2106	3.5077	2.5326	.2833	.1957
2107	3.5403	2.5534	.2802	.1926
2108	3.5717	2.5701	.2776	.1891
2109	3.5989	2.5811	.2750	.1856
2110	3.6287	2.5992	.2724	.1825
2111	3.6568	2.6207	.2688	.1784
2112	3.6889	2.6426	.2650	.1656
2113	3.7155	2.6612	.2610	.1527
2114	3.7408	2.6826	.2563	.1387
2115	3.7653	2.7039	.2512	.1239
2116	3.7892	2.7217	.2459	.1101
2117	3.8130	2.7391	.2403	.1041
2118	3.8365	2.7572	.2347	.0961
2119	3.8598	2.7726	.2289	.0865
2120	3.8821	2.7893	.2235	.0759
2121	3.9042	2.8030	.2181	.0627
2122	3.9254	2.8183	.2132	.0476
2123	3.9470	2.8268	.2085	.0328
2124	3.9691	2.8314	.2043	.0181
2125	3.9901	2.7559	.2002	.0028
2126	4.0123	2.7654	.1962	-.0131
2127	4.0316	2.7706	.1920	-.0280
2128	4.0508	2.7780	.1879	-.0424
2129	4.0690	2.7817	.1837	-.0559
2130	4.0868	2.7816	.1795	-.0769
2131	4.1048	2.7731	.1752	-.0958
2132	4.1228	2.7645	.1706	-.1061
2133	4.1403	2.7628	.1659	-.1161
2134	4.1573	2.7514	.1611	-.1511
2135	4.1740	2.7347	.1563	-.1783
2136	4.1898	2.7267	.1517	-.2073
2137	4.2055	2.7144	.1472	-.2443
2138	4.2206	2.7056	.1429	-.2837
2139	4.2356	2.6818	.1384	-.2977
2140	4.2496	2.6716	.1340	-.3123
2141	4.2632	2.6643	.1293	-.3245
2142	4.2763	2.6578	.1244	-.3353
2143	4.2892	2.5652	.1195	-.3425
2144	4.3011	2.5516	.1148	-.3574
2145	4.3123	2.5356	.1102	-.3718
2146	4.3237	2.5265	.1060	-.4123
2147	4.3347	2.2602	.1021	-.4568
2148	4.3452	2.2443	.0981	-.5045
2149	4.3550	2.2126	.0946	-.5471
2150	4.3640	2.1910	.0911	-.5814
2151	4.3730	2.1398	.0880	-.5897
2152	4.3813	2.1141	.0852	-.5941
2153	4.3904	2.0831	.0827	-.5957
2154	4.3958	2.0600	.0801	-.5757
2155	4.4024	2.0366	.0777	-.5616
2156	4.4095	2.0209	.0751	-.5467
2157	4.4165	1.9155	.0725	-.5605
2158	4.4247	1.8820	.0698	-.5701
2159	4.4322	1.5847	.0671	-.5993
2160	4.4387	1.5287	.0642	-.6279
2161	4.4432	1.4817	.0617	-.6616
2162	4.4526	1.4329	.0592	-.6668
2163	4.4579	1.3986	.0567	-.6755
2164	4.4669	1.3720	.0542	-.6740

cg2003

2165	4.4732	1.3484	.0516	-.6724
2166	4.4792	1.3274	.0487	-.6372
2167	4.4850	1.2356	.0456	-.5971
2168	4.4901	1.2184	.0427	-.5537
2169	4.4948	1.1822	.0394	-.5151
2170	4.4982	.8663	.0362	-.4753
2171	4.5014	.8448	.0328	-.4625
2172	4.5046	.7974	.0293	-.4529
2173	4.5072	.7708	.0258	-.4730
2174	4.5098	.6926	.0224	-.4890
2175	4.5121	.6599	.0190	-.5067
2176	4.5127	.6392	.0160	-.5234
2177	4.5146	.6142	.0133	-.5108
2178	4.5160	.5850	.0109	-.4700
2179	4.5168	.5693	.0090	-.4359
2180	4.5171	.5529	.0074	-.4013
2181	4.5177	.5337	.0057	-.3668
2182	4.5176	.5177	.0038	-.3621
2183	4.5175	.5004	.0019	-.3578
2184	4.5171	.4839	-.0001	-.3530
2185	4.5170	.4689	-.0021	-.3483
2186	4.5186	.1789	-.0038	-.3450
2187	4.5160	.1603	-.0060	-.3424
2188	4.5152	.1429	-.0086	-.3404
2189	4.5151	.1267	-.0113	-.3391
2190	4.5153	.1129	-.0140	-.3380
2191	4.5167	.1047	-.0168	-.3388
2192	4.5168	.0630	-.0188	-.3395
2193	4.5161	.0414	-.0207	-.3128
2194	4.5129	-.0181	-.0223	-.2860
2195	4.5105	-.0463	-.0240	-.2594
2196	4.5088	-.0580	-.0258	-.2310
2197	4.5070	-.0726	-.0277	-.2029
2198	4.5054	-.0969	-.0297	-.2032
2199	4.5029	-.1091	-.0319	-.2012
2200	4.4933	-.1268	-.0341	-.1986
2201	4.4871	-.1438	-.0360	-.1913
2202	4.4832	-.1559	-.0376	-.1826
2203	4.4797	-.1702	-.0391	-.1734
2204	4.4761	-.2032	-.0405	-.1671
2205	4.4730	-.2172	-.0423	-.1606
2206	4.4691	-.2316	-.0440	-.1600
2207	4.4655	-.2457	-.0450	-.1618
2208	4.4605	-.2612	-.0453	-.1653
2209	4.4564	-.2768	-.0454	-.1702
2210	4.4514	-.2926	-.0450	-.1762
2211	4.4476	-.3087	-.0444	-.1788
2212	4.4438	-.3229	-.0446	-.1812
2213	4.4398	-.3365	-.0454	-.1812
2214	4.4352	-.3487	-.0462	-.1789
2215	4.4304	-.3647	-.0470	-.1771
2216	4.4251	-.3793	-.0479	-.1782
2217	4.4195	-.3956	-.0486	-.1769
2218	4.4116	-.4094	-.0491	-.1780
2219	4.4078	-.4336	-.0494	-.1802
2220	4.4039	-.4636	-.0498	-.1805
2221	4.4001	-.4998	-.0502	-.1808
2222	4.3949	-.5282	-.0506	-.1826
2223	4.3899	-.5423	-.0508	-.1818
2224	4.3865	-.5543	-.0509	-.1814
2225	4.3815	-.5781	-.0506	-.1843
2226	4.3745	-.5875	-.0505	-.1835
2227	4.3685	-.5948	-.0504	-.1816

cg2003

2228	4.3631	-.6149	-.0505	-.1798
2229	4.3580	-.6389	-.0507	-.1750
2230	4.3530	-.6412	-.0513	-.1692
2231	4.3480	-.6751	-.0519	-.1658
2232	4.3433	-.6944	-.0525	-.1628
2233	4.3383	-.6925	-.0527	-.1610
2234	4.3333	-.7140	-.0529	-.1608
2235	4.3282	-.7309	-.0529	-.1615
2236	4.3232	-.7379	-.0527	-.1607
2237	4.3181	-.7637	-.0525	-.1623
2238	4.3128	-.7943	-.0526	-.1636
2239	4.3076	-.7908	-.0530	-.1639
2240	4.3026	-.8111	-.0535	-.1647
2241	4.2968	-.8402	-.0539	-.1673
2242	4.2910	-.8487	-.0544	-.1678
2243	4.2858	-.8685	-.0548	-.1687
2244	4.2802	-.8779	-.0550	-.1702
2245	4.2742	-.8960	-.0551	-.1671
2246	4.2686	-.9094	-.0551	-.1639
2247	4.2634	-.9131	-.0553	-.1603
2248	4.2590	-.9479	-.0555	-.1520
2249	4.2531	-.9549	-.0557	-.1444
2250	4.2475	-.9754	-.0561	-.1381
2251	4.2413	-1.0093	-.0563	-.1319
2252	4.2347	-1.0156	-.0564	-.1281
2253	4.2291	-1.0345	-.0564	-.1305
2254	4.2239	-1.0558	-.0565	-.1356
2255	4.2181	-1.0700	-.0566	-.1400
2256	4.2125	-1.0803	-.0568	-.1454
2257	4.2072	-1.1001	-.0570	-.1481
2258	4.2033	-1.1050	-.0572	-.1470
2259	4.1973	-1.1011	-.0574	-.1439
2260	4.1903	-1.1145	-.0576	-.1417
2261	4.1842	-1.0996	-.0579	-.1374
2262	4.1785	-1.1086	-.0583	-.1336
2263	4.1729	-1.1165	-.0587	-.1319
2264	4.1672	-1.1259	-.0592	-.1285
2265	4.1616	-1.1617	-.0598	-.1268
2266	4.1555	-1.1930	-.0603	-.1278
2267	4.1500	-1.2234	-.0606	-.1287
2268	4.1447	-1.2404	-.0616	-.1318
2269	4.1386	-1.2565	-.0618	-.1374
2270	4.1328	-1.2687	-.0631	-.1426
2271	4.1260	-1.2811	-.0634	-.1469
2272	4.1192	-1.2958	-.0649	-.1489
2273	4.1129	-1.3044	-.0659	-.1471
2274	4.1064	-1.3145	-.0675	-.1414
2275	4.0998	-1.3249	-.0678	-.1353
2276	4.0931	-1.3355	-.0690	-.1280
2277	4.0868	-1.3465	-.0691	-.1236
2278	4.0807	-1.3578	-.0692	-.1206
2279	4.0750	-1.3693	-.0692	-.1201
2280	4.0691	-1.3788	-.0693	-.1198
2281	4.0545	-1.3860	-.0693	-.1205
2282	4.0560	-1.4110	-.0695	-.1208
2283	4.0380	-1.4216	-.0694	-.1221
2284	4.0416	-1.4362	-.0694	-.1235
2285	4.0250	-1.4464	-.0694	-.1250
2286	4.0161	-1.4617	-.0695	-.1260
2287	4.0113	-1.4689	-.0696	-.1274
2288	4.0065	-1.4838	-.0688	-.1289
2289	4.0009	-1.4938	-.0688	-.1291
2290	3.9952	-1.5096	-.0675	-.1286

cg2003

2291	3.9861	-1.5203	-.0671	-.1278
2292	3.9804	-1.5359	-.0654	-.1265
2293	3.9743	-1.5451	-.0642	-.1261
2294	3.9683	-1.5587	-.0625	-.1270
2295	3.9600	-1.5691	-.0620	-.1285
2296	3.9540	-1.5877	-.0607	-.1307
2297	3.9487	-1.6006	-.0606	-.1327
2298	3.9421	-1.6136	-.0605	-.1333
2299	3.9353	-1.6229	-.0603	-.1341
2300	3.9290	-1.6375	-.0599	-.1353
2301	3.9233	-1.6528	-.0593	-.1368
2302	3.9177	-1.6672	-.0585	-.1390
2303	3.9120	-1.6727	-.0579	-.1404
2304	3.9065	-1.6819	-.0576	-.1415
2305	3.9019	-1.6917	-.0573	-.1426
2306	3.8962	-1.7240	-.0572	-.1430
2307	3.8907	-1.7344	-.0572	-.1422
2308	3.8849	-1.7495	-.0571	-.1409
2309	3.8795	-1.7621	-.0570	-.1387
2310	3.8732	-1.7743	-.0569	-.1378
2311	3.8672	-1.7893	-.0569	-.1382
2312	3.8619	-1.8092	-.0560	-.1399
2313	3.8563	-1.8226	-.0551	-.1401
2314	3.8527	-1.8419	-.0543	-.1408
2315	3.8464	-1.8564	-.0535	-.1385
2316	3.8403	-1.8704	-.0527	-.1343
2317	3.8341	-1.8846	-.0529	-.1292
2318	3.8268	-1.9020	-.0531	-.1245
2319	3.8210	-1.9102	-.0533	-.1160
2320	3.8153	-1.9168	-.0535	-.1080
2321	3.8100	-1.9228	-.0538	-.0998
2322	3.8040	-1.9287	-.0541	-.0917
2323	3.7982	-1.9525	-.0542	-.0852
2324	3.7921	-1.9737	-.0548	-.0844
2325	3.7975	-1.9877	-.0550	-.0844
2326	3.7913	-1.9957	-.0551	-.0860
2327	3.7853	-2.0028	-.0553	-.0883
2328	3.7793	-2.0065	-.0557	-.0916
2329	3.7728	-2.0125	-.0555	-.0923
2330	3.7667	-2.0195	-.0557	-.0917
2331	3.7603	-2.0136	-.0558	-.0896
2332	3.7544	-1.9929	-.0570	-.0866
2333	3.7488	-1.9991	-.0581	-.0829
2334	3.7431	-2.0099	-.0591	-.0806
2335	3.7372	-2.0207	-.0602	-.0798
2336	3.7318	-2.0299	-.0614	-.0802
2337	3.7213	-2.0598	-.0614	-.0813
2338	3.7165	-2.0791	-.0612	-.0840
2339	3.7106	-2.0938	-.0610	-.0895
2340	3.7041	-2.1041	-.0609	-.0947
2341	3.6983	-2.1160	-.0607	-.0996
2342	3.6926	-2.1102	-.0606	-.1042
2343	3.6865	-2.1243	-.0607	-.1076
2344	3.6805	-2.1357	-.0605	-.1064
2345	3.6744	-2.1449	-.0613	-.1019
2346	3.6685	-2.1525	-.0619	-.0977
2347	3.6630	-2.1607	-.0624	-.0940
2348	3.6569	-2.1707	-.0629	-.0892
2349	3.6492	-2.1787	-.0638	-.0866
2350	3.6442	-2.1875	-.0635	-.0881
2351	3.6391	-2.1967	-.0634	-.0893
2352	3.6339	-2.2061	-.0636	-.0898
2353	3.6273	-2.2151	-.0638	-.0926

cg2003

2354	3.6216	-2.2254	-.0639	-.0954
2355	3.6159	-2.2349	-.0641	-.0962
2356	3.6093	-2.2469	-.0642	-.0976
2357	3.6029	-2.2611	-.0642	-.1000
2358	3.5875	-2.2499	-.0642	-.0992
2359	3.5827	-2.2674	-.0643	-.0989
2360	3.5775	-2.2812	-.0642	-.1005
2361	3.5714	-2.2854	-.0642	-.1000
2362	3.5651	-2.2856	-.0641	-.0998
2363	3.5600	-2.3099	-.0638	-.1018
2364	3.5538	-2.3214	-.0634	-.1010
2365	3.5458	-2.3266	-.0622	-.1019
2366	3.5398	-2.3506	-.0611	-.1043
2367	3.5342	-2.3641	-.0600	-.1042
2368	3.5281	-2.3635	-.0590	-.1057
2369	3.5219	-2.3788	-.0580	-.1088
2370	3.5160	-2.3934	-.0581	-.1085
2371	3.5100	-2.3865	-.0581	-.1067
2372	3.5045	-2.4061	-.0579	-.1083
2373	3.4998	-2.4240	-.0576	-.1055
2374	3.4941	-2.4211	-.0578	-.1037
2375	3.4885	-2.4387	-.0577	-.1026
2376	3.4829	-2.4559	-.0576	-.1039
2377	3.4776	-2.4532	-.0577	-.1015
2378	3.4721	-2.4687	-.0578	-.1049
2379	3.4663	-2.4867	-.0576	-.1050
2380	3.4607	-2.4846	-.0576	-.1058
2381	3.4551	-2.5087	-.0577	-.1058
2382	3.4501	-2.5084	-.0578	-.1083
2383	3.4431	-2.5261	-.0581	-.1060
2384	3.4373	-2.5227	-.0582	-.1077
2385	3.4319	-2.5457	-.0582	-.1092
2386	3.4266	-2.5457	-.0581	-.1088
2387	3.4172	-2.5695	-.0579	-.1094
2388	3.4117	-2.5691	-.0577	-.1122
2389	3.4065	-2.5924	-.0577	-.1132
2390	3.4010	-2.5889	-.0574	-.1136
2391	3.3955	-2.6150	-.0574	-.1193
2392	3.3894	-2.6128	-.0578	-.1211
2393	3.3837	-2.6382	-.0584	-.1225
2394	3.3778	-2.6342	-.0584	-.1241
2395	3.3721	-2.6595	-.0587	-.1262
2396	3.3663	-2.6608	-.0591	-.1247
2397	3.3615	-2.6771	-.0590	-.1255
2398	3.3560	-2.6976	-.0587	-.1235
2399	3.3509	-2.6964	-.0588	-.1241
2400	3.3459	-2.7113	-.0587	-.1214
2401	3.3409	-2.7412	-.0583	-.1220
2402	3.3340	-2.7422	-.0578	-.1199
2403	3.3293	-2.7595	-.0571	-.1204
2404	3.3225	-2.7888	-.0565	-.1190
2405	3.3133	-2.7905	-.0561	-.1180
2406	3.3063	-2.7923	-.0558	-.1141
2407	3.3007	-2.8196	-.0557	-.1134
2408	3.2946	-2.8232	-.0557	-.1099
2409	3.2885	-2.8433	-.0555	-.1056
2410	3.2830	-2.8420	-.0555	-.1047
2411	3.2777	-2.8419	-.0553	-.1012
2412	3.2722	-2.8686	-.0546	-.0969
2413	3.2678	-2.8655	-.0538	-.0984
2414	3.2637	-2.8911	-.0530	-.0971
2415	3.2595	-2.8917	-.0521	-.0942
2416	3.2548	-2.8925	-.0512	-.0952

cg2003

2417	3.2501	-2.9187	-.0508	-.0970
2418	3.2448	-2.9155	-.0506	-.0963
2419	3.2397	-2.9140	-.0506	-.0984
2420	3.2343	-2.9368	-.0509	-.1013
2421	3.2292	-2.9380	-.0512	-.1004
2422	3.2241	-2.9405	-.0516	-.1013
2423	3.2190	-2.9680	-.0518	-.1019
2424	3.2134	-2.9714	-.0519	-.1001
2425	3.2079	-2.9737	-.0520	-.0984
2426	3.2033	-3.0033	-.0519	-.1018
2427	3.1984	-3.0049	-.0518	-.1010
2428	3.1938	-3.0034	-.0517	-.1005
2429	3.1880	-3.0358	-.0516	-.1019
2430	3.1818	-3.0429	-.0515	-.1039
2431	3.1760	-3.0459	-.0515	-.1031
2432	3.1700	-3.0745	-.0516	-.1041
2433	3.1645	-3.0748	-.0519	-.1012
2434	3.1596	-3.0751	-.0522	-.1013
2435	3.1552	-3.1019	-.0529	-.1015
2436	3.1508	-3.1023	-.0534	-.0991
2437	3.1464	-3.1062	-.0537	-.0966
2438	3.1413	-3.1083	-.0536	-.0968
2439	3.1364	-3.1318	-.0534	-.0952
2440	3.1312	-3.1386	-.0529	-.0929
2441	3.1262	-3.1428	-.0526	-.0939
2442	3.1212	-3.1426	-.0523	-.0929
2443	3.1160	-3.1801	-.0524	-.0923
2444	3.1106	-3.1816	-.0530	-.0938
2445	3.1039	-3.1852	-.0537	-.0949
2446	3.0974	-3.1791	-.0545	-.0938
2447	3.0917	-3.2086	-.0551	-.0947
2448	3.0846	-3.2177	-.0556	-.0964
2449	3.0803	-3.2216	-.0556	-.0955
2450	3.0746	-3.2290	-.0557	-.0920
2451	3.0704	-3.2237	-.0558	-.0900
2452	3.0653	-3.2628	-.0556	-.0913
2453	3.0603	-3.2654	-.0553	-.0932
2454	3.0557	-3.2715	-.0549	-.0921
2455	3.0503	-3.2777	-.0543	-.0937
2456	3.0449	-3.2742	-.0537	-.0961
2457	3.0348	-3.3096	-.0535	-.0951
2458	3.0302	-3.3101	-.0534	-.0946
2459	3.0252	-3.3167	-.0535	-.0934
2460	3.0204	-3.3235	-.0537	-.0929
2461	3.0146	-3.3313	-.0539	-.0924
2462	3.0095	-3.3374	-.0540	-.0919
2463	3.0039	-3.3473	-.0539	-.0916
2464	2.9990	-3.3456	-.0533	-.0917
2465	2.9944	-3.3837	-.0529	-.0920
2466	2.9895	-3.3866	-.0523	-.0922
2467	2.9839	-3.3926	-.0519	-.0926
2468	2.9788	-3.4009	-.0514	-.0915
2469	2.9738	-3.4092	-.0514	-.0933
2470	2.9681	-3.4176	-.0511	-.0942
2471	2.9628	-3.4261	-.0509	-.0958
2472	2.9572	-3.4348	-.0507	-.0942
2473	2.9522	-3.4442	-.0505	-.0931
2474	2.9471	-3.4534	-.0504	-.0920
2475	2.9429	-3.4612	-.0503	-.0911
2476	2.9380	-3.4739	-.0502	-.0892
2477	2.9327	-3.4826	-.0499	-.0897
2478	2.9266	-3.4919	-.0497	-.0894
2479	2.9220	-3.5013	-.0491	-.0886

cg2003

2480	2.9171	-3.5103	-.0485	-.0878
2481	2.9125	-3.5201	-.0479	-.0868
2482	2.9078	-3.5288	-.0474	-.0864
2483	2.9030	-3.5379	-.0467	-.0850
2484	2.8982	-3.5459	-.0465	-.0785
2485	2.8933	-3.5549	-.0461	-.0734
2486	2.8890	-3.5642	-.0457	-.0685
2487	2.8834	-3.5735	-.0454	-.0637
2488	2.8788	-3.5822	-.0451	-.0642
2489	2.8743	-3.5907	-.0449	-.0694
2490	2.8695	-3.5935	-.0447	-.0727
2491	2.8640	-3.6004	-.0445	-.0754
2492	2.8633	-3.6084	-.0444	-.0721
2493	2.8580	-3.6169	-.0444	-.0656
2494	2.8536	-3.6249	-.0442	-.0601
2495	2.8496	-3.6330	-.0442	-.0552
2496	2.8455	-3.6342	-.0442	-.0508
2497	2.8412	-3.5916	-.0443	-.0530
2498	2.8366	-3.6131	-.0441	-.0546
2499	2.8318	-3.6235	-.0444	-.0564
2500	2.8272	-3.6347	-.0444	-.0579
2501	2.8232	-3.6851	-.0444	-.0591
2502	2.8185	-3.6898	-.0442	-.0598
2503	2.8144	-3.6917	-.0442	-.0608
2504	2.8098	-3.6960	-.0439	-.0660
2505	2.8048	-3.6460	-.0439	-.0704
2506	2.8002	-3.6640	-.0439	-.0748
2507	2.7954	-3.6799	-.0439	-.0789
2508	2.7904	-3.6867	-.0440	-.0780
2509	2.7858	-3.6973	-.0441	-.0723
2510	2.7801	-3.7062	-.0442	-.0684
2511	2.7771	-3.7163	-.0442	-.0648
2512	2.7726	-3.7322	-.0442	-.0673
2513	2.7696	-3.7365	-.0441	-.0731
2514	2.7654	-3.7440	-.0441	-.0777
2515	2.7620	-3.7523	-.0439	-.0819
2516	2.7581	-3.7604	-.0437	-.0855
2517	2.7537	-3.7678	-.0434	-.0827
2518	2.7484	-3.7763	-.0433	-.0802
2519	2.7438	-3.7867	-.0430	-.0776
2520	2.7392	-3.7948	-.0430	-.0752
2521	2.7347	-3.8021	-.0431	-.0731
2522	2.7300	-3.8091	-.0434	-.0716
2523	2.7256	-3.8163	-.0437	-.0705
2524	2.7212	-3.8235	-.0440	-.0704
2525	2.7170	-3.8309	-.0440	-.0699
2526	2.7124	-3.8384	-.0439	-.0690
2527	2.7075	-3.8455	-.0438	-.0688
2528	2.7031	-3.8533	-.0436	-.0684
2529	2.6992	-3.8605	-.0434	-.0678
2530	2.6953	-3.8631	-.0433	-.0675
2531	2.6912	-3.8657	-.0431	-.0674
2532	2.6868	-3.8721	-.0429	-.0667
2533	2.6825	-3.8790	-.0426	-.0662
2534	2.6782	-3.8864	-.0423	-.0660
2535	2.6738	-3.8936	-.0419	-.0657
2536	2.6694	-3.8997	-.0417	-.0654
2537	2.6651	-3.9066	-.0414	-.0655
2538	2.6612	-3.9132	-.0413	-.0662
2539	2.6572	-3.9199	-.0411	-.0669
2540	2.6526	-3.9342	-.0409	-.0679
2541	2.6476	-3.9378	-.0406	-.0688
2542	2.6436	-3.9421	-.0404	-.0693

cg2003

2543	2.6397	-3.9495	-.0401	-.0696
2544	2.6362	-3.9562	-.0400	-.0698
2545	2.6323	-3.9629	-.0399	-.0700
2546	2.6283	-3.9697	-.0398	-.0703
2547	2.6246	-3.9764	-.0397	-.0698
2548	2.6205	-3.9833	-.0400	-.0691
2549	2.6166	-3.9901	-.0402	-.0694
2550	2.6130	-3.9966	-.0403	-.0693
2551	2.6090	-4.0033	-.0404	-.0689
2552	2.6056	-4.0107	-.0406	-.0693
2553	2.6020	-4.0186	-.0404	-.0698
2554	2.5980	-4.0251	-.0404	-.0693
2555	2.5938	-4.0323	-.0405	-.0693
2556	2.5898	-4.0404	-.0406	-.0694
2557	2.5855	-4.0472	-.0407	-.0695
2558	2.5815	-4.0545	-.0407	-.0696
2559	2.5775	-4.0614	-.0408	-.0698
2560	2.5736	-4.0683	-.0410	-.0698
2561	2.5651	-4.0717	-.0412	-.0699
2562	2.5622	-4.0855	-.0413	-.0699
2563	2.5590	-4.0895	-.0414	-.0697
2564	2.5555	-4.0943	-.0412	-.0693
2565	2.5519	-4.1003	-.0410	-.0688
2566	2.5473	-4.1083	-.0408	-.0682
2567	2.5432	-4.1157	-.0405	-.0677
2568	2.5391	-4.1225	-.0399	-.0675
2569	2.5353	-4.1293	-.0395	-.0667
2570	2.5315	-4.1350	-.0391	-.0663
2571	2.5275	-4.1436	-.0387	-.0661
2572	2.5232	-4.1512	-.0384	-.0661
2573	2.5187	-4.1581	-.0384	-.0655
2574	2.5151	-4.1649	-.0383	-.0655
2575	2.5109	-4.1715	-.0381	-.0652
2576	2.5078	-4.1782	-.0379	-.0644
2577	2.5045	-4.1834	-.0378	-.0636
2578	2.5007	-4.1892	-.0377	-.0628
2579	2.4965	-4.1956	-.0376	-.0621
2580	2.4935	-4.2020	-.0374	-.0613
2581	2.4889	-4.2080	-.0372	-.0605
2582	2.4857	-4.2141	-.0369	-.0599
2583	2.4821	-4.2197	-.0368	-.0594
2584	2.4783	-4.2268	-.0367	-.0590
2585	2.4746	-4.2334	-.0367	-.0589
2586	2.4713	-4.2387	-.0367	-.0589
2587	2.4675	-4.2444	-.0366	-.0588
2588	2.4642	-4.2493	-.0363	-.0585
2589	2.4601	-4.2546	-.0361	-.0583
2590	2.4556	-4.2599	-.0360	-.0579
2591	2.4523	-4.2660	-.0358	-.0575
2592	2.4487	-4.2732	-.0357	-.0570
2593	2.4455	-4.2765	-.0357	-.0567
2594	2.4416	-4.2826	-.0356	-.0566
2595	2.4379	-4.2897	-.0353	-.0565
2596	2.4342	-4.2961	-.0350	-.0567
2597	2.4306	-4.3010	-.0347	-.0569
2598	2.4272	-4.3071	-.0339	-.0570
2599	2.4240	-4.3130	-.0333	-.0568
2600	2.4213	-4.3188	-.0329	-.0567
2601	2.4177	-4.3237	-.0327	-.0565
2602	2.4141	-4.3290	-.0326	-.0561
2603	2.4099	-4.3341	-.0330	-.0557
2604	2.4070	-4.3396	-.0331	-.0557
2605	2.4035	-4.3451	-.0329	-.0553

cg2003

2606	2.4007	-4.3518	-.0327	-.0551
2607	2.3970	-4.3581	-.0325	-.0548
2608	2.3943	-4.3634	-.0324	-.0547
2609	2.3919	-4.3687	-.0322	-.0543
2610	2.3879	-4.3743	-.0320	-.0541
2611	2.3897	-4.3794	-.0318	-.0540
2612	2.3842	-4.3846	-.0316	-.0540
2613	2.3791	-4.3898	-.0317	-.0538
2614	2.3753	-4.3946	-.0319	-.0536
2615	2.3712	-4.4004	-.0323	-.0535
2616	2.3679	-4.4057	-.0329	-.0534
2617	2.3655	-4.4122	-.0333	-.0533
2618	2.3629	-4.4167	-.0339	-.0532
2619	2.3595	-4.4227	-.0343	-.0530
2620	2.3565	-4.4266	-.0345	-.0526
2621	2.3529	-4.4322	-.0346	-.0523
2622	2.3503	-4.4366	-.0345	-.0520
2623	2.3477	-4.4418	-.0339	-.0516
2624	2.3447	-4.4481	-.0335	-.0513
2625	2.3412	-4.4529	-.0333	-.0512
2626	2.3343	-4.4583	-.0332	-.0508
2627	2.3311	-4.4637	-.0331	-.0507
2628	2.3278	-4.4702	-.0329	-.0505
2629	2.3243	-4.4760	-.0326	-.0507
2630	2.3209	-4.4807	-.0324	-.0507
2631	2.3176	-4.4855	-.0324	-.0506
2632	2.3143	-4.4877	-.0324	-.0505
2633	2.3104	-4.4932	-.0323	-.0504
2634	2.3073	-4.4986	-.0323	-.0502
2635	2.3049	-4.5037	-.0321	-.0442
2636	2.3022	-4.5079	-.0318	-.0386
2637	2.2991	-4.5126	-.0315	-.0335
2638	2.2960	-4.5183	-.0314	-.0288
2639	2.2929	-4.5234	-.0313	-.0244
2640	2.2916	-4.5282	-.0310	-.0260
2641	2.2891	-4.5331	-.0308	-.0273
2642	2.2861	-4.5388	-.0306	-.0281
2643	2.2830	-4.5434	-.0305	-.0286
2644	2.2784	-4.5482	-.0304	-.0289
2645	2.2757	-4.5531	-.0303	-.0294
2646	2.2715	-4.5585	-.0303	-.0300
2647	2.2678	-4.5640	-.0303	-.0305
2648	2.2646	-4.5114	-.0304	-.0309
2649	2.2613	-4.5200	-.0305	-.0311
2650	2.2583	-4.5303	-.0307	-.0309
2651	2.2552	-4.5383	-.0307	-.0307
2652	2.2527	-4.5444	-.0307	-.0306
2653	2.2497	-4.5505	-.0305	-.0302
2654	2.2468	-4.5555	-.0302	-.0300
2655	2.2439	-4.5606	-.0299	-.0354
2656	2.2411	-4.5663	-.0295	-.0404
2657	2.2385	-4.5720	-.0289	-.0447
2658	2.2358	-4.5809	-.0283	-.0488
2659	2.2330	-4.5858	-.0279	-.0528
2660	2.2302	-4.5905	-.0276	-.0511
2661	2.2273	-4.5955	-.0275	-.0498
2662	2.2243	-4.6000	-.0275	-.0493
2663	2.2213	-4.6042	-.0278	-.0487
2664	2.2178	-4.6087	-.0279	-.0484
2665	2.2144	-4.6140	-.0282	-.0474
2666	2.2118	-4.6169	-.0284	-.0463
2667	2.2090	-4.6231	-.0286	-.0453
2668	2.2063	-4.6262	-.0287	-.0448

cg2003

2669	2.2047	-4.6307	-.0288	-.0441
2670	2.2031	-4.6338	-.0287	-.0440
2671	2.2010	-4.6385	-.0284	-.0438
2672	2.1978	-4.6427	-.0281	-.0435
2673	2.1949	-4.6489	-.0277	-.0431
2674	2.1913	-4.6534	-.0273	-.0428
2675	2.1883	-4.6585	-.0269	-.0426
2676	2.1841	-4.6609	-.0267	-.0427
2677	2.1824	-4.6671	-.0268	-.0428
2678	2.1784	-4.6698	-.0270	-.0431
2679	2.1753	-4.6727	-.0272	-.0425
2680	2.1724	-4.6781	-.0275	-.0417
2681	2.1697	-4.6851	-.0275	-.0406
2682	2.1671	-4.6880	-.0275	-.0396
2683	2.1648	-4.6924	-.0273	-.0383
2684	2.1625	-4.6935	-.0272	-.0378
2685	2.1603	-4.6989	-.0269	-.0373
2686	2.1581	-4.7023	-.0266	-.0373
2687	2.1559	-4.7083	-.0262	-.0375
2688	2.1535	-4.7116	-.0257	-.0368
2689	2.1509	-4.7163	-.0251	-.0372
2690	2.1481	-4.7206	-.0247	-.0385
2691	2.1455	-4.7263	-.0241	-.0392
2692	2.1425	-4.7220	-.0237	-.0397
2693	2.1399	-4.7260	-.0235	-.0411
2694	2.1370	-4.7289	-.0234	-.0409
2695	2.1337	-4.7349	-.0233	-.0402
2696	2.1305	-4.7354	-.0235	-.0394
2697	2.1280	-4.7411	-.0236	-.0382
2698	2.1260	-4.7421	-.0236	-.0370
2699	2.1241	-4.7481	-.0235	-.0370
2700	2.1222	-4.7568	-.0234	-.0370
2701	2.1209	-4.7528	-.0232	-.0372
2702	2.1190	-4.7654	-.0230	-.0374
2703	2.1162	-4.7780	-.0227	-.0382
2704	2.1170	-4.7755	-.0224	-.0382
2705	2.1140	-4.7827	-.0221	-.0382
2706	2.1116	-4.7846	-.0220	-.0381
2707	2.1088	-4.7841	-.0219	-.0375
2708	2.1057	-4.7893	-.0220	-.0373
2709	2.1037	-4.7909	-.0221	-.0362
2710	2.1009	-4.7917	-.0222	-.0341
2711	2.0990	-4.7966	-.0226	-.0327
2712	2.0962	-4.7983	-.0228	-.0318
2713	2.0934	-4.8034	-.0229	-.0272
2714	2.0915	-4.8052	-.0230	-.0244
2715	2.0888	-4.8087	-.0229	-.0229
2716	2.0870	-4.8134	-.0226	-.0215
2717	2.0844	-4.8175	-.0223	-.0207
2718	2.0826	-4.8198	-.0226	-.0228
2719	2.0800	-4.8229	-.0221	-.0243
2720	2.0782	-4.8247	-.0217	-.0254
2721	2.0760	-4.8289	-.0219	-.0301
2722	2.0742	-4.8313	-.0221	-.0365
2723	2.0717	-4.8337	-.0217	-.0443
2724	2.0696	-4.8365	-.0221	-.0518
2725	2.0676	-4.8422	-.0225	-.0597
2726	2.0657	-4.8145	-.0219	-.0635
2727	2.0634	-4.8217	-.0215	-.0653
2728	2.0617	-4.8309	-.0212	-.0656
2729	2.0593	-4.8377	-.0210	-.0660
2730	2.0576	-4.8430	-.0214	-.0659
2731	2.0502	-4.8472	-.0217	-.0659

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2732	2.0556	-4.8514	-.0224	-.0653
2733	2.0530	-4.8561	-.0229	-.0681
2734	2.0455	-4.8986	-.0234	-.0706
2735	2.0437	-4.9241	-.0232	-.0728
2736	2.0421	-4.9414	-.0237	-.0747
2737	2.0398	-4.9461	-.0235	-.0767
2738	2.0378	-4.9512	-.0227	-.0754
2739	2.0405	-4.9550	-.0228	-.0742
2740	2.0374	-4.9577	-.0228	-.0732
2741	2.0334	-4.9602	-.0224	-.0686
2742	2.0315	-4.9631	-.0220	-.0618
2743	2.0229	-4.9645	-.0222	-.0539
2744	2.0280	-4.9682	-.0217	-.0461
2745	2.0194	-4.9692	-.0213	-.0381
2746	2.0179	-4.9740	-.0219	-.0341
2747	2.0159	-4.9786	-.0223	-.0326
2748	2.0146	-4.9841	-.0225	-.0324
2749	2.0130	-4.9877	-.0226	-.0324
2750	2.0116	-4.9904	-.0220	-.0325
2751	2.0096	-4.9933	-.0215	-.0327
2752	2.0078	-4.9964	-.0205	-.0328
2753	2.0056	-4.9995	-.0198	-.0327
2754	2.0037	-5.0026	-.0191	-.0321
2755	2.0014	-5.0040	-.0193	-.0313
2756	1.9994	-5.0085	-.0189	-.0305
2757	1.9968	-5.0122	-.0191	-.0299
2758	1.9946	-5.0154	-.0193	-.0295
2759	1.9927	-5.0184	-.0194	-.0296
2760	1.9909	-5.0227	-.0196	-.0304
2761	1.9890	-5.0258	-.0196	-.0317
2762	1.9872	-5.0284	-.0197	-.0329
2763	1.9864	-5.0307	-.0196	-.0341
2764	1.9848	-5.0331	-.0195	-.0352
2765	1.9826	-5.0355	-.0193	-.0358
2766	1.9807	-5.0382	-.0192	-.0361
2767	1.9783	-5.0386	-.0189	-.0359
2768	1.9763	-5.0416	-.0188	-.0359
2769	1.9742	-5.0449	-.0186	-.0361
2770	1.9727	-5.0506	-.0186	-.0363
2771	1.9704	-5.0535	-.0186	-.0364
2772	1.9687	-5.0568	-.0186	-.0363
2773	1.9656	-5.0656	-.0186	-.0359
2774	1.9650	-5.0724	-.0184	-.0356
2775	1.9619	-5.0768	-.0185	-.0353
2776	1.9613	-5.0803	-.0185	-.0347
2777	1.9584	-5.0838	-.0184	-.0342
2778	1.9569	-5.0874	-.0183	-.0340
2779	1.9548	-5.0908	-.0183	-.0330
2780	1.9536	-5.0943	-.0176	-.0314
2781	1.9519	-5.0974	-.0172	-.0282
2782	1.9507	-5.1019	-.0168	-.0256
2783	1.9488	-5.1046	-.0166	-.0230
2784	1.9473	-5.1069	-.0164	-.0209
2785	1.9453	-5.1053	-.0164	-.0193
2786	1.9437	-5.1064	-.0164	-.0198
2787	1.9437	-5.1092	-.0164	-.0197
2788	1.9376	-5.1120	-.0159	-.0196
2789	1.9368	-5.1124	-.0156	-.0194
2790	1.9363	-5.1157	-.0155	-.0193
2791	1.9349	-5.1195	-.0154	-.0200
2792	1.9339	-5.1173	-.0153	-.0205
2793	1.9334	-5.1206	-.0158	-.0211
2794	1.9318	-5.1074	-.0162	-.0216

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2795	1.9295	-5.1165	-.0160	-.0221
2796	1.9280	-5.1198	-.0159	-.0220
2797	1.9258	-5.1234	-.0163	-.0220
2798	1.9242	-5.1269	-.0166	-.0217
2799	1.9220	-5.1302	-.0166	-.0219
2800	1.9210	-5.1332	-.0173	-.0222
2801	1.9236	-5.1363	-.0177	-.0238
2802	1.9210	-5.1397	-.0174	-.0238
2803	1.9174	-5.1429	-.0172	-.0246
2804	1.9158	-5.1526	-.0170	-.0254
2805	1.9129	-5.1499	-.0168	-.0263
2806	1.9111	-5.1506	-.0167	-.0257
2807	1.9095	-5.1525	-.0167	-.0261
2808	1.9082	-5.1549	-.0171	-.0258
2809	1.9069	-5.1577	-.0174	-.0265
2810	1.8999	-5.1602	-.0176	-.0273
2811	1.8990	-5.1602	-.0175	-.0272
2812	1.8993	-5.1632	-.0173	-.0283
2813	1.8978	-5.1662	-.0167	-.0292
2814	1.8975	-5.1693	-.0164	-.0270
2815	1.8956	-5.1600	-.0162	-.0250
2816	1.8947	-5.1692	-.0163	-.0240
2817	1.8926	-5.1768	-.0160	-.0227
2818	1.8911	-5.1819	-.0158	-.0220
2819	1.8885	-5.1856	-.0157	-.0234
2820	1.8875	-5.1811	-.0156	-.0237
2821	1.8857	-5.1826	-.0155	-.0239
2822	1.8850	-5.2004	-.0158	-.0256
2823	1.8828	-5.2051	-.0161	-.0263
2824	1.8830	-5.2072	-.0161	-.0261
2825	1.8807	-5.2089	-.0163	-.0265
2826	1.8796	-5.2058	-.0162	-.0264
2827	1.8770	-5.1918	-.0163	-.0260
2828	1.8754	-5.1969	-.0161	-.0263
2829	1.8727	-5.2023	-.0162	-.0255
2830	1.8713	-5.2056	-.0160	-.0246
2831	1.8688	-5.2091	-.0162	-.0241
2832	1.8679	-5.2159	-.0162	-.0224
2833	1.8659	-5.2118	-.0165	-.0212
2834	1.8648	-5.2158	-.0163	-.0225
2835	1.8641	-5.2222	-.0164	-.0241
2836	1.8616	-5.2252	-.0160	-.0254
2837	1.8607	-5.2276	-.0159	-.0265
2838	1.8573	-5.2319	-.0154	-.0269
2839	1.8571	-5.2310	-.0152	-.0256
2840	1.8546	-5.2389	-.0148	-.0250
2841	1.8545	-5.2414	-.0148	-.0245
2842	1.8518	-5.2435	-.0145	-.0233
2843	1.8517	-5.2460	-.0143	-.0223
2844	1.8491	-5.2480	-.0140	-.0220
2845	1.8479	-5.2496	-.0138	-.0206
2846	1.8459	-5.2521	-.0136	-.0200
2847	1.8452	-5.2483	-.0135	-.0197
2848	1.8434	-5.2536	-.0136	-.0191
2849	1.8428	-5.2558	-.0137	-.0185
2850	1.8400	-5.2575	-.0139	-.0181
2851	1.8397	-5.2591	-.0140	-.0167
2852	1.8383	-5.2601	-.0140	-.0158
2853	1.8375	-5.2623	-.0140	-.0152
2854	1.8353	-5.2639	-.0141	-.0151
2855	1.8361	-5.2625	-.0140	-.0148
2856	1.8343	-5.2651	-.0141	-.0151
2857	1.8335	-5.2687	-.0142	-.0155

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2858	1.8312	-5.2688	-.0142	-.0160
2859	1.8298	-5.2729	-.0144	-.0161
2860	1.8273	-5.2761	-.0143	-.0164
2861	1.8263	-5.2750	-.0144	-.0168
2862	1.8240	-5.2788	-.0144	-.0171
2863	1.8232	-5.2793	-.0147	-.0174
2864	1.8208	-5.2753	-.0147	-.0171
2865	1.8202	-5.2780	-.0150	-.0170
2866	1.8178	-5.2798	-.0150	-.0167
2867	1.8171	-5.2824	-.0150	-.0161
2868	1.8151	-5.2840	-.0148	-.0158
2869	1.8143	-5.2859	-.0146	-.0158
2870	1.8111	-5.2904	-.0144	-.0160
2871	1.8115	-5.2918	-.0142	-.0169
2872	1.8086	-5.2950	-.0140	-.0178
2873	1.8094	-5.2959	-.0136	-.0184
2874	1.8064	-5.2979	-.0132	-.0189
2875	1.8066	-5.2985	-.0130	-.0192
2876	1.8037	-5.3008	-.0127	-.0188
2877	1.8031	-5.3003	-.0125	-.0185
2878	1.8008	-5.3014	-.0125	-.0184
2879	1.8006	-5.3041	-.0125	-.0184
2880	1.7979	-5.3063	-.0125	-.0187
2881	1.7974	-5.3076	-.0126	-.0190
2882	1.7955	-5.3101	-.0127	-.0195
2883	1.7954	-5.3142	-.0127	-.0197
2884	1.7933	-5.3153	-.0127	-.0200
2885	1.7932	-5.3168	-.0126	-.0201
2886	1.7926	-5.3186	-.0126	-.0198
2887	1.7923	-5.3193	-.0125	-.0195
2888	1.7899	-5.3211	-.0124	-.0193
2889	1.7893	-5.3219	-.0125	-.0189
2890	1.7868	-5.3263	-.0124	-.0184
2891	1.7861	-5.3297	-.0125	-.0183
2892	1.7837	-5.3326	-.0125	-.0183
2893	1.7834	-5.3354	-.0127	-.0183
2894	1.7811	-5.3376	-.0127	-.0184
2895	1.7809	-5.3392	-.0129	-.0189
2896	1.7784	-5.3402	-.0130	-.0191
2897	1.7782	-5.3413	-.0131	-.0191
2898	1.7758	-5.3417	-.0130	-.0188
2899	1.7755	-5.3410	-.0129	-.0184
2900	1.7733	-5.3435	-.0127	-.0174
2901	1.7731	-5.3454	-.0125	-.0167
2902	1.7698	-5.3470	-.0122	-.0166
2903	1.7708	-5.3491	-.0118	-.0168
2904	1.7679	-5.3520	-.0115	-.0170
2905	1.7687	-5.3540	-.0111	-.0180
2906	1.7657	-5.3559	-.0107	-.0190
2907	1.7664	-5.3572	-.0105	-.0196
2908	1.7635	-5.3608	-.0104	-.0201
2909	1.7633	-5.3607	-.0104	-.0204
2910	1.7611	-5.3635	-.0105	-.0202
2911	1.7604	-5.3636	-.0107	-.0209
2912	1.7585	-5.3664	-.0110	-.0216
2913	1.7589	-5.3661	-.0112	-.0235
2914	1.7569	-5.3692	-.0113	-.0260
2915	1.7585	-5.3752	-.0114	-.0289
2916	1.7563	-5.3760	-.0113	-.0258
2917	1.7565	-5.3776	-.0111	-.0285
2918	1.7554	-5.3818	-.0109	-.0245
2919	1.7555	-5.3824	-.0107	-.0257
2920	1.7524	-5.3858	-.0105	-.0211

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2921	1.7523	-5.3860	-.0105	-.0269
2922	1.7481	-5.3866	-.0107	-.0259
2923	1.7490	-5.3868	-.0110	-.0305
2924	1.7456	-5.4003	-.0113	-.0295
2925	1.7454	-5.4041	-.0116	-.0337
2926	1.7430	-5.4157	-.0120	-.0325
2927	1.7432	-5.4213	-.0120	-.0318
2928	1.7411	-5.4276	-.0120	-.0315
2929	1.7419	-5.3776	-.0118	-.0308
2930	1.7394	-5.4408	-.0115	-.0306
2931	1.7400	-5.3834	-.0112	-.0291
2932	1.7378	-5.4421	-.0109	-.0277
2933	1.7376	-5.3870	-.0107	-.0253
2934	1.7354	-5.4447	-.0105	-.0227
2935	1.7353	-5.4426	-.0105	-.0195
2936	1.7327	-5.4411	-.0104	-.0226
2937	1.7332	-5.4432	-.0104	-.0198
2938	1.7305	-5.4448	-.0105	-.0238
2939	1.7308	-5.4466	-.0106	-.0225
2940	1.7289	-5.4458	-.0107	-.0272
2941	1.7288	-5.4479	-.0107	-.0212
2942	1.7267	-5.4452	-.0107	-.0216
2943	1.7271	-5.4478	-.0106	-.0161
2944	1.7242	-5.4493	-.0105	-.0162
2945	1.7244	-5.4508	-.0104	-.0108
2946	1.7221	-5.4529	-.0103	-.0094
2947	1.7230	-5.4545	-.0103	-.0039
2948	1.7201	-5.4563	-.0103	-.0014
2949	1.7208	-5.4577	-.0103	.0000
2950	1.7185	-5.4594	-.0103	.0018
2951	1.7186	-5.4609	-.0103	.0029
2952	1.7159	-5.4621	-.0102	.0000
2953	1.7163	-5.4632	-.0101	-.0002
2954	1.7143	-5.4642	-.0100	.0008
2955	1.7143	-5.4654	-.0099	.0013
2956	1.7121	-5.4633	-.0097	.0008
2957	1.7124	-5.4644	-.0094	.0013
2958	1.7100	-5.4669	-.0093	.0014
2959	1.7105	-5.4518	-.0090	.0016
2960	1.7080	-5.4142	-.0088	.0017
2961	1.7084	-5.4445	-.0086	.0020
2962	1.7057	-5.4528	-.0088	.0013
2963	1.7064	-5.4518	-.0088	.0010
2964	1.7038	-5.4435	-.0090	.0007
2965	1.7051	-5.4486	-.0091	.0010
2966	1.7021	-5.4513	-.0094	-.0007
2967	1.7035	-5.4521	-.0094	-.0059
2968	1.7000	-5.4558	-.0095	-.0083
2969	1.7023	-5.4562	-.0094	-.0102
2970	1.7021	-5.4529	-.0094	-.0124
2971	1.7005	-5.4583	-.0093	-.0139
2972	1.6986	-5.4567	-.0093	-.0110
2973	1.6986	-5.4622	-.0093	-.0106
2974	1.6975	-5.4601	-.0094	-.0109
2975	1.6962	-5.4655	-.0093	-.0110
2976	1.6936	-5.4636	-.0095	-.0101
2977	1.6936	-5.4623	-.0098	-.0093
2978	1.6910	-5.4631	-.0099	-.0102
2979	1.6912	-5.4645	-.0100	-.0126
2980	1.6892	-5.4667	-.0102	-.0136
2981	1.6897	-5.4684	-.0103	-.0155
2982	1.6876	-5.4696	-.0100	-.0177
2983	1.6878	-5.4702	-.0099	-.0185

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2984	1.6856	-5.4713	-.0098	-.0183
2985	1.6859	-5.4722	-.0097	-.0194
2986	1.6839	-5.4710	-.0094	-.0203
2987	1.6841	-5.4719	-.0093	-.0204
2988	1.6820	-5.4750	-.0067	-.0543
2989	1.6823	-5.4747	-.0093	-.0776
2990	1.6802	-5.4682	-.0067	-.0548
2991	1.6806	-5.4869	.0000	.0000
2992	1.6783	-5.5007	.0000	.0000
2993	1.6787	-5.4913	.0000	.0000
2994	1.6765	-5.4980	.0000	.0000
2995	1.6769	-5.5029	.0000	.0000
2996	1.6746	-5.5001	.0000	.0000
2997	1.6750	-5.5047	.0000	.0000
2998	1.6721	-5.5033	.0000	.0000
2999	1.6733	-5.5107	.0000	.0000
3000	1.6705	-5.5056	.0000	.0000

19. 2003-02-03

cc: Chris Miller <Chris.Miller@noaa.gov>, mhughes@ltrr.arizona.edu,
dverardo@nsf.gov, mann@virginia.edu, broecker@ldeo.columbia.edu, rfweiss@ucsd.edu,
k.briffa@uea.ac.uk, drdendro@ldeo.columbia.edu
date: Mon, 3 Feb 2003 09:55:14 -0800
from: Jeff Severinghaus <jseveringhaus@ucsd.edu>
subject: Re: [Fwd: tree rings and late 20th century warming]
to: Phil Jones <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, "Thomas
R Karl" <Thomas.R.Karl@noaa.gov>, Ray Bradley <rbradley@geo.umass.edu>
<x-rich>Gentlemen:

Please accept my apologies if I have gotten the story wrong. I am not a specialist in the tree-ring field, and was simply reporting what I saw in the Briffa and Osborne paper, several other papers, and what several tree-ring people have told me in conversations. I agree, we need to keep the level of misinformation out there down to a minimum! I regret adding to it.

I am still confused, however, about Mike's explanation for the Briffa and Osborne paper's curve appearing flat after 1950 AD. Can you try explaining this again, Mike, please? I don't understand how aligning could change the slope of a curve. The curves appear to continue to 1990 AD or so, and the Esper et al. curve continues to 1993. So the explanation that the records only go up to 1980 doesn't seem to hold in this case. The dashed black line is the instrumental record for warm-season >20 N latitudes and it does indeed diverge from the tree-ring records in the 1980s. Can you help me out here?

Sincerely,

Jeff

At 4:36 PM +0000 2/3/03, Phil Jones wrote:

<excerpt> Tom,

Mike's answer is a fair response. Jeff has mixed some facts up and this is maybe

because we've never explained them clearly enough. There are two facts:

1. There are few tree-core series that extend beyond the early 1980s. This is because

many of the sites we're using were cored before the early 1980s. So most tree-ring records just don't exist post 1980.

2. The majority of the recent warming is post-1980, so no proxy would pick this up.

This warming has been large and it would be good to go back and see if the trees have

picked it up. It would give more faith in tree-ring reconstructions, but any reconstruction

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method is being pushed to the limit by the rate of temperature rise over the late 20th

century. Applies to other proxies but you have to note the following:

It is important to remember that locally few regions exhibit statistically significant warming. Highly significant at the hemispheric level, but not great at the local level due to high level's of variability. The spatial scales are important and this is difficult to get across.

Cheers

Phil

At 09:15 03/02/03 -0500, Michael E. Mann wrote:

<excerpt>

Dear Tom,

Have no fear, Jeff has still got his facts wrong, even after going back and checking once...

First off, I never made any such comment to Jeff--he clearly misunderstood comments that I made at EGS a year ago in response to a question he asked. Of course, it is well known that there are a *number* of competing explanations [*this* is what I said--to quote this as offering "no explanation" is a bit unfair Jeff, don't you think? As I recall, I even invited Tim Osborn in the audience to add his own comments--but he had little to say] for the fact that *high latitude*, primarily *summer responsive*, tree-ring *density* data have exhibited a noticeable decline in the past few decades in the amplitude of their response to temperature variability. We have discussed this issue time and again in our own work, and Keith Briffa, Malcolm Hughes, and many others have published on this, w/ competing possible explanations (stratospheric ozone changes, incidentally, is the least plausible to me of multiple competing, more plausible explanations that have been published). See e.g.:

Vaganov, E.A., M.K. Hughes, A.V. Kirdyanov, F.H. Schweingruber, and P.P. Silkin, Influence of Snowfall and Melt Timing on Tree Growth in Subarctic Eurasia, *Nature*, *400* (July 8), 149-151, 1999.

It should *also* be noted that we used essentially none of these data in the multiproxy Mann/Bradley/Hughes (MBH) reconstruction, and that the MBH reconstruction tracks the instrumental record quite well through the very end of our calibration interval (1980--it stops then because there are far fewer paleo records available after 1980). This was shown in our 1998 *Nature* article quite clearly, and of course remains true today. Jeff made the mistake of only looking at the Briffa & Osborn paper, which doesn't properly align the 20th century means of the various reconstructions and instrumental record.

An *appropriate* alignment of all the records is provided in IPCC, and in the attached *Science* *perspective* from last year. This shows how well the Mann et al reconstruction (and several model-based estimates) track the entire instrumental record. There are some good reasons that some of the other purely tree-ring based reconstructions differ in their details, in addition to the greater influence of the recent high-latitude density decline issue, and these are discussed in IPCC and the *Science* piece. Of course, we have in our own work provided detailed calibration and verification statistics that establish the skill in our reconstruction in capturing the details of both the modern instrumental record, and independent, withheld earlier instrumental data (19th century and, more sparsely, 18th century), and we publish *uncertainties* that are based on rigorous analysis of the calibration and cross-validation residuals. I know that Jeff has seen me talk on this many times, and probably has read our work (I would hope), so I'm frankly a bit disappointed at the comments. I would have liked to think

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that he would have approached us first, before broadcasting a message full of factual errors.

Please let me, or any of the others know, if we can provide any further information that would help to clarify (rather than obscure!) the facts,

cheers,

mike

At 07:49 AM 2/3/2003 -0500, Thomas R Karl wrote:

<excerpt>

Colleagues,

Correct me if I am wrong, but I always thought the failure was a lack of tree cores subsequent to the 1980s. Please correct me if I am wrong, and if Jeff is correct, then indeed we have a significant implication.

Tom

----- Original Message -----

Subject: tree rings and late 20th century warming

Date: Sun, 2 Feb 2003 16:15:04 -0800

From: Jeff Severinghaus

<<mailto:jseveringhaus@ucsd.edu><jseveringhaus@ucsd.edu>

To: <<mailto:Thomas.R.Karl@noaa.gov>Thomas.R.Karl@noaa.gov

<fixed>Dear Dr. Karl,

I enjoyed your presentation yesterday at the MIT Global Change forum.

You

may recall that I asked about the failure of tree rings to record the 20th

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cg2003

Jeff

copies to Ray Weiss, Wally Broecker
</fixed>Jeff Severinghaus
Associate Professor of Geosciences
Scripps Institution of Oceanography
University of California, San Diego 92093-0244
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</excerpt>
<fontfamily><param>Courier_New</param>

</fontfamily>
Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434)
982-2137

<<http://www.evsc.virginia.edu/faculty/people/mann.shtml>http://www.evsc.virginia.edu/faculty/people/mann.shtml
</excerpt>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
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UK

</excerpt>

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Attachment Converted: "c:\eudora\attach\Briffa&Osborn.pdf"

Jeff Severinghaus
Associate Professor of Geosciences
Scripps Institution of Oceanography
University of California, San Diego 92093-0244
(858) 822-2483 voice
(858) 822-3310 fax

2545. 2003-02-03

cc: Chris Miller <Chris.Miller@noaa.gov>, mhughes@ltrr.arizona.edu,
dverardo@nsf.gov, broecker@ldeo.columbia.edu, rfweiss@ucsd.edu,
k.briffa@uea.ac.uk, drdendro@ldeo.columbia.edu

date: Mon, 03 Feb 2003 14:16:20 -0500

from: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

subject: Re: [Fwd: tree rings and late 20th century warming]

to: Jeff Severinghaus <jseveringhaus@ucsd.edu>, Phil Jones <p.jones@uea.ac.uk>,
Thomas R Karl <Thomas.R.Karl@noaa.gov>, Ray Bradley <rbradley@geo.umass.edu>

Jeff,

Choice of aligning has no influence on the slope of the curve, it simply changes the mean

baseline for comparison. The Mann et al reconstruction has the same amplitude increase as

the full Northern Hemisphere annual mean instrumental record over the calibration interval

(1900-1980). On this simple point, there is no debate. And this seems to be the

origin of
 your misunderstanding of the issues involved.
 Briffa & Osborn use a slightly different convention from that used elsewhere
 (e.g. IPCC and
 in the attached Science piece which I've re-sent for the benefit of your
 expanded recipient
 list), and by their convention the instrumental record is observed to lie
 ever-so-slightly
 above the MBH reconstruction over the entire interval available for comparison
 (mid 19th
 century-> 1980). This difference is actually quite small, so I'm not sure why
 we're even
 discussing it in the first place. It, however, does not in any case impact a
 comparison of
 the trends in the two series, which match remarkably well over that same
 interval. This is
 despite the fact that the MBH reconstruction represents the entire Northern
 Hemisphere
 (which gets half of its contribution from the tropics i.e., latitudes < 30N)
 while the
 instrumental series shown by Briffa & Osborn is only the extratropics north of
 20N.
 This is old stuff, and I would guess that the others cc'd in on this message
 (Ray,
 Malcolm, Keith, Phil) are not interested in re-hashing these old discussions.
 The state of
 the science here has moved well beyond these semantic and/or conventional
 arguments,
 focusing instead on detailed intercomparisons of methods and data (employing
 rigorous
 diagnostics of reconstructive fidelity (collaborative between
 Bradley/Briffa/Hughes/Jones/Mann/Osborn/Rutherford). There is little
 disagreement between
 us on the broad trends when seasonal and spatial sampling issues, and differing
 conventions
 for e.g. defining reference periods, have been taken appropriately into account.
 I hope you find that the above information clarifying Jeff. Due to other demands
 on my
 time, I have to sign out now on this series of exchanges.
 best regards,
 Mike Mann
 At 09:55 AM 2/3/03 -0800, Jeff Severinghaus wrote:
 Gentlemen:
 Please accept my apologies if I have gotten the story wrong. I am not a
 specialist in
 the tree-ring field, and was simply reporting what I saw in the Briffa and
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 The dashed black line is the instrumental record for warm-season >20 N
 latitudes and it

cg2003

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At 4:36 PM +0000 2/3/03, Phil Jones wrote:

Tom,

Mike's answer is a fair response. Jeff has mixed some facts up and this is maybe

because we've never explained them clearly enough. There are two facts:

1. There are few tree-core series that extend beyond the early 1980s. This is because

many of the sites we're using were cored before the early 1980s. So most tree-ring

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just don't exist post 1980.

2. The majority of the recent warming is post-1980, so no proxy would pick this up.

This warming has been large and it would be good to go back and see if the trees have

picked it up. It would give more faith in tree-ring reconstructions, but any reconstruction

method is being pushed to the limit by the rate of temperature rise over the late 20th

century. Applies to other proxies but you have to note the following:

It is important to remember that locally few regions exhibit statistically significant

warming. Highly significant at the hemispheric level, but not great at the local level

due to high level's of variability. The spatial scales are important and this is

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get across.

Cheers

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Have no fear, Jeff has still got his facts wrong, even after going back and checking

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I made at EGS a year ago in response to a question he asked. Of course, it is well know

that there are a number of competing explanations [this is what I said--to quote this as

offering "no explanation" is a bit unfair Jeff, don't you think? As I recall, I even

invited Tim Osborn in the audience to add his own comments--but he had little to say]

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data have exhibited a notable decline in the past few decades in the amplitude of their

response to temperature variability. We have discussed this issue time and again in our

own work, and Keith Briffa, Malcolm Hughes, and many others have published on this, w/

competing possible explanations (stratospheric ozone changes, incidentally, is the least

plausible to me of multiple competing, more plausible explanations that have been

published). See e.g.:

Vaganov, E.A., M.K. Hughes, A.V. Kirdyanov, F.H. Schweingruber, and P.P.

cg2003

Silkin,

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properly align the 20th century means of the various reconstructions and instrumental

record.

An appropriate alignment of all the records is provided in IPCC, and in the attached

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(and several model-based estimates) track the entire instrumental record. There are some

good reasons that some of the other purely tree-ring based reconstructions differ in

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instrumental record, and independent, withheld earlier instrumental data (19th century

and, more sparsely, 18th century), and we publish uncertainties that are based on

rigorous analysis of the calibration and cross-validation residuals. I know that Jeff

has seen me talk on this many times, and probably has read our work (I would hope), so

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At 07:49 AM 2/3/2003 -0500, Thomas R Karl wrote:

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indeed we have a significant implication. Tom

----- Original Message -----

Subject: tree rings and late 20th century warming

Date: Sun, 2 Feb 2003 16:15:04 -0800

cg2003

From: Jeff Severinghaus
<[1]mailto:jseveringhaus@ucsd.edu><jseveringhaus@ucsd.edu>
To: <[2]mailto:Thomas.R.Karl@noaa.gov>Thomas.R.Karl@noaa.gov

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Personally, I think that the tree ring records should be able to reproduce the instrumental record, as a first test of the validity of this proxy. To me it casts doubt on the integrity of this proxy that it fails this test.

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Jeff Severinghaus

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

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Attachment Converted: "c:\eudora\attach\MannPersp2002.pdf"

4355. 2003-02-03

cc: Chris Miller <Chris.Miller@noaa.gov>, mhughes@ltrr.arizona.edu,
dverardo@nsf.gov, broecker@ldeo.columbia.edu, rfweiss@ucsd.edu,
k.briffa@uea.ac.uk, drdendro@ldeo.columbia.edu
date: Mon, 03 Feb 2003 15:03:24 -0500
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Jeff,

One final point I didn't respond to, upon re-reading your previous email: My comments about the baseline period issue only refers to comparisons of the instrumental record against the MBH reconstruction (as shown in the Briffa & Osborn piece). Unlike the MBH reconstruction, which tracks the instrumental record well through the end of the calibration interval (1980), the Esper et al reconstruction indeed doesn't show any warming after 1950 or so, which defies evidence from the instrumental record. This is similar to what has been noted, as discussed in the previous emails, with high-latitude summer-temperature sensitive maximum latewood tree-ring density chronologies (e.g. Briffa and coworkers) and it may relate to the same factors that have been discussed in that context. This generally doesn't appear to be a problem with tree ring width data, at least those available through 1980.

Once again, the wisest approach is to make use of all annually-resolved proxy information available...

That's my final word on this, promise...

mike

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cg2003

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Prof. Phil Jones

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[5][http://www.evsc.virginia.edu/faculty/people/mann.\[6\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[6].shtml)

3058. 2003-02-04

date: Tue, 04 Feb 2003 21:43:25 -0800

from: Global Dialogue 2004 <gdufour@globalcommunitywebnet.com>

subject: Uncle SAM is at the bottom of the Scale of Human and Earth Rights

Uncle SAM is at the bottom of the Scale of Human and Earth Rights

The Newsletter can be found at the following location:

February 2003 Newsletter

([1]<http://www.telusplanet.net/public/gdufour/NewsF.html>)

There are no costs in reading our Newsletters

([2]<http://www.telusplanet.net/public/gdufour/Newsletters.htm>).

The Table of Contents of the Newsletter is shown here.

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3.0 Articles

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destruction

B) Global Ministry of Women's Rights

C) Peace movement of the Earth Community Organization (ECO)

D) Count down of world population

E) Earth Ministry of Water Resources

F) Community rights (2) on the Scale of Human and Earth Rights

G) The roots of the 'mad empire' wish for oil

H) Conflicts and wars between nations

Local community issues: Regional District of Nanaimo, B.C., Canada

Assessment proposal

I) Uncle SAM is at the bottom of the Scale of Human and Earth Rights

cg2003

J) Regional District of Nanaimo ecosystems and natural heritage at the midnight hour

K) Biophysical and economic assessment of Mount Benson and surrounding ecosystems in the context of a Vancouver Island Urban and Rural Development Master Plan

L) Mount Benson Preservation Society

* Meeting of the Mount Benson Preservation Society

* AGENDA for the meeting

* Constitution of the Mount Benson Preservation Society

* By-Laws of the Mount Benson Preservation Society

* Membership Form of the Mount Benson Preservation Society

* Explanation of the four interacting circles

Letter to the Board of Directors of the Regional District of Nanaimo

Letter to His Worship, Gary Richard Korpan, Mayor of the City of Nanaimo

Letter to Honourable Stan Hagen, Minister of Sustainable Resource Management of British Columbia, Canada

Petition to save Mount Benson and surrounding ecosystems

May the DIVINE WILL come into our lives and show us the way.

May our higher purpose in life bring us closer to the Soul of Humanity and God.

Germain Dufour, President

[3]Earth Community Organization (ECO) and [4]Interim Earth Government

Apt. 201, 59 Nicol Street, Nanaimo, British Columbia, Canada V9R 4S7

Website of the Earth Community Organization and of the Interim Earth Government

[5]<http://www.telusplanet.net/public/gdufour/>

Email addresses

[6]gdufour@globalcommunitywebnet.com

[7]gdufour@telusplanet.net

4108. 2003-02-05

cc: Scott Rutherford <srutherford@gso.uri.edu>, Zhang <zz9t@virginia.edu>, mann@virginia.edu, Tim Osborne <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Irina Fast <f14@zedat.fu-berlin.de>, mhughes@lrr.arizona.edu, rbradley@geo.umass.edu

date: Wed, 05 Feb 2003 13:19:29 -0500

from: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

subject: Re: program code

to: f14@zedat.fu-berlin.de

Dear Irina,

The code we used in Mann/Bradley/Hughes 1998 was not changed or "improved", but there may

be different versions of the code floating around, and in a previous email to Uli Cubasch,

I indicated that I was not sure the version you have (from Tim Osborn), is identical to

the version we used in our original paper (it would require some work on my part to insure

it gives precisely the same results, and I don't have the time to do that). I suspect,

however, that the code is the same as the one we used in our paper and any differences, if

they exist, should be minor (as long as the code compiles and runs correctly on the

platform you have--the possible platform-dependence of Fortran is a potential cause for

concern here).

Numerous people have coded up our method independently, including Ed Zorita, w/ whom I

believe your group has a close collaboration, and my graduate student Zhang has successfully coded this up independently in Matlab (it's a short script, which didn't take

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Zhang long to write anyway). I'm copying this message to Zhang, so that he can provide you with his matlab version of the code if you are interested. Because Zhang's version is in Matlab, it should run correctly, independently of the particular platform (an advantage over the fortran code) [As an aside, on a pedagogical note, I would still encourage you to code this up yourself].

As I indicated in a previous email to Uli, the selection of the optimal subset of EOFs to retain is not automated in the code, and you need to do that yourself...The methodology we used is described in detail in our publications.

We have tested this method against the approach our group now uses for climate field reconstruction (Schneider RegEM approach), and find that the results are similar, but the cross-validation statistics improve slightly w/ the RegEM approach, which we now favor and use in place of the old, Mann et al approach.

Details of this latter approach are described in these two manuscripts (as well as the original paper by Schneider referenced within):

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies',
Geophysical Research Letters, 29 (10), 1501, doi: 10.1029/2001GL014554, 2002.
available at:
[1]ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.[2]pdf
Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., Climate Field
Reconstruction Under Stationary and Nonstationary Forcing, Journal of Climate, 16, 462-479, 2003.
available at:
[3]ftp://holocene.evsc.virginia.edu/pub/mann/Rutherfordetal-Jclim03.pdf

The RegEM code is available over the web, and Scott Rutherford can provide you with the ftp side if you are interested. It, too, is available only in matlab.

I hope you find this information of help.

Best of luck w/ your research,

mike mann

At 06:10 PM 2/5/03 +0100, Irina Fast wrote:

Dear Michael,

I believe that you have not heard about me as yet. My name is Irina Fast. Since the January 2003 I am a PhD student at the Free University in Berlin in the framework of the EU-Project SOAP. My supervisor is Ulrich Cubasch. At the SOAP's start-up meeting it was proposed to use your multiproxy calibration method (published in 1998) for the joint analysis of model simulations and proxydata.

Because your method was essential improved since 1998 I would like to know if you kann provide us with your program code.

We could try to code your approach ourselves, but we do not know if this kind of analysis will success in our case. In the case of failure we will have to search for other analyses methodes. And the timespan for the data processing is rather short. Naturally you will not miss our gratitude and acknowledgement.

I apologise for my mistakes in this letter.

Best regards

Irina Fast

--

Irina Fast
Freie Universität Berlin

cg2003

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Germany
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phone: +49 (0)30 838 711 22
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Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[4]<http://www.evsc.virginia.edu/faculty/people/mann.>[5]shtml

3835. 2003-02-07

date: Fri Feb 7 16:39:09 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Voting on "VALUES"
to: c.goodess@uea, i.harris@uea, j.burgess@uea, j.palutikof@uea, m.kelly@uea,
p.jones@uea, d.lister@uea, m.haylock@uea, c.hanson@uea, m.ekstrom@uea,
andrew.matthews@uea, m.salmon@uea
Dear all Hub session attendees,
I just wanted to follow up on one part of last Monday's hub session, viz. the
initial
voting on "values".
The second part of this voting was quite revealing - particularly that it showed
up the
widespread (though not ubiquitous) feeling that our current situation restricts
our
creativity and innovation.
The first part of the voting was supposed to guide us in our drafting of a CRU
"values
statement". But unfortunately the question asked of you didn't really help us
with this -
because most or all of the values were obviously very admirable and hence
received high
marks. What we really wanted to ask each of you was "which of these values do
you consider
the *most* important to include in a CRU values statement?" Thus even if
particular values
are very admirable, you might not consider it important that they are explicitly
included
in a "values statement". A "values statement" would set out the ethos which CRU
would
strive to follow, in undertaking our activities (which are themselves covered by
our
existing "mission statement" - see CRU flyer for this, as I couldn't find it on
our
website).
Please select the SIX values that you would most like to form part of a CRU
"values
statement". Simply reply listing the SIX numbers from the list below (1-25) -
despite not
having the Hub software to collate this, I will keep your responses anonymous by
transferring the numbers to a voting form and deleting your email.
Thanks
Tim
1. Aim to influence national/international policymaking
2. Working at the "cutting edge"; doing work that matters to governments and

the wider

community.

3. Promotion of a sustainable environment
4. Contribution to a popular understanding of environmental issues
5. Contribution to UEA's environmental research profile
6. Links with end users of climate research
7. Collaboration with colleagues in other institutions
8. Interactions with 'customers'/stakeholders' bridging academic and non-academic world
9. Creativity and innovation; an environment that supports innovation
10. High quality - research/education/administration; an environment that supports high quality research
11. Diverse people, fun and always changing work environment
12. Open communication
13. Open decision making; non-hierarchical; entrepreneurial
14. Respect for other viewpoints, courtesy, supportiveness
15. Equal opportunities; tolerance/acceptance of cultural and religious diversity
16. Healthy and safe environment
17. Appropriate and working equipment
18. Valuing staff and students; promotion of self-esteem and self-respect
19. Respect for privacy
20. Respect for family responsibilities
21. Integrity
22. Intellectual freedom and freedom of expression
23. Job security; planned career progression
24. Recognition and support of individuals' desires for personal and professional development; reaching our potential
25. Training opportunities

1816. 2003-02-09

cc: Ray Bradley <rbradley@geo.umass.edu>, k.briffa@uea.ac.uk,
mhughes@ltrr.arizona.edu, Phil Jones <p.jones@uea.ac.uk>, t.osborn@uea.ac.uk
date: Sun, 09 Feb 2003 16:53:34 -0700
from: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>
subject: Re:

to: Scott Rutherford <srutherford@gso.uri.edu>

Deear Scott - please find attached the mss with some suggested changes - I have used track changes. My comments are very similar to Tim's. We could drop the CE and the mixed hybrid. I'm not so sure about dropping the Esper comparison, but son't fell strongly about it either way. After making the changes, it also occurred to me that the criterion for weighting the high or low frequency components could give too much weight to poor low frequency records with no high frequency and undervalue records with good signal in both wavebands, so that when it comes to the next generation of reconstructions we should use a different approach to weighting.

Cheers, Malcolm
Malcolm Hughes
Professor of Dendrochronology
Laboratory of Tree-Ring Research
University of Arizona
Tucson, AZ 85721
520-621-6470
fax 520-621-8229

1. 2003-02-10

date: Mon, 10 Feb 2003 12:10:13 -0000
from: "R Warren" <R.Warren@uea.ac.uk>
subject: RE: Report - Research on Stabilisation - 17 Jan 2003
to: "'Mike Hulme'" <m.hulme@uea.ac.uk>

cg2003

Dear Mike,

Many thanks for this. Reading the report I see how very linked the whole meeting was to the scientific needs of DEFRA and it has a great bearing on how I might design the CIAS. The main message of course, is that there is a great interest on DEFRA's part in following up the UNFCCC article 2 aim of studying stabilisation targets. Various other interesting points are the poss. definition of dangerous CC with respect to the carbon cycle; and the precise recommendations of which stabilisation scenarios should be looked in to. I have also never seen Nigel Arnell present the results of the Fast Track project and that would have been really useful for me since it seems to be state of the art.

We had a theme 1 meeting a couple of weeks later and I found that I wasn't in a good position to join in discussions about key decisions on modelling plans in an informed way - as a result of my being so in the dark.

I really feel strongly that I should have been there and would have very much liked to come, but I was not told it was happening till the day before - at the theme 1 meeting in Southampton - (even though I'd said to one or two people that I wanted to go to the next meeting with DEFRA!). I would have expressed a wish to come, had I not (by that time) arranged another much less important meeting for the same day. I also didn't know, whether the meeting was the kind of meeting that one can invite oneself to - e.g. might have been a private meeting of an expert panel of which I wasn't a member, or may be there were already too many people from Tyndall there, etc.

Jonathan and I have already talked about this (and he told me about what happened at the meeting regarding economic modelling) and agreed that I need to develop a relationship with DEFRA and get involved in some (but not all) of the meetings we have with DEFRA. That is, we've agreed that I need to be at the really key stakeholder meetings like this one, and to meet occasionally with key (potential!) stakeholders like DEFRA. i.e. that in order to make good decisions about the flagship project, I need to have contact and visibility with key stakeholders.

If there are other key meetings very much linked to the scientific aims of the integrated modelling like this one, I would like to come, particularly if they are with DEFRA. However, this doesn't mean I want to go to every meeting - I don't have time - it is just the really key ones like this.

I've also noticed that DW has written an outline proposal - I guess that Tyndall is contributing to this - and again its content must have a lot of bearing on the future design of CIAS, since your section 6 shows that it discusses integrated assessment - but I didn't know about this proposal until I received your e-mail today - are we contributing to it and if so who is writing it?

Rachel

Dr. Rachel Warren
Senior Research Fellow
Tyndall Centre (HQ)
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ

Tel: 01603 593912
Fax: 01603 593901

-----Original Message-----

From: Mike Hulme [mailto:m.hulme@uea.ac.uk]

Sent: 08 February 2003 12:14

To: r.warren@uea.ac.uk; Kanako Tanaka; n.adger@uea.ac.uk;
simon.shackley@umist.ac.uk

Subject: Fwd: Report - Research on Stabilisation - 17 Jan 2003

Attached are some meeting notes from DEFRA concerning the meeting David

cg2003

WarriLOW convened on 17th Jan. concerning research into stabilisation levels and IPCC/UNFCCC agenda's.

Mike

```
>From: "Noguer, Maria (GA)" <maria.noguer@DEFRA.GSI.GOV.UK>
>To: "'parryml@aol.com'" <parryml@aol.com>, "'n.w.arnell@soton.ac.uk'"
>      <n.w.arnell@soton.ac.uk>, "'r.nicholls@mdx.ac.uk'"
>      <r.nicholls@mdx.ac.uk>, "'John.furlong@dti.gsi.gov.uk'"
>      <John.furlong@dti.gsi.gov.uk>, "'T-Foy@dfid.gov.uk'"
>      <T-Foy@dfid.gov.uk>,
>      "Parker, Miles (SD)" <miles.parker@DEFRA.GSI.GOV.UK>,
>      "Rose, Michael (LMID)" <michael.rose@defra.gsi.gov.uk>,
>      "Tauhid, Sayeeda (EPE)" <Sayeeda.Tauhid@defra.gsi.gov.uk>,
>      "Pittini, Michele (EPE)" <michele.pittini@DEFRA.GSI.GOV.UK>,
>      "Hendry, Sarah (GA)" <Sarah.Hendry@defra.gsi.gov.uk>,
>      "WarriLOW, David (GA)" <David.WarriLOW@defra.gsi.gov.uk>,
>      "Johnson, Cathy (GA)" <Cathy.Johnson@defra.gsi.gov.uk>,
>      "Noguer, Maria (GA)" <maria.noguer@DEFRA.GSI.GOV.UK>,
>      "Wilkins, Diana (GA)" <Diana.Wilkins@defra.gsi.gov.uk>,
>      "Penman, Jim (GA)" <Jim.Penman@defra.gsi.gov.uk>,
>      "'bacallander.msg.mod@gtnet.gov.uk'"
>      <bacallander.msg.mod@gtnet.gov.uk>,
>      "'h.j.schellnhuber@uea.ac.uk'" <h.j.schellnhuber@uea.ac.uk>,
>      "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>,
>      "'alex.haxeltine@uea.ac.uk'" <alex.haxeltine@uea.ac.uk>,
>      <J.kohler@uea.ac.uk>, "'J.kohler@uea.ac.uk'"
>      <J.kohler@uea.ac.uk>,
>      "'sraper@awi-bremerhaven.de'" <sraper@awi-bremerhaven.de>,
>      "'p.jones@uea.ac.uk'" <p.jones@uea.ac.uk>,
>      "'d.viner@uea.ac.uk'"
>      <d.viner@uea.ac.uk>, "'geoff.jenkins@metoffice.com'"
>      <geoff.jenkins@metoffice.com>, "'Peter.cox@metoffice.com'"
>      <Peter.cox@metoffice.com>, "'mike.Harrison@metoffice.com'"
>      <mike.Harrison@metoffice.com>,
>      "'paul.vanderlinden@metoffice.com'"
>      <paul.vanderlinden@metoffice.com>,
>      "'Kathy.maskell@metoffice.com'"
>      <Kathy.maskell@metoffice.com>, "'mgrc@ceh.ac.uk'"
>      <mgrc@ceh.ac.uk>,
>      "'cgrapley@bas.ac.uk'" <cgrapley@bas.ac.uk>,
>      "'hqpo@nerc.ac.uk'"
>      <hqpo@nerc.ac.uk>, "'a.j.thorpe@reading.ac.uk'"
>      <a.j.thorpe@reading.ac.uk>, "'ppn@nerc.ac.uk'"
>      <ppn@nerc.ac.uk>,
>      "'ben.sykes@bbsrc.ac.uk'" <ben.sykes@bbsrc.ac.uk>,
>      "'adrian.alsop@esrc.ac.uk'" <adrian.alsop@esrc.ac.uk>,
>      "'peter.bates@epsrc.ac.uk'" <peter.bates@epsrc.ac.uk>,
>      "'b.j.hoskins@reading.ac.uk'" <b.j.hoskins@reading.ac.uk>,
>      "'chris.west@ukcip.org.uk'" <chris.west@ukcip.org.uk>,
>      "'richenda.connell@ukcip.org.uk'"
>      <richenda.connell@ukcip.org.uk>,
>      "'tom.downing@sei.se'" <tom.downing@sei.se>,
>      "'j.skea@psi.org.uk'"
>      <j.skea@psi.org.uk>, "'anver.ghazi@cec.eu.int'"
>      <anver.ghazi@cec.eu.int>,
>      "'John.f.mitchell@metoffice.com'"
>      <John.f.mitchell@metoffice.com>,
>      "'Jhg@ceh.ac.uk'" <Jhg@ceh.ac.uk>, "'f.berkhout@Sussex.ac.uk'"
>      <f.berkhout@Sussex.ac.uk>, "'s.r.sorrel@sussex.ac.uk'"
>      <s.r.sorrel@sussex.ac.uk>, "'dennis.anderson@ic.ac.uk'"
>      <dennis.anderson@ic.ac.uk>, "'doug.d.mckay@si.shell.com'"
>      <doug.d.mckay@si.shell.com>, "'robert.gross@ic.ac.uk'"
>      <robert.gross@ic.ac.uk>, "'terry.barker@econ.cam.ac.uk'"
>      <terry.barker@econ.cam.ac.uk>, "'jgs@soc.soton.ac.uk'"
```

cg2003

> <jgs@soc.soton.ac.uk>, "Bobb, Letitia (GA)"
> <letitia.bobb@DEFRA.GSI.GOV.UK>
>Subject: Report - Research on Stabilisation - 17 Jan 2003
>Date: Wed, 29 Jan 2003 12:45:08 -0000
>X-Mailer: Internet Mail Service (5.5.2655.55)
>
>Dear colleagues,
>
>Please find attached the draft report of the meeting that took place 17
Jan
>2003 on Further Research on Stabilisation.
>
>Let me take this opportunities to thank all the participants for a very
>fruitful discussion and especially the speakers for introducing the
>different Agenda items.
>
>I would like to ask you to spend a few minutes reading this draft
report and
>to send me any comments that you may have. Also I would like to know if
you
>are happy for the Report to be sent to others in Europe and elsewhere
(WCRP,
>IGPP). Please send me your comments no later than 7 February.
>
>The presentations from the speakers will be sent to you in a separate
email.
>
>Regards,
>
>Maria Noguer
>
>
>
> <<Draft Report - Further Research on Stabilisation.doc>>
>-----
>-----
>Dr Maria Noguer
>Global Atmosphere Division
>Department for Environment, Food and Rural Affairs
>3/B4 Ashdown House
>123 Victoria Street
>London SW1E 6DE
>Telephone: +44 (0)207 944 5437; GTN: 3533 5437
>Email: maria.noguer@defra.gsi.gov.uk
>-----
>-----
>
>
>

1157. 2003-02-12

cc: colin.glass@fco.gov.uk, gina.ebanks-petrie@gov.ky, kevin.mowbray@fco.gov.uk
date: Wed, 12 Feb 2003 15:54:13 +0000
from: e903 <E.Tompkins@uea.ac.uk>
subject: FWD: RE: Climate change : Environment Fund proposal
to: j.darch@uea.ac.uk
Dear Janice,
I am delighted to let you know that we have the note from the director of the
department of the environment supporting the bid.
I am attaching the final proposal for you to confirm figures. This then needs
to be sent to colin.glass@fco.gov.uk and kevin.mowbray@fco.gov.uk.

cg2003

Obviously as I am already 11 days behind schedule I would appreciate it if the process could be expedited.

Many thanks

Emma

>===== Original Message From "Ebanks-Petrie, Gina" <Gina.Ebanks-Petrie@gov.ky>
=====

Hi Emma:

This is to confirm that the Department of Environment very strongly supports the bid and that the Caymanian counterpart (who will be Lisa-Ann Hurlston, Environmental Assessment Officer in the department) is keen and willing to participate in the project as outlined. In addition, the funding for the counterpart as proposed is suitable. As you know, Lisa is an extremely capable individual and I believe that this project has great possibilities with her involvement and your guidance.

I hope that the late submission of the bid will not hamper its chances of approval.

Will be in touch soon.

Regards,

Gina

PS Please let me know if this e-mail will suffice or whether I need to submit a formal letter on DoE letterhead.

Gina Ebanks-Petrie
Director, Department of Environment
Cayman Islands Government
P.O.Box 486GT
Grand Cayman
Cayman Islands, BWI
T: 345 949 8469
F: 345 949 4020
E: gina.ebanks-petrie@gov.ky

-----Original Message-----

From: e903 [mailto:E.Tompkins@uea.ac.uk]
Sent: Tuesday, February 11, 2003 12:08 PM
To: Gina.Ebanks-Petrie@gov.ky
Cc: Denise.Dudgeon

Subject: RE: Climate change : Environment Fund proposal

<< File: PAF030211.doc >> Dear Gina and Denise

I am attaching a final draft of the proposal. Unfortunately it cannot be approved by our UEA administrator until we have an email/letter from Gina saying:

1) you support it;
2) for the amount stated in the proposal the Caymanian researcher will be able

to undertake the duties as described in the proposal.

However, until I receive that I wanted to send this to you to show you the changes that have been made to make it more specific small islands and hence other OTs and also to include a comparison of adaptive capacity with other OTs.

The issue you raised about how to avoid it being a talking shop is one I will address in a covering letter - it is always difficult with research to prove that research will generate to change- when many factors generate change....I think this project provides the ground-support to make the mainstreaming of climate change response a long term proposition i.e. 1) a Caymanian counterpart in Dept of Planning (if possible); 2) profile raising activities (interviews with public and private sector 3) workshop with decision makers.

The outputs i.e. the papers and the manual are then there to support further developments...however it is awareness-raising and education that I think will make this project more than a talk shop.

I look forward to hearing from you soon Gina.

Hope all is well with you both

All the best

cg2003

Emma

>===== Original Message From e903 <E.Tompkins@uea.ac.uk> =====

>Dear Denise,

>Thank you very much for these comments - they are very helpful. I will
address

>the points you have raised and will re-submit the proposal.

>

>I am a little constrained at the moment as my laptop was stolen yesterday
from

>Southampton where I am visiting - so am having to make do on temporary

>facilities - and without the original documents. However I will endeavour
to

>re-submit as soon as possible.

>

>Many thanks for your time and thoughts on this proposal.

>All the best

>Emma

>>===== Original Message From Denise.Dudgeon@fco.gov.uk =====

>>Dear Gina and Emma

>>

>>The comments back from our Climate Change Team are :

>>

>>- this project would be more valuable if it demonstrated how this work
>>could be applied internationally to other OTs and to other Small Island
>>States. Maybe some clarification of the Tyndall Centre's work on
indicators

>>of adaptive capacity would clarify this?

>>

>>- could you provide more information/explanation of "indicators for
>>adaptive capacity"

>>

>>- we are wary of projects which just produce paper. Although the
workshop

>>for decision makers is planned, can you demonstrate better the tangible
>>benefits to come out of this project? How do we know that this project
will

>>not result in simply more documentation and another talking shop?

>>

>>I hope you don't mind these frank comments! If you can re-shape the
>>proposal slightly, it would be worth you submitting it for assessment. It
>>will be assessed along with other climate change projects (rather than
with

>>other OT projects) so there shouldn't be a problem with prioritising
>>alongside other Cayman proposals. The proposal will need to be submitted
as

>>soon as possible to Colin Glass (colin.glass@fco.gov.uk

>><mailto:colin.glass@fco.gov.uk>) who will in turn pass it to Derek

>>Pasquill, the Fund Manager.

>>

>>I hope this is helpful.

>>

>>Denise

>>

>>Denise Dudgeon

>>Biodiversity Team (Overseas Territories)

>>Environment Policy Department

>>Tel: 020 7008 2725

>>Fax: 020 7008 4076

>>Email: denise.dudgeon@fco.gov.uk

>>www.fco.gov.uk/environment

>>

>>

>>

cg2003

>>-----Original Message-----

>>From: Denise Dudgeon

>>Sent: 05 February 2003 17:28

>>To: Kevin Mowbray* Grand Cayman -UBS; 'Ebanks-Petrie, Gina'

>>Cc: Emma L. Tompkins (E-mail); Derek Pasquill

>>Subject: RE: Climate change : Environment Fund proposal

>>

>>

>>Dear Kevin

>>

>>Your email crossed with mine, I think, advising that I am seeking the
>>comments of our Climate Change Team.

>>

>>However, I like the thought of presenting this as an "cross-OT" bid. Emma
>>would need to re-word the proposal slightly to emphasise that Cayman
Islands

>>were being used as a pilot project, and that the results of the project
>>would be disseminated to all the OTs, with the idea of using this as a
model

>>for others to follow.

>>

>>There are already 3 "cross-OT" project proposals, including the one from
>>Iain.

>>

>>However, let's see first what our Climate Change team say. I expect to be
>>back in touch with you soon.

>>

>>Denise

>>

>>Denise Dudgeon

>>Biodiversity Team (Overseas Territories)

>>Environment Policy Department

>>Tel: 020 7008 2725

>>Fax: 020 7008 4076

>>Email: denise.dudgeon@fco.gov.uk

>>www.fco.gov.uk/environment

>>

>>

>>

>>-----Original Message-----

>>From: Kevin Mowbray* Grand Cayman -UBS

>>Sent: 05 February 2003 17:00

>>To: 'Ebanks-Petrie, Gina'

>>Cc: Emma L. Tompkins (E-mail); Denise Dudgeon; Derek Pasquill

>>Subject: RE: Climate change : Environment Fund proposal

>>

>>

>>

>>Dear Gina,

>>

>>Many thanks for your input on this proposal.

>>

>>I too am happy to lend support to this very worthwhile project,
particularly

>>as it would serve as a useful way to take forward your Department's
>>medium-term plan on Climate Change. But I would endorse your caveat that
>>our support should not be to the detriment of the other three projects
that

>>we have submitted from Cayman for funding from the Environment Fund in the
>>next financial year. As the proposal should be of general interest to all
>>the OTs, perhaps it can, as you suggest, be submitted in its own right,
>>though this will still put it and Iain Orr's project in direct competition
>>for funding against our other bids unless there is some way of making them

cg2003

a

>>lower priority. Perhaps EPD can advise on this.

>>

>>Best regards.

>>

>>

>>Kevin Mowbray

>>Staff Officer to H.E. the Governor

>>George Town

>>Grand Cayman

>>

>>Tel: 345 244 2434

>>Fax: 345 945 4131

>>Email: Kevin.Mowbray@fco.gov.uk

>>

>>5 February 2003

>>

>>

>>

>>-----Original Message-----

>>From: Ebanks-Petrie, Gina [mailto:Gina.Ebanks-Petrie@gov.ky]

>>Sent: Tuesday, February 04, 2003 9:23 AM

>>To: Kevin.Mowbray@fco.gov.uk; Denise.Dudgeon@fco.gov.uk

>>Cc: Emma L. Tompkins (E-mail)

>>Subject: RE: Climate change : Environment Fund proposal

>>

>>

>>

>>Kevin and Denise:

>>

>>

>>

>>Sorry to be so late in responding but have been busy with visiting beach

>>erosion expert! The whole issue of Climate change has been on the DoE's

>>short to medium term work plan for the last two years. Denise you will

>>recall that I flagged it as an issue that I thought the UK could assist

>>with

>>when we first met here in Cayman. I have been talking with Emma about

>>collaborating on this issue for some time now but, as always, resources

>>have been an issue. Nevertheless, Emma has managed to do some preliminary work

>>here. So, I very much support this bid although I would not like it to

>>compete with the bids coming directly from Cayman. I see that the project

>>also supports several of the recommendations in the recent report on "The

>>Impacts of Global Climate Change on the UK Overseas Territories" by Sear,

>>Hulme, Adger and Brown. Would it then be possible for this project to be

>>considered in its own right as opposed to being lumped in with the Cayman

>>projects? Please let me have your views.

>>

>>

>>

>>

>>

>>

>>

>>Gina Ebanks-Petrie

>>

>>Director, Department of Environment

>>

>>Cayman Islands Government

>>

>>P.O.Box 486GT

>>

cg2003

>>Grand Cayman
>>
>>Cayman Islands, BWI
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>>T: 345 949 8469
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>>E: gina.ebanks-petrie@gov.ky
>>
>>
>>
>>-----Original Message-----
>>From: Kevin.Mowbray@fco.gov.uk [mailto:Kevin.Mowbray@fco.gov.uk]
>>Sent: Monday, February 03, 2003 9:43 AM
>>To: Denise.Dudgeon@fco.gov.uk
>>Cc: Gina.Ebanks-Petrie@gov.ky
>>Subject: RE: Climate change : Environment Fund proposal
>>
>>
>>
>>Denise,
>>
>>
>>
>>Many thanks.
>>
>>
>>
>>I subsequently received a copy of the proposal but have not had time to do
>>anything with it as yet. I saw Gina briefly on Saturday night and had a
>>quick discussion with her about the bid, though she too has not had time
>>to
>>look at it in detail. I am not against the bid in principle but could
>>have
>>done with more of a heads-up on it and would not want this bid to dilute
>>our
>>other bids for funding.
>>
>>
>>
>>Let's see what Gina has to say on it before I put it through to the
>>Governor
>>for our comments.
>>
>>
>>
>>Best regards.
>>
>>
>>
>>Kevin Mowbray
>>Staff Officer to H.E. the Governor
>>George Town
>>Grand Cayman
>>
>>Tel: 345 244 2434
>>Fax: 345 945 4131
>>Email: Kevin.Mowbray@fco.gov.uk
>>
>>3 February 2003
>>
>>

cg2003

>>
>>-----Original Message-----
>>From: Denise Dudgeon
>>Sent: Monday, February 03, 2003 4:54 AM
>>To: Kevin Mowbray* Grand Cayman -UBS
>>Cc: Colin Glass
>>Subject: Climate change : Environment Fund proposal
>>
>>Kevin
>>
>>
>>
>>This lady came in to my office on Friday morning (no appointment, she just
>>popped her head round the door). She introduced herself as Emma Tompkins
>>and
>>works for the Tyndall Centre for Climate Change. She said she's been
>>working with Gina Ebanks-Petrie on climate change issues and, with Gina's
>>support, she was going to put in a bid to the Environment Fund. I know
>>little more. I advised her to send the proposal to you in the first
>>instance as the proposal would need your/the Governor's approval. I
>>haven't
>>seen a copy of the proposal yet.
>>
>>Denise
>>
>>Denise Dudgeon
>>Biodiversity Team (Overseas Territories)
>>Environment Policy Department
>>Tel: 020 7008 2725
>>Fax: 020 7008 4076
>>Email: denise.dudgeon@fco.gov.uk
>>www.fco.gov.uk/environment
>>
>>
>>
>>-----Original Message-----
>>From: Kevin Mowbray* Grand Cayman -UBS
>>Sent: 31 January 2003 17:17
>>To: Denise Dudgeon
>>Subject: RE: DFID meeting UEA - 3rd Feb
>>
>>Denise,
>>
>>
>>
>>Am I missing something here? I have no idea what this proposal is about.
>>It
>>has not to my knowledge been cleared with us in advance and so will not
>>receive our support.
>>
>>
>>
>>Plus we are up to our necks in the Euro Bank aftermath (I have given 34
>>press interviews in two weeks) and so cannot focus on other issues at this
>>time.
>>
>>
>>
>>Best regards.
>>
>>
>>
>>Kevin Mowbray

cg2003

>>Staff Officer to H.E. the Governor

>>George Town

>>Grand Cayman

>>

>>Tel: 345 244 2434

>>Fax: 345 945 4131

>>Email: Kevin.Mowbray@fco.gov.uk

>>

>>31 January 2003

>>

>>

>>

>>-----Original Message-----

>>From: Denise Dudgeon

>>Sent: Friday, January 31, 2003 11:16 AM

>>To: 'e.tompkins@uea.ac.uk'

>>Cc: Colin Glass; Kevin Mowbray* Grand Cayman -UBS; Meghna Patel

>>Subject: RE: DFID meeting UEA - 3rd Feb

>>

>>Emma

>>

>>

>>

>>Just a quick reply to say as soon as your proposal is ready, you should send

>>it to the Staff Officer in the Cayman Islands (Kevin.Mowbray@fco.gov.uk
>><mailto:Kevin.Mowbray@fco.gov.uk>) for his approval and the authorisation
>>of the Governor. Kevin then sends it to Colin Glass, who is the Cayman
>>Islands Desk Officer here in our Overseas Territories Department. Colin
>>adds his comments and then submits it direct to Derek Pasquill, the Fund
>>Manager, copied to me for information.

>>

>>

>>

>>I'm not sure if anyone from here is attending next week's meeting - possibly

>>Meg Patel from our Climate Change team might be involved.

>>

>>

>>

>>It was nice to meet you too yesterday. I hope your cold clears up soon!

>>

>>Denise

>>

>>Denise Dudgeon

>>Biodiversity Team (Overseas Territories)

>>Environment Policy Department

>>Tel: 020 7008 2725

>>Fax: 020 7008 4076

>>Email: denise.dudgeon@fco.gov.uk

>>www.fco.gov.uk/environment

>>

>>

>>

>>-----Original Message-----

>>From: Emma L. Tompkins [mailto:e.tompkins@uea.ac.uk]

>>Sent: 31 January 2003 15:48

>>To: Denise.Dudgeon@fco.gov.uk

>>Subject: FW: DFID meeting UEA - 3rd Feb

>>

>>Dear Denise

>>

>>Thanks for the email. It was also good to meet you.

cg2003

>>
>>
>>
>>I am battling away here with the proposal - although may not get it in by
>>the end of the day due to a heavy head cold which has descended! However I
>>will try.
>>
>>
>>
>>I thought you might be interested in this meeting that we have arranged
with
>>DfID on Monday - a rough agenda is attached. I am sure that someone from
>>FCO is coming along -although I do not have the name.
>>
>>
>>
>>Thanks for making the time to speak to me, I will be in touch again soon.
>>
>>All the best
>>
>>Emma
>>
>>
>>
>>
>>
>>-----Original Message-----
>>From: Neil Adger [mailto:N.Adger@uea.ac.uk]
>>Sent: 27 January 2003 15:36
>>To: Katrina Brown; e.tompkins@uea.ac.uk; Suraje Dessai; d.conway@uea.ac.uk
>>Cc: m.hulme@uea.ac.uk; Alex Haxeltine; Nick Brooks; Jouni Paavola; Vanessa
>>McGregor
>>Subject: DFID meeting UEA - 3rd Feb
>>
>>
>>
>>
>>
>>Folks
>>
>>
>>
>>Please find below the finalised timetable for the DFID meeting next week.
It
>>is in the Callendar Room beginning with coffee and tea and 11.30. There
are
>>seven visitors from DFID, FCO and DEFRA. The most senior person, I think,
is
>>Tom Foy who leads the climate change team in the Environment Policy Dept
of
>>DFID.
>>
>>
>>
>>
>>You are invited to lunch with us at the Sports Park - please confirm your
>>attendance with Vanessa.
>>
>>
>>
>>
>>>From Kate, Declan and Suraje, please let Vanessa know your audio-visual
>>needs for the session - you can give informal talks, or use overheads.
>>Please note that those sessions are only 15-20 minutes long, so only short
>>presentations. Vanessa is compiling packs to distribute to the visitors so

cg2003

>>please include project material or offprints related to development and
>>climate issues - please send to Vanessa this week.

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>>Many thanks

>>
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>>
>>Neil

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>>
>>
>>11.30-1.00

>>
>>
>>
>>Brief introduction to Tyndall research from Prof. Mike
Hulme

>>
>>
>>
>>DFID Climate Policy and Actions covering:
>>
>>Responsibilities of DFID in the development and
climate
>>policy area

>>
>>What DFID is doing about this - development activities
>>and research

>>
>>Future needs and priorities

>>
>>
>>
>>Overview of Tyndall projects with developing country and
>>international development dimensions (Neil Adger)

>>
>>
>>1-2pm Lunch (Sports Park)

>>
>>
>>
>>2.00-3.00 pm Tyndall research efforts on international development,
climate
>>and policy

>>
>>
>>
>>Short discussions from a selection of Tyndall activities

>>
>>1 Overseas territories and adaptation to climate
change
>>(Emma Tompkins)

>>
>>2 Development and equity in and forest-based
mitigation
>>(Katrina Brown)

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>>
>> 3 East African long term climate change and water
>>availability (Declan Conway)
>>
>> 4 What is dangerous climate change? And why does it
>>matter? (Suraje Dessai)
>>
>>
>>3.00-3.30 pm Discussion of future needs and directions
>>
>>
>>3.30pm Leave for 4pm train to London.
>>
>>
>>
>>*****
>>For more information on the Foreign and Commonwealth Office visit
>><http://www.fco.gov.uk> For information about the UK visit
>><http://www.i-uk.com>
>>
>>Please note that all messages sent and received by members of the Foreign
>>and Commonwealth Office and its missions overseas may be monitored
>>centrally.
>>This is done to ensure the integrity of the system.
>>
>>*****
>>
>>
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>>
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>>
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>><http://www.gov.ky>
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Visit the official website of the Cayman Islands Government at
<http://www.gov.ky>

4802. 2003-02-12

cc: Roland Fuchs <RFuchs@agu.org>, e.l.jones
date: Wed Feb 12 09:37:56 2003

cg2003

from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: Fwd: Responding to Climate Change 2003 - COP9
to: Neil Leary <NLeary@agu.org>

Neil,

I think we interacted with a company called Ramtor re. the publication you refer to. We

put an advert and a page of text in both the 2001 and 2002 publications - and paid for it.

We have not yet decided about 2003.

I am copying this to my colleague - Dr Elaine Jones, Tyndall's Business Liaison Manager -

since it was she who negotiated this on our behalf and handled all the correspondence

[Elaine - could you give Neil a brief run-down of the rates/content etc. as we used it?

Elaine - do we have a spare copy we could send to Neil?]

Certainly quite a few heavyweight businesses and research establishments advertised. And

it gets to all delegates. How effective it is of course is a matter of judgement - I can't

give you any hard evidence about this.

Best wishes,

Mike

At 18:08 10/02/03 -0500, Neil Leary wrote:

Dear Mike,

START was contacted today by a firm called Entico about a publication called "Responding

to Climate Change" that they are preparing for distribution at COP9 (see below). He said

that this is an "official" publication that is produced for the UNFCCC Secretariat that

get's high visibility at the CoPs. He wants to know if START would be interested in

being included in the publication (with a focus on AIACC). By this he means purchase

advertising space as well as to provide a paper or case study that would be included in

the publication.

The Tyndall Centre is listed as a partner. Do you know anything about Entico and their

publication? Is this worth learning more about and considering as a possible way to

inform more people (COP delegates in particular) about AIACC?

I looked at their web page briefly. Under case studies, research and training, the

entries include Columbia University's Earth Institute and a Cambridge-MIT collaboration. So it looks like they have some solid programs that have

decided to use

them for outreach. But I really don't know anything about them.

If you have any opinions I would be glad to have them.

Thanks,

Neil

Date: Mon, 10 Feb 2003 17:44:32 +0000

From: Adam Bumpus <ab@entico.com>

Subject: Responding to Climate Change 2003 - COP9

To: RFuchs@agu.org, NLeary@agu.org

Importance: Normal

X-Priority: 3 (Normal)

Dear all,

Following our conversation earlier today, and as promised, I include the attachment

regarding the publication 'Responding to Climate Change' for COP9 in December 2003.

cg2003

As we discussed, the publication will go into the hands of all the delegates and attendees at the meeting as a window on the major and most innovative players in the climate change arena. This is really about the communication of effective climate change networks and solutions, and to provide conference-wide profiling and support of those organisations taking part at the conference.

The publication is an official observer at the meeting, and thus takes a high profile to promote the work of the organisations we work with. This will also be the case for the side events that we will run for select members of our partners. For the START secretariat, and in addition to the advertisement, we would also like to invite you to be involved with a contribution to the editorial of the publication, in the form of a case study or white paper (approximately 800) in order to build in more detail the solutions you present.

All the materials in the publication will also be presented on the associated dedicated website ([1]www.rtcc.org), with links to all the partners involved, and driving traffic through to their websites through active links. RTCC 2002 is still on-line for you to help substantiate our credentials and other partnerships.

Please feel free to contact me if you have any questions. I thank you in advance and look forward to speaking with you on Monday.

Kind regards
Adam Bumpus
Entico Corporation
Victoria Chambers
16-18 Strutton Ground
London
SW1P 2HP
UK
t: +44 (0) 20 7799 2222
f: +44 (0) 20 7340 2868
m / cell : +44 (0) 7739 170792
[2]www.entico.com
[3]www.rtcc.org

--

Neil A. Leary
Science Director
Assessments of Impacts and Adaptations to Climate Change (AIACC)
The International START Secretariat
2000 Florida Avenue NW, Suite 200
Washington, DC 20009 USA
Phone: 1 202 462 2213
Fax: 1 202 457 5859
Email: nleary@agu.org
Website: [4]www.start.org

1401. 2003-02-15

date: Sat Feb 15 13:51:14 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: LEAD paper and MAGICC/SCENGEN
to: "xianfu" <xianfu@waikato.ac.nz>
xianfu,

cg2003

A revised version is attached with a few tracked comments in blue. Note: the references have no standard formatting - this needs attention by someone. Otherwise basically OK.

Let me know what LEAD will do with it.

John Ashton has cancelled his visit on 28 Feb. unfortunately - hopefully it will be re-arranged.

Keep me informed about Tom and MAGICC. The only reason I can think he says not to use the SRES forced GCM runs is because he argues that the results from CMIP (all 1% p.a. forced)

give a "cleaner" GHG signal to pattern-scale. But the SRES forced GCM results are better

to use directly than pattern-scaling. There is also the complication of aerosols.

There are no "right" answers here - all of this involves approximations. Tom will always

want to do things his way - and for now he clearly wants to keep "control" of the model.

Mike

At 14:38 09/02/03 +1300, you wrote:

Mike,

Hope all s well in Tyndall.

Attached is the paper we did for LEAD. They have sent me a version with reviewers

comments and suggestions for changes they are all rather minor, really. I responded to

a few of them but there are a couple I am unsure about. Could you please go through the

comments from reviewers (in either yellow or very light blue) and my responses (in

red)? Thanks. I hope this is the real final round of revision.

On MAGICC/SCENGEN: Tom Wigley has agreed to offer his version of MAGICC/SCENGEN and

technical support (largely through me) to AIACC community. He sent me some rather out

of date technical notes on what he did with this version. I am very confused by what he

was trying to do and what he commented on the SRES forced GCM experiments (essentially

he was saying that it is WRONG to directly use GCM outputs!). Also, from his correspondences to Neil Leary which Neil passed on to me, Tom does not seem

keen to involve Tyndall in upgrading or distributing his version of the model.

Anyway, I am

waiting for him to send me the software (which he is still debugging) and some updated

technical notes. If you wish, I could send you the technical notes once I have them.

Neil has asked me to do a tutorial on it at the Jo burg workshop since Tom is not able

to make it.

Slowly adapting to everything here but finding it hard at times to be so far away from

everywhere else and everybody else.

with best regards,

Xianfu

2661. 2003-02-15

cc: "Andy Wright" <Andy.Wright@umist.ac.uk>, "Geoff Levermore" <geoff.levermore@umist.ac.uk>, "John Turnpenny" <j.turnpenny@uea.ac.uk>

cg2003

date: Sat Feb 15 16:50:30 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: RE: Using Scaling Factors for HadRM3 data
to: "David Chow" <david.chow@umist.ac.uk>

David,

A few answers and comments:

- the scaling factors (Table 7) are derived from globally-averaged surface air temperatures (dry bulb), not from locally derived (e.g. UK) data. It has to be done this way for the

pattern-scaling method we employed to make any sense

- just because you cannot reconstruct values for intermediate periods is no indication of

how the scaling factors were derived; pattern-scaling is a method that has a long history

(see attached note for explanation) and can at best only ever be an approximation

- there are several reasons why you will never get full agreement between pattern-scaled

estimates and raw/direct model output: - the signal (being scaled is not well-defined

relative to the noise); climate change is in any case not a linear function; regional

climate will not behave linearly in relation to global climate.

- in your exercise of using A2 to reconstruct B2 through pattern-scaling, it matters

whether you used 30 years or 90 years of GCM data (i.e., there was an ensemble of three A2

simulations). You should use the maximum number of years possible to define your "signal".

In the end, I am not sure what you are trying to achieve here - prove that pattern-scaling

is good or bad as a method? This is a complex subject and Tim Mitchell here in HQ wrote a

whole PhD thesis about it! I don't think it is relevant for your research. You should

feel confident in using the UKCIP02 scenarios and Hadley data as supplied.

I hope this helps,

Mike

At 09:54 11/02/03 +0000, David Chow wrote:

Dear Mike,

In relation to my Tyndall work I'd like to know is how the scaling factors (Table 7 on

p. 43 of UKCIP02 report) were derived from the global data. (Presumably it was just global dry bulb.) An equation and any

relevant references would be very useful. Were the factors based on 15 min data, hourly, daily or

monthly? We want to use the factors to derive percentiles for the 2020s and 2050s.

I have conducted some analysis on spells of data for temperature and solar radiance

using HadCM3 and HadRM3 data that may be of interest.

Chart 1 shows the differences between different model runs compared with real observed

data for 1976-1990. It can be seen that HadCM3 data (dark columns) are significantly

colder than what was observed in real life, not just with the average values, but also

for the extremes. So the obvious thing is to use HadRM3 data, which seem to be more

accurate.

However, the problem with using HadRM3 data is that there is only data for

cg2003

1960-1990 and
2070-2100. Periods in between need to be interpolated, using Table 7 on p. 43
in the
UKCIP02 Scientific Report (April 2002). I presumed the values from this table
are
obtained from analysis using HadCM3 data. The Report does not specify exactly
how they
were obtained but the obvious thing to use would be average temperature. So I
did a
quick check to see what the scaling factors are for temperature in HadCM3.
Chart 2 shows
the results. It appears that the extreme cold data (dotted line) and extreme
hot data
(solid lines) have significantly different scaling factors. The average data
also have a
trend different to what the Report suggests. (The thick blue line), which
suggests that
the scaling factors are not simply based on average temperature.
There are 2 ways of obtaining HadRM3 (B2 senario) data for 2080s. One is to
use the
actual data provided by the database (selection), and the other metod is to
use results
from the A2 senario and apply the pattern-scaling factors. If the scaling
factors are
reliable, the 2 sets of results should be similar. However, Table 1 shows that
there is
a significant difference, with the pattern-scaled data "over-estimating" the
increase.
In particular, one would expect the median (50%) value to be close to the mean
and hence
accurately pattern scaled (with perhaps lower correlation for the extremes),
but the
median difference values are typically about 10% higher than the data-derived
values.

Thank you very much for helping.

Regards,

David Chow

Research Assistant

Manchester Centre of Civil and Construction Engineering

UMIST

M60 1QD

Tel. 07879 447760

e-mail. [1]david.chow@umist.ac.uk

50. 2003-02-17

date: Mon, 17 Feb 2003 11:03:16 -0800 (PST)
from: Eric Steig <steig@geophys.washington.edu>
subject: review of Holocene paper by Masson-Delmotte et al.
to: k.briffa@uea.ac.uk

Dear Keith,

Following is my review of Masson-Delmotte et al., "Common millennial-scale
variability". My apologies for the delay.

I would prefer anononimity for this review.

I have reproduced the peer review form below.

Sincerely,

Eric Steig

Paper title: Common millennial scale variability of Antarctic and Southern
Ocean temperatures during the past 5000 years reconstructed
from EPICA Dome C ice core

Author(s): V. Masson-Delmotte, B. Stenni, and J. Jouzel

Checklist:

cg2003

Title: Suitable. I suggest inserting "the" before "EPICA".

Introduction: requires revision

Discussion: sufficient

Abstract: requires revision

Methods: better description and error bars required for spectral analysis

Conclusions: generally sound but spectral analysis overinterpreted

Scope: international interest/interdisciplinary interest/general
significance is apparent

Length: appropriate

Language and style: English requires work of copy-editor;
generally well organized

Referencing: some additional references required

Figures: not all are essential; some revision required

Recommendation:

(1)/(2) Should be acceptable after modification and resubmission

Importance Rating: Major Contribution

Detailed comments:

The authors have made significant contributions to the interpretation of deuterium and deuterium excess from ice cores. Especially in the last few years they have demonstrated the utility of deuterium excess as a proxy for sea surface temperatures. The great advantage of their approach is that both local (ice sheet) temperatures and distant (sea surface) temperatures are obtained from a single ice core record, largely eliminating ambiguities about relative age. This paper provides new deuterium (δD) and deuterium excess (ϵ_s) data from the EPICA ice core at Dome C, Antarctica. A 2-D isotopic model is used to calculate linear functions relating δD and ϵ_s to site and source temperature, allowing conversion of the isotope ratios to useful climate variables. This approach has been used previously and shown to be very reasonable; it is probably even more reasonable for the Holocene than the last glacial period, since boundary conditions are changing less, especially on the millennial timescales emphasized in the paper.

The paper is overall well organized, but there is too little description of the deuterium and deuterium excess, which some readers will not be familiar with. I suggest adding a short paragraph, prior to the one that begins "Here we focus..." which provides more reference to previous theoretical work on deuterium excess. Missing especially from the references is the Kavanaugh and Cuffey paper from the Greenland IGS meeting, and the Cuffey and Vimeux paper from Nature. Also, reference is made to the Stenni et al. paper from Science, but the reader has to guess what was in that paper. A brief description of that paper and its conclusions would be appropriate.

The paper reaches three main conclusions. First, that the early Holocene optimum occurs early in Antarctica than at lower latitudes; second, that site and source temperatures co-vary after about 5000 years ago (which is tentatively attributed to an increase in ENSO-type variability); third, that there is significant temperature variability on timescales of ~800 years at the Antarctic site, but not at lower latitudes. Each of these conclusions is important, if correct, because each provides insights into how the climate system has evolved through the Holocene. I find particularly interesting the suggestion that the millennial-scale variability in the Antarctic is probably of regional origin, since it does not appear to occur at lower latitudes and is on a different timescale than the often-discussed 1500-year cycles of the North Atlantic. Overall, I think the conclusions are sound, but I am skeptical about some aspects.

The interpretation of various "periodicities" in the data is overstated. In fact, no confidence intervals are shown in the spectral analysis plots, unless perhaps the dashed line shown is supposed to be the 95% confidence (?). The very different spectra obtained for the 0-3, 3-6, 6-9 and 9-12 ka intervals are interpreted as meaningful changes in the physical processes involved (e.g. "some periodicities only appear in the last thousands of years"). A more conservative conclusion would be that none

cg2003

of these "periodicities" are actually significant, relative to red noise. Use of a more conservative spectral analysis routine, with a greater number of degrees of freedom (MTM notoriously uses too few degrees of freedom), would doubtless result in virtually no significant peaks being detected. I would advise against listing multiple "significant" periodicities (e.g. on page 8, it says "220, 176, 150, 110....") and focus instead on the much more interesting result that the deuterium is "redder" than the deuterium excess, which implies (as the authors state) that the millennial-scale power is of local rather than global origin.

Related to this, Vimeux, Masson-Delmotte and others reported a 900-year periodicity in deuterium excess from Taylor Dome. Why does this show up in xs (only) from Taylor Dome and dd (only) at Dome C?

The supposed connection with solar variability during the last 2000 years is entirely unconvincing and I suggest deleting it. No statistical work is shown to suggest that this is significant.

Since the paper should be revised, I will not comment on specific grammatical or typographical errors here. Overall, the English is fine but should be looked over by a good copy editor prior to publication. Two important things though:

1) "western" and "eastern Antarctica" is incorrect! Use the terms "West Antarctica" and "East Antarctica". These are place names, not geographic directions.

2) On page 10, the term "inversed" refers to the mathematical inversion, but as written it implies that the graphs are upside down. I would say "calculated" instead of "inversed."

3) Figures: Figure 1 should show other ice core sites discussed in the text (as well as well-known sites like Vostok). Figure 5, 8a: justification should be given for using the "reshaped" harmonic spectra; otherwise (preferably) these should not be used, as they are appropriate for electronic signal processing and are of dubious use in climate research where narrow band signals would be a major discovery! On both figures confidence intervals should be shown and explained.

220. 2003-02-17

cc: <energy2@enda.sn>, <ysokona@enda.sn>, <youba@hotmail.com>, <harro.van.asselt@ivm.vu.nl>, <joyeeta.gupta@ivm.vu.nl>, <marleen.van.de.kerkhof@ivm.vu.nl>, <matthijs.hisschemoller@ivm.vu.nl>, <emilio@ppe.ufrj.br>, <bert.metz@ivm.nl>, <marcel.berk@ivm.nl>, "Leena Srivastava" <leena@teri.res.in>, "Preety Bhandari" <preetyb@teri.res.in>, <alex.haxeltine@uea.ac.uk>, <m.hulme@uea.ac.uk>

date: Mon, 17 Feb 2003 14:48:27 +0530

from: "Ulka Kelkar" <ulkak@teri.res.in>

subject: RE: The HOT Research Protocol is finished

to: <edan.rotenberg@ivm.vu.nl>

Hi Edan,

This is in continuation of my earlier email regarding preparations for the regional stakeholder interviews. We are now in the process of compiling a database of potential interviewees. In addition to TERI's contacts, we are tapping the following sources:

- Government focal points and NGOs accredited with the UNFCCC
- Asian partners/members of the World Business Council for Sustainable Development

- Members of the Climate Action Network (CAN) South and South-east Asia networks
Any suggestions or contact information would be most welcome.

Second, going by the UNFCCC classification, we plan to include stakeholders from Middle East countries in our survey. Some Former Soviet Union countries are also included in Asia, but we want to check if RIVM would be covering any of these (Armenia, Azerbaijan, Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, Uzbekistan, and Ukraine).

Third, we would like to hear from the other regional partners about the number of interviews that they plan to conduct, as well as their approach to the questionnaire.

cg2003

Finally, we are following the Tyndall Centre discussion forum with great interest.
Best regards,
Ulka.

>>> "Edan Rotenberg" <edan.rotenberg@ivm.vu.nl> 02/05/03 08:57pm >>>
Hello,

Thanks very much to Ulka and everyone at TERI for getting the ball rolling on these questionnaires. Joyeeta is out of the office today but she would doubtless like to see the questionnaire. I have spoken to Marcel Berk and Alex Haxeltine - they will both be reviewing the questionnaire to see if it will give them the information they need to provide useful scientific input. Also, there are a few comments still to come in from RIVM on the HOT Research Protocol, so it is not yet finished. I hope to be able to send out a finalized protocol next week, and am looking forward to receiving the questionnaires prepared by other Regional Partners.

best wishes to all,

Edan Rotenberg

p.s. I don't have a graduate degree, so I'm not a doctor. Please just call me Edan.

-----Original Message-----

From: Ulka Kelkar [mailto:ulkak@teri.res.in]

Sent: Wed 2/5/2003 3:01 PM

To: nwohslowhmygodyess@yahoo.com

Cc: energy2@enda.sn; ysokona@enda.sn; youba@hotmail.com; Edan Rotenberg; Harro van Asselt; joyeetagupta@ivm.vu.nl; Marleen van de Kerkhof; Matthijs Hisschemöller; emilio@ppe.ufrj.br; bert.metz@rivm.nl; marcel.berk@rivm.nl; Leena Srivastava; Preety Bhandari; alex.haxeltine@uea.ac.uk; m.hulme@uea.ac.uk

Subject: Re: The HOT Research Protocol is finished

Dear Dr Rotenberg,

Thank you for your email, and for the finalised protocol. I am attaching a draft copy of the questionnaire that we are developing for the regional stakeholder interviews. In this draft we have tried to limit the number of questions, and to simplify the language wherever possible. The questionnaire would be sent to the identified stakeholders with a covering letter explaining the background and purpose of this exercise.

We look forward to hearing from the other partners about the approach adopted for their respective regions, and would welcome their suggestions regarding this questionnaire.

Thanks and warm regards,

Ulka.

Ulka Kelkar
Research Associate and Area Convener
Centre for Global Environment Research
T E R I
New Delhi - 110 003 / India
Tel + 91 11 2468 2100 and 2468 2111 (ext 2315)
Fax + 91 11 2468 2144 and 2468 2145
Web www.teriin.org

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The Hon'ble President of India, Dr A P J Abdul Kalam, will inaugurate DSDS 2003.

Theme: "The Message from WSSD: Translating Resolve into Action for a Sustainable Future"

6-9 February 2003, New Delhi

Details at www.teriin.org/dsds/

2831. 2003-02-19

cc: a.minns@uea.ac.uk

date: Wed, 19 Feb 2003 11:46:00 -0000

from: "Prof B.E. Launder" <brian.launder@umist.ac.uk>

subject: Re: (Fwd) Re: (Fwd) Climate Change/ Environmental Issues

to: Mike Hulme <m.hulme@uea.ac.uk>

<x-rich>Mike:

To respond to your second-paragraph query, Mia began by asking for someone to go against Professor Stott but, I guess, picking up the vibes I put out in my last note to her, she has broadened her possible list of topics. Frankly, I wouldn't myself want to take a strongly 'anti' position on any of the power options whether it be nuclear or even fossil-fuel (because, in the latter case, carbon sequestration offers interesting prospects especially if bolted onto coal-fired power). So, I would have thought that the Tyndall Centre would have most to contribute (and to gain) from an articulate response to Professor Stott.

Brian

Date sent: Wed, 19 Feb 2003 09:01:38 +0000

To: brian.launder@umist.ac.uk

From: Mike Hulme <m.hulme@uea.ac.uk>

Subject: Re: (Fwd) Re: (Fwd) Climate Change/ Environmental Issues

Copies to: a.minns@uea.ac.uk

<FontFamily><param>Times New Roman</param><bigger>Brian,✠

✠

Yes, indeed, Professor Stott is known to me and I have had a couple of radio debates with him in the past. His expertise is biogeography, although he seems to have made a reputation for himself (amongst the media, not the scientists) in disputing the credibility of climate models✠

✠

I will give this some thought and discuss with Asher; by the way, the last email from Mia seemed to be looking for an anti-fossil or an anti-nuclear speaker from Tyndall rather than someone to debate with Stott - or have I mis-read?✠

✠

Mike✠

✠

✠

✠

At 17:41 18/02/03 +0000, you wrote:✠

<paraindent><param>left</param><color><param>0100,0100,0100</param>Mike: ✠

✠

Here's a succession of emails exchanged between Mia Nybrant at ✠

</color>the Scientific Alliance (SA) and myself. While I've attached most of them, hopefully just her latest with this commentary will be sufficient for you to be able to respond. The SA is holding a 1-day discussion at the Royal Institution just a couple of weeks before our event. Though the underlying motivation for their event is somewhat similar to ours it is much broader in its coverage as it's a

whole-day affair); I attach the outline programme that she sent up with her original email. ♪

♪

Anyway, around her third email she asked if the Tyndall Centre could contribute a speaker. I quizzed her as to the role and as you will see from her latest message she wants to have someone to go head-to-head against Professor Stott, who I gather does not believe in global warming. It seems to me that what is best needed to counter his arguments would be a real expert in the science. Since that isn't really Tyndall North's area, I thought I'd check with you whether anyone at UEA would seem just right. Simon (who is simply unavailable on May 6th) commented to me that Stott is a pretty tricky and slippery debater so one would want to be confident that Tyndall could go in with a fire-proof case. ♪

♪

I look forward to your thoughts, ♪

♪

Brian ♪

<color><param>0100,0100,0100</param>----- Forwarded message follows
-----</color> ♪

Dear Brian ♪

Many thanks. ♪ The opening session on "CO2 reduction policy" will focus on ♪
the observational science. We expect Professor Stott to outline some of the ♪
difficulties associated with the prediction of climate change including a ♪
discussion of data collection and computer modelling. We are looking for a ♪
counterpart speaker to present the case for confidence in climate change ♪
data and computer modelling. ♪
The later sessions on renewables, fossil fuels and nuclear will address the ♪
engineering strategies available to achieve CO2 emission reductions. ♪ We are ♪
still looking for speakers to present "The case against nuclear energy: risk ♪
vs. reward" and "The case against fossil fuels" and would be happy if the ♪
Tyndall Centre would prefer to put forward a speaker for one of these ♪
session in preference to "The science behind the policy of CO2 ♪
reduction". ♪

I look forward to hearing from you ♪

Kindest regards ♪

Mia ♪

----- Original Message ----- ♪

> Mia: ♪

> ♪

> I'm forwarding your invitation to some of my colleagues to invite ♪

> their input (to me) on who at the Tyndall Centre might best be able ♪

> to contribute to your meeting. The question of CO2 reduction is ♪

> such a big field and it may be necessary to learn the angle or ♪

> specialty area of Professor Stott before deciding who would best ♪

> complement his talk. Is he concerned with the

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observational ✧
> science of CO2 growth and the environmental impacts;
or is he into ✧
> the engineering/economic strategies for alleviating the
problem? It ✧
> is this last aspect that Tyndall North is especially focused
on. ✧
> ✧
> Brian Launder ✧

> > Dear Professor Launder ✧
> ✧
> > Thank you very much for forwarding the information to
your ✧
communications ✧
> > officer and for providing publicity for our event. ♪ We
will do our ✧
utmost to ✧
> > return the favour. ♪ I will send you copies of the
brochure when it is ✧
ready, ✧
> > which will have the price and details of speakers etc on
it (let's ✧
discuss ✧
> > nearer the time how many copies you would like). ✧
> > However, some of the speakers so far are Professor
Phillip Stott, ✧
University ✧
> > of London, speaking in the session on CO2 Reduction
Policy; Phillip ✧
Wolfe, ✧
> > Renewable Power Association speaking on
Renewables; and Dr Nick Riley, ✧
> > British Geological Survey on Fossil Fuels. ✧
> > I would like to invite you, or appropriate colleague, to
speak on the ✧
> > session on CO2 Reduction Policy and debate the issue
with Professor ✧
Stott. ✧
> > This would include 15-20 minutes presentation followed
directly by a ✧
short ✧
> > Q&A and a more general Panel Discussion at the end
of the day. ✧
> > ✧
> > Please do not hesitate to call me on 020 7484 5094 or
email me with any ✧
> > queries you may have. ♪ I look forward to hearing from
you. ✧
> > ✧
> > Kindest regards ✧
> > ✧
> > Mia ✧
> > ✧
> > ----- Original Message ----- ✧
> > From: "Prof B.E. Launder"
<<brian.launder@umist.ac.uk> ✧
> > To: "Mia Nybrant" <<mianybrant@scientific-
alliance.org> ✧
> > Sent: Tuesday, February 18, 2003 9:21 AM ✧
> > Subject: Re: (Fwd) Climate Change/ Environmental
Issues ✧

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> > ✧
> > ✧
> > > Dear Ms Nybrant: ✧
> > > ✧
> > > Thanks for getting back in touch with me. I will
forward your email ✧
> > > and its attachment to our communications officer
requesting that ✧
> > > he should alert the Tyndall Community to your 1-day
debate. I've ✧
> > > marked the date in my diary in the hope that I will be
able to attend ✧
> > > myself. It would be good to know the names of
speakers. Are ✧
> > > these nearly ready to be disclosed? Also, we would
want to know ✧
> > > of any charges for participating in "Powering the
UK's Future". With ✧
> > > this extra information we'll be happy to give your
event all the ✧
> > > publicity we can. ✧
> > > ✧
> > > Best wishes, ✧
> > > ✧
> > > Brian Launder ✧
> > > ✧
> > > ✧
> > >

> > > > Dear Professor Launder ✧
✧
> > > > Many thanks for your email and I am sorry for the
slight delay in my ✧
> > > > response. We would be delighted to forward any
information about ✧
your ✧
> > > > debate to our members, as well as posting the
details on our ✧
website. ✧
✧
> > > > We are organising a conference on 6 May at the
Royal Institution ✧
> > > > entitled ✧
> > > > "2020 vision -Powering the UK's Future". The
aim is to outline the ✧
> > > > scientific arguments for and against a policy of
CO2 reductions and, ✧
in ✧
> > > > the ✧
> > > > light of the Government's Energy White Paper, to
discuss what role ✧
> > > > different ✧
> > > > forms of energy supply can contribute toward this
goal. I have ✧
attached ✧
> > > > a ✧
> > > > draft outline of the conference for your information
and I would be ✧
very ✧
> > > > interested to hear your comments on it. Please
also let me know if ✧
you ✧
> > > > have ✧

cg2003

cc: "Mike Hulme (E-mail)" <m.hulme@uea.ac.uk>, "Marcel Berk (E-mail)" <marcel.berk@rivm.nl>, "Joyeeta Gupta" <joyeeta.gupta@ivm.vu.nl>
date: Fri, 21 Feb 2003 10:46:21 +0100
from: "Edan Rotenberg" <edan.rotenberg@ivm.vu.nl>
subject: Tyndall's role in the HOT project
to: "Alex Haxeltine (E-mail)" <alex.haxeltine@uea.ac.uk>

Dear Alex,

After meeting with Marcel Berk and Joyeeta Gupta I've been asked to get in touch with you

to start fleshing out what Tyndall's role in the HOT project will look like.

This involves

dealing with two types of scientific support - the background paper which will serve as

input to the meetings, and scientific support at the meetings themselves.

Meetings:

What do you think is the best way to provide scientific support at the meetings?

We hope

that you will be in touch with all of the regional partners in relation to their science

requirements.

Background Paper:

Marcel thought it would be wise if you prepared a draft of the scientific input in advance,

and simply made modifications as you saw fit when the stakeholder surveys came back. Also,

please note that we added another line to the scientific input description in the research

protocol: asking Tyndall to include some general background on vulnerability and adaptive

capacity in the background data. This is because we feel that the discussions, especially

in the south, are going to involve adaptation and a discussion of the costs. Since

vulnerability depends on both the severity of climate change impacts and the adaptive

capacity of those affected some understanding of the scientific discussions on adaptive

capacity and vulnerability will probably be helpful in the regional meetings.

Additionally, we are aware that the European Climate Forum is also planning a one day

conference on defining dangerous climate change to be based on a position paper being

prepared by Bill Hare of Greenpeace. Do you think that his paper could be a good piece

from which to start building the scientific input? Even if not it may be useful in helping

to move along the dialogue at regional meetings (an independent source of ideas that no one

is attached to). Can we get a copy of that paper when it is finished?

Joyeeta would like to schedule a teleconference with you for sometime next week - is the

25th good?

I hope to hear back from you shortly, and wish you well. On a personal note it looks like

I won't be applying to UEA after all - my girlfriend and I talked it over and decided that

we aren't going to move to the UK for 2-5 years b/c we would start settling in and making

contacts very far from home. Nonetheless, thanks for taking the time to talk with me about

UEA, and I look forward to meeting you at some point (thought probably not this time around

- I leave the IVM on March 10th).
Best wishes,
Edan Rotenberg

Edan Rotenberg, Guest Researcher
Institute for Environmental Studies,
Vrije Universiteit
De Boelelaan 1087
1081 HV Amsterdam, The Netherlands
Tel: +31-20-4449562 Fax: +31-20-4449553
E-mail: edan.rotenberg@ivm.vu.nl
[1]<http://www.vu.nl/IVM/research/ihdp-it/>

1403. 2003-02-25

date: Tue, 25 Feb 2003 15:10:09 -0000
from: "Max Beran" <maxberan@oldboot.demon.co.uk>
subject: Tree rings and the Mann hockey stick
to: <k.briffa@uea.ac.uk>

Dear Keith

I deliver courses on global change in Oxford and area and one of the matters that comes up is the Mann hockey stick and its implications (Mann-made climate change:-). It has been given enormous prominence both in terms of its message about the recent and "deep" past, and in terms of its portents. Its use as the take-home message from the policymakers summary of the IPCC-TAR demonstrates this clearly.

I am aware that the detailed form of the curve conflicts with what is known about well attested features of the millennial climate (weak, if any, signatures of medieval warming and the little ice age), but what is exercising me more is what it says about trees themselves (I know it is multi-proxy but as I understand it, dendrochronology rules).

That tree-ring contribution to the temperature reconstruction obviously uses a numerical expression of the sensitivity between temperature and tree ring width/density. I don't have any numbers but if these are anything like the ones you show in Figure 2 of your 1998 Nature paper, then there is an approximate one-to-one between the standardised departures of April-September temperature and tree ring width or density.

Two areas of concern are (a) the situation up to the present, and (b) the future.

The present

Given that the standard deviation of the yearly values of summer average temperature is of the order of 0.5 degree, this is coincidentally about the same difference as between pre-industrial times and now. This implies that there ought to have been a similar one standard deviation growth in tree rings. (Again I've no access to real numbers but I guess we are talking a mm or 3). At an individual site and year this is doubtless well within the noise level, but would be expected to shine through when maintained over a number of years and sites. I tried to compare this with Figure 7 of your 1998 Royal Society paper but got mixed up with whether this shows the annual values of the BAI (as implied by the text) or the annual values of the year-on-year "change-in-BAI" as in the caption. If the former, one might have expected some sort of compound interest pattern, if the latter an even faster growing pattern (compound compound). Perhaps the modesty of the rise is indicative of the reversal of the sensitivity between tree rings and temperature that is visible in the post-1940 data on Figure 6 of the Royal Society paper.

How do you reconcile this reversal in the sign of the relationship between tree growth and temperature, and Figure 6 in general, with the statements elsewhere in the paper saying there has been a non-climatic "enhancement" of tree growth?

If there has indeed been a reversal in the sign of the sensitivity this would imply a very large reduction in NPP as a result of the conspiracy

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between ring width and wood density. One might then ask why this post-1940 sensitivity is not a more reliable basis for backward reconstruction? I know you tend towards non-climatic explanations (notwithstanding my confusion over the direction) but for my money this explanation could be at least as legitimately aimed at the period from 1880 to 1940. Huge proportional changes in land use and industrial pollution in that era make current incursions look relatively speaking benign. Just look at population, agricultural area, industrial outputs and emissions data to see this.

The future

The climate models, bless'em, indicate a temperature increase of the order of less than 5 to more than 10 standard deviations by the 2080s. Accepting the robustness of the sensitivities implicit in the Hockey stick reconstruction (much used to tune and confirm GCM behaviour), that suggests to me that we can anticipate a similar order of growth in tree ring width and density? I can't picture what the standard deviation of the density series might be in relation to the mean, but I would hazard a guess that applying this to the tree ring width alone would lead to a more than doubling of today's BAI. The overall effect on NPP of such a dramatic shift in growth behaviour would surely turn the current 60-ish Gt to well over 100 Gt. If only a modest fraction was turned into NBU this could make a mighty hole in emissions and would be good news at least over the lifetimes of the trees. And all this is would put the benefit of CO2 fertilisation completely in the shade.

Seems to me we have a classic checks-and-balances situation here. The climate modellers (and the policy makers) implicitly accept the tree-ring to climate sensitivity as far as the past is concerned. This bolsters their belief in the forward projections of temperature with all that that implies for impacts and policy. By their own logic, they should then also accept that trees (far and away the dominant living carbon pool) would continue in their positive temperature-driven response and provide a hefty negative feedback acting via the land carbon cycle. In all seriousness though, does anyone really believe trees would respond so dramatically. We'd know about it from physiology and see some signal in latitude clines - as far as I know they don't exist, but you'd know for sure.

So at what point does the tree ring to temperature sensitivity break down? And what might its impacts be on the hockey stick and through that the GCM tuning? Have there been other periods when your post-1940 reversal occurred perhaps due to macroclimatic forces? Could these also account for the discrepancy between the hockey stick and what we thought we used to know about the climate since 1000 AD?

Any thoughts on any of the above would be delightedly received. You may even save a soul from falling into the embrace of the sainted Lomborg!

Max Beran

1 The Croft

East Hagbourne

Didcot OX11 9LS

Tel: 01235 812493

Fax: 0870 054 7384

4735. 2003-03-01

cc: "Mike Hulme" <m.hulme@uea.ac.uk>

date: Sat, 1 Mar 2003 13:17:55 +1300

from: "Xianfu Lu" <xianfu@waikato.ac.nz>

subject: RE: climate change politics in China

to: <miriam.schroeder@student.uni-tuebingen.de>

Dear Miriam Schroeder,

Because I work with Professor Hulme and I am from China, he forwarded to me your enquiry on the WWF Climate Change Scenarios Series for China and international NGO collaborative activities in the area of climate change in the country.

For the WWF climate change scenarios booklet, you could download from

<http://www.cru.uea.ac.uk/~mikeh/research/wwfscenarios.html>. Note that the final version is only available in Chinese, while the English version is a draft only.

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As for other co-operations among international NGOs in China, what I am aware of is the WWF China country office runs certain projects in the area of energy and climate change, with funding from European countries. Details on these projects can be found at <http://www.wwfchina.org/english/loca.php?loca=96>. Another rather active international NGO in China is called LEAD (Leadership for Environment and Development)-China (<http://www.lead.org.cn>). They are now preparing for a few initiatives which address the issue of climate change in one way or another. It would be worthwhile contacting the relevant people at the above organizations and they should be able to provide you some useful information.

Hope this helps.

Xianfu Lu

>Date: Thu, 27 Feb 2003 01:54:17

>To: <m.hulme@uea.ac.uk>

>Subject: climate change politics in China

>From: <miriam.schroeder@student.uni-tuebingen.de>

>X-Mailer: TWIG 2.7.6

>X-Antivirus: checked by Antivir Milter 1.0.0.8; AVE 6.18.0.2; VDF 6.18.0.9

>

>Dear Professor Hulme,

>

>I am a student from the University of Tuebingen, Germany. Currently I am
>writing my master's thesis on the international cooperation in climate
>change politics in China. Taking a political science perspective, I am going
>to analyze how the international cooperation between international and
>Chinese NGOs has advanced the value of climate protection in China.

>

>So far, I have found a lot of literature about the cooperation activities
>among scientists, but little about cooperation among NGOs with regard to
>climate change. The only work I have found from an international NGO's view
>on Chinese climate change is your book "Climate change due to the greenhouse
>effect and its implications for China". Gland, Switzerland: World Wide Fund
>for Nature, 1992.

>

>Unfortunately, it seems not to be available in any university library - at
>least not in Germany. Is there any way you could send it to me maybe as a
>PDF document? Or is it still on store with some WWF office?

>

>Do you have any further suggestions which other literature might be helpful?

>

>Thank you very much for your help.

>

>Best regards,

>

>Miriam Schroeder

5015. 2003-03-03

date: Mon, 3 Mar 2003 13:18:36 -0000

from: "Stephanie Ferguson" <stephanie.ferguson@ukcip.org.uk>

subject: UKCIP news

to: "Stephanie Ferguson" <stephanie.ferguson@ukcip.org.uk>

Dear Colleague

1. Building Knowledge for a Changing Climate initiative and report launch
2. Climate change and the housing industry event
3. Working with Business strategy consultation
4. Working with local government
5. Climate change and demand for water
6. IPCC to consider carbon sequestration and regional climate forecasts in 4th assessment
7. Regional scenario maps on website
8. Communications feedback
9. Forward look
1. Building Knowledge for a Changing Climate initiative and report launch

A joint £2-million EPSRC/UKCIP research initiative on the impacts of climate change on the built environment is to be launched by Minister for Environment and the

Agri-environment, Rt Hon Michael Meacher MP, today 3 March at an event held in conjunction with the

Institution of Civil Engineers. A report outlining the six research projects and wider

research agenda, developed with stakeholders, is also published today. For a hard copy,

please contact UKCIP, or see [1]www.ukcip.org.uk/built_enviro/built_enviro.html

for more information and to download.

2. Climate change and the housing industry event

UKCIP are among the speakers at a CIRIA event in London on 4th June, to be launched by Rt

Hon Michael Meacher MP. Other speakers are from Office of the Deputy Prime Minister, the

Association of British Insurers, and Arup. For further information, see [2]www.ciria.org.

3. Working with Business strategy consultation

UKCIP wants to engage a wider section of the business community, so that the many different

sectors can start to consider how they will be affected by climate change. To build on

existing relationships, we plan to develop identify new sectors and create a new section of

our website, accessible from the home page. We would welcome your views about our draft

strategy, recently discussed with the government's Advisory Committee for Business and the

Environment (ACBE). See [3]www.ukcip.org.uk/business/business.htm

4. Working with local government

UKCIP and the Innovation and Development Agency (IdeA) have agreed to produce information

for local authorities on climate change impacts on the full set of tasks undertaken by

them. UKCIP and IdeA will work with the LGA, ODPM, the devolved administrations, and the

members of the "Councils for Climate Protection" to produce a checklist of impacts against

which local authorities can test their actions and policies. This follows the drafting of

an initial document by Andy Reisinger, on secondment with UKCIP from the NZ government last

December. If you would like to be involved in writing the next draft, contact Chris West or

Steve Waller at IdeA ([4]steve.waller@idea.gov.uk).

5. Climate change and demand for water

A new study on the demand for water has been completed and is now available via our

website: [5]www.ukcip.org.uk/water_demand/water_demand.html. Funded by Defra, the study was

undertaken to ensure that climate change was considered not just from a water supply

perspective, but also in relation to demand. A stakeholder forum to discuss the findings

will be held in the Spring. Consideration is being given to production of a summary report.

6. IPCC to consider carbon sequestration and regional climate forecasts in 4th assessment

The politically sensitive issues of carbon sequestration and regional climate forecasts are

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to form part of the fourth assessment report by the Intergovernmental Panel on Climate Change (IPCC). The assessment, due in 2007, was discussed by some 300 IPCC members in Paris on 19-21 February. Sequestration was chosen as the subject for a special report, separate from the main assessment. After the meeting, Rajendra Pachauri, director of the Tata Energy Research Institute in New Delhi and chair of the IPCC, confirmed that more detailed regional models of the impact of climate change would be considered by the assessment's authors. For more information, see [6]www.ipcc.ch.

7. Regional scenario maps on website
New maps based on UKCIP02 climate scenario data are now available on the website. They provide a regional focus of the UK-wide maps seen previously. See [7]www.ukcip.org.uk/climate_change/future_uk.html.

8. Communications feedback
Brief update on your responses: 65 returned. All like e-news! Fact sheets for different sectors are also in demand, and we will be getting on with these in the coming months, in line with our Working with Business strategy. Mixed views as to whether hard copies of reports are needed, but people are generally more positive about downloading material than they were a couple of years ago. Media relations is not a general priority, but useful to raise awareness of climate change or to help meet specific objectives. It varies as to how much people have seen references to UKCIP. Discussion groups are not much in demand - you don't have time!

This will be useful information for the new communications manager, so many thanks for returning the forms. We'll contact you directly if you made specific comments or expressed interest in training or discussion groups.

9. Forward look
BBC Radio 4's Archers is due to include a climate change theme in the coming weeks, when Spring comes early. Not sure of dates, but regular listeners will enjoy. Climate adaptation: risk, uncertainty and decision-making (launch date tbc please contact UKCIP if you're waiting to use this report)
West Midlands scoping study launch (Summer 03)
We look forward to meeting you at one of the above events or hearing from you via phone or email.

Regards
Chris West
Director, UKCIP
Based at the University of Oxford and funded by DEFRA, UKCIP helps organisations assess how they will be affected by climate change, so they can prepare for its impacts. UK Climate Impacts Programme (UKCIP) Draft Strategy for Working with Business
March 2003

INTRODUCTION
UKCIP has successfully worked with some business sectors (e.g. water and insurance) over a number of years to raise awareness of climate change. See Annex 1 on existing business/UKCIP links. The current issue is how to engage with the wider business

community

that has its own specific needs and diverse interests.

Aims

To develop a strategy to help meet the business sector's needs for information on climate

change impacts, and on the adaptation options available.

1. It should complement the work of the Advisory Committee on Business and the Environment

(ACBE) and be consistent with ACBE's recommendations to Government;

2. It should support UK domestic policy on climate change impacts and adaptation by synergy

with the inter-departmental process and other activities as appropriate;

3. It should address climate change impacts as part of the wider sustainable development

issue;

4. It should seek to address issues identified as important by business itself in a way

accessible to business people;

5. It should increase the capacity of business to address its own needs;

6. It should ideally use existing mechanisms for delivery that have earned some trust from

business;

7. It should focus as much on engaging the community and on raising awareness as on the

development of research agendas.

ONGOING ACTIVITIES

UKCIP has been working with the private sector for a number of years. This includes:

a. Almost 50 private sector organisations have funded, or been on the steering committees

of regional scoping studies;

b. There are water companies on the steering committees of the studies Regional Integrated

Assessment of Climate Change Impacts (REGIS) and Climate Change and the Demand for Water

(CC:DEW);

c. The Crown Estate is a major funder of the Marine Biodiversity and Climate Change project

(MarClim);

d. Anglian Water and Nottcutts Nurseries are funders for the Gardens study. A list of

businesses with UKCIP links is provided in Annex 1.

Water Industry

UKCIP is working with the water companies on both the supply and demand sides. The industry

has used UKCIP climate change scenarios to look at water supply changes, and UKCIP and the

water companies are engaged in a study CC-DEW looking at water demand changes due to

climate change. The results of this study will be used in the fourth periodic review of

water prices. A final stakeholder workshop is scheduled for Spring 2003 to coincide with

the publication of the CC-DEW report. Subject to resources, UKCIP intends to produce a

separate leaflet for distribution based upon the executive summary of the report.

Built Environment

UKCIP has entered into a major three-year research initiative, in partnership with the

Engineering and Physical Science Research Council (EPSRC), which aims to provide an

assessment of the impacts of climate change on the built environment. The

programme will address impacts on: urban areas; the construction sector; transport infrastructure; the energy sector; coastal and river engineering and water resources; and cultural heritage.

This provides a major new opportunity in developing research linked to the needs of stakeholders in this very large sector. The initiative represents the start of a new mode of operation by the research councils, and UKCIP will need to continue to be very actively engaged within the process to ensure ongoing stakeholder involvement.

Businesses have been engaged in the following ways: The 2001 research fora in London and Edinburgh were attended by about 180 built environment professionals. Of these, approx 80% of the London attendees were from the private sector, compared to 30% at the Edinburgh event. Railtrack, Scottish Water, National Grid, the Association of British Insurers and Buro Happold were on the shortlisting panel that reviewed expressions of interest to EPSRC.

It was made clear to researchers that projects would only be funded under the initiative if stakeholders were committed to being project partners. The projects chosen by EPSRC include numerous private sector organisations as collaborators, from engineering firms, energy producers and distributors, transport companies, water companies, port operators, professional institutes and the insurance industry. These have all committed time and in some cases, data and resources, to the projects. Details of the projects, in the context of a wider research agenda, are outlined in a short report available from UKCIP.

A Stakeholder Forum, to be set up by UKCIP, will oversee the portfolio will have a majority of private sector organisations.

Tools, Data and Communication

Guidance on handling risk and uncertainty in decision-making will be published in Spring 2003;

A methodology for costing the impacts of climate change will be published shortly afterwards;

UKCIP will prepare a document that describes how to undertake a UKCIP study and use the UKCIP toolkit;

UKCIP is preparing a checklist for individual organisations/companies to use to assess their vulnerability to climate change, to be available via the UKCIP website. (This would complement ACBE's "Sector specific risk and opportunities from predicted changes to UK climate"). It will be illustrated with concrete examples of impacts, either already present or anticipated and planned for.

These tools should prove very useful for UKCIP stakeholders and in particular should help to engage the business community, as the risk and uncertainty guidance is framed around the "normal" project decision-making cycle that many businesses use; the costings methodology

will enable businesses, for the first time, to begin to attach a monetary value to climate change impacts and compare them to the costs of adaptation.

PROPOSED COURSE OF ACTION

A three point course of action is proposed:

1. UKCIP will work with a selected Pilot Partner Group of Trade Associations to enable them

to deliver useful, accurate and sector-relevant information to their members;

2. Repeat the methodology developed in 1 to access Other Routes into Business;

3. Manage the ongoing programme;

1. Pilot Partner Group of Trade Associations

UKCIP are working with the Department of Trade and Industry, the Confederation of British Industry, and the devolved administrations to identify a small number (20?) of Trade

Associations suitable as UKCIP partner organisations, based on previous experience and expressed interest.

UKCIP would like to build on the work already done by ACBE in this area, possibly using the

same group of Trade Associations that ACBE has had contact with.

The intention is then to ask the Pilot Partner Group of Trade Associations to each identify

an individual who would come to UKCIP (for a few days together or over several visits?)

for:

a. An introduction to the UKCIP programme;

b. Training in climate impacts, UKCIP methods, quality control;

c. Assistance with mining existing study reports and library materials;

d. Assembly of a set of impacts/adaptation options relevant to their business.

The partners would then return to their host Trade Associations to write/lead a team

writing business- and sector-friendly factsheets. UKCIP will check the technical content of

the factsheets. These would then be distributed by the Trade Associations. Many of these

bodies already provide fact-sheets; some will have provided climate change fact-sheets,

often from a mitigation viewpoint. Others already have house newsletters, magazines,

websites, etc. The issue of intellectual property rights and branding will need to be

addressed.

At this stage it is probably best not to emphasise the ensuing links from any preliminary

scoping study to a more expensive and off-putting full research project, but a factsheet

would advise that UKCIP can help with further work. Over-emphasis on the science and

research interest will put off many business readers.

For this first group, individual or very limited numbers would be hosted at each time, in

order to build up experience. Organisers of the Hampshire and Isle of Wight Summit

requested such access and help with producing fact-sheets on 14 issues to be covered at the

Summit, so we have some experience of how this can work. A parallel exercise to build

understanding of climate change impacts and increase adaptation capacity among local

authorities is being undertaken by UKCIP in conjunction with the Improvement and Development Agency and the Local Government Association. Useful lessons should

be learnt

from these two exercises.

2. Other Routes into Business

Once the methodology is thus trialled and evaluated, further groups of bodies could be

selected or invited to undertake similar work. At a national level, such bodies are the

Trade Association Federation, British Chambers of Commerce, Business in the Community,

Trade Unions, large individual companies and others; a further set of Trade Associations

and the professional Institutes would cover other business sectors while at a regional/local level there are Regional Development Agencies, Chambers of Commerce, and

other business-focussed groups. Individuals engaged in the first pilot group could provide

the basis for an advisory panel, if this was useful.

This stage would have to be decoupled physically from UKCIP premises to allow more

individuals to participate, but each would receive less assistance, and arrangements for

wider access to existing material would have to be provided.

3. Manage the ongoing programme

At this stage it would be worth approaching all Trade Associations and Chambers of Commerce

to publicise the existence of a package consisting of:

a. Guidance on the training available;

b. Templates for paper and web-based fact-sheets;

c. Core presentation material and a Q&A briefing for use by the named person;

d. Information on the network of partners who have undertaken this task, and who would be

able to assist further iterations;

e. Limited access to a named individual at UKCIP for assistance. This package could, given

sufficient resources, form another tool in the UKCIP toolbox entitled "How to assemble a

climate change impacts factsheet for your members".

There would be a gradual and open process of moving from tight UKCIP control of quality

where its own brand is involved to a situation where partners have more autonomy to produce

their own brand of product.

Continuing enquiries about producing further member fact-sheets will be referred to the

tool above, while UKCIP will deal constructively with any intention expressed to undertake

further work. The provision of ongoing assistance to the network of partners will have

resource implications. In parallel with this awareness-raising stream, there will be

opportunities to press for some sectoral or generic business-relevant studies into the

impacts of Climate Change and Adaptation Options.

COMMUNICATIONS STRATEGY TO SUPPORT WORK WITH PRIVATE SECTOR

UKCIP sees its work with business as being primarily about communication, and much is

already included above. However a number of specifics can be highlighted:

a. An initial meeting with the Trade Association would be needed to gain their support;

b. UKCIP would supply the UKCIP logo for production by the trade association. Factsheets

would be branded by the trade association;

c. UKCIP and each Trade Association would issue a joint news release to their trade media

to announce that they were working together to provide information to a given sector, and that a fact-sheet and advice was available from both;

d. The UKCIP e-newsletter will announce plans for the initiative and publicise the factsheets as they become available. The Trade Association would do the same via their own newsletters and communications;

e. A new business page on the UKCIP Website will be created in the first instance, with links to ACBE and the Trade Association factsheets, outlining our plans to work in partnership with businesses, explaining that we recognise that different sectors will have different levels of concern and impacts.

f. Depending on the level of take-up, we could develop the site to become a gateway for business, with links to each partner organisation's site;

g. UKCIP will provide introductory leaflets to the Trade Association, and reference copies of other tools and reports;

h. Consideration will be given to a display panel outlining these partnerships. UKCIP could provide material for Trade Association production of display panels on climate change impacts and adaptation;

i. As UKCIP stakeholders, the partner Trade Associations would receive details of events and the UKCIP e-newsletter and would be invited to user fora. The format for the next user forum will be determined in 12 months' time, but a business slant has already been proposed.

Annex 1 - Some Businesses and Organisations involved in UKCIP studies

Acordis
 Anglian Water
 Arla Foods Plc
 Arkleton Trust
 Asda Stores Limited
 Associated British Ports
 Association of British Insurers
 Association of Electricity Producers
 Atkins
 Avesta Polarit
 AXA Insurance
 Cadbury Plc
 Carlton TV
 CBI
 Country Land & Business Association
 Country Life Magazine
 Crown Estate
 Duchy of Cornwall
 East Midlands Airport
 Federation of Small Businesses
 Food Technopole
 Humber Growers Ltd
 London Electricity
 London Tourist Board
 Manchester Airport
 Marsh UK Ltd
 Midlands Environment Business Club
 NatWest Bank
 Northumbrian Water

cg2003

North West Water
Norwich Union
Notcutts Nurseries
PROSPER
Rolls Royce plc
Severn Trent Water
Shepherd Building Group
South West Tourism
South West Water
St George plc
SWEB
SWEL
Tarmac plc
Thames Water
Toyota UK
TXU Europe Power Ltd
Wessex Water
Westcountry Television
Westcountry Tourist Board
Wilkinson
Welsh Water
Yorkshire Electricity
Yorkshire Tourist Board
Yorkshire Water Services

Stephanie Ferguson
Administrative Assistant
UK Climate Impacts Programme
Union House, 12-16 St Michael's Street, Oxford OX1 2DU
Tel. 01865 431254 Fax. 01865 432077
email [8]stephanie.ferguson@ukcip.org.uk
[9]www.ukcip.org.uk

2140. 2003-03-07

date: Fri, 7 Mar 2003 13:55:53 -0000
from: "Nigel Arnell" <n.w.arnell@soton.ac.uk>
subject: Re: Policy Stakeholders and Tyndall
to: "Mike Hulme" <m.hulme@uea.ac.uk>

Mike,
I think John has a good point here: profile-raising is important, and if we can convene a few "for information.." briefing sessions (as I think we discussed earlier), it would be very helpful. Perhaps as a first stage we could try to find out who the key UK policy stakeholders are in each department, and decide on how best to approach them (or even whether we are already doing enough..).

I've just come off the DEFRA web site climate change pages - and there is no link to Tyndall (even though there are links to Hadley and CRU)!

Regards
Nigel

-- Original Message -----

From: [1]Mike Hulme
To: [2]alex.haxeltine@uea.ac.uk ; [3]h.j.Schellnhuber@uea.ac.uk ;
[4]n.adger@uea.ac.uk ;
[5]N.w.Arnell ; [6]simon.shackley@umist.ac.uk ; [7]a.watkinson@uea.ac.uk ;
[8]jenkins@fs5.ee.umist.ac.uk ; [9]j.kohler@econ.cam.ac.uk ;
[10]kevin.anderson@umist.ac.uk
Sent: Friday, March 07, 2003 10:03 AM
Subject: Fwd: Policy Stakeholders and Tyndall

cg2003

Dear All,

An interesting note from John Murlis. I would be interested in your thoughts.

We either take this seriously or else interpret it as unwarranted heavy-handedness and bias from David Warrilow. I don't think DfT, DFID, OST or even FCO would share this view given the interactions we have had recently, and even another part of DEFRA (flooding/coastal) given Andrew's recent conversations. DTI I couldn't comment on. But

are we being complacent?

Your views and reactions are invited.

Mike

From: "John Murlis" <[11]john.murlis@btinternet.com>

To: "Mike Hulme" <[12]m.hulme@uea.ac.uk>

Subject: Policy Stakeholders

Date: Thu, 6 Mar 2003 15:58:23 -0000

X-Mailer: Microsoft Outlook Express 6.00.2800.1106

Mike,

I had lunch yesterday with David Warrilow and we discussed the progress of hat Tyndall

Centre. He is very supportive of the work you are doing, but feels that some of the

policy stakeholders do not have sufficient contact with the Centre. The consequence of

this, he fears, is that the Centre is not recognised as essential and does not figure

highly in current awareness. When the Centre comes up for refunding, it will be

important that key policy customers, in DEFRA, DTI, DFID and FCO, for example, recognise

the Centre and rate its work highly.

This links to my concern that we have not been able to fund sufficient agenda setting

activities. With agenda setting work there would be a natural connection to the policy

communities and as yet I feel we have not engaged sufficiently at a high level.

With this in mind, may I suggest a round of activities to engage this very important

constituency? I would be very happy to help with a charm offensive in Departments. It

would clearly need planning, but I suggest that you start by identifying someone as your

"Policy Ambassador" and set a senior level meeting with key policy contacts to be held

before the July Annual Forum.

Please give me a call if you wish to discuss.

Regards,

John

John Murlis

PhD DIC FRES FRMS

41, Royal Crescent,

London W11 4SN

Telephone +44 (0)20 7602 0161

Facsimile +44 (0)20 7603 9165

Mobile +44 (0)7785 745 452

[13]john.murlis@btinternet.com

2550. 2003-03-07

date: Fri, 7 Mar 2003 15:13:38 +0100

from: "Marjan Minnesma" <marjan.minnesma@ivm.vu.nl>

cg2003

subject: FW: Latest version of ENSEMBLES WP structure

to: "Bert Metz" <bert.metz@rivm.nl>, "Mike" <m.hulme@uea.ac.uk>

Dear Mike and Bert,

I hope you had some chance to sit together and think about how to organise or get together

to organise workpackage 2 of EFIEA. I would be happy to meet you anywhere in Europe, and

preferably in the coming month, as I would not like to loose too much time in getting some

things off the ground before the summer.

Please read Richards remarks as well, which are in a way indirect criticisms on EFIEA, and

see if it is of any use for us.

Friendly

Marjan

Mrs. mr.drs. M.E. Minnesma MBA

Institute for Environmental Studies (IVM)

De Boelelaan 1087

1081 HV Amsterdam

tel: +31 20 444 9597

fax: +31 20 444 9553

www.vu.nl/ivm

-----Original Message-----

From: Bettina Menne [SMTP:BME@who.it]

Sent: Friday, March 07, 2003 12:29 PM

To: 'Richard Tol'; Bettina Menne

Cc: Marjan Minnesma

Subject: RE: Latest version of ENSEMBLES WP structure

Richard for your explanation:

Within the European Ministerial Conference cycle I have to organize at the next Environment and Health Ministries conference (Budapest, June, 2004) a ROUND-TABLE on energy, sustainable development and health.

For this I am in charge of the development of a short "background document" with evidence based information on this topic.

The issue is interesting for us under four aspects:

How does energy generation, distribution and transmission affect human health ?

-Internalization of external costs

-Includes high concerns such as EMF, nuclear waste, etc

How do current and future energy policies affect health?

-HIA and promotion of win-win strategies

How can we deal with energy poverty?

-E.g. fuel poverty initiative in the United Kingdom

How can the health sector become more energy efficient?

Hope this helps

Thanks again

Bettina

-----Original Message-----

From: Richard Tol [[1]mailto:tol@dkrz.de]

Sent: 07 March 2003 12:17

To: Bettina Menne

Cc: Marjan. Minnesma@Ivm. Vu. Nl

Subject: RE: Latest version of ENSEMBLES WP structure

> Dear Richard first of all do you know of any project which looks more into > details at energy impacts and security?

I've been trying to push EFIEA2 in that direction -- but, although most people see the connection between oil and security, and between oil and climate, so far most people are blind to the connection between oil and climate -- silly really -- a lot of Arabs really hate us, and the only thing that restrains them is that we buy their stuff -- if we decide to use solar power instead, there'll be economic misery and much less restraint in Arabia there's also an overlooked link between dislocated people (e.g., sea level rise) and terrorism

4730. 2003-03-09

date: Sun, 09 Mar 2003 12:03:17 -0800
from: Earth Government <earthgov@shaw.ca>
subject: Press release from Earth Government

Press release from Earth Government
FOR IMMEDIATE RELEASE

This Press release from Earth Government is found at

[1]<http://members.shaw.ca/earthgov/HNewsPR04.htm>

Let it be Peace within Earth Community

March 8th, 2003

To all Peoples of the Earth,

War is the greatest violation of human rights that one people can inflict on another. It

brings deaths and injuries, starvation, diseases, millions of people losing their homes and

livelihoods, and massive destruction of property. Children and teenagers are placed in

internment camps, and several are often forced to serve as soldiers. War not only corrupts

the morals of soldiers, it leads to a decline in the morality of the whole nation.

Political and military leaders are always convinced that their particular war is

justified. From their point of view, there are several reasons to go to war: loyalty to

allies, religion, a thirst for power, greed, ancient grievances to be settled, or the desire to

alleviate suffering among their people. A non-violent settlement to a conflict would

always be more advantageous. War is self-defeating because it cannot secure what it sets out

to achieve, protection against attack. The hatred for the enemy whipped up by war and the

desire for revenge among the losers leads to an accursed vicious circle from which there

is no escape. The difference between aggressive and defensive, or just and unjust

wars, is

ridiculous. They are tags each side adopted to suit its interests. War and militarism

destroy civil liberties within a nation.

What happens to a person's conscience when he/she wears the uniform of the soldier? It

is enslaved to the state. He must kill when ordered. No government, whether democratic or

despotic, can allow the soldier to decide what to do according to his conscience. That

would undermine discipline and the power to fight.

The Earth Community claims that everyone on Earth should be able to live in peace. This

peace movement is about courage. Not the courage it takes to go into battle but the

courage to organize resistance to war when a bloody taste for it inflames the world, and

the threat of prison in a nation where the human rights and freedom of expression have

diminished significantly. It is about the courage to say NO to the war industry. It is

an industry that destroys life on Earth, corrupts society, and violates

morality. Military

intervention in the affairs of other nations is wrong. There are other ways, there are peaceful ways, ways that are not based on profit-making and the gain of power for itself.

We are

conscientious objectors, non-resistants.

There are several reasons for objecting to war: religious, moral and political. People

have different degrees of refusal to go to war. Absolute pacifists such as the Earth

Community

will not cooperate with any preparation for war as decided by the war industry, let

alone war itself. Others will accept some kinds of service so long as they are not forced to fight.

And others are willing to fight in "just wars." The idea of the "just war" began with

St. Augustine. Late in the fourth century he argued that the good Christian, barred from

doing

violence on his own behalf, could take arms in a war that was just. Several theologians

now say that the standards for a just war are:

a) War must be the last resort and used only after other means have failed.

b) War must be declared to redress rights actually violated or for defence against

unjust demands backed by the threat of force. It must not be fought simply to satisfy national pride or to further economic or territorial gain.

c) The war must be openly and legally declared by a legal government.

d) There must be a reasonable chance of winning.

e) The means used must be in proportion to the ends sought.

f) Soldiers must distinguish between armies and civilians and not kill civilians

in purpose.

g) The winner must not require the utter humiliation of the loser.

It can be debated whether any war has ever satisfied all these reasonable conditions.

The people of the Earth Community are dedicated in using our resources to resolve

conflict, promote democracy, and fight hunger, terrorism, disease, and human and Earth

rights

abuses. In order to bring about the event of peace, the Earth Community is offering

other good organizations around the world to work together to bring warring parties to

peace.

We can accomplish this task by concrete actions such as:

a) Tracking armed conflicts within and between nations around the world and

offering assistance in dispute resolution;

b) Promoting human and Earth rights and democracy;

c) Monitoring democratic elections; and

d) Educating the public about the advantages of a peaceful solution to any

conflict.

The Earth Community Organization (ECO) has given back responsibility to every citizen on

Earth. Everyone shares responsibility for the present and future well-being of life within

Earth Community. We will work together in working out sound solutions to local and global problems. It would be wrong and dishonest to blame it all on the leader of a country.

Most problems in the world must find solutions at the local and global community levels

(and not assume that the leader alone is responsible and will handle it). There is a wisdom

in the ways of very humble people that needs to be utilized. Every humble person

deserves to have ideas respected, and encouraged to develop his or her own life for the better.

Sound solutions to help manage and sustain Earth will very likely be found this way.

Everyone can help assess the needs of the planet and propose sound solutions for its proper

management, present and future. Everyone can think of better ideas to sustain all life

on Earth and realize these ideas by conducting positive and constructive actions. When

there is a need to find a solution to a problem or a concern, a sound solution would be to

choose a measure or conduct an action, if possible, which causes reversible damage as opposed

to a measure or an action causing an irreversible loss; that is the grassroots process.

The Earth Community Organization can help people realized their actions by coordinating

efforts efficiently together.

The responsibility of a peacemaker is to settle differences through compromise and

negotiation before they erupt into violence. Conflicting views do not have to bring about

fighting. War is an irreversible solution to a problem. War is never an appropriate solution to resolve a conflict.

The worst environmental degradation happens in wars. Farm products in fields and

livestock are abandoned, there is no more control on toxic wastes, and water, air, and land are

polluted. People are displaced and feel no longer responsible for the quality of life in

their communities. Historically, the industrialized nations have caused the most damage to

the environment, with their careless technology and policies. Emissions from factories and

vehicles have caused ozone depletion and acid rain. Leaders of the wealthier nations must

be willing to accept responsibility for past mistakes and to help pay the financial burden

for environmental protection of the developing nations. This is the most damaging conflict

of interests between the rich industrialized countries and those that are poor and

struggling just for existence. The Earth Community must help wealthy and poorer

cg2003

nations
reach a
better understanding of each other's needs. All aspects are interrelated:
peace, human
rights and the environment. The poor is more concerned with ending starvation,
finding a
proper shelter and employment, and helping their children to survive.
Environmental
issues become meaningless to the poor. In reality, all concerns are
interrelated. As soon
as the
environment is destroyed beyond repair, human suffering is next. Ecology has
no
boundaries. All nations suffer the effects of air pollution, global warming,
loss of
biodiversity, soil
erosion, acid rain, ozone depletion, silting of streams, and countless of
other
environmental problems. This was the reason for proposing to the Earth Community
the Scale
of
Human and Earth Rights.
The Earth Community wants to provide a forum where international conflicts
could be
argued and resolved peacefully. Because of hatred and mistrust, disputing
parties always
find
it difficult to express constructive ideas or proposals. A face-to-face
meeting may not
even be possible. The Earth Community offers to be a trusted third party that
would carry
ideas back and forth, put forward new proposals until both sides agree. When
both
parties feel they have gained more than they have lost from the process, the
outcome is a
win-win settlement for peace.
May the DIVINE WILL come into our lives and show us the way.
May our higher purpose in life bring us closer to the Soul of Humanity and God.
Germain Dufour, President
[2]Earth Community Organization (ECO) and [3]Earth Government
Website of the Earth Community Organization and of Earth Government
[4]<http://www.telusplanet.net/public/gdufour/>
[5]<http://members.shaw.ca/earthgov>
Email addresses
[6]gdufour@globalcommunitywebnet.com
[7]gdufour@telusplanet.net
[8]earthgov@shaw.ca

1502. 2003-03-11

date: Tue, 11 Mar 2003 18:05:14 -0000
from: "Prof B.E. Launder" <brian.launder@umist.ac.uk>
subject: (Fwd) Re: INQUIRY INTO "THE FUTURE OF AVIATION" From: Richard
to: M.hulme@uea.ac.uk, n.jenkins@umist.ac.uk, h.j.schellnhuber@uea.ac.uk,
simon.shackley@umist.ac.uk
<x-rich><color><param>0100,0100,0100</param>The Royal Academy of Engineering
requested my input on "The
future of Aviation". I provided the response that follows. I realize
that most of it wasn't really in the area or from the point of view
they were seeking input but I don't think that matters.
Brian

----- Forwarded message follows -----

</color>From: <color><param>0000,0000,8000</param>Prof B.E.

cg2003

Launder <<brian.launder@umist.ac.uk></color>
To: <<Hearns@raeng.co.uk></color> <0000,0000,8000</param>"Hearn Sylvia (Miss)"
<0000,0000,8000</param>Re: INQUIRY INTO "THE
<0000,0000,8000</param>Subject: <0000,0000,8000</param>FUTURE OF AVIATION" From: Richard Ploszek</bold></color>
Copies to:
<0000,0000,8000</param>Response@raeng.co.uk</color>
Send reply to:
<0000,0000,8000</param>brian.launder@umist.ac.uk</color>
Date sent: <0000,0000,8000</param>Tue, 11 Mar 2003
15:35:16 -0000</color>

<bold><italic>Dear Mr Ploszek:
I'm replying in my role as regional director of the Tyndall
Centre for Climate Change Research. My concern is here
exclusively concerned with reducing the CO2 emission from
aircraft. While at present the proportion of CO2 associated
directly with aircraft movement is small, because air traffic is
growing and there is at least a vision for reducing CO2 from
automobiles, it is likely that releases from aircraft will become
a substantial contributor in the next decade if nothing is done.
* A major "driver" of the present pattern of evolution is that
there is no tax levied on aviation fuel. This is a problem that
MUST be tackled, desirably at a world level but at least within
a European framework.
* Moreover, if the Government introduces a carbon tax, that
should also be applied to aircraft movements. [Again
international agreements are required]
* The above development would provide a stimulus to the
development of fail-safe designs for hydrogen-fuelled aircraft.
* It seems to me that more should be done to develop modes
of aerial transport that consumed much less fuel per passenger
mile than existing schemes (whether or not one is adopting
hydrogen as a fuel). Perhaps there is scope for the return of the
airship ...or perhaps some hybrid, intermediate version
between an airship and a conventional aeroplane. Of course,
this would also greatly reduce the problem from the other
main pollutant source with aircraft: noise! No long runways
would be needed either.
* In connection with the above, with leisure transport,
especially high speed is not of great advantage since at
present, for European flights, actual flying time is less than
(often MUCH less than) one-fifth of the total journey time from
home to hotel. So a comfortable airship flight at just 400km/hr
might be very attractive, especially if the fuel savings were
properly factored into the ticket price.
* Although the photographs released of the NASA solar-
powered aircraft are both intriguing and impressive, it seems
likely that the commercial implementation of such a scheme is
a long way off. Nevertheless, it's surely a goal to be pursued.
* Finally, more need to be done to ELIMINATE short internal
flights in this country by providing
<u>highspeed</u></italic></bold> <bold><italic>rail links,
including direct links between airport terminals.
</italic></bold>> House of Commons Transport Select Committee
> Inquiry into "The Future of Aviation"
>
> The House of Commons Transport Select Committee has been
holding an inquiry
> into "The Future of Aviation" since November last year, the terms
of
> reference for which can be found at
>

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www.parliament.uk/parliament_committees/transport_committee/tc

221102.cmf via

> the Parliament website.

> The Committee has decided that it should deal with the problem of aircraft

> noise and environmental pollution separately from the mainstream of the

> inquiry and has asked The Academy to submit evidence on the subject.

> In particular, the Committee would like to hear what Fellows consider the

> future trends in aircraft design are and how these might impact on forecasts

> of environmental damage. This is in particular reference to noise on

> take-off and approach, and fuel consumption. It would be helpful if you

> could indicate what you believe the key drivers are for improvements and

> whether these conflict with commercial goals. The Committee would also be

> interested on your views on likely timescales for new technologies to be

> introduced and to achieve significant market penetration. Local air quality

> around airports is also a consideration and the Committee would also be

> interested to hear your views on this subject.

> I would be pleased to receive your comments by Friday 28th March, or sooner

> if possible via e-mail to responses@raeng.co.uk. Should you be unable to

> respond, for whatever reason, I would like to thank you for considering this

> request.

>

> Richard Ploszek

> Assistant Manager, Engineering Policy

<color><param>0100,0100,0100</param>----- End of forwarded message -----</nofill>

Professor B. E. Launder, FRS, FREng

MAME Department,

UMIST, PO Box 88,

Manchester M60 1QD, UK

Telephone: 0161-200-3701

Fax : 0161-200-3723

</x-rich>

1889. 2003-03-11

cc: k.briffa@uea.ac.uk,jto@u.arizona.edu,drdendro@ldeo.columbia.edu,keith.alverson@pages.unibe.ch,mmaccrac@comcast.net,jto@u.arizona.edu,mann@virginia.edu

date: Tue, 11 Mar 2003 08:14:49 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Fwd: Soon & Baliunas

to: Phil Jones <p.jones@uea.ac.uk>,rbradley@geo.umass.edu,mhughes@ltrr.arizona.edu,srutherford@gso.uri.edu,tcrowley@duke.edu

Thanks Phil,

(Tom: Congrats again!)

The Soon & Baliunas paper couldn't have cleared a 'legitimate' peer review process

anywhere. That leaves only one possibility--that the peer-review process at

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Climate

Research has been hijacked by a few skeptics on the editorial board. And it isn't just De

Frietas, unfortunately I think this group also includes a member of my own department...

The skeptics appear to have staged a 'coup' at "Climate Research" (it was a mediocre

journal to begin with, but now its a mediocre journal with a definite 'purpose').

Folks might want to check out the editors and review editors:

[1]<http://www.int-res.com/journals/cr/crEditors.html>

In fact, Mike McCracken first pointed out this article to me, and he and I have discussed

this a bit. I've cc'd Mike in on this as well, and I've included Peck too. I told Mike that

I believed our only choice was to ignore this paper. They've already achieved what they

wanted--the claim of a peer-reviewed paper. There is nothing we can do about that now, but

the last thing we want to do is bring attention to this paper, which will be ignored by the

community on the whole...

It is pretty clear that thee skeptics here have staged a bit of a coup, even in the

presence of a number of reasonable folks on the editorial board (Whetton, Goodess, ...). My

guess is that von Storch is actually with them (frankly, he's an odd individual, and I'm

not sure he isn't himself somewhat of a skeptic himself), and without von Storch on their

side, they would have a very forceful personality promoting their new vision.

There have been several papers by Pat Michaels, as well as the Soon & Baliunas paper, that

couldn't get published in a reputable journal.

This was the danger of always criticising the skeptics for not publishing in the "peer-reviewed literature". Obviously, they found a solution to that--take over a journal!

So what do we do about this? I think we have to stop considering "Climate Research" as a

legitimate peer-reviewed journal. Perhaps we should encourage our colleagues in the climate

research community to no longer submit to, or cite papers in, this journal. We would also

need to consider what we tell or request of our more reasonable colleagues who currently

sit on the editorial board...

What do others think?

Mike

At 08:49 AM 3/11/2003 +0000, Phil Jones wrote:

Dear All,

Apologies for sending this again. I was expecting a stack of emails this morning

in

response, but I inadvertently left Mike off (mistake in pasting) and picked up Tom's

old

address. Tom is busy though with another offspring !

I can I looked briefly at the paper last night and it is appalling - worst word

think of today

without the mood pepper appearing on the email ! I'll have time to read more at the

weekend

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as I'm coming to the US for the DoE CCPP meeting at Charleston. Added Ed,
Peck and
Keith A.
onto this list as well. I would like to have time to rise to the bait, but
I have so
much else on at
the moment. As a few of us will be at the EGS/AGU meet in Nice, we should
consider what
to do there.
The phrasing of the questions at the start of the paper determine the
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1900 and their MWP 800-1300, there appears (at my quick first reading) no
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synchronicity of the cool/warm periods. Even with the instrumental record, the
early and
late
20th century warming periods are only significant locally at between 10-20%
of grid
boxes.
Writing this I am becoming more convinced we should do something - even
if this is
just
to state once and for all what we mean by the LIA and MWP. I think the
skeptics will
use
this paper to their own ends and it will set paleo back a number of years if
it goes
unchallenged.
I will be emailing the journal to tell them I'm having nothing more to
do with it
until they
rid themselves of this troublesome editor. A CRU person is on the editorial
board, but
papers
get dealt with by the editor assigned by Hans von Storch.
Cheers
Phil
Dear all,
Tim Osborn has just come across this. Best to ignore probably, so don't
let it
spoil your
day. I've not looked at it yet. It results from this journal having a number
of
editors. The
responsible one for this is a well-known skeptic in NZ. He has let a few
papers
through by
Michaels and Gray in the past. I've had words with Hans von Storch about
this, but got
nowhere.
Another thing to discuss in Nice !
Cheers
Phil
X-Sender: f055@pop.uea.ac.uk
X-Mailer: QUALCOMM Windows Eudora Version 5.1
Date: Mon, 10 Mar 2003 14:32:14 +0000

cg2003

To: p.jones@uea
From: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Soon & Baliunas
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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

31. 2003-03-12

cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, rbradley@geo.umass.edu,
mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, k.briffa@uea.ac.uk,
mann@virginia.edu
date: Wed, 12 Mar 2003 16:16:16 +0000
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: Fwd: Soon & Baliunas
to: "Michael E. Mann" <mann@virginia.edu>, Tom Crowley <tcrowley@duke.edu>, Phil
Jones <p.jones@uea.ac.uk>
<x-flowed>

This is an excellent idea, Mike, IN PRINCIPLE at least. In practise, however, it raises some interesting results (as I have found when attempting this myself) that may be difficult to avoid getting bogged down with discussing.

The attached .pdf figure shows an example of what I have produced (NB. please don't circulate this further, as it is from work that is currently being finished off - however, I'm happy to use it here to illustrate my point). I took 7 reconstructions and re-calibrated them over a common period and against an observed target series (in this case, land-only, Apr-Sep, >20N - BUT I GET SIMILAR RESULTS WITH OTHER CHOICES, and this re-calibration stage is not critical). You will have seen figures similar to this in stuff Keith and I have published. See the coloured lines in the attached figure. In this example I then simply took an unweighted average of the calibrated series, but the weighted average obtained via an EOF approach can give similar results. The average is shown by the thin black line (I've ignored the potential problems of series covering different periods). This was all done with raw, unsmoothed data, even though 30-yr smoothed curves are plotted in the figure.

The thick black line is what I get when I re-calibrate the average record against my target observed series. THIS IS THE IMPORTANT BIT. The *re-calibrated* mean of the reconstructions is nowhere near the mean of the reconstructions. It has enhanced variability, because averaging the reconstructions results in a redder time series (there is less common variance between the reconstructions at the higher frequencies compared with the lower frequencies, so the former averages out to leave a smoother

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curve) and the re-calibration is then more of a case of fitting a trend (over my calibration period 1881-1960) to the observed trend. This results in enhanced variability, but also enhanced uncertainty (not shown here) due to fewer effective degrees of freedom during calibration.

Obviously there are questions about observed target series, which series to include/exclude etc., but the same issue will arise regardless: the analysis will not likely lie near to the middle of the cloud of published series and explaining the reasons behind this etc. will obscure the message of a short EOS piece.

It is, of course, interesting - not least for the comparison with borehole-based estimates - but that is for a separate paper, I think.

My suggestion would be to stick with one of these options:

- (i) a single example reconstruction;
- (ii) a plot of a cloud of reconstructions;
- (iii) a plot of the "envelope" containing the cloud of reconstructions (perhaps also the envelope would encompass their uncertainty estimates), but without showing the individual reconstruction best guesses.

How many votes for each?

Cheers

Tim

At 15:32 12/03/03, Michael E. Mann wrote:

>p.s. The idea of both a representative time-slice spatial plot emphasizing
>the spatial variability of e.g. the MWP or LIA, and an EOF analysis of all
>the records is a great idea. I'd like to suggest a small modification of
>the latter:

>

>I would suggest we show 2 curves, representing the 1st PC of two different
>groups, one of empirical reconstructions, the other of model simulations,
>rather than just one in the time plot.

>

>Group #1 could include:

>

>1) Crowley & Lowery

>2) Mann et al 1999

>3) Bradley and Jones 1995

>4) Jones et al, 1998

>5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD
>reconstruction]

>6) Esper et al [yes, no?--one series that differs from the others won't
>make much of a difference]

>

>I would suggest we scale the resulting PC to the CRU 1856-1960 annual
>Northern Hemisphere mean instrumental record, which should overlap w/ all
>of the series, and which pre-dates the MXD decline issue...

>

>Group #2 would include various model simulations using different forcings,
>and with slightly different sensitivities. This could include 6 or so
>simulation results:

>

>1) 3 series from Crowley (2000) [based on different solar/volcanic
>reconstructions],

>2) 2 series from Gerber et al (Bern modeling group result) [based on
>different assumed sensitivities]

>1) Bauer et al series (Claussen group EMIC result) [includes 19th/20th
>century land use changes as a forcing].

>

>I would suggest that the model's 20th century mean is aligned with the
>20th century instrumental N.Hem mean for comparison (since this is when we
>know the forcings best).

>

>

>I'd like to nominate Scott R. as the collector of the time series and the
>performer of the EOF analyses, scaling, and plotting, since Scott already

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>has many of the series and many of the appropriate analysis and plotting
>tools set up to do this.
>
>We could each send our preferred versions of our respective time series to
>Scott as an ascii attachment, etc.
>
>thoughts, comments?
>
>thanks,
>
>mike
>
>At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:
>>Thanks Tom,
>>
>>Either would be good, but Eos is an especially good idea. Both Ellen M-T
>>and Keith Alverson are on the editorial board there, so I think there
>>would be some receptiveness to such a submission.t
>>
>>I see this as complementary to other pieces that we have written or are
>>currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are
>>doing for Science on the MWP) and this should proceed entirely
>>independently of that.
>>
>>If there is group interest in taking this tack, I'd be happy to contact
>>Ellen/Keith about the potential interest in Eos, or I'd be happy to let
>>Tom or Phil to take the lead too...
>>
>>Comments?
>>
>>mike
>>
>>At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:
>>>>
>>>
>>>
>>>
>>>
>>>
>>>
>>>Phil et al,
>>>
>>>I suggest either BAMS or Eos - the latter would probably be better
>>>because it is shorter, quicker, has a wide distribution, and all the
>>>points that need to be made have been made before.
>>>
>>>rather than dwelling on Soon and Baliunas I think the message should be
>>>pointedly made against all of the standard claptrap being dredged up.
>>>
>>>I suggest two figures- one on time series and another showing the
>>>spatial array of temperatures at one point in the Middle Ages. I
>>>produced a few of those for the Ambio paper but already have one ready
>>>for the Greenland settlement period 965-995 showing the regional nature
>>>of the warmth in that figure. we could add a few new sites to it, but
>>>if people think otherwise we could of course go in some other direction.
>>>
>>>rather than getting into the delicate question of which paleo
>>>reconstruction to use I suggest that we show a time series that is an
>>>eof of the different reconstructions - one that emphasizes the
>>>commonality of the message.
>>>
>>>Tom

>>>
>>>
>>>>Dear All,
>>>> I agree with all the points being made and the multi-authored
>>>> article would be a good idea,
>>>> but how do we go about not letting it get buried somewhere. Can we
>>>> not address the
>>>> misconceptions by finally coming up with definitive dates for the LIA
>>>> and MWP and
>>>> redefining what we think the terms really mean? With all of us and
>>>> more on the paper, it should
>>>> carry a lot of weight. In a way we will be setting the agenda for
>>>> what should be being done
>>>> over the next few years.
>>>> We do want a reputable journal but is The Holocene the right
>>>> vehicle. It is probably the
>>>> best of its class of journals out there. Mike and I were asked to
>>>> write an article for the EGS
>>>> journal of Surveys of Geophysics. You've not heard of this - few
>>>> have, so we declined. However,
>>>> it got me thinking that we could try for Reviews of Geophysics. Need
>>>> to contact the editorial
>>>> board to see if this might be possible. Just a thought, but it
>>>> certainly has a high profile.
>>>> What we want to write is NOT the scholarly review a la Jean Grove
>>>> (bless her soul) that
>>>> just reviews but doesn't come to anything firm. We want a critical
>>>> review that enables
>>>> agendas to be set. Ray's recent multi-authored piece goes a lot of
>>>> the way so we need
>>>> to build on this.
>>>>
>>>> Cheers
>>>> Phil
>>>>
>>>>
>>>>>At 12:55 11/03/03 -0500, Michael E. Mann wrote:
>>>>>HI Malcolm,
>>>>>
>>>>>>Thanks for the feedback--I largely concur. I do, though, think there
>>>>>>is a particular problem with "Climate Research". This is where my
>>>>>>colleague Pat Michaels now publishes exclusively, and his two closest
>>>>>>colleagues are on the editorial board and review editor board. So I
>>>>>>promise you, we'll see more of this there, and I personally think
>>>>>>there *is* a bigger problem with the "messenger" in this case...
>>>>>
>>>>>>But the Soon and Baliunas paper is its own, separate issue too. I too
>>>>>>like Tom's latter idea, of a more hefty multi-authored piece in an
>>>>>>appropriate journal (Paleoceanography? Holocene?) that seeks to
>>>>>>correct a number of misconceptions out there, perhaps using Baliunas
>>>>>>and Soon as a case study ('poster child?'), but taking on a slightly
>>>>>>greater territory too.
>>>>>
>>>>>>Question is, who would take the lead role. I *know* we're all very busy,
>>>>>
>>>>>mike
>>>>>
>>>>>> At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:
>>>>>>>I'm with Tom on this. In a way it comes back to a rant of mine
>>>>>>>to which some of you have already been victim. The general
>>>>>>>point is that there are two arms of climatology:
>>>>>>> neoclimatology - what you do based on instrumental records

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>>>>>and direct, systematic observations in networks - all set in a
>>>>>very Late Holocene/Anthropocene time with hourly to decadal
>>>>>interests.
>>>>>paleoclimatology - stuff from rocks, etc., where major changes
>>>>>in the Earth system, including its climate, associated with
>>>>>major changes in boundary conditions, may be detected by
>>>>>examination of one or a handful of paleo records.
>>>>>Between these two is what we do - "mesoclimatology" -
>>>>>dealing with many of the same phenomena as neoclimatology,
>>>>>using documentary and natural archives to look at phenomena
>>>>>on interannual to millennial time scales. Given relatively small
>>>>>changes in boundary conditions (until the last couple of
>>>>>centuries), mesoclimatology has to work in a way that is very
>>>>>similar to neoclimatology. Most notably, it depends on heavily
>>>>>replicated networks of precisely dated records capable of
>>>>>being either calibrated, or whose relationship to climate may
>>>>>be modeled accurately and precisely.
>>>>>Because this distinction is not recognized by many (e.g.
>>>>>Sonnechkin, Broecker, Karlen) we see an accumulation of
>>>>>misguided attempts at describing the climate of recent
>>>>>millennia. It would be better to head this off in general, rather
>>>>>than draw attention to a bad paper. After all, as Tom rightly
>>>>>says, we could all nominate really bad papers that have been
>>>>>published in journals of outstanding reputation (although there
>>>>>could well be differences between our lists).
>>>>>End of rant, Cheers, Malcolm
>>>>> > Hi guys,
>>>>> >
>>>>> > junk gets published in lots of places. I think that what could be
>>>>> > done is a short reply to the authors in Climate Research OR a SLIGHTLY
>>>>> > longer note in a reputable journal entitled something like "Continuing
>>>>> > Misconceptions About interpretation of past climate change." I kind
>>>>> > of like the more pointed character of the latter and submitting it as
>>>>> > a short note with a group authorship carries a heft that a reply to a
>>>>> > paper, in no matter what journal, does not.
>>>>> >
>>>>> > Tom
>>>>> >
>>>>> >
>>>>> >
>>>>> > > Dear All,
>>>>> > > Apologies for sending this again. I was expecting a stack of
>>>>> > > emails this morning in
>>>>> > > response, but I inadvertently left Mike off (mistake in pasting)
>>>>> > > and picked up Tom's old
>>>>> > > address. Tom is busy though with another offspring !
>>>>> > > I looked briefly at the paper last night and it is appalling -
>>>>> > > worst word I can think of today
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>>>>> > > as I'm coming to the US for the DoE CCPP meeting at Charleston.
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>>>>> > > onto this list as well. I would like to have time to rise to the
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>>>>> > > to do there.
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>>>>> > > with their LIA being 1300-

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>>>>> > >1900 and their MWP 800-1300, there appears (at my quick first
>>>>> > >reading) no discussion of
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>>>>> > > 20th century warming periods are only significant locally at
>>>>> > >between 10-20% of grid boxes.
>>>>> > > Writing this I am becoming more convinced we should do
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>>>>> > > to state once and for all what we mean by the LIA and MWP. I think
>>>>> > >the skeptics will use
>>>>> > > this paper to their own ends and it will set paleo back a number of
>>>>> > >
>>>>> > >years if it goes
>>>>> > > unchallenged.
>>>>> > >
>>>>> > > I will be emailing the journal to tell them I'm having
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>>>>> > > rid themselves of this troublesome editor. A CRU person is on the
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>>>>> > > get dealt with by the editor assigned by Hans von Storch.
>>>>> > >
>>>>> > > Cheers
>>>>> > > Phil
>>>>> > >
>>>>> > > Dear all,
>>>>> > > Tim Osborn has just come across this. Best to ignore
>>>>> > >probably, so don't let it spoil your
>>>>> > > day. I've not looked at it yet. It results from this journal
>>>>> > >having a number of editors. The
>>>>> > > responsible one for this is a well-known skeptic in NZ. He has let
>>>>> > >
>>>>> > >a few papers through by
>>>>> > > Michaels and Gray in the past. I've had words with Hans von Storch
>>>>> > >
>>>>> > >about this, but got nowhere.
>>>>> > > Another thing to discuss in Nice !
>>>>> > >
>>>>> > > Cheers
>>>>> > > Phil
>>>>> > >
>>>>> > >>X-Sender: f055@pop.uea.ac.uk
>>>>> > >>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>>>> > >>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>>>> > >>To: p.jones@uea
>>>>> > >>From: Tim Osborn <t.osborn@uea.ac.uk>
>>>>> > >>Subject: Soon & Baliunas
>>>>> > >>
>>>>> > >>
>>>>> > >>Dr Timothy J Osborn | phone: +44 1603 592089
>>>>> > >>Senior Research Associate | fax: +44 1603 507784
>>>>> > >>Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
>>>>> > >>School of Environmental Sciences | web-site: University of East
>>>>> > >>Anglia _____| <http://www.cru.uea.ac.uk/~timo/> Norwich NR4
>>>>> > >>7TJ | sunclock: UK |
>>>>> > >><http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>>>>> > >
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>>>>> > >Climatic Research Unit Telephone +44 (0) 1603 592090
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>>>>> > >University of East Anglia
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>>>>> > >NR4 7TJ

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>>>>> > >UK
>>>>> > >-----
>>>>> > >-----
>>>>> > >
>>>>> > >
>>>>> > >Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
>>>>> > >/CARO) (00016021)
>>>>> >
>>>>> >
>>>>> > --
>>>>> > Thomas J. Crowley
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>>>>> > Nicholas School of the Environment and Earth Sciences
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>>>>> >
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>>>>> > 919-684-5833 fax
>>>>>
>>>>>Malcolm Hughes
>>>>>Professor of Dendrochronology
>>>>>Laboratory of Tree-Ring Research
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>>>>>Tucson, AZ 85721
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>>>>> University of Virginia
>>>>> Charlottesville, VA 22903
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>>>>> http://www.evsc.virginia.edu/faculty/people/mann.shtml
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>>>>>UK
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>>>>>103 Old Chem Building Duke University
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```

cg2003

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>> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
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> Charlottesville, VA 22903
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> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
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UK	http://www.cru.uea.ac.uk/~timo/sunclock.htm

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404. 2003-03-12

cc: rbradley@geo.umass.edu,mhughes@lrr.arizona.edu,srutherford@gso.uri.edu,
k.briffa@uea.ac.uk,mann@virginia.edu
date: wed, 12 Mar 2003 12:12:02 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Fwd: Soon & Baliunas
to: Tim Osborn <t.osborn@uea.ac.uk>,Tom Crowley <tcrowley@duke.edu>, Phil Jones
<p.jones@uea.ac.uk>

Dear Tim,

Thanks for your rapid replies and your help. This is all very useful.

Well, lets see what this gives...

There are some notable differences just between our relative comparisons of the different

series which must have something to do with the relative scaling and aligning of the

series. The position of Crowley and Lowery, in particular, is quite inconsistent between

our respective comparisons. When we scale the various series to the full N. Hem
instrumental annual mean CRU record 1856-1980, we get a a very different

relative ordering

of the different series, as shown in the attached figure from my Science
perspective piece

from last year

This should not, however, influence the EOF decomposition if all series are
zero-mean and

standardized prior to the EOF analysis, but the scaling and alignment of the
result, in the

end, will be sensitive to all of these various issues.

So, in short, lets see what we get, and then discuss any

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similarities/differences w/ your

result, then make a decision as to what to show in the Eos piece. I'm sure we can come up

w/ something we're all happy with...

Please do send us your & Keith's preferred version of the MXD

reconstruction--we'll collect

the others from the individual sources (most we already have, I think)...

mike

At 04:53 PM 3/12/2003 +0000, Tim Osborn wrote:

At 16:29 12/03/03, Michael E. Mann wrote:

but there are many variables here [not the least of which is the choice of scaling the

series to an extratropical summer mean, which as we have argued before, we don't think

is appropriate for a full N. Hem mean because of changes in meridional temperature

gradient over time, and the choice of calibration period--I wonder if

1856-1960 or

1856-1980 gives a more stable result).

True, but as I indicated I have tried alternatives. The attached is what I get with

annual mean temperature as the target series - still taken only from land >20N though

[but I have extracted that domain from your spatial reconstructions to produce the time

series that I used for "Mann et al." - which should make it reasonably appropriate back

to 1400 at least]. I have also tried different calibration periods (including not

calibrating against instrumental data at all!). All give qualitatively similar results

- see attached .pdf and compare with the first one I sent.

The point is, that (I believe) the approach will introduce a *new* result and while that

is interesting it wouldn't be appropriate for a short EOS piece - and having found this

out, I was trying to save you the effort.

But, on reflection, it would be good if you went ahead and did this anyway, because the

results might well be useful to publish in another paper, even if they weren't deemed

suitable for the EOS piece.

I could provide the 7 series that I have used, but would prefer that you got them from

the original sources to ensure that you have the most up-to-date/correct versions.

Cheers

Tim

Dr Timothy J Osborn

Senior Research Associate

Climatic Research Unit

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ

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Attachment Converted: "c:\eudora\attach\mannpersp2002.gif"

716. 2003-03-12

cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tom Crowley
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t.osborn@uea.ac.uk, mann@virginia.edu
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p.s. The idea of both a representative time-slice spatial plot emphasizing the spatial

variability of e.g. the MWP or LIA, and an EOF analysis of all the records is a great idea.

I'd like to suggest a small modification of the latter:

I would suggest we show 2 curves, representing the 1st PC of two different groups, one of empirical reconstructions, the other of model simulations, rather than just one in the time plot.

Group #1 could include:

- 1) Crowley & Lowery
- 2) Mann et al 1999
- 3) Bradley and Jones 1995
- 4) Jones et al, 1998
- 5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD reconstruction]
- 6) Esper et al [yes, no?--one series that differs from the others won't make

much of a difference]

I would suggest we scale the resulting PC to the CRU 1856-1960 annual Northern Hemisphere

mean instrumental record, which should overlap w/ all of the series, and which pre-dates

the MXD decline issue...

Group #2 would include various model simulations using different forcings, and with

slightly different sensitivities. This could include 6 or so simulation results:

- 1) 3 series from Crowley (2000) [based on different solar/volcanic reconstructions],
- 2) 2 series from Gerber et al (Bern modeling group result) [based on different assumed sensitivities]

1) Bauer et al series (Claussen group EMIC result) [includes 19th/20th century land use changes as a forcing].

I would suggest that the model's 20th century mean is aligned with the 20th century

instrumental N.Hem mean for comparison (since this is when we know the forcings best).

I'd like to nominate Scott R. as the collector of the time series and the performer of the

EOF analyses, scaling, and plotting, since Scott already has many of the series and many of

the appropriate analysis and plotting tools set up to do this.

We could each send our preferred versions of our respective time series to Scott as an

ascii attachment, etc.

thoughts, comments?

thanks,

mike

At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:

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Thanks Tom,
Either would be good, but Eos is an especially good idea. Both Ellen M-T and Keith Alverson are on the editorial board there, so I think there would be some receptiveness to such a submission.
I see this as complementary to other pieces that we have written or are currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are doing for Science on the MWP) and this should proceed entirely independently of that.
If there is group interest in taking this tack, I'd be happy to contact Ellen/Keith about the potential interest in Eos, or I'd be happy to let Tom or Phil to take the lead too...
Comments?
mike
At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:
Phil et al,
I suggest either BAMS or Eos - the latter would probably be better because it is shorter, quicker, has a wide distribution, and all the points that need to be made have been made before.
rather than dwelling on Soon and Baliunas I think the message should be pointedly made against all of the standard claptrap being dredged up.
I suggest two figures- one on time series and another showing the spatial array of temperatures at one point in the Middle Ages. I produced a few of those for the Ambio paper but already have one ready for the Greenland settlement period 965-995 showing the regional nature of the warmth in that figure. we could add a few new sites to it, but if people think otherwise we could of course go in some other direction.
rather than getting into the delicate question of which paleo reconstruction to use I suggest that we show a time series that is an eof of the different reconstructions - one that emphasizes the commonality of the message.
Tom
Dear All,
I agree with all the points being made and the multi-authored article would be a good idea,
but how do we go about not letting it get buried somewhere. Can we not address the misconceptions by finally coming up with definitive dates for the LIA and MWP and redefining what we think the terms really mean? With all of us and more on the paper, it should carry a lot of weight. In a way we will be setting the agenda for what should be being done over the next few years.
We do want a reputable journal but is The Holocene the right vehicle. It is probably the best of its class of journals out there. Mike and I were asked to write an article for

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the EGS
journal of Surveys of Geophysics. You've not heard of this - few have, so we declined.

However,
it got me thinking that we could try for Reviews of Geophysics. Need to contact the editorial board to see if this might be possible. Just a thought, but it certainly has a high profile.

What we want to write is NOT the scholarly review a la Jean Grove (bless her soul)

that just reviews but doesn't come to anything firm. We want a critical review that enables

agendas to be set. Ray's recent multi-authored piece goes a lot of the way so we need

to build on this.

Cheers

Phil

At 12:55 11/03/03 -0500, Michael E. Mann wrote:

Hi Malcolm,

Thanks for the feedback--I largely concur. I do, though, think there is a particular problem with "Climate Research". This is where my colleague Pat Michaels now publishes

exclusively, and his two closest colleagues are on the editorial board and review editor

board. So I promise you, we'll see more of this there, and I personally think there *is*

a bigger problem with the "messenger" in this case...

But the Soon and Baliunas paper is its own, separate issue too. I too like Tom's latter

idea, of a more hefty multi-authored piece in an appropriate journal (Paleoceanography?

Holocene?) that seeks to correct a number of misconceptions out there, perhaps using

Baliunas and Soon as a case study ('poster child'?), but taking on a slightly greater

territory too.

Question is, who would take the lead role. I *know* we're all very busy, mike

At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

I'm with Tom on this. In a way it comes back to a rant of mine to which some of you have already been victim. The general point is that there are two arms of climatology:

neoclimatology - what you do based on instrumental records and direct, systematic observations in networks - all set in a very Late Holocene/Anthropocene time with hourly to decadal interests.

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be modeled accurately and precisely.

Because this distinction is not recognized by many (e.g. Sonnechkin, Broecker, Karlen) we see an accumulation of misguided attempts at describing the climate of recent millennia. It would be better to head this off in general, rather than draw attention to a bad paper. After all, as Tom rightly says, we could all nominate really bad papers that have been published in journals of outstanding reputation (although there could well be differences between our lists).

End of rant, Cheers, Malcolm

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> junk gets published in lots of places. I think that what could be
> done is a short reply to the authors in Climate Research OR a SLIGHTLY
> longer note in a reputable journal entitled something like "Continuing
> Misconceptions About interpretation of past climate change." I kind
> of like the more pointed character of the latter and submitting it as
> a short note with a group authorship carries a heft that a reply to a
> paper, in no matter what journal, does not.

>

> Tom

>

>

>

> > Dear All,

> > Apologies for sending this again. I was expecting a stack of
> > emails this morning in

> > response, but I inadvertently left Mike off (mistake in pasting)
> > and picked up Tom's old

> > address. Tom is busy though with another offspring !

> > I looked briefly at the paper last night and it is appalling -
> > worst word I can think of today

> > without the mood pepper appearing on the email ! I'll have time to
> > read more at the weekend

> > as I'm coming to the US for the DoE CCPP meeting at Charleston.

> > Added Ed, Peck and Keith A.

> > onto this list as well. I would like to have time to rise to the
> > bait, but I have so much else on at

> > the moment. As a few of us will be at the EGS/AGU meet in Nice, we
> > should consider what

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> > between 10-20% of grid boxes.

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> > to state once and for all what we mean by the LIA and MWP. I think
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> > this paper to their own ends and it will set paleo back a number of
> >

> > years if it goes

> > unchallenged.

> >

> > I will be emailing the journal to tell them I'm having

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> > nothing more to do with it until they
> > rid themselves of this troublesome editor. A CRU person is on the
> > editorial board, but papers
> > get dealt with by the editor assigned by Hans von Storch.
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Dear All,

I like Phil's suggestion. I think such a piece would do a lot of good for the field. When something as full of half-truths/mis-truths as the S&B piece is put forth, it would be very useful to have a peer-reviewed review like this, which we all have endorsed through co-authorship, to point to in response. This way, when we get the inevitable "so what do you have to say about this" from our colleagues, we already have a self-contained, thorough rejoinder to point to. I'm sure we won't all agree on every detail, but there is enough commonality in our views on the big issues to make this worthwhile.

Perhaps Phil can go ahead and contact the editorial board at "Reviews of Geophysics" and see if they're interested. If so, Phil and I (and anyone else interested) could take the lead with this, and then we can entrain everyone else in as we proceed with a draft, etc.

mike

p.s. Keith: I hope you're feeling well, and that your recovery proceeds quickly!

At 10:02 AM 3/12/2003 +0000, Phil Jones wrote:

Dear All,

I agree with all the points being made and the multi-authored article would be a good idea, but how do we go about not letting it get buried somewhere. Can we not address the misconceptions by finally coming up with definitive dates for the LIA and MWP and redefining what we think the terms really mean? With all of us and more on the paper, it should carry a lot of weight. In a way we will be setting the agenda for what should be being done over the next few years.

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3366. 2003-03-12

cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tom Crowley
<tcrowley@duke.edu>, rbradley@geo.umass.edu,
mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, k.briffa@uea.ac.uk,
t.osborn@uea.ac.uk, mann@virginia.edu
date: Wed, 12 Mar 2003 10:08:55 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Fwd: Soon & Baliunas
to: Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>

Thanks Tom,
Either would be good, but Eos is an especially good idea. Both Ellen M-T and
Keith Alverson
are on the editorial board there, so I think there would be some receptiveness
to such a
submission.

I see this as complementary to other pieces that we have written or are
currently writing

(e.g. a review that Ray, Malcolm, and Henry Diaz are doing for Science on the
MWP) and this
should proceed entirely independently of that.

If there is group interest in taking this tack, I'd be happy to contact
Ellen/Keith about

the potential interest in Eos, or I'd be happy to let Tom or Phil to take the
lead too...

Comments?

mike

At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:

Phil et al,

I suggest either BAMS or Eos - the latter would probably be better because it
is
shorter, quicker, has a wide distribution, and all the points that need to be
made have
been made before.

rather than dwelling on Soon and Baliunas I think the message should be
pointedly made

against all of the standard claptrap being dredged up.

I suggest two figures- one on time series and another showing the spatial
array of

temperatures at one point in the Middle Ages. I produced a few of those for
the Ambio

paper but already have one ready for the Greenland settlement period 965-995
showing the

regional nature of the warmth in that figure. we could add a few new sites to

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it, but
if people think otherwise we could of course go in some other direction.
rather than getting into the delicate question of which paleo reconstruction
to use I
suggest that we show a time series that is an eof of the different
reconstructions - one
that emphasizes the commonality of the message.
Tom
Dear All,
I agree with all the points being made and the multi-authored article
would be a
good idea,
but how do we go about not letting it get buried somewhere. Can we not
address the
misconceptions by finally coming up with definitive dates for the LIA and MWP
and
redefining what we think the terms really mean? With all of us and more on
the paper,
it should
carry a lot of weight. In a way we will be setting the agenda for what should
be being
done
over the next few years.
We do want a reputable journal but is The Holocene the right vehicle. It
is
probably the
best of its class of journals out there. Mike and I were asked to write an
article for
the EGS
journal of Surveys of Geophysics. You've not heard of this - few have, so we
declined.
However,
it got me thinking that we could try for Reviews of Geophysics. Need to
contact the
editorial
board to see if this might be possible. Just a thought, but it certainly has
a high
profile.
What we want to write is NOT the scholarly review a la Jean Grove (bless
her soul)
that
just reviews but doesn't come to anything firm. We want a critical review
that enables
agendas to be set. Ray's recent multi-authored piece goes a lot of the way so
we need
to build on this.
Cheers
Phil
At 12:55 11/03/03 -0500, Michael E. Mann wrote:
Hi Malcolm,
Thanks for the feedback--I largely concur. I do, though, think there is a
particular
problem with "Climate Research". This is where my colleague Pat Michaels now
publishes
exclusively, and his two closest colleagues are on the editorial board and
review editor
board. So I promise you, we'll see more of this there, and I personally think
there *is*
a bigger problem with the "messenger" in this case...
But the Soon and Baliunas paper is its own, separate issue too. I too like
Tom's latter
idea, of a more hefty multi-authored piece in an appropriate journal
(Paleoceanography?)

Holocene?) that seeks to correct a number of misconceptions out there, perhaps using

Baliunas and Soon as a case study ('poster child?'), but taking on a slightly greater territory too.

Question is, who would take the lead role. I *know* we're all very busy, mike

At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

I'm with Tom on this. In a way it comes back to a rant of mine to which some of you have already been victim. The general point is that there are two arms of climatology:

neoclimatology - what you do based on instrumental records and direct, systematic observations in networks - all set in a very Late Holocene/Anthropocene time with hourly to decadal interests.

paleoclimatology - stuff from rocks, etc., where major changes in the Earth system, including its climate, associated with major changes in boundary conditions, may be detected by examination of one or a handful of paleo records.

Between these two is what we do - "mesoclimatology" - dealing with many of the same phenomena as neoclimatology, using documentary and natural archives to look at phenomena on interannual to millennial time scales. Given relatively small changes in boundary conditions (until the last couple of centuries), mesoclimatology has to work in a way that is very similar to neoclimatology. Most notably, it depends on heavily replicated networks of precisely dated records capable of being either calibrated, or whose relationship to climate may be modeled accurately and precisely.

Because this distinction is not recognized by many (e.g. Sonnechkin, Broecker, Karlen) we see an accumulation of misguided attempts at describing the climate of recent millennia. It would be better to head this off in general, rather than draw attention to a bad paper. After all, as Tom rightly says, we could all nominate really bad papers that have been published in journals of outstanding reputation (although there could well be differences between our lists).

End of rant, Cheers, Malcolm

> Hi guys,

>

> junk gets published in lots of places. I think that what could be done is a short reply to the authors in Climate Research OR a SLIGHTLY longer note in a reputable journal entitled something like "Continuing Misconceptions About interpretation of past climate change." I kind of like the more pointed character of the latter and submitting it as a short note with a group authorship carries a heft that a reply to a paper, in no matter what journal, does not.

>

> Tom

>

>

>

> > Dear All,

> > Apologies for sending this again. I was expecting a stack of > emails this morning in

> > response, but I inadvertently left Mike off (mistake in pasting)

> > and picked up Tom's old

> > address. Tom is busy though with another offspring !

> > I looked briefly at the paper last night and it is appalling - > > worst word I can think of today

> > without the mood pepper appearing on the email ! I'll have time to

> > read more at the weekend

> > as I'm coming to the US for the DoE CCPP meeting at Charleston.

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> > Added Ed, Peck and Keith A.
> > onto this list as well. I would like to have time to rise to the
> > bait, but I have so much else on at
> > the moment. As a few of us will be at the EGS/AGU meet in Nice, we
> > should consider what
> > to do there.
> > The phrasing of the questions at the start of the paper
> > determine the answer they get. They
> > have no idea what multiproxy averaging does. By their logic, I
> > could argue 1998 wasn't the
> > warmest year globally, because it wasn't the warmest everywhere.
> > with their LIA being 1300-
> > 1900 and their MWP 800-1300, there appears (at my quick first
> > reading) no discussion of
> > synchronicity of the cool/warm periods. Even with the instrumental
> > record, the early and late
> > 20th century warming periods are only significant locally at
> > between 10-20% of grid boxes.
> > Writing this I am becoming more convinced we should do
> > something - even if this is just
> > to state once and for all what we mean by the LIA and MWP. I think
> > the skeptics will use
> > this paper to their own ends and it will set paleo back a number of
> >
> > years if it goes
> > unchallenged.
> >
> > I will be emailing the journal to tell them I'm having
> > nothing more to do with it until they
> > rid themselves of this troublesome editor. A CRU person is on the
> > editorial board, but papers
> > get dealt with by the editor assigned by Hans von Storch.
> >
> > Cheers
> > Phil
> >
> > Dear all,
> > Tim Osborn has just come across this. Best to ignore
> > probably, so don't let it spoil your
> > day. I've not looked at it yet. It results from this journal
> > having a number of editors. The
> > responsible one for this is a well-known skeptic in NZ. He has let
> >
> > a few papers through by
> > Michaels and Gray in the past. I've had words with Hans von Storch
> >
> > about this, but got nowhere.
> > Another thing to discuss in Nice !
> >
> > Cheers
> > Phil
> >
> >> X-Sender: f055@pop.uea.ac.uk
> >> X-Mailer: QUALCOMM Windows Eudora Version 5.1
> >> Date: Mon, 10 Mar 2003 14:32:14 +0000
> >> To: p.jones@uea
> >> From: Tim Osborn <t.osborn@uea.ac.uk>
> >> Subject: Soon & Baliunas
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> > Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
> > /CARO) (00016021)
> >

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2859. 2003-03-13

date: Thu Mar 13 15:33:36 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: extreme events as catalysts
to: i.lorenzoni,s.nicholson-cole@uea.ac.uk,t.oriordan@uea.ac.uk,t.dockerty
... following on our questions and discussion at the seminar today - which,
Irene and
Sophie, I thought was superbly put together (well done!) - you might like to
look at this
research project from Clare Johnson at Middlesex flood hazard group being funded
by the new
ESRC Environment and Human Behaviour Programme:
[1]<http://www.psi.org.uk/ehb/projectsjohnson.html>.
I realise that changes in regulatory behaviour re. adaptation is not the same as
changing
underlying lifestyles and mitigation, but I still think for climate change we
rely
fundamentally on people's personal experiences - the more extreme the better -
coinciding
with scientific narratives giving meaning to those experiences in order to
motivate
behavioral change.
Mike

1790. 2003-03-17

date: Mon, 17 Mar 2003 21:00:29 +0000 (GMT Standard Time)
from: Irene Lorenzoni <I.Lorenzoni@uea.ac.uk>
subject: Re: RE: extreme events as catalysts
to: Trudie Dockerty <t.dockerty@uea.ac.uk>, s.nicholson-cole@uea.ac.uk, Mike Hulme
<m.hulme@uea.ac.uk>
Dear Mike, Trudie and Sophie
thanks for your interesting comments. I agree with the
importance of extreme events as foci for public and
governmental opinion - my view is that if they are
sufficiently broad in terms of the physical impacts caused
(e.g. widespread flooding in Germany as the visitor we had
last Thursday was mentioning, which provided the catalyst
for the German government to legislate on flood protection
etc) they will almost certainly lead to 'reactive'
behaviour. They might also induce also proactive
mitigative behaviour.
However, from my experience and research work I would also
agree with Trudie's point on the fact that the general
public perceive climate change as episodic ie manifesting
itself when extremes occur. For mitigative as well as
adaptive behaviour to take place, I have often heard the
argument that 'climate change' needs to be present in
people's daily lives. They should be reminded that it is a

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continuously occurring and evolving phenomenon, either in the form of added premiums for renewable energies, reduced railpass as encouragement for alternative modes of transport, regular coverage on the media.

In other words, I agree that extreme events can be catalysts for change for those affected and for governmental policy, but for mitigation to take place seriously, climate change should be on everyone's daily agenda.

Irene

On Thu, 13 Mar 2003 18:59:11 -0000 Trudie Dockerty

<t.dockerty@uea.ac.uk> wrote:

> Thanks for this - haven't had a read yet but to press the
> point - I do think from my experiences of visiting
> government agencies, NGOs and local authorities that there
> is a growing awareness of the issue but people
> don't necessarily link cause and effect i.e. their actions
> to climate change - we all need empowering through better
> educational programmes that make these links - i.e. switch
> off the light - save energy - save power station emissions
> - reduce ghg - reduce climate change. Most programmes stop
> at the 'save energy' stage and people don't make the link.
> Also it would be quite interesting to do a study to see if
> there is a trend (beginning prior to the formation of IPCC)
> in the frequency with which climate change is mentioned in
> the media - I feel sure an increased 'nagging' through an
> ever increasing range of reports is keeping the issue at
> least in the back of people's minds and this increasing
> discomfort will eventually push people into taking steps in
> the right direction - if they are presented with practical
> actions they can take. The biggest feedback I have had in
> talking to people about climate change is how powerless
> they feel to do anything about it - there is a huge
> opportunity I think to engage people through 'show and
> tell'. We have to start with small measures to engage in
> the process and reduce the resistance to bigger actions
> that will be needed before long. I don't disagree with
> you at all but that can't be The End - surely somehow we
> must try and do something alongside waiting for the 'aha'
> moment to dawn after each catastrophe?

>
> Anyway! No reply necessary - just adding my thoughts - hope
> you don't mind!

>
> regards

> Trudie

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> -----Original Message-----

> From: Mike Hulme [mailto:m.hulme@uea.ac.uk]

> Sent: 13 March 2003 15:34

> To: i.lorenzoni@uea.ac.uk; s.nicholson-cole@uea.ac.uk;

> t.oriordan@uea.ac.uk; t.dockerty@uea.ac.uk

> Subject: extreme events as catalysts

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> I realise that changes in regulatory behaviour re.

> adaptation is not the same as changing underlying

> lifestyles and mitigation, but I still think for climate

> change we rely fundamentally on people's personal

> experiences - the more extreme the better - coinciding with

> scientific narratives giving meaning to those experiences

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> in order to motivate behavioral change.

>

> Mike

>

>

Ms Irene Lorenzoni
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1926. 2003-03-17

date: Mon, 17 Mar 2003 16:43:34 +0100
from: Armin Haas <haas@pik-potsdam.de>
subject: AMS project
to: Alex Haxeltine <Alex.Haxeltine@uea.ac.uk>, Alexander Wokaun <wokaun@psi.ch>, Anco Lankreijer <lana@geo.vu.nl>, Andrew Jordan <a.jordan@uea.ac.uk>, Antonella Battaglini <antonella.battaglini@pik-potsdam.de>, Antoni Rosell <antoni.rosell@uab.es>, Antonio Navarra <navarra@ingv.it>, Armin Haas <haas@pik-potsdam.de>, "Asbjørn Torvanger" <asbjorn.torvanger@cicero.uio.no>, "baldur.eliasson@ch.abb.com" <baldur.eliasson@ch.abb.com>, "Benito Müller" <benito.mueller@philosophy.oxford.ac.uk>, Bert Metz <bert.metz@rivm.nl>, "bhare@ams.greenpeace.org" <bhare@ams.greenpeace.org>, "Brian O'Neill" <oneill@iiasa.ac.at>, Carlo Carraro <ccarraro@unive.it>, Carlo Jaeger <carlo.jaeger@pik-potsdam.de>, Catherine Boemare <boemare@centre-cired.fr>, Christian Azar <fritca@fy.chalmers.se>, Christian Flachsland <christian.flachsland@pik-potsdam.de>, Christos Giannakopoulos <cgiannak@meteo.noa.gr>, Claudia Kemfert <kemfert@uni-olderburg.de>, Daniel Droste <d.droste@consultants.mvv.de>, Eberhard Jochem <eberhard.jochem@isi.fhg.de>, Eberhard Jochem <jochem@cepe.mavt.ethz.ch>, Elaine Jones <e.l.jones@uea.ac.uk>, Elas Hunfeld <els.hunfeld@falw.vu.nl>, Felicity Thomas <ft@ier.uni-stuttgart.de>, Ferenc Toth <toth@iiasa.ac.at>, Francis Johnson <francis.johnson@sei.se>, Frank Thomalla <frank.thomalla@pik-potsdam.de>, Fred Langeweg <Fred.Langeweg@rivm.nl>, Gary Yohe <gyohe@wesleyan.edu>, "gberz@munichre.com" <gberz@munichre.com>, Gernot Klepper <gklepper@ifw.uni-kiel.de>, HALLEGATTE Stephane <Stephane.Hallegatte@lmd.jussieu.fr>, Harald Bradke <hb@isi.fhg.de>, Heike Zimmermann-Timm <heike.zimmermann-timm@pik-potsdam.de>, Helga Kromp-Kolb <kromp-ko@tornado.boku.ac.at>, Henning Jappe <h.jappe@consultants.mvv.de>, Henning Niemeyer <h.niemeyer@consultants.mvv.de>, Henry Neufeldt <neufeldt@ife-le.de>, Herve Le Treut <letreut@lmd.ens.fr>, "Jaap C. Jansen" <j.jansen@ecn.nl>, Jan Rotmans <j.Rotmans@icis.unimaas.nl>, Jean Palutikof <j.palutikof@uea.ac.uk>, Jean-Charles Hourcade <hourcade@centre-cired.fr>, Jeroen Aerts <jeroen.aerts@ivm.vu.nl>, Jeroen van der Sluijs <j.p.vandersluijs@chem.uu.nl>, Joan David Tabara <jdtabara@terra.es>, John Schellnhuber <h.j.schellnhuber@uea.ac.uk>, John Turnpenny <j.turnpenny@uea.ac.uk>, Jon Hovi <jon.hovi@stv.uio.no>, "Jonathan Köhler" <j.kohler@uea.ac.uk>, Juergen Kurths <juergen@agnld.uni-potsdam.de>, "juergen.engelhard@rwerheinbraun.com" <juergen.engelhard@rwerheinbraun.com>, "Karen O'Brien" <karen.obrien@cicero.uio.no>, Katrin Gerlinger <Katrin.Gerlinger@pik-potsdam.de>, "Klaus Böswald" <klaus.boeswald@factorag.ch>, Klaus Hasselmann <klaus.hasselmann@dkrz.de>, Kornelis Blok <K.Blok@chem.uu.nl>, Leen Hordijk <hordijk@iiasa.ac.at>, Lennart Olsson <lennart.olsson@miclu.lu.se>, Liudmila Romaniuk <Romaniuk@mail.lanck.net>, Manfred Stock <manfred.stock@pik-potsdam.de>, Marco Berg <marco.berg@factorag.ch>, Marcus Lindner <Marcus.Lindner@efi.fi>, Marina Fischer-Kowalski <marina.fischer-kowalski@univie.ac.at>, Marjan Minnesma <Marjan.Minnesma@ivm.vu.nl>, Mark Rounsevell <rounsevell@geog.ucl.ac.be>, Martin Claussen <Martin.Claussen@pik-potsdam.de>, Martin Kaltschmitt <kaltschmitt@ife-le.de>, Martin Parry <martin.parry@uea.ac.uk>, " martin.welp" <martin.welp@pik-potsdam.de>, Mike Hulme <m.hulme@uea.ac.uk>, Monika Ritt <Monika.ritt@falw.vu.nl>, MVV C&E Berlin Tom Mansfield <mansfield@euweb.de>, MVV

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Wolfgang Lucht <wolfgang.Lucht@pik-potsdam.de>

Dear all,

Attached please find the first minutes of the strategy committee. We are building on the documents attached to my first e-mail to you so as to stepwise define the project and develop the proposal.

Many of you have been asked to make specific contributions and we are grateful for the timely responses we get. In particular, we have asked the members of the research committee to specialize on specific work domains in the short run, so as to move fast forward with the writing. We will soon mail out a first raw version of the proposal and ask for additional input.

By now, for most work domains not only individual contributors, but also working groups facilitated by the work domain coordinators have been formed. They work via e-mail and teleconferences, and they work well - thank you.

For your convenience, here comes the list of domain coordinators:

Scenarios: Sander van der Leeuw

Adaptation: Pier Vellinga

Mitigation: Alexander Wokaun

Strategies: John Schellnhuber

Cross-cutting activities: Klaus Hasselmann

On our second decision-making session, we have taken a first round of decisions about project partners. As these are of particular importance, I communicate the key decision here without waiting for the second minutes (these need to be checked at the next meeting of the strategy committee): we have decided that all institutions represented either in the strategy or the research committee will be asked to be project partners.

For your convenience here comes the list of these institutions:

CEPE / ETH

CICERO

CIREN

CNRS

ECF
IIASA
ISI
Lund University
Nansen Institute
Paul Scherrer Institute / ETH
PIK
SEI
Tyndall Centre
Vrije Universiteit Amsterdam
Wageningen University

The list is not closed yet, and no budget decisions have been taken so far. Both issues will be addressed soon, of course. Clearly, the EU wants to see proposals organized around a small number of competent and powerful institutions. At the same time, it seems that sub-contracting will be very difficult in the 6th framework program. Therefore, we will have a long list of partners with the understanding that all partners see this project as a major opportunity to take advantage of research synergies and to gain visibility, an opportunity they are actually willing to invest in. We have no time for efforts to make a lot of money for oneself where there is an opportunity to make critical discoveries for the whole community - discoveries that will no doubt direct additional money flows towards those who make them. Besides the on-going writing process and the definitions of partners and budgets, the form-filling exercise will keep us busy in the weeks to come. As this is critical, too, it may be useful to start talking with the relevant officers in your institution so as to make sure that they can handle things on short notice by the end of this month. With this background, I look forward to a process of discovery carried out at a European scale in view of global risks, and I believe that it is just about time for Europe to get its act together when it comes to global risks.

Best regards,
Carlo

Attachment Converted: "c:\eudora\attach\ams 12 m 03 sc minutes 09-03-03.rtf"

3421. 2003-03-20

cc: Mike Hulme <m.hulme@uea.ac.uk>
date: Thu, 20 Mar 2003 21:25:05 -0800 (PST)
from: Stephen H Schneider <shs@stanford.edu>
subject: Re: Fw: Justice and Adaptation Meeting - September 2003
to: Neil Adger <N.Adger@uea.ac.uk>

Many apologies Neil for my rude delay. I have just about cleared that I don't have a conflict in Stockholm, and if I go the the Berlin IPCC Scoping meeting 2 in Berlin it is two days before your meeting. My docs say that as long as my next two test have only background levels of nasty cells, I don't need any treatments and thus can travel--treatments mean immune compromise and eliminate airplanes for a while. So my Bayesian priors on this were around 80%--until you mentioned a big stumbling block. I cannot now, nor unless they withdraw the Lomborg book as science and apologize to the scientific community for a scientific fraud, nor can I EVER work with Chris Harrison. His talk at the AAAS was deceitful maneuvering--wrapping him self up in an authors right to speak and citing all the university visits Lomborg was paid to go to as proof how important his "challenge" was. Pure deceit. Chris didn't mention that at each BL was roundly trounced by angry folks. Lomborg is a non-rewritable CD. If a debater wins a point, he just repeats his litany at the end. That Harrison should cover his butt for such an egregious error of scientifically incompetent reviewing is the part that is unpardonable, if not unethical. I have no problems with a 150 page polemic--many of us write them. But we don't do 500 pages and 3000 references--elliptically selected from the happy end of the literature of

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four fields: climatology, demography, conservation biology and energy systems. The odds of that if every one of those field had only random skill is 1 out of 16--that all four got in wrong in the same direction--overstatement. This is NOT random, it is egggregious scientific fraud on the part of Lomborg and until Harrison comes clean with the scientific community and denounces The Skeptical Environmentalist as political polemic, not science, neither I nor most ecologists I know will have any thing to do with them. CUP lost the Millennium Assessment because of a peasant revolt from ecologists--and they deserved it.

I feel very strongly that the incompetent peers CUP used are no excuse for the horrific failure of scientific peer review. If Harrison keeps his duck and cover polemics, then I and many of my colleagues will continue to excoriate him as dishonest and covering up fraud. So if you use Cambridge, then no thanks. Sorry to be so blunt, but only a full retraction from Harrison will satify me and most of my ecologist friends who have literally wasted months refuting the Lomborgian polemics and then having to endure specious accusations that we don't want to "hear the other side". IN science there aren't two sides, but many outcomes and many subjective probabilities attached to each. Lomborg--with Harrison covering him up still!--did not talk in subjective probabilities, just selective ranges and point values. He can't even do statistics right!. Scientific peer review is not about equality, but quality. Equality is everyone should get an opportunity to vet his/her science to the knowledge community--what CUP peer review was supposed to do. They must have selected dead above the ears reviewers if any at all, and thus this whole horrendous caper was launched--by the way, CUP and Lomborg are crying all the way to the bank.

I guess, Neil, you've figured out I'm pretty pissed about CUP and at this moment cannot have anything to do with them--especially the soical side of the house that should have dismissed Harrison for his continuing coverup--especially after the release. DON't know what that means for my attendance, but I doubt you should be leveraged by the strong views of one participant, so if you keep CUP and Harrison in the mix, I'll just make my looked-forward-to vistit to Tyndall at another occassion. Between the dangerous moron I have for a President and the dishonesty of Lomborg and Harrison, I just don't need to be asscociated with more such stress. Life, as I've learned the hard way, is too short to spend it fighting polemicists at every turn, when there are so many decent people in the world.

I'm sorry I will likely miss your decent crowd--many people I like and respect on the invite list--because of this unshakable position of mine. Perhaps there will be another time. Cheers, Steve
On Tue, 18 Mar 2003, Neil Adger wrote:

> Dear Steve

>

> I am chasing you up on the Justice and Adaptation Meeting 7th-9th Sepetember
> here at UEA.

>

> I noted in my diary that your schedule should be appparent by now and that
> you can confirm (hopefully) that you can make it.

>

> The programme is now fairly well firmed up - you will have had a note a few
> weeks ago from John Turnpenny here in Tyndall concerning providing an exact
> title and a few sentence abstract or summary of your talk. I very muich hope
> you can confirm your participation and can give us some intial thoughts on
> the paper. I know that John Schellnhuber and Mike Hulme (among others) are
> keen for you to visit and very much looking forward to the Sustainability
> Days events.

>

> The other reason for asking for a confirmed title is that a publisher has
> already expressed an interest in a resulting book from the conference. This
> is Chris Harrison of CUP, who tells me that he met you at a recent AAAS
> session on politicisation of science. As you know Chris was the editor

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> responsible for publishing Lomborg's book. I really hope this doesn't put
> you off! We do not intend to publish a 'Lomborg'! For one, we will be
> undergoing proper review processes. And for two, we are not necessarily
> committed to CUP.

> I look forward to hearing from you.

> Best wishes

> Neil

> ----- Original Message -----

> From: "Stephen H Schneider" <shs@stanford.edu>

> To: "Neil Adger" <N.Adger@uea.ac.uk>

> Sent: Friday, January 24, 2003 3:52 AM

> Subject: Re: Fw: Justice and Adaptation Meeting - Spetember 2003

> > Many thanks Neil for the reminder of the original invitation--just about
> > got to that among 1000 backed up e-mails from 3 weeks away from
> > computers--thank god for SPAM, I can delete about 500 of the old messages
> > in seconds each!

> > In any case, a quick look at your meeting has two strong positives:
> > first I've promised Mike and John that I'd visit soon and have so far been
> > delinquent in that, and second, it is a very interesting program you've
> > put together. The negatives are my overtraveling and a possible conflict
> > with the Beijer institute of Ecol Econ meetings in early Sept of which I
> > am a regular participant--but the dates aren't set yet. So if you can take
> > a tentative yes for another month or so, that will have to suffice. If you
> > must know for sure now, then better ask someone with a saner life and a
> > clearer schedule. I suspect--subjective probabilities always from me
> > about future events!--about a 70% chance I'll do it, but my priors won't
> > get revised for probably another 6 weeks if you can wait. Thanks again,
> > and my best wishes to you, Mike and John for the wonderful new and
> > exciting programs building at UEA. Cheers, Steve

> > On Wed, 8 Jan 2003, Neil Adger wrote:

> > > Dear Steve

> > > In case you didn't receive this, or are still contemplating it, I copy
> > below

> > > again my message from 17th December. Look forward to hearing from you.

> > > Best wishes for 2003

> > > Yours sincerely

> > > Neil Adger

> > > >

> > > > shs@stanford.edu

> > > > 17th December 2002

> > > > Dear Professor Schneider

> > > > Justice in Adaptation to Climate Change

> > > > I hope this finds you well. I write on behalf of myself and my
> > colleagues

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> > > to
> > > invite you to give a paper an this upcoming event we are planning for
> > > 6-8th
> > > September 2003 here at the University of East Anglia.
> > > >
> > > > The conference is part of a strategic assessment we are undertaking on
> the
> > > > justice and equity aspects of adaptation actions. But it also forms
> part
> > > of
> > > > Third Sustainability Days, a week of events here at UEA celebrating,
> among
> > > > other things, the opening of the new Zuckerman Institute for
> Connective
> > > > Environmental Research which will host Tyndall Centre, CESERGE and
> other
> > > > interdisciplinary research centres. It promises to be an eventful
> week.
> > > The
> > > > justice conference will straddle some of these events, including the
> > > > inaugural Zuckerman Institute lecture on Monday 7th September. Hence
> there
> > > > will be an opportunity to interact with other scientists and
> researchers
> > > > within the third Sustainability Days.
> > > >
> > > > Information on the justice conference is attached. We would very much
> like
> > > > you to contribute an important paper on the third issue (in the
> questions
> > > > listed) of dangerous climate change following your writing on this
> issue.
> > > > This should address not only the need for adaptation and the need for
> > > > quantified assessment of the likelihood of alternative futures, but
> also
> > > > the
> > > > implications of developments in this area for framing adaptation
> response.
> > > > We see your contribution as a key paper which frames some of the
> > > > discussions
> > > > on optimal adaptation.
> > > >
> > > > The conference will have about 30 invited participants and will be run
> in
> > > > plenary throughout. We are inviting key philosophers, economists,
> climate
> > > > scientists and geographers to explore theoretical and applied areas of
> > > > these
> > > > justice questions through the two full days of discussions. We will
> also
> > > > present our own work being undertaken by CSERGE, FIELD and IIED on
> > > > international to local dimensions of the problem. As a starting point
> I
> > > > attach a copy of a Tyndall Centre Working Paper 23 which outlines our
> > > > framework.
> > > >
> > > > If you agree, we will expect a written paper by 1st July 2003 for
> > > > circulation prior to the meeting. We plan to publish all the
> commissioned
> > > > papers in an edited book with a highly quality academic publishing
> house.
> > > > Papers, following the conference and discussions, will be reviewed and
> > > will

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of modern 20th century history, I think you have understood the lessons of that century

well - it is regrettable that the French have not.

I for one will not be turning my back on the Labour Party.

Yours sincerely,

Professor Mike Hulme

At 18:20 20/03/2003 +0000, you wrote:

20 March 2003

Dear Colleague

I am writing to you following the House of Commons vote earlier this week and the beginning of military action in Iraq.

Our party has held its discussions on this issue without rancour and with respect for each others' views. There are deeply held views and that is natural, for there are few more serious choices a country can face than whether or not to take part in military action.

The Government has taken the decision to use military action to ensure the disarmament of Iraq, not because we have any quarrel with the people of Iraq - in fact they have suffered more than anyone under the tyrannical Iraqi regime. We have done so to enforce the many UN resolutions on Iraq and its weapons of mass destruction which have been passed over the years.

For many years the Labour Party has firmly supported attempts to stop the proliferation of weapons of mass destruction, but I can assure you that there was no inevitability about military action in Iraq. Saddam Hussein could have chosen to comply with the UN and disarm peacefully. Instead, for twelve years he defied its decisions, misled its inspectors and used every means possible to hold on to and develop his chemical and biological weapons.

In addition his brutal dictatorship has engaged in a sustained campaign of repression against his own people. The death and torture camps, barbaric prisons for political opponents and routine beatings for anyone suspected of disloyalty are well documented. If Saddam Hussein's regime continues in this way, many more Iraqi people will be killed and tortured in the future.

All the while he has hoped that division between countries and uncertain public opinion in the democracies would weaken our resolve and allow him to carry on in power unchecked.

What he has failed to understand is that democracy and open debate are strengths not weaknesses. In all matters, however, there comes a point when a judgement has to be made. Having taken our decision, this country will now pursue our aims with firm resolve and with determination.

Yet if we only disarm Saddam, we will not have completed our task. It is also vital that the world engages in a sustained humanitarian effort to help the people of Iraq after their years of living under such a repressive regime. Sixty per cent of the Iraqi population is today dependent on food aid, despite the fact that the Oil for Food Programme allows Saddam to sell as much oil as he wants in order to provide food for his people.

That situation cannot continue. As I said in the debate in the House of Commons this week, the United Nations should be authorised to meet the humanitarian needs of the Iraqi people. Iraq's territorial integrity should be protected and Iraq's oil revenues, which some people falsely claim are a reason for military action, should be put in a trust fund for the Iraqi people administered through the UN.

I also know that many in the Labour Party care deeply about the plight of people whose lives are being devastated by lack of progress in the Middle East peace process.

That's why last Friday's announcement by President Bush agreeing to publish the Middle East Roadmap is such a significant step. It provides the route to a permanent, two state solution with clear phases and target dates aimed at progress through steps by both sides in all the relevant areas. And the destination is a final and comprehensive settlement of the Israel - Palestinian conflict by 2005. I am determined that we should use all our influence to secure the implementation of this vision for the future of the Middle East.

Our vision for the future of Iraq is of a country free of repression able to

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live peacefully alongside its neighbours and develop in a way its own people choose. It is I believe a progressive vision.

We may face difficult times ahead but the decision we have taken is right. It is important now that our party and our country come together and support our armed forces in the task they face.

Yours sincerely,

Rt Hon Tony Blair MP

Leader of the Labour Party

m.hulme@uea.ac.uk

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The Labour Party

2461. 2003-03-21

date: Fri Mar 21 15:59:36 2003

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Re: Fwd: Soon & Baliunas

to: Tom Crowley <tcrowley@duke.edu>

Tom - sorry for the delay in replying. I am interested in doing this, and can change the

figure layout. The reason for the slow delay is that I was attempting to estimate

uncertainty ranges on the re-calibrated composite - but haven't succeeded yet! The

difficulty is that I cannot simply combine the published uncertainty ranges under the

assumption that they are independent series - because they have common proxy data in,

especially early on. On the other hand, I cannot simply use the calibration statistics of

the composite to estimate uncertainty ranges, since that ignores the deterioration in

reliability early on that occurs in some of the constituent reconstructions because of

fewer proxy records early on. I'm trying to figure out how to combine the two.

Anyway, I

hope to sort this out next week.

Best regards

Tim

At 16:04 13/03/03, you wrote:

tim,

I like what you have done and do think it could be used in a paper, if we choose to go

ahead with it. I would suggest however that you plot the previously published reconstructions in dashed lines so that the composite stands out better - you

also do

not have a label for the composite. needs to be inserted. can you try a different

color to distinguish the composite from the observations? say brown?

we are now doing simulations back to 8000 BP - would it be possible to obtain your

composite reconstruction to compare with our results? maybe we can do a separate paper

comparing the model with long composite.

regards, Tom

This is an excellent idea, Mike, IN PRINCIPLE at least. In practise, however, it raises

some interesting results (as I have found when attempting this myself) that may be

difficult to avoid getting bogged down with discussing.

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The attached .pdf figure shows an example of what I have produced (NB. please don't circulate this further, as it is from work that is currently being finished off - however, I'm happy to use it here to illustrate my point). I took 7 reconstructions and re-calibrated them over a common period and against an observed target series (in this case, land-only, Apr-Sep, >20N - BUT I GET SIMILAR RESULTS WITH OTHER CHOICES, and this re-calibration stage is not critical). You will have seen figures similar to this in stuff Keith and I have published. See the coloured lines in the attached figure. In this example I then simply took an unweighted average of the calibrated series, but the weighted average obtained via an EOF approach can give similar results. The average is shown by the thin black line (I've ignored the potential problems of series covering different periods). This was all done with raw, unsmoothed data, even though 30-yr smoothed curves are plotted in the figure. The thick black line is what I get when I re-calibrate the average record against my target observed series. THIS IS THE IMPORTANT BIT. The *re-calibrated* mean of the reconstructions is nowhere near the mean of the reconstructions. It has enhanced variability, because averaging the reconstructions results in a redder time series (there is less common variance between the reconstructions at the higher frequencies compared with the lower frequencies, so the former averages out to leave a smoother curve) and the re-calibration is then more of a case of fitting a trend (over my calibration period 1881-1960) to the observed trend. This results in enhanced variability, but also enhanced uncertainty (not shown here) due to fewer effective degrees of freedom during calibration. Obviously there are questions about observed target series, which series to include/exclude etc., but the same issue will arise regardless: the analysis will not likely lie near to the middle of the cloud of published series and explaining the reasons behind this etc. will obscure the message of a short EOS piece. It is, of course, interesting - not least for the comparison with borehole-based estimates - but that is for a separate paper, I think. My suggestion would be to stick with one of these options:

- (i) a single example reconstruction;
- (ii) a plot of a cloud of reconstructions;
- (iii) a plot of the "envelope" containing the cloud of reconstructions (perhaps also the envelope would encompass their uncertainty estimates), but without showing the individual reconstruction best guesses.

How many votes for each?

Cheers
Tim

At 15:32 12/03/03, Michael E. Mann wrote:
p.s. The idea of both a representative time-slice spatial plot emphasizing the spatial

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variability of e.g. the MWP or LIA, and an EOF analysis of all the records is a great idea. I'd like to suggest a small modification of the latter:
I would suggest we show 2 curves, representing the 1st PC of two different groups, one of empirical reconstructions, the other of model simulations, rather than just one in the time plot.
Group #1 could include:
1) Crowley & Lowery
2) Mann et al 1999
3) Bradley and Jones 1995
4) Jones et al, 1998
5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD reconstruction]
6) Esper et al [yes, no?--one series that differs from the others won't make much of a difference]
I would suggest we scale the resulting PC to the CRU 1856-1960 annual Northern Hemisphere mean instrumental record, which should overlap w/ all of the series, and which pre-dates the MXD decline issue...
Group #2 would include various model simulations using different forcings, and with slightly different sensitivities. This could include 6 or so simulation results:
1) 3 series from Crowley (2000) [based on different solar/volcanic reconstructions],
2) 2 series from Gerber et al (Bern modeling group result) [based on different assumed sensitivities]
1) Bauer et al series (Claussen group EMIC result) [includes 19th/20th century land use changes as a forcing].
I would suggest that the model's 20th century mean is aligned with the 20th century instrumental N.Hem mean for comparison (since this is when we know the forcings best).
I'd like to nominate Scott R. as the collector of the time series and the performer of the EOF analyses, scaling, and plotting, since Scott already has many of the series and many of the appropriate analysis and plotting tools set up to do this.
We could each send our preferred versions of our respective time series to Scott as an ascii attachment, etc.
thoughts, comments?
thanks,
mike
At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:
Thanks Tom,
Either would be good, but Eos is an especially good idea. Both Ellen M-T and Keith Alverson are on the editorial board there, so I think there would be some receptiveness to such a submission.
I see this as complementary to other pieces that we have written or are currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are doing for Science on the MWP) and this should proceed entirely independently of that.
If there is group interest in taking this tack, I'd be happy to contact Ellen/Keith

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about the potential interest in Eos, or I'd be happy to let Tom or Phil to
take the lead
too...
Comments?
mike
At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:
Phil et al,
I suggest either BAMS or Eos - the latter would probably be better because it
is shorter, quicker, has a wide distribution, and all the points that need to be
made have been made before.
rather than dwelling on Soon and Baliunas I think the message should be
pointedly made
against all of the standard claptrap being dredged up.
I suggest two figures- one on time series and another showing the spatial
array of temperatures at one point in the Middle Ages. I produced a few of those for
the Ambio paper but already have one ready for the Greenland settlement period 965-995
showing the regional nature of the warmth in that figure. we could add a few new sites to
it, but
if people think otherwise we could of course go in some other direction.
rather than getting into the delicate question of which paleo reconstruction
to use I suggest that we show a time series that is an eof of the different
reconstructions - one
that emphasizes the commonality of the message.
Tom
Dear All,
I agree with all the points being made and the multi-authored article
would be a good idea,
but how do we go about not letting it get buried somewhere. Can we not
address the misconceptions by finally coming up with definitive dates for the LIA and MWP
and redefining what we think the terms really mean? With all of us and more on
the paper,
it should carry a lot of weight. In a way we will be setting the agenda for what should
be being done
over the next few years.
We do want a reputable journal but is The Holocene the right vehicle. It
is probably the best of its class of journals out there. Mike and I were asked to write an
article for the EGS
journal of Surveys of Geophysics. You've not heard of this - few have, so we
declined.
However,
it got me thinking that we could try for Reviews of Geophysics. Need to
contact the editorial
board to see if this might be possible. Just a thought, but it certainly has
a high profile.
What we want to write is NOT the scholarly review a la Jean Grove (bless
her soul)
that

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just reviews but doesn't come to anything firm. We want a critical review that enables agendas to be set. Ray's recent multi-authored piece goes a lot of the way so we need to build on this.

Cheers

Phil

At 12:55 11/03/03 -0500, Michael E. Mann wrote:

Hi Malcolm,

Thanks for the feedback--I largely concur. I do, though, think there is a particular problem with "Climate Research". This is where my colleague Pat Michaels now publishes exclusively, and his two closest colleagues are on the editorial board and review editor board. So I promise you, we'll see more of this there, and I personally think there *is*

a bigger problem with the "messenger" in this case...

But the Soon and Baliunas paper is its own, separate issue too. I too like Tom's latter

idea, of a more hefty multi-authored piece in an appropriate journal (Paleoceanography?

Holocene?) that seeks to correct a number of misconceptions out there, perhaps using

Baliunas and Soon as a case study ('poster child'?), but taking on a slightly greater territory too.

Question is, who would take the lead role. I *know* we're all very busy, mike

At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

I'm with Tom on this. In a way it comes back to a rant of mine to which some of you have already been victim. The general point is that there are two arms of climatology:

neoclimatology - what you do based on instrumental records and direct, systematic observations in networks - all set in a very Late Holocene/Anthropocene time with hourly to decadal interests.

paleoclimatology - stuff from rocks, etc., where major changes in the Earth system, including its climate, associated with major changes in boundary conditions, may be detected by examination of one or a handful of paleo records.

Between these two is what we do - "mesoclimatology" - dealing with many of the same phenomena as neoclimatology, using documentary and natural archives to look at phenomena on interannual to millennial time scales. Given relatively small changes in boundary conditions (until the last couple of centuries), mesoclimatology has to work in a way that is very similar to neoclimatology. Most notably, it depends on heavily replicated networks of precisely dated records capable of being either calibrated, or whose relationship to climate may be modeled accurately and precisely.

Because this distinction is not recognized by many (e.g. Sonnechkin, Broecker, Karlen) we see an accumulation of misguided attempts at describing the climate of recent millennia. It would be better to head this off in general, rather than draw attention to a bad paper. After all, as Tom rightly says, we could all nominate really bad papers that have been published in journals of outstanding reputation (although there could well be differences between our lists).

End of rant, Cheers, Malcolm

Hi guys,

junk gets published in lots of places. I think that what could be done is a short reply to the authors in Climate Research OR a SLIGHTLY

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longer note in a reputable journal entitled something like "Continuing Misconceptions About interpretation of past climate change." I kind of like the more pointed character of the latter and submitting it as a short note with a group authorship carries a heft that a reply to a paper, in no matter what journal, does not.

Tom

> Dear All,
> Apologies for sending this again. I was expecting a stack of
> emails this morning in
> response, but I inadvertently left Mike off (mistake in pasting)
> and picked up Tom's old
> address. Tom is busy though with another offspring !
> I looked briefly at the paper last night and it is appalling -
> worst word I can think of today
> without the mood pepper appearing on the email ! I'll have time to
> read more at the weekend
> as I'm coming to the US for the DoE CCPP meeting at Charleston.
> Added Ed, Peck and Keith A.
> > onto this list as well. I would like to have time to rise to the
> bait, but I have so much else on at
> the moment. As a few of us will be at the EGS/AGU meet in Nice, we
> should consider what
> to do there.
> The phrasing of the questions at the start of the paper
> determine the answer they get. They
> have no idea what multiproxy averaging does. By their logic, I
> could argue 1998 wasn't the
> warmest year globally, because it wasn't the warmest everywhere.
> With their LIA being 1300-
> 1900 and their MWP 800-1300, there appears (at my quick first
> reading) no discussion of
> synchronicity of the cool/warm periods. Even with the instrumental
> record, the early and late
> 20th century warming periods are only significant locally at
> between 10-20% of grid boxes.
> Writing this I am becoming more convinced we should do
> something - even if this is just
> to state once and for all what we mean by the LIA and MWP. I think
> the skeptics will use
> this paper to their own ends and it will set paleo back a number of
> years if it goes
> unchallenged.
>
> I will be emailing the journal to tell them I'm having
> nothing more to do with it until they
> rid themselves of this troublesome editor. A CRU person is on the
> editorial board, but papers
> get dealt with by the editor assigned by Hans von Storch.
>
> Cheers
> Phil
>
> Dear all,
> Tim Osborn has just come across this. Best to ignore
> probably, so don't let it spoil your
> day. I've not looked at it yet. It results from this journal
> having a number of editors. The
> responsible one for this is a well-known skeptic in NZ. He has let
>
> a few papers through by
> Michaels and Gray in the past. I've had words with Hans von Storch
>

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>about this, but got nowhere.
> Another thing to discuss in Nice !
>
> Cheers
> Phil
>
>>X-Sender: f055@pop.uea.ac.uk
>>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>To: p.jones@uea
>>From: Tim Osborn <t.osborn@uea.ac.uk>
>>Subject: Soon & Baliunas
>>
>>
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>Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
>/CARO) (00016021)
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4687. 2003-03-21

cc: "Mike Hulme" <m.hulme@uea.ac.uk>
date: Fri, 21 Mar 2003 12:03:47 -0000
from: "Neil Adger" <N.Adger@uea.ac.uk>
subject: Re: Fw: Justice and Adaptation Meeting - September 2003
to: "Stephen H Schneider" <shs@stanford.edu>
Dear Steve
Thanks for the forthright message.

cg2003

We wish you to come to the conference. We will not be using CUP as a publisher.

The important issue for us is the intellectual debate and taking forward ideas that may be of use to society. The right to publishing these ideas will be given to a publisher who everyone in the group believes to be sharing these aims.

I agree with your sentiments and position over CUP - I believe that the publishers see the Lomborg affair only in terms of sales, though was not aware that they still defend the scientific nature of Lomborg's polemic. I was also not aware of the efforts being made to ensure that CUP does not publish the MA etc. Your information and views will be of use to Mike and the rest of us in Tyndall since we are contemplating a book series. I raised the possibility of CUP in my message precisely because I believed you had strong views on them.

So hopefully with the hurdle cleared that we will not be approaching CUP or Chris Harrison, we can still count on your 80 percent assurance of attending. We strongly believe that our meeting will be a worthwhile and stimulating event.

I also sympathise with having a 'dangerous moron for a President' - indeed the world has gone mad. So let's make a difference in what we can do to promote justice and equity.

I look forward to hearing from you.

Best wishes

Neil

cc Mike

----- Original Message -----

From: "Stephen H Schneider" <shs@stanford.edu>

To: "Neil Adger" <N.Adger@uea.ac.uk>

Cc: "Mike Hulme" <m.hulme@uea.ac.uk>

Sent: Friday, March 21, 2003 5:25 AM

Subject: Re: Fw: Justice and Adaptation Meeting - September 2003

> Many apologies Neil for my rude delay. I have just about cleared that I
> don't have a conflict in Stockholm, and if I go the the Berlin IPCC
> Scoping meeting 2 in Berlin it is two days before your meeting. My docs
> say that as long as my next two test have only background levels of nasty
> cells, I don't need any treatments and thus can travel--treatments mean
> immune compromise and eliminate airplanes for a while. So my Bayesian
> priors on this were around 80%--until you mentioned a big stumbling
> block. I cannot now, nor unless they withdraw the Lomborg book as
> science and apologize to the scientific community for a scientific fraud,
> nor can I EVER work with Chris Harrison. His talk at the AAAS was
> deceitful maneuvering--wrapping him self up in an authors right to speak
> and citing all the university visits Lomborg was paid to go to as proof
> how important his "challenge" was. Pure deceit. Chris didn't mention
> that at each BL was roundly trounced by angry folks. Lomborg is a
> non-rewritable CD. If a debater wins a point, he just repeats his litany
> at the end. That Harrison should cover his butt for such an egregious
> error of scientifically incompetent reviewing is the part that
> is unpardonable, if not unethical. I have no problems with a 150 page
> polemic--many of us write them. But we don't do 500 pages and 3000
> references--elliptically selected from the happy end of the literature of
> four fields: climatology, demography, conservation biology and energy
> systems. The odds of that if every one of those field had only random
> skill is 1 out of 16--that all four got in wrong in the same
> direction--overstatement. This is NOT random, it is egregious scientific
> fraud on the part of Lomborg and until Harrison comes clean with the
> scientific community and denounces The Skeptical Environmentalist as
> political polemic, not science, neither I nor most ecologists I know will
> have any thing to do with them. CUP lost the Millennium Assessment because
> of a peasant revolt from ecologists--and they deserved it.
> I feel very strongly that the incompetent peers CUP used are no excuse
> for the horrific failure of scientific peer review. If Harrison keeps his
> duck and cover polemics, then I and many of my colleagues will continue to

cg2003

> excoriate him as dishonest and covering up fraud. So if you use Cambridge,
> then no thanks. Sorry to be so blunt, but only a full retraction from
> Harrison will satisfy me and most of my ecologist friends who have
> literally wasted months refuting the Lomborgian polemics and then having
> to endure specious accusations that we don't want to "hear the other
> side". IN science there aren't two sides, but many outcomes and many
> subjective probabilities attached to each. Lomborg--with Harrison
> covering him up still!--did not talk in subjective probabilities, just
> selective ranges and point values. He can't even do statistics right!.
> Scientific peer review is not about equality, but quality. Equality is
> everyone should get an opportunity to vet his/her science to the knowledge
> community--what CUP peer review was supposed to do. They must have
> selected dead above the ears reviewers if any at all, and thus this whole
> horrendous caper was launched--by the way, CUP and Lomborg are crying all
> the way to the bank.
> I guess, Neil, you've figured out I'm pretty pissed about CUP and at
> this moment cannot have anything to do with them--especially the soical
> side of the house that should have dismissed Harrison for his continuing
> coverup--especially after the release. DON't know what that means for my
> attendance, but I doubt you should be leveraged by the strong views of one
> participant, so if you keep CUP and Harrison in the mix, I'll just make my
> looked-forward-to visit to Tyndall at another occasion. Between the
> dangerous moron I have for a President and the dishonesty of
> Lomborg and Harrison, I just don't need to be associated with more such
> stress. Life, as I've learned the hard way, is too short to spend it
> fighting polemicists at every turn, when there are so many decent people
> in the world.
> I'm sorry I will likely miss your decent crowd--many people I like
> and respect on the invite list--because of this unshakable
> position of mine. Perhaps there will be another time. Cheers, Steve

> On Tue, 18 Mar 2003, Neil Adger wrote:

> > Dear Steve

> > I am chasing you up on the Justice and Adaptation Meeting 7th-9th
September
> > here at UEA.

> > I noted in my diary that your schedule should be apparent by now and
that

> > you can confirm (hopefully) that you can make it.

> > The programme is now fairly well firmed up - you will have had a note a
few

> > weeks ago from John Turnpenny here in Tyndall concerning providing an
exact

> > title and a few sentence abstract or summary of your talk. I very much
hope

> > you can confirm your participation and can give us some initial thoughts
on

> > the paper. I know that John Schellnhuber and Mike Hulme (among others)
are

> > keen for you to visit and very much looking forward to the
Sustainability

> > Days events.

> > The other reason for asking for a confirmed title is that a publisher
has

> > already expressed an interest in a resulting book from the conference.
This

> > is Chris Harrison of CUP, who tells me that he met you at a recent AAAS
> > session on politicisation of science. As you know Chris was the editor

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> > responsible for publishing Lomborg's book. I really hope this doesn't
put
> > you off! We do not intend to publish a 'Lomborg'! For one, we will be
> > undergoing proper review processes. And for two, we are not necessarily
> > committed to CUP.
> >
> > I look forward to hearing from you.
> >
> > Best wishes
> >
> > Neil
> >
> >
> > ----- Original Message -----
> > From: "Stephen H Schneider" <shs@stanford.edu>
> > To: "Neil Adger" <N.Adger@uea.ac.uk>
> > Sent: Friday, January 24, 2003 3:52 AM
> > Subject: Re: Fw: Justice and Adaptation Meeting - Spetember 2003
> >
> >
> > > Many thanks Neil for the reminder of the original invitation--just
about
> > > got to that among 1000 backed up e-mails from 3 weeks away from
> > > computers--thank god for SPAM, I can delete about 500 of the old
messages
> > > in seconds each!
> > > In any case, a quick look at your meeting has two strong positives:
> > > first I've promised Mike and John that I'd visit soon and have so far
been
> > > delinquent in that, and second, it is a very interesting program
you've
> > > put together. The negatives are my overtraveling and a possible
conflict
> > > with the Beijer institute of Ecol Econ meetings in early Sept of which
I
> > > am a regular participant--but the dates arent set yet. SO if you can
take
> > > a tentative yes for another month or so, that will have to suffice. If
you
> > > must know for sure now, then better ask someone with a saner life and
a
> > > clearer schedule. I suspect--subjective probabilities always from me
> > > about future events!--about a 70% chance I'll do it, but my priors
won't
> > > get reviesed for probably another 6weeks if you can wait. Thanks
again,
> > > and my best wishes to you, Mike and John for the wonderful new and
> > > exciting programs building at UEA. Cheers, Steve
> > >
> > > On Wed, 8 Jan 2003, Neil Adger wrote:
> > >
> > > > Dear Steve
> > > >
> > > > In case you didn't receive this, or are still contemplating it, I
copy
> > > > below
> > > > again my message from 17th December. Look forward to hearing from
you.
> > > >
> > > > Best wishes for 2003
> > > >
> > > > Yours sincerely

cg2003

> > > >
> > > > Neil Adger
> > > >
> > > > >
> > > > > shs@stanford.edu
> > > > >
> > > > > 17th December 2002
> > > > >
> > > > > Dear Professor Schneider
> > > > >
> > > > > Justice in Adaptation to Climate Change
> > > > >
> > > > > I hope this finds you well. I write on behalf of myself and my
> > colleagues
> > > > to
> > > > > invite you to give a paper an this upcoming event we are planning
for
> > > > 6-8th
> > > > > September 2003 here at the University of East Anglia.
> > > > >
> > > > > The conference is part of a strategic assessment we are
undertaking on
> > the
> > > > > justice and equity aspects of adaptation actions. But it also
forms
> > part
> > > > of
> > > > > Third Sustainability Days, a week of events here at UEA
celebrating,
> > among
> > > > > other things, the opening of the new Zuckerman Institute for
> > Connective
> > > > > Environmental Research which will host Tyndall Centre, CESERGE and
> > other
> > > > > interdisciplinary research centres. It promises to be an eventful
> > week.
> > > > The
> > > > > justice conference will straddle some of these events, including
the
> > > > > inaugural Zuckerman Institute lecture on Monday 7th September.
Hence
> > there
> > > > > will be an opportunity to interact with other scientists and
> > researchers
> > > > > within the third Sustainability Days.
> > > > >
> > > > > Information on the justice conference is attached. We would very
much
> > like
> > > > > you to contribute an important paper on the third issue (in the
> > questions
> > > > > listed) of dangerous climate change following your writing on this
> > issue.
> > > > > This should address not only the need for adaptation and the need
for
> > > > > quantified assessment of the likelihood of alternative futures,
but
> > also
> > > > the
> > > > > implications of developments in this area for framing adaptation
> > response.
> > > > > We see your contribution as a key paper which frames some of the
> > > > discussions

cg2003

> > > Stanford, CA 94305-5020 U.S.A.

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> > > Tel: (650)725-9978

> > > Fax: (650)725-4387

> > > shs@stanford.edu

> > >

> >

>

> -----

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>

1730. 2003-03-24

date: Mon, 24 Mar 2003 12:24:55 +0100

from: "Michel Smitall" <MSmitall@unfccc.int>

subject: UNFCCC media advisory -- Joke Waller-Hunter urges

to: "Climate Change Info Mailing List" <climate-l@lists.iisd.ca>

UNFCCC media advisory

Joke Waller-Hunter urges a global carbon-constrained energy matrix
GENEVA, 24 March 2003 - Dealing with climate change hinges on an integrated
management of natural resources. Speaking at the World Meteorological Day
celebration in Geneva, the Executive Secretary of UNFCCC, Ms Joke
Waller-Hunter reminded the international community that climate change is a
problem that cannot be solved speedily. The effect of emissions of
greenhouse gases today will change the climate many decades into the
future, both because of the time lag in the removal of carbon dioxide from
the atmosphere and because of the long time it takes to warm the oceans.
Conversely, it is also true that current emission reductions will take a
long time to limit the change in the climate.

Waller-Hunter emphasized the fact that humanity has to develop less carbon
intensive means of satisfying its requirements for personal comfort,
transportation and mechanical work. This will imply a change in the
existing infrastructure in industrialized countries and the choice of a
sustainable path for developing countries. The energy sector is especially
critical. Policies here must clearly take into account the requirement for
a less carbon intensive energy matrix. "Action is required as a matter of
urgency", she said.

In view of the commitment by the Government of the Russian Federation to
seek the ratification of the Kyoto Protocol by Parliament, the Convention's
Executive Secretary expects that the treaty will enter into force this
year. "When this condition is met, the Clean Development Mechanism (CDM)
established by the Protocol will become an important practical exercise in
international cooperation aiming at ensuring a more sustainable path of
development", Waller-Hunter said.

1 - Full statement by Joke Waller-Hunter, UNFCCC Executive Secretary

2 - Opening address to the "Second WMO Conference on Women in Meteorology
and Hydrology"

3 - Webcast of latest CDM Executive Board meeting

4 - Press release of the World Meteorological Organisation

5 - WMO booklet: "Our Future Climate"

1 - Full statement

by Joke Waller-Hunter, UNFCCC Executive Secretary, on occasion of the World Meteorological Day 2003, "Our Future Climate" on 24 March 2003, in Geneva :
<http://unfccc.int/press/stat2003/jwh240303.pdf>

2 - Opening address

by Joke Waller-Hunter to the "Second WMO Conference on Women in Meteorology and Hydrology" on 24 March 2003 in Geneva :
<http://unfccc.int/press/stat2003/statem-wmo-240303.pdf>

3 - Webcast of latest CDM Executive Board meeting

The "Eighth meeting of the Executive Board to the CDM" took place from 19-20 March 2003 in Bonn, Germany. UNFCCC provided a full webcast of this meeting which also features the conclusion and summary of decisions.
<http://www.meta-fusion.com/kunden/unfccc/cdm030319/archiv.html>

4 - Press release

WMO calls for timely global action on climate. Recent occurrences of floods, tropical cyclones, droughts and other extreme weather- and climate-related events could well be glimpses of what a change in climate could bring upon us. The future cost of inaction to protect climate is expected to exceed by far the cost of timely action. Every year, the World Meteorological Day (WMD) is celebrated to commemorate the entry-into-force, on 23 March 1950, of the Convention of the World Meteorological Organization (WMO). The theme of this year is "Our future climate". National Meteorological and Hydrological Services throughout the world will celebrate WMD 03, and a ceremony will take place at WMO Headquarters in Geneva. <http://www.wmo.ch/web/Press/Press.html#pr>

5 - WMO booklet: "Our Future Climate"
<http://www.wmo.ch/wmd/pdf/wmd2003.pdf>

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 - Archives of Climate-L and Climate-L News are available online at:
<http://www.iisd.ca/email/climate-L.htm>

311. 2003-03-25

cc: J.skea@psi.org.uk, m.hulme@uea.ac.uk, PARRYML@aol.com,
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lorraine.hamid@fco.gov.uk, alan.apling@dfit.gsi.gov.uk
 date: Tue, 25 Mar 2003 05:55:43 EST

cg2003

from: PARRYML@aol.com
subject: initial vision of IPCC WGII
to: Sophia.Oliver@DEFRA.GSI.GOV.UK

Dear Sophia:

In reply to your circular about the IPCC's AR4, I am copying below an outline vision developed and discussed by the Bureau of WGII (Impacts and Adaptation). It is fluid.

There is also a strawman 25-chapter outline that reflects this broad 5-way division of the issues, but I think distribution of this would be premature since it is likely to change radically and could imply a level of detail in planning that has not yet been achieved.

It will however be tabled at the First Scoping Meeting (FSM), and following that meeting, I hope to be able to circulate something further.

The comments you provide to the FSM will be most helpful

STARTS

Key issues provisionally identified and discussed by WGII Bureau (at meetings in August and December 2002, and April 2003), that should be addressed in the WGII 4th Assessment Report

include:

1. Are there attributable impacts which are observable now? How far is it possible to distinguish between effects of natural climate variability and those of possible (early onset of) human-induced climate change. What can be learned from adaptation to natural variability as a basis for planning for adaptation to climate change.

2. What are the likely effects of future unmitigated climate change? This was partially answered in TAR, but attention is needed to these key sub-questions:

" What are the implied effects under different development pathways (eg as indicated by the SRES marker scenarios)?

" Are there critical levels of climate change where exceedance leads to non-linear or irreversible effects? Can this help inform the question: What is a dangerous climate change?

" What would be the effect of altered extreme weather events and patterns?

" What are the uncertainties/probabilities attached to these estimates of impact?

" Can we identify the regional and local as well as the global aspects of these issues?

Are there key vulnerable regions and sectors?

" Can we achieve a more balanced approach by rigorously analysing the balance between opportunities and challenges presented by potential impacts (noting there may have been, in

some cases, a tendency to emphasise negative effects).

3. How much of these effects could be avoided or reduced by adaptation?

" What are current and future estimated adaptive capacities?

" How would these capacities vary under different development pathways (eg SRES)?

" How could these be enhanced? At what cost, etc

4. What would be the estimated impacts/adaptation requirements under different levels of mitigation? (eg under different stabilisation scenarios for different development pathways). We need both:

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" Global assessments.

" Regional and local case studies

5. Can we begin to conduct some robust comparable analyses of mitigation and adaptation?

For example, what are the relative costs of these two responses and what combinations of

them might be most effective? This would require cross-cutting work with WG3.

ENDS

Kind regards,

Martin

5074. 2003-03-25

date: Tue, 25 Mar 2003 10:01:05 -0000

from: "Oliver, Sophia (GA)" <Sophia.Oliver@DEFRA.GSI.GOV.UK>

subject: Scoping for IPCC Fourth Assessment Report

to: "'J.skea@psi.org.uk'" <J.skea@psi.org.uk>, "'m.hulme@uea.ac.uk'"

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<rodger.lightbody@doeni.gov.uk>, "Dare, Barry (NAWAD)"

<Barry.Dare@Wales.GSI.Gov.UK>, "Prosser, Havard (NAWAD)"

<Havard.Prosser@Wales.GSI.Gov.UK>, "'john.houghton@jri.org.uk'"

<john.houghton@jri.org.uk>, DL - GAALL <DL-GAALL@DEFRA.GSI.GOV.UK>, "Derwent,

Henry (CEER)" <Henry.Derwent@defra.gsi.gov.uk>, "Dalton, Howard (SD)"

<howard.dalton@DEFRA.GSI.GOV.UK>, "Parker, Miles (SD)"

<miles.parker@DEFRA.GSI.GOV.UK>, "'terry.carrington@dti.gsi.gov.uk'"

<terry.carrington@dti.gsi.gov.uk>, "'tim_foy@dfid.gsi.gov.uk'"

<tim_foy@dfid.gsi.gov.uk>, "'robert.mason@fco.gov.uk'" <robert.mason@fco.gov.uk>,

"'lorraine.hamid@fco.gov.uk'" <lorraine.hamid@fco.gov.uk>,

"'alan.apling@dft.gsi.gov.uk'" <alan.apling@dft.gsi.gov.uk>

Dear all

The IPCC Secretariat wrote to governments recently to ask for comments on the structure and scope of the Fourth Assessment Report (AR4). This is the first meeting in a process which culminates in the final approval of the scope of the AR4 at the IPCC Plenary session in November 2003. It is important to get our ideas on the table as early as possible. The suggestions, which need to be received by the 28th March, will be given to participants at an expert meeting during April on the scope of the AR4.

The attached document, prepared in GA, is a first draft outlining preliminary ideas for the AR4 in terms of overall structure and approach, and lists specific areas of science that may require attention in the report. I would be grateful if you could email me with any comments that you

cg2003

have on the proposals, or any suggestions for areas that we have omitted.
Please reply by 9am on the 28th March - apologies for the short deadline.

Yours,
Sophia

<<AR4scoping_consult.doc>>

Dr Sophia Oliver
Global Atmosphere Division
DEFRA
3/C2 Ashdown House
123 Victoria Street, London SW1E 6DE
Tel: 020 7944 5232
Fax: 020 7944 5219
sophia.oliver@defra.gsi.gov.uk
Attachment Converted: "c:\eudora\attach\AR4scoping_consult.doc"

1776. 2003-03-27

date: Thu, 27 Mar 2003 16:05:07 -0800
from: Earth Government <earthgov@shaw.ca>
subject: Press release from Earth Government and April Newsletter
Press release from Earth Government and April Newsletter

FOR IMMEDIATE RELEASE

This Press release from Earth Government is found at

[1]<http://members.shaw.ca/earthgov/HNewsPR05.htm>

Formation of Earth Government for the good of all

March 27th, 2003

To all Peoples of the Earth,

Earth has long been waiting for a truly global governing body based on universal values,

human rights, global concepts and democracy. Earth Government might as well be created now,

there is no longer any reason to wait. We are the Earth Community, and we will form the

Earth Government. Earth management is a priority and is a duty by every responsible person.

A democratically elected Earth Government will now be formed, and we want you to reflect on

future effects of such an event on the history of humanity. Certainly one will expect

extraordinary changes: a reorganizing of human activities all over the planet; participation by all societies on the planet in solving local and global

problems; new

alliances forming; north meeting with south (eradication of poverty will be the price to

pay to get votes from the south) in order to gather more votes within the newly created

Earth Government to satisfy power struggles between European, Asian and Western countries;

adoption of democratic principles, human and Earth rights, global concepts, and universal

values by every human being; expansion of consciousness; gathering and coordinating of

forces to resolve social and political problems in a peaceful way (no more conflicts or

wars); gathering and coordinating of forces (technologies, scientific research, exploration

work, human resources, etc.) to resolve global problems such as global climate, environment, availability of resources, poverty, employment, etc. Thousands more changes!

Let your heart and mind reflect on 'the good' of a democratically elected Earth Government.

Everyone is part of Earth Community by birth and therefore everyone has a right

to vote.

Everyone should be given a chance to vote. Decisions will be made democratically.

Earth Government is proposing that:

- a) different nations may require different political systems at different times
- b) a democratic system is not a "must have it" to be a responsible member nation

of the

Earth Government

c) all democracies are to be upgraded, or improved upon, to be a responsible member nation

of the Earth Government. The Scale of Human and Earth Rights and the Charter of the Earth

Government are the newly added requirements to all democratic systems of the world.

In today's Earth Government it is important for our survival to cooperate globally on

several aspects such as peace, security, pollution in the air, water and land, drug trade,

shelving the war industry, keeping the world healthy, enforcing global justice for all,

eradicating poverty worldwide, replacing the Universal Declaration of Human Rights by the

Scale of Human and Earth Rights, and entrenching the Charter of Earth Government as a way

of life for the good of all.

Earth needs urgently a world system of governance. The United Nations fail to satisfy the

needs of the people of the 21st Century. It has never improved upon the old ways and

thinking of the middle of the 20th Century. Its voting system no longer satisfy the 6.157

billion people on Earth. The challenges are different and require a world organization up

for dealing with the needs of all these people.

During the past several years, the Earth Government has been pleading the United Nations

leaders to make changes in the UN organizational structure and ways of doing things. There

has been an urgent need for fundamental changes in the United Nations organization. The

decision of the United States Government to invade the Middle East nations and Afghanistan

has shown to be a result of this incapacity for changes on the part of the United Nations.

A lack of leadership at the United Nations is a major threat to the security of the world.

The world wants a true democratic world organization. The UN is not!

The most fundamental requirement of a world organization is a democratic system of voting.

Democracy must be a priority. The right that the greatest number of people has by virtue of

its number (50% plus one) is a human right. It should be respected. The actual UN system of

voting is undemocratic, unfair and noone likes it. It does not work! Earth Government has

proposed a voting system based on democracy.

Of the 190 Member States of the United Nations, it takes only one of the five permanent

members to overthrow any decision or proposal during a meeting. This means 1/189 or 0.5% of

the membership is more powerful than the remaining 99.5%. If that is not a dictatorship, what

is it? It does not say much about democracy at the UN. More like a dictatorship of

the five

permanent members. In the Preamble of the Charter of the United Nations, it says "WE THE PEOPLES OF THE UNITED NATIONS " but in fact it should say "WE THE FIVE PERMANENT MEMBERS".

The voting system for Earth Government is very simple and practical. One representative per million people. If all countries in the world had decided now to participate with this

process we would have today 6,114 elected representatives to form Earth Government. They

would form the Legislative body of Earth Government. They could actually all stay home to

govern or from some place in their communities. Today communications are more than good

enough to allow voting and discussing issues, etc. through the Internet and video

conferencing. That would cut cost of governing down to a minimum, at least administrative

costs. The Executive body would also govern in this way to cut cost down to a minimum.

Ministers can administer their Ministries from where they live if they wish to. There will

be a place for the Headquarters. We will show that it costs very little to administer Earth

Government, and that we can achieve immense results. There is no limit to the good the

Earth Government can achieve in the world. Think! What can do a unified 6.114 billion

people determined to make things work to keep Earth healthy?

For the first time in human history, and the first time this millennium, humanity has

proposed a benchmark:

- * formation of Earth Government
- * formation of global ministries in all important aspects of our lives
- * the Scale of Human and Earth Rights as a replacement to the Universal

Declaration of

Human Rights

* an evolved Democracy based on the Scale of Human and Earth Rights and the Charter of

the Earth Government

* a central organization for Earth management, the restoration of the planet and Earth

governance: the Global Community Assessment Centre (GCAC)

* the Earth Court of Justice to deal with all aspects of the Governance and Mangement of

the Earth

* a new impetus given to the way of doing business and trade

* more new, diversified (geographical, economical, political, social, business,

religious) symbiotical relationships between nations, communities, businesses, for the

good and well-being of all

* the event and formation of the human family and the Soul of Humanity

* proposal to reform the United Nations, the World Trade Organization, the World Bank,

the IMF, NAFTA, FTAA, and to centralize them under Earth Government, and these organizations will be asked to pay a global tax to be administered by Earth

Government

* the Peace Movement of the Earth Government and shelving of the war industry from

humanity

* a global regulatory framework for capitals and corporations that emphasizes

global

corporate ethics, corporate social responsibility, protection of human and Earth rights,

the environment, community and family aspects, safe working conditions, fair wages and

sustainable consumption aspects

* the ruling by the Earth Court of Justice of the abolishment of the debt of the poor or

developing nations as it is really a form of global tax to be paid annually by the rich

or industrialized nations to the developing nations

* establishing freshwater and clean air as primordial human rights

The political system of an individual country does not have to be a democracy.

Political

rights of a country belong to that country alone. Democracy is not to be enforced by anyone

and to anyone or to any community. Every community can and should choose the

political

system of their choice with the understanding of the importance of such a right on the

Scale of Human and Earth Rights. On the other hand, representatives to Earth Government

must be elected democratically in every part of the world. An individual country may have

any political system at home but the government of that country will have to ensure (and

allow verification by Earth Government) that representatives to Earth Government have been

elected democratically. This way, every person in the world can claim the birth right of

electing a democratic government to manage Earth: the rights to vote and elect representatives to form the Earth Government.

In order to elect representatives to Earth Government it is proposed the following:

A. Each individual government in the world will administer the election of representatives to Earth Government with an NGO and/or members of Earth

Government be

allowed to verify all aspects of the process to the satisfaction of all parties

involved.

B. Representatives be elected every five years to form a new Earth Government.

C. It is proposed here that there will be one elected representative per 1,000,000

people. A population of 100 million people will elect 100 representatives. This process

will create a feeling of belonging and participating to the affairs of the Earth

Community and Earth Government.

D. A typical community of a million people does not have to be bounded by a geographical

or political border. It can be a million people living in many different locations all

over the world. The Global Community is thus more fluid and dynamic. We need to let go

the archaic ways of seeing a community as the street where I live and contained by a

border. Many conflicts and wars will be avoided by seeing ourselves as people with a

heart, a mind and a soul, and as part of a community with the same.

E. Earth population is now 6.114 billion people. If all representatives had been elected

this year there would be 6,114 representatives to form Earth Government. They would be

the Legislative elected body of Earth Government. They would participate in some ways in

choosing the Executive and Judiciary bodies of Earth Government.

Humanity has now a Vision of the Earth in the years to come and a sense of direction.

May the DIVINE WILL come into our lives and show us the way.

May our higher purpose in life bring us closer to the Soul of Humanity and God.

Germain Dufour, President

Earth Community Organization (ECO) and Earth Government

The Newsletter can be found at the following location:

April 2003 Newsletter

[2]<http://members.shaw.ca/earthgov/NewsA.htm>

There are no costs in reading our Newsletters

([3]<http://members.shaw.ca/earthgov/EarthGovernment.htm>).

The Table of Contents of the Newsletter is shown here.

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- 5.0 Letter to the Moslem and the Arab Peoples
- 6.0 Letter to Jiang Zemin and Zhu Rongji of China, and to the Chinese People
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 - Improved Democracy, Nonviolence, and Peace
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 - Ecological Integrity
 - Social and Economic Justice
 - A new symbiotical relationship between that of spirituality and the protection of the global life-support systems
 - Scale of Human and Earth Right
 - Earth Court of Justice
 - Charter of Earth Government

May the DIVINE WILL come into our lives and show us the way.

May our higher purpose in life bring us closer to the Soul of Humanity and God.

Germain Dufour, President

[4]Earth Community Organization (ECO) and [5]Earth Government

cg2003

Website of the Earth Community Organization and of Earth Government

[6]<http://www.telusplanet.net/public/gdufour/>

[7]<http://members.shaw.ca/earthgov>

Email addresses

[8]gdufour@globalcommunitywebnet.com

[9]gdufour@telusplanet.net

[10]earthgov@shaw.ca

5062. 2003-03-27

date: Thu, 27 Mar 2003 22:51:47 +0100

from: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

subject: Re: AMS-Europe - WP1.3

to: Mike Hulme <m.hulme@uea.ac.uk>, Rik Leemans <Rik.Leemans@rivm.nl>, Rik Leemans <Rik.Leemans@wur.nl>

<x-flowed>

Dear Mike,

this time to you and Rik only:

I am having second thoughts about the thing below. I recognise that it might not be easy to extract those 75k from either John or Jonathan's WPs in AMS. Hence I wonder: if I convinced Rik to provide some of that sum out of 1.3, would it help? I think it should be possible, but perhaps not for the total.

In your notes, you mention us as possible recipient of that money: I am not so sure about that. Despite not knowing what Tim and you have concluded about Tim's visit to PIK, I rather feel that this is a CRU/Tyndall only job. If specific PIK resources are considered helpful (e.g. some of the long term records) then we should be able to provide those to you (and hence "ourselves") free of charge.

I also wonder what the Paris role could be for the climate information: Hallegatte mentions IPSL, but what could they contribute? If it's just another set of GCM runs, then those should come free of charge, I think. If additional partners are needed, then I sometimes wonder about Tim Carter - isn't he sort of a co-owner of your approach?

So hence I am voting, not for a work package, but for a clearly identifiable activity, led by you or Tim, for the climate information - receiving those 75k from two places, WP1.3 and another one which yet needs to be identified (I'll keep looking for it, but it MIGHT perhaps be one of Tyndall WPs).

Does that sound useful?

Best wishes,

Wolfgang

On 27/03/03 17:35, Wolfgang Cramer wrote:

> Dear Mike,

>

> you know how much I am in favour of this - so thank you very much for making this more specific than I would be able to do myself.

>

> From a management point of view, I would nevertheless support Carlo's views as follows

>

> - we stick to the fixed budget size WP structure (and probably also to the list of WPs we have now)

>

> - the 75k for the work of Tim Mitchell (which Carlo, too, thinks are very well justified) should be budgeted into one of the three (?) WPs led by the Tyndall Centre

>

> I understand that we could also take a distributed approach where everyone gives just a little, but I believe the administrative burden of that would be horrendous.

>

> Would that work?

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>
> Best wishes,
>
> Wolfgang
>
> On 27/03/03 17:27, Mike Hulme wrote:
>
>> Dear Rik - and other Scenarios WD people,
>>
>> Following 24 hours of some confusion - and having talked with
>> Jean-Charles and Wolfgang (I have tried to raise Carlo Jaeger today
>> for clarification, but with no luck, so I am still a little in the
>> dark) - it seems you are well on track for developing the WP1.3.
>>
>> May I therefore make sure you have seen the attached document from me
>> which circulated a week or so ago, concerning the role of climate
>> information in AMS-Europe. I have seen your comment that WP1.3
>> should *not* be about climate information - historic and future - and
>> whilst I can agree it should not necessarily be *primarily* about
>> climate information (although it could be if AMS wanted it so), then
>> it must at least pay some attention to climate information (otherwise
>> we are *entirely* dependent upon whatever climate information other
>> people and projects may just happen to produce - and as we know,
>> these things rarely happen to conform to people's needs just by
>> chance!). There seems to be a need to connect the storylines in
>> WP1.1 and economics of WP1.2, including inter alia stabilisation
>> pathways, with climate information and this connection is likely to
>> be unique to AMS-Europe (i.e., ENSEMBLES is unlikely to re-orient
>> itself, unresourced, to do this).
>>
>> So this is the main thrust of my short set of notes and I hope that
>> you can consider these when drafting the WP1.3 - which I have not yet
>> seen.
>>
>> Best wishes,
>>
>> Mike
>
>
>
>
>
--

Wolfgang Cramer, Department of Global Change and Natural Systems
Potsdam Institute for Climate Impact Research, PO Box 60 12 03
D-14412 Potsdam, Germany, Tel.: +49-331-288-2521, Fax: -2600
mail:Wolfgang.Cramer@pik-potsdam.de, www.pik-potsdam.de/~cramer

</x-flowed>

1330. 2003-03-28

date: Fri Mar 28 12:49:53 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: Borehole temperatures
to: "Henry N. Pollack" <hpollack@umich.edu>

Dear Henry,
many thanks for your help with parameter values of rock properties etc. while I was visiting Simon Tett at the Hadley Centre earlier this week. We took deep (4 m) soil temperatures from their model simulation under volcanic and solar forcings, and diffused them down using a 1D diffusion model. With little overall warming trend in this simulation, they obviously don't agree with the observed "transient profile", so not much point in circulating our results just yet. But when the

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simulation with anthropogenic forcings too is completed in the next couple of months, we should get some interesting results.

Best wishes

Tim

4192. 2003-03-28

cc: Rik Leemans <Rik.Leemans@rivm.nl>, Wolfgang Cramer

<Wolfgang.Cramer@pik-potsdam.de>

date: Fri, 28 Mar 2003 12:54:39 +0100

from: Rik Leemans <Rik.Leemans@rivm.nl>

subject: Re: AMS-Europe - WP1.3

to: Mike Hulme <m.hulme@uea.ac.uk>

I agree with you. But if we include you, we have to exclude Tim, because the shares become just too small.

I further do not know, if we can actually use the grant to fund PhD-student or that is only for post-docs. If the latter, the Wageningen fee is about 90000 euros a year. I want to have at least a post-doc for 2 years: ie 180000, Wolfgang would get the same for Dagmar.

if we provide Mark Rounsevell with 25000 to provide some land-use scenario input, that would leave 65 for you Mike. Unfortunately, this does not include any travel, equipment, etc.

I would actually like to use a dutch PHD-student (who had a 5-year msc education) to perform the work. Then the costs come down for me to 160000 euros for 4 years work of a person. Much more cost effective.

So I am still in doubt how to share, please give suggestions here!

Mike Hulme

<m.hulme@uea.ac.uk> To: Wolfgang Cramer
<Wolfgang.Cramer@pik-potsdam.de>, Rik Leemans
c.uk> <Rik.Leemans@rivm.nl>

cc:

28-03-03 12:22

Subject: Re: AMS-Europe - WP1.3

Rik and Wolfgang,

I have spoken with Tim Carter also he is not sure at all what FEI will be asked to do for WP1.3 (and also very sceptical that ENSEMBLES will really make progress in "designer climate information"). I would therefore make a stronger argument for a resource person in WP1.3, ~€75k, to be able to develop consistent climate information in the Scenarios WD, and which will be useful in the other AMS Work Domains. I suggest that person be Tim Mitchell here in Tyndall.

As I have said to Carlo, if WP1.3 does not address this - even in this basic minimum way - the AMS-Europe project will be a "climate-information free zone" - this is not what we want.

Mike

At 09:39 28/03/2003 +0100, Wolfgang Cramer wrote:

>Rik, thanks for that clarification... I still think we should actually
>support Tim Mitchell partly through this WP. I am also happy about any
>involvement of Tim - but I would like Mike's views on whether this makes
>for a productive addition here.

>

>Wolfgang

>

>On 28/03/03 09:25, Rik Leemans wrote:

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>
>>Dear Mike,
>>
>>Sorry to be so little communicative, but things were changing so fast and I
>>did not know how to effectively proceed and simultaneously involve all the
>>necessary player.
>>I just returned to the office after a day on a very different topic the
>>World Barley, malt, and Beer conference and saw your mail. Sorry therefor
>>for my late response.
>>
>>When I was asked to coordinate WP1.3 last week, I did not have a clue on
>>what should go into it. Early this week, therefore I drafted the document
>>that you saw, which had quite some overlap with the other Wps. This were
>>my
>>preliminary ideas. I still strongly believe that we should do some of the
>>impacts (from emissions to impacts) in this wp. Climate is a part of that
>>but not the only one. For me the innovative part is not only to base the
>>impact assessment on the quantitative climate scenarios but also on the
>>qualitative narratives, which help to define resilience, sensitivity etc.
>>Additionally from a systemic point of view the interactions between
>>climate, impacts and concentrations are important. This was my main
>>philosophy in drafting the WPs.
>>
>>I also had a call with Tim Carter on his involvement in other project and
>>learned several thing from him. He was very hesitant to become involved,
>>although some money for Suzanne for literature review (She did a great
job
>>on collecting scenarios information for forestry developments in EU
>>countries) was always welcome.
>>
>>I have also been thinking to involve the IMAGE group a little stronger (I
>>am not with them any more) but are hesitating because they want to focus
on
>>the mitigation strategies.
>>
>>I had indeed made the assumption that for the climate scenarios, we
should
>>use off-the shell material but I believe that it could be a good idea to
>>actually grant a little more money into to get some, for impact
assessment,
>>important climate variables from the latest runs.
>>
>>Brian is organising at 11.00 a telephone conference to discuss the wps,
why
>>do you not try to be involved in that discussion (I'll give brian a call
to
>>invite you as well).
>>
>>Rik
>>
>>
>>
>>
>>
>>Mike
>> Hulme
>>
>>
>> <m.hulme@uea.a To: "Leemans, Rik"
>> <Rik.Leemans@wur.nl>, alex.haxeltine@uea.ac.uk,
>> c.uk> mike.hulme@uae.ac.uk,
>> wolfgang.cramer@pik-potsdam.de, hallegatte@centre-cired.fr,

cg2003

>> hourcade@centre-cired.fr, Brian
>> O'NEILL <oneill@iiasa.ac.at>, naki@iiasa.ac.at,
>> 27-03-03 17:27 stapelbroek@hetnet.nl,
>> Rik.Leemans@rivm.nl, "S.E. van der Leeuw"
>> <vanderle@wanadoo.fr>,
>> t.mitchell@uea.ac.uk
>> CC:
>> Carlo.Jaeger@pik-potsdam.de, Armin Haas
>> <haas@pik-potsdam.de>
>> Subject: AMS-Europe -
>> WP1.3
>>
>>
>>
>>
>>
>>Dear Rik - and other Scenarios WD people,
>>
>>Following 24 hours of some confusion - and having talked with
Jean-Charles
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>>
>>So this is the main thrust of my short set of notes and I hope that you
can
>>
>>consider these when drafting the WP1.3 - which I have not yet seen.
>>
>>Best wishes,
>>
>>Mike
>>(See attached file: Climate information in AMS.doc)
>>
>>
>
>
>--

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>-----
>Wolfgang Cramer, Department of Global Change and Natural Systems
>Potsdam Institute for Climate Impact Research, PO Box 60 12 03
>D-14412 Potsdam, Germany, Tel.: +49-331-288-2521, Fax: -2600
>mail:Wolfgang.Cramer@pik-potsdam.de, www.pik-potsdam.de/~cramer
>-----
>

4516. 2003-03-29

cc: Mike Hulme <m.hulme@uea.ac.uk>
date: Sat, 29 Mar 2003 00:32:18 +0100
from: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
subject: Re: AMS-Europe - WP1.3
to: Rik Leemans <Rik.Leemans@rivm.nl>
<x-flowed>

Another attempt, Rik (I assume all these numbers include travel, computer and overhead, right?):

WUR Ph.D. student 170000

postdoc PIK 170000

Tim Mitchell 75000

Mark Rounsevell 35000

This adds up to 450k, which I think should be possible to get. The numbers, as I see them, DO now include travel and equipment, and overhead of course. As far as PIK is concerned, I use an inverse formula that gives me person-months from this, plus the other expenses.

What do you think?

Wolfgang

On 03/28/2003 12:54 PM, Rik Leemans wrote:

>I agree with you. But if we include you, we have to exclude Tim, because
>the shares become just too small.

>

>I further do not know, if we can actually use the grant to fund PhD-student
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>

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>

>

>

>

> Mike Hulme

> <m.hulme@uea.ac.uk> To: Wolfgang Cramer
<Wolfgang.Cramer@pik-potsdam.de>, Rik Leemans
> <Rik.Leemans@rivm.nl>

> cc:

> 28-03-03 12:22 Subject: Re: AMS-Europe - WP1.3

>

>

>
>
>
>
>
>Rik and Wolfgang,
>
>I have spoken with Tim Carter also he is not sure at all what
>FEI will be asked to do for WP1.3 (and also very sceptical that ENSEMBLES
>will really make progress in "designer climate information"). I would
>therefore make a stronger argument for a resource person in WP1.3, ~€75k,
>to be able to develop consistent climate information in the Scenarios WD,
>and which will be useful in the other AMS Work Domains. I suggest that
>person be Tim Mitchell here in Tyndall.
>
>As I have said to Carlo, if WP1.3 does not address this - even in this
>basic minimum way - the AMS-Europe project will be a "climate-information
>free zone" - this is not what we want.
>
>Mike
>
>
>At 09:39 28/03/2003 +0100, Wolfgang Cramer wrote:
>
>
>>Rik, thanks for that clarification... I still think we should actually
>>support Tim Mitchell partly through this WP. I am also happy about any
>>involvement of Tim - but I would like Mike's views on whether this makes
>>for a productive addition here.
>>
>>Wolfgang
>>
>>On 28/03/03 09:25, Rik Leemans wrote:
>>
>>
>>
>>>Dear Mike,
>>>
>>>Sorry to be so little communicative, but things were changing so fast and
>>>
>>>
>>>I
>>>
>>>did not know how to effectively proceed and simultaneously involve all
>>>
>>>
>>>the
>>>
>>>necessary player.
>>>I just returned to the office after a day on a very different topic the
>>>World Barley, malt, and Beer conference and saw your mail. Sorry therefor
>>>for my late response.
>>>
>>>When I was asked to coordinate WP1.3 last week, I did not have a clue on
>>>what should go into it. Early this week, therefore I drafted the document
>>>that you saw, which had quite some overlap with the other Wps. This were
>>>
>>>
>>>my
>>>
>>>preliminary ideas. I still strongly believe that we should do some of the

cg2003

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>>>impacts (from emissions to impacts) in this wp. Climate is a part of that
>>>but not the only one. For me the innovative part is not only to base the
>>>impact assessment on the quantitative climate scenarios but also on the
>>>qualitative narratives, which help to define resilience, sensitivity etc.
>>>Additionally from a systemic point of view the interactions between
>>>climate, impacts and concentrations are important. This was my main
>>>philosophy in drafting the WPs.
>>>
>>>I also had a call with Tim Carter on his involvement in other project and
>>>learned several thing from him. He was very hesitant to become involved,
>>>although some money for Suzanne for literature review (She did a great
>>>
>>>
>job
>
>
>>>on collecting scenarios information for forestry developments in EU
>>>countries) was always welcome.
>>>
>>>I have also been thinking to involve the IMAGE group a little stronger (I
>>>am not with them any more) but are hesitating because they want to focus
>>>
>>>
>on
>
>
>>>the mititgation strategies.
>>>
>>>I had indeed made the assumption that for the climate scenarios, we
>>>
>>>
>should
>
>
>>>use off-the shell material but I believe that it could be a good idea to
>>>actually grant a little more money into to get some, for impact
>>>
>>>
>assessment,
>
>
>>>important climate variables from the latest runs.
>>>
>>>Brian is organising at 11.00 a telephone conference to discuss the wps,
>>>
>>>
>why
>
>
>>>do you not try to be involved in that discussion (I'll give brian a call
>>>
>>>
>to
>
>
>>>invite you as well).
>>>
>>>Rik
>>>
>>>
>>>
>>>
>>>
```

cg2003

```
>>>                                Mike
>>>Hulme
>>>
>>>                                <m.hulme@uea.a          To:      "Leemans, Rik"
>>><Rik.Leemans@wur.nl>, alex.haxeltine@uea.ac.uk,
>>>                                c.uk>                mike.hulme@uae.ac.uk,
>>>wolfgang.cramer@pik-potsdam.de, hallegatte@centre-cired.fr,
>>>                                hourcade@centre-cired.fr, Brian
>>>
>
>
>>>O'NEILL <oneill@iiasa.ac.at>, naki@iiasa.ac.at,
>>>                                27-03-03 17:27          stapelbroek@hetnet.nl,
>>>Rik.Leemans@rivm.nl, "S.E. van der Leeuw"
>>>                                <vanderle@wanadoo.fr>,
>>>t.mitchell@uea.ac.uk
>>>                                cc:
>>>Carlo.Jaeger@pik-potsdam.de, Armin Haas
>>><haas@pik-potsdam.de>
>>>                                Subject:      AMS-Europe -
>>>WP1.3
>>>
>>>
>>>
>>>Dear Rik - and other Scenarios WD people,
>>>
>>>Following 24 hours of some confusion - and having talked with
>>>
>>>>Jean-Charles
>
>
>>>and wolfgang (I have tried to raise Carlo Jaeger  today for
>>>
>>>>clarification,
>
>
>>>but with no luck, so I am still a little in the dark) - it seems you are
>>>well on track for developing the WP1.3.
>>>
>>>May I therefore make sure you have seen the attached document from me
>>>
>>>>which
>
>
>>>circulated a week or so ago, concerning the role of climate information
>>>
>>>>in
>
>
>>>AMS-Europe. I have seen your comment that WP1.3 should *not* be about
>>>climate information - historic and future - and whilst I can agree it
>>>should not necessarily be *primarily* about climate information (although
>>>it could be if AMS wanted it so), then it must at least pay some
>>>
```


cg2003

```
>>>
>attention
>
>
>>>to climate information (otherwise we are *entirely* dependent upon
>>>
>>>
>whatever
>
>
>>>climate information other people and projects may just happen to produce
>>>
>>>
>-
>
>
>>>and as we know, these things rarely happen to conform to people's needs
>>>just by chance!). There seems to be a need to connect the storylines in
>>>WP1.1 and economics of WP1.2, including inter alia stabilisation
>>>
>>>
>pathways,
>
>
>>>with climate information and this connection is likely to be unique to
>>>AMS-Europe (i.e., ENSEMBLES is unlikely to re-orient itself, unresourced,
>>>to do this).
>>>
>>>So this is the main thrust of my short set of notes and I hope that you
>>>
>>>
>can
>
>
>>>consider these when drafting the WP1.3 - which I have not yet seen.
>>>
>>>Best wishes,
>>>
>>>Mike
>>>(See attached file: Climate information in AMS.doc)
>>>
>>>
>>>
>>--
>>-----
>>Wolfgang Cramer, Department of Global Change and Natural Systems
>>Potsdam Institute for Climate Impact Research, PO Box 60 12 03
>>D-14412 Potsdam, Germany, Tel.: +49-331-288-2521, Fax: -2600
>>mail:Wolfgang.Cramer@pik-potsdam.de, www.pik-potsdam.de/~cramer
>>-----
>>
>>
>>
>
>
>
>
>
>
>
>
```

--

Wolfgang Cramer, Department of Global Change and Natural Systems
Potsdam Institute for Climate Impact Research, PO Box 60 12 03
D-14412 Potsdam, Germany, Tel.: +49-331-288-2521, Fax: -2600
mail:Wolfgang.Cramer@pik-potsdam.de, www.pik-potsdam.de/~cramer

</x-flowed>

1668. 2003-04-01

cc: Mike Hulme <m.hulme@uea.ac.uk>, John Schellnhuber <H.J.Schellnhuber@uea.ac.uk>,
"S.E. van der Leeuw" <vanderle@wanadoo.fr>, Carlo Jaeger
<carlo.jaeger@pik-potsdam.de>
date: Tue, 01 Apr 2003 09:15:11 +0200
from: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
subject: Re: ... "in agreement" ...
to: Klaus Hasselmann <klaus.hasselmann@dkrz.de>

<x-flowed>

Ok, Klaus, then I think we are in pretty good agreement here, certainly on the content. Budget- and management.-wise, we now have a minimalistic approach (like everywhere else in FP6) on doing only the utmost necessary, which is achieved by ensuring 75k€ for a postdoc with precisely the skills you outline (Tim Mitchell) in the world-leading center for precisely this work (CRU-Tyndall, or Tyndall/CRU, or whatever). I would have loved to give them more, but I think we failed to discuss this point appropriately early on in the game. Remember that precisely what is said about climate here also counts for land use change, an even more massive driver of global change in many regions (and we assign even less to it).

I also think that many of these content issues (which Mike, I and others have been hammering in, among other places, two recent EU Concerted Actions, appropriately named ECLAT) will require adequate discussion at AMS meetings in the future. On the optimistic front, I note that, at this late hour, apparently ENSEMBLES is recognising this need as well, and is installing a matching (not overlapping) action by involving Tim Carter of the Finnish Environment Institute, another core member of our earlier joint activities.

Best wishes,

Wolfgang

On 04/01/2003 07:47 AM, Klaus Hasselmann wrote:

> Dear Wolfgang:

> I have not been involved in the ENSEMBLES discussion, apart from the
> joint AMS/ENSEMBLES teleconference a few days ago, in which only Dave
> Griggs participated from ENSEMBLES. My comments are my summary of the
> teleconference as I understood and interpreted it, but has not been
> discussed with others yet.

>

> You underline my point that providing useful information for the user
> from GCM scenario runs is largely a matter of communication. The users
> know what they need, but only the climate modellers can provide the
> information. If the climate modellers simply dump the massive outputs
> of their simulations on AMS-WP 1.3, the poor WP 1.3 scientists will
> be swamped and drown. Also, much of the input they need will probably
> not be stored, or stored in a manner which will be very costly to
> retrieve. What is needed is a clear strategy, geared to the user
> needs, on

>

> a) what is stored, and in what format, from the scenario runs, and

>

> b) what software tools will be available for the post-processing
> needed to transform the output of the scenario runs into useful
> information for the impacts community.

cg2003

>
> This requires more than just a "pointer" in WP 1.3, but somebody, or
> rather a few people, who are willing to work in close collaboration
> with the ENSEMBLES modellers, and to whom the ENSEMBLES modellers
> would listen. And there will be a fair amount of coding work to be
> done on the post-processing side.
>
> Cheers
> Klaus
>
> At 22:37 31.03.2003 +0200, Wolfgang Cramer wrote:
>
>> Dear Klaus,
>>
>> I keep pondering about this. It seems you are indicating that 1.3 is
>> hardly needed and should be replaced by a pointer to ENSEMBLES?
>>
>> One could start a whole discussion here. In fact, the result of last
>> week's consultations with ENSEMBLES, for example, is, as I see it,
>> even worse than I imagined: not only do the "climate modelling
>> centers" have no consideration whatsoever of the possible "user
>> needs" - they keep reporting negative progress. For example, despite
>> the massive build-up of computing power, this power is being invested
>> into atmospheric process studies almost exclusively. Not even the
>> most evident anthropogenic land surface feedbacks are dealt with in
>> any credible way, and I don't even want to speak about the needs for
>> off-line scenarios for impact assessment.
>>
>> The information that is being produced, the little it is, is mostly
>> grossly inadequate for impact studies - and this is not a problem of
>> the impact models, neither of the ill-informed impact modellers, but
>> it is because most atmospheric modelling teams stay clear of the
>> complexities involved in making appropriate choices in assembling
>> climate information.
>>
>> This is why a small, but significant part of 1.3 needs to go to
>> Tyndall/CRU and an entirely statistical operation there. The most
>> part, however, is used to define the nuts&bolts of the scenarios
>> (scenarios, emissions, climate, land use, etc.) that the
>> vulnerability and adaptation crowd in AMS needs. I am aware that time
>> could have been spent usefully during recent months to develop this
>> better, and we all know what we have been occupied with in reality.
>> But if there is no support in the strategy committee for the
>> development of credible, geographically comprehensive baseline and
>> scenario information, then AMS will be just a talkshop where people
>> forever debate about terminology. I would not want to be part of it.
>>
>> Hence I hope we can avoid throwing away the efforts we have made to
>> generate a useful Scenarios WD and a useful WP1.3. We are almost there.
>>
>> Herzliche Grüße,
>>
>> Wolfgang
>>
>> On 03/31/2003 04:46 PM, Mike Hulme wrote:
>>
>>> ... except maybe Klaus Hasselmann. John Schellnhuber sent me this
>>> message from Klaus on Saturday, with a different take on WP1.3. Is
>>> Klaus's idea going anywhere?
>>>
>>> Mike
>>>
>>>> ----- Original Message -----

cg2003

>>>> From: "Klaus Hasselmann" <klaus.hasselmann@dkrz.de>
>>>> To: "S.E. van der Leeuw" <vanderle@wanadoo.fr>; "S.E. van der Leeuw"
>>>> <vanderle@mae.u-paris10.fr>; <Carlo.Jaeger@pik-potsdam.de>
>>>> Cc: "Armin Haas" <haas@pik-potsdam.de>; "Armin Haas"
>>>> <haas@pik-potsdam.de>;
>>>> "Alexander Wokaun" <wokaun@psi.ch>; "John Schellnhuber"
>>>> <h.j.schellnhuber@uea.ac.uk>; "Klaus Hasselmann"
>>>> <klaus.hasselmann@dkrz.de>;
>>>> "Pier Vellinga" <pier.vellinga@falw.vu.nl>; "S.E. van der Leeuw"
>>>> <vanderle@wanadoo.fr>; "S.E. van der Leeuw"
>>>> <vanderle@mae.u-paris10.fr>;
>>>> "Sebastian Gallehr" <gallehr@e5.org>; "Pier Vellinga" <vell@geo.vu.nl>
>>>> Sent: Saturday, March 29, 2003 9:58 PM
>>>> Subject: WP-1.3

>>>>
>>>>
>>>> > Dear Sanders and Carlo, and other strategists:
>>>> >
>>>> > Attached please find the short description of WP 1.3 as I
>>>> understood it.
>>>> >
>>>> > I would imagine you would need much less than the 450,000 Euro
>>>> per work
>>>> > Package to establish the link as I described WP 1.3. Perhaps you
>>>> would
>>>> > prefer to modify the reference value of 450,000 Euro beyond the
>>>> 10% swing
>>>> > we decided in the Strategy Committee for an individual WP, while
>>>> sticking
>>>> > with the average value of 450,000 Euro for the domain. I would
>>>> support
>>>> this.
>>>> >
>>>> > Cheers
>>>> > Klaus

>>>>
>>>> -----
>>>> ----

>>>>
>>>>
>>>> > Klaus Hasselmann
>>>> > Max Planck Institute for Meteorology
>>>> > Bundesstrasse 55, 20146 Hamburg, Germany
>>>> > Voice: +49-(0)40-41173-236
>>>> > Fax: +49-(0)40-41173-250
>>>> > Email: hasselmann@dkrz.de
>>>> > URL: <http://www.mpimet.mpg.de>
>>>> >

>>>
>>
>>
>>
>> --

>> -----
>> Wolfgang Cramer, Department of Global Change and Natural Systems
>> Potsdam Institute for Climate Impact Research, PO Box 60 12 03
>> D-14412 Potsdam, Germany, Tel.: +49-331-288-2521, Fax: -2600
>> mail:Wolfgang.Cramer@pik-potsdam.de, www.pik-potsdam.de/~cramer
>> -----

>>
>
> Klaus Hasselmann
> Max Planck Institute for Meteorology

cg2003

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> Email: hasselmann@dkrz.de
> URL: http://www.mpimet.mpg.de
>
>
>
--

Wolfgang Cramer, Department of Global Change and Natural Systems
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</x-flowed>

4082. 2003-04-02

date: Wed Apr 2 09:22:11 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: Borehole temperatures
to: "Henry N. Pollack" <hpollack@umich.edu>

At 16:23 31/03/03, you wrote:

Is there a brief write-up somewhere that I can read to become familiar with how the Hadley models handle land surface processes and the coupling of radiative forcing with the ground?
Will you be in Nice? I will have a paper (disastrously scheduled for the afternoon of the last day) that shows there is much more coherence in the spatial structure between the borehole reconstructions and the SAT than Mike Mann would have you believe.

Henry, unfortunately I won't be able to come to Nice this year (I hoped to, but no time).

I also had a paper scheduled for the Friday afternoon when I went last year, so I tried to

change it to a poster - but that was scheduled at an even worse time: Friday evening!! I'm

sure nobody was there by then (I certainly wasn't). I'd be interested in your results

concerning the veracity of the spatial information provided by the borehole network - I've

tried computing pattern correlations against observed twentieth century warming trends

under various amounts of spatial smoothing, with ambiguous results.

Anyway, to answer your question about the Hadley land-surface model component...

I'm not

sure whether this is what you would consider to be brief, but try this:

Cox, PM et al., 1999. 'The impact of new land surface physics on the GCM simulation of

climate and climate sensitivity' Climate Dynamics, 15 (3), 183-103.

Best wishes

Tim

3559. 2003-04-03

date: Thu, 03 Apr 2003 13:29 -0500
from: GRLonline@agu.org
subject: 2003GL017425 Request to Review from Geophysical Research Letters
to: K.Briffa@uea.ac.uk

Dear Dr. Briffa:

would you be willing and available to review "A 2,326-year tree-ring record of climate variability on the northeastern Qinghai-Tibetan Plateau" by Qi-Bin Zhang,

cg2003

Guodong Cheng, Tandong Yao, Xingcheng Kang, submitted for possible publication in the Geophysical Research Letters.

The manuscript's abstract is:

The climate on the Qinghai-Tibetan Plateau is sensitive to global changes.

High-resolution climate proxy records covering the last two millennia in this region are scarce yet essential to evaluation of the patterns, synchronicity and spatial extent of past climatic changes including those in the Medieval Warm Period (MWP) and the Little Ice Age (LIA). Here we present a 2326-year tree-ring record of spring precipitation for Dulan area of northeastern Qinghai-Tibetan Plateau. We find that a moist interval spanning A.D. 929-1031 occurs in the MWP, with the peak occurring around A.D. 974. Signals associated with the LIA are also recorded in the tree rings. The greatest change during the last two millennia seems to occur in the second half of the 4th century. Our tree-ring data will facilitate intercontinental comparisons of large-scale synoptic climate variability for the last two millennia.

If you agree to review this manuscript, I would ask for your comments within 14 days from your acceptance.

To ACCEPT, click on the link below:

<<http://grl-submit.agu.org/cgi-bin/main.plex?el=A5K4ZQX1A5mTr4D2A9ykoZf8XpJS8EK0EcaPh5OgZ>>

If you are unable to review this manuscript at this time, I would appreciate any suggestions of other potential reviewers who would be qualified to examine this manuscript. (Via reply e-mail.)

To DECLINE, click on the link below:

<<http://grl-submit.agu.org/cgi-bin/main.plex?el=A7K1ZQX7A5mTr3E3A9ykoZf8XpJS8EK0EcaPh5OgZ>>

If you have any questions or need more information feel free to reply to this e-mail.

Thank you for your consideration and support of Geophysical Research Letters.

Sincerely,

James Famiglietti

Associate Editor

Geophysical Research Letters

556. 2003-04-07

date: Mon Apr 7 13:11:00 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: Re: paper?

to: Jan Esper <esper@wsl.ch>

Jan

it is a paper by Soon and Baliunas published 2003 but I can't remember where.

It is

concerned with MWP particularly and has engendered a lot of annoyance among palaeo types.

I mentioned it because the issue of scaling is relevant to their poor conclusions. I think

it may have been in JGR. I think you can track it pretty easily via the web

Keith

At 02:07 PM 4/7/03 +0200, you wrote:

Keith

you mentioned a paper about absolute temperature amplitudes on the phone. I guess, I

didn't get the authors correctly. Could you send me the reference again?

Thank you

Jan

--

Dr. Jan Esper

Swiss Federal Research Institute WSL

Zuercherstrasse 111, 8903 Birmensdorf

cg2003

Switzerland
Phone: +41-1-739 2510
Fax: +41-1-739 2215
Email: esper@ws1.ch

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

799. 2003-04-07

cc: tcrowley@duke.edu
date: Mon Apr 7 15:03:19 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
to: "Jenkins, Geoff" <geoff.jenkins@metoffice.com>, "Parker, Dave"
<deparker@metoffice.com>, "Tett, Simon" <sfbtett@metoffice.com>,
"Folland, Chris" <chris.folland@metoffice.com>, "Stott, Peter"
<pastott@metoffice.com>, "Jones, Gareth" <gsjones@metoffice.com> Bcc:
pnl_group.all

Simon and all

sorry to have been somewhat silent recently . I am now back at work after my operation and

eager to state that what Tom says here is right on the nail.

I believe passionately that we have a long way to go to get realistic and accurate

(absolute) measures of Hemispheric temperatures over the last millennium and earlier .

However, we must not lose sight of the fact that the "best evidence" is certainly in

support of unprecedented (truly mean Hemispheric and annual) warming in the 20th century

and recent decades. The modern (instrumental) indications of Hemispheric warmth are

(almost literally) incomparably superior to those based on our high-resolution proxy

records (with their narrow coverage and largely summer seasonal bias) . Even pushing the

few individual records to their maximum warmth limit , the most sensible interpretation of

the data does provide much of a case for equivalent warmth in any "Medieval" period (or on

any timescale). Those who prefer to believe in a globally warmer Medieval period largely

fall back on poorly resolved , even more selective evidence that has real problems e.g.

interpretable signal (temp. versus precip.) ; qualitative measurement ; non-deconvolved

lagged responses, and geographical bias that is at least as poor as our high-resolution

data. The science is not progressed without overcoming these problems. Our own desire to

recognize and address the limitations of our own data in the search for accurate and

absolute climate histories should not be confused with a clear expression that "as we

stand" the evidence against unprecedented recent warming does not carry the day.

At 09:57 AM 4/4/03 -0500, Tom Crowley wrote:

Keith, forgot to cc you on this, Tom

Date: Fri, 4 Apr 2003 09:56:40 -0500

cg2003

To: Simon Tett <simon.tett@metoffice.com>

From: Tom Crowley <tcrowley@duke.edu>

Subject: Re: [simon.brown@metoffice.com: PRESS: 20th century is neither the warmest century nor the century with the most extreme weather of the past 1000 years]

Cc: "Jenkins, Geoff" <geoff.jenkins@metoffice.com>, "Parker, Dave" <deparker@metoffice.com>, "Tett, Simon" <sfbtett@metoffice.com>, "Folland, Chris" <chris.folland@metoffice.com>, "Stott, Peter" <pastott@metoffice.com>, "Jones, Gareth" <gsjones@metoffice.com>
Bcc: pnl_group.all

X-Attachments:

Simon,
along with others I was contacted by a New York Times reporter on the Soon and Baliunas paper - I know that Phil is chagrined by the Soon and Baliunas paper. Some of us are thinking about writing some type of rebuttal. at least three main problems in that paper are:

1) they show no data - only report what others state (sort of a pseudo-Bayesian expert assessment).
2) they report various multi-decadal warmings from different places, totally ignoring that they occur at different times - this was the point I made earlier in a paper I wrote in Ambio (others too have made the same point).
3) the reporting is suspect - in the description of my ambio paper they state that the data coverage was worldwide - it was not - all the data points were from the mid-high latitudes northern hemisphere, but the composite was compared against the northern hemisphere instrumental record.
They also state that I conclude that there was no Medieval Warm Period. Yet the title of my paper was "How warm was the Medieval Warm Period?" I do state that there was such a thing as a period in the Middle Ages warmer than the Little Ice Age - just that peak composite warming was no greater than the mid-20th century warming.
the reason that Soon and Baliunas have gotten a lot of attention about this is that the conservative publicity machine in the U.S. has contacts in high places - the rest of us could write the most eloquent, rigorous rebuttal and proof in the world and it would at best wind up in the trash bin of some Congressional committee.

Regards, Tom

Keith, Tom

Baliunus and Soon are stirring things again -- does what they say make sense. Tom I think you have said that the late medieval warm period is not a coherent thing...

Simon

----- Start of forwarded message -----

Content-return: allowed

Date: Fri, 04 Apr 2003 10:32:18 +0100

From: "Brown, Simon" <simon.brown@metoffice.com>

Subject: PRESS: 20th century is neither the warmest century nor the century

with the most extreme weather of the past 1000 years
To: "Jenkins, Geoff" <geoff.jenkins@metoffice.com>,

cg2003

"Parker, Dave" <deparker@metoffice.com>,
"Tett, Simon" <sfbtett@metoffice.com>,
"Folland, Chris" <chris.folland@metoffice.com>,
"Stott, Peter" <pastott@metoffice.com>,
"Jones, Gareth" <gsjones@metoffice.com>

Content-type: text/plain; charset=iso-8859-1

Importance: high

X-Priority: 1

Dear all,

as usual with the media it's they need the answer yesterday. Comments please.

Simon.

> -----Original Message-----

> From: Clarke, Sean

> Sent: 04 April 2003 10:03

> To: Brown, Simon

> Subject: FW: Past climate records

> Importance: High

>

>

> Hello Simon,

>

> As discussed, please find below the e-mail from Robert Matthews of the
> Sunday Telegraph. Could you please get back to him ASAP.

>

> Many thanks

>

>

> Sean

>

>

>

> -----Original Message-----

> From: Robert Matthews [SMTP:r.matthews@physics.org]

> Sent: 04 April 2003 09:49

> To: sean.clarke@metoffice.com

> Subject: Past climate records

> Importance: High

>

> Hi Sean

>

> Here's the press release. I'd very much appreciate any comments from Geoff
> et al about what this review means for statements we often hear that "This
> year is the warmest /among the warmest on record". As this is usually
> taken to be since records began in 1659, during which we were in the midst
> of the Little Ice Age, does this research outlined below mean that current
> record-breaking years may not be as significant as once thought ?

>

> many thanks

> Robert

> =====

>

> Harvard-Smithsonian Center for Astrophysics

>

> Release No: 03-10

> For Immediate Release: March 31, 2003

>

> Cambridge, MA -- A review of more than 200 climate studies led by
> researchers at the Harvard-Smithsonian Center for Astrophysics has
> determined that the 20th century is neither the warmest century nor the
> century with the most extreme weather of the past 1000 years. The review
> also confirmed that the Medieval Warm Period of 800 to 1300 A.D. and the
> Little Ice Age of 1300 to 1900 A.D. were worldwide phenomena not limited

> to the European and North American continents. While 20th century
 > temperatures are much higher than in the Little Ice Age period, many
 > parts of the world show the medieval warmth to be greater than that of the
 > 20th century.

>
 > Smithsonian astronomers Willie Soon and Sallie Baliunas, with co-authors
 > Craig Idso and Sherwood Idso (Center for the Study of Carbon Dioxide and
 > Global Change) and David Legates (Center for Climatic Research, University
 > of Delaware), compiled and examined results from more than 240 research
 > papers published by thousands of researchers over the past four decades.
 > Their report, covering a multitude of geophysical and biological climate
 > indicators, provides a detailed look at climate changes that occurred in
 > different regions around the world over the last 1000 years.

>
 > "Many true research advances in reconstructing ancient climates have
 > occurred over the past two decades," Soon says, "so we felt it was time to
 > pull together a large sample of recent studies from the last 5-10 years
 > and look for patterns of variability and change. In fact, clear patterns
 > did emerge showing that regions worldwide experienced the highs of the
 > Medieval Warm Period and lows of the Little Ice Age, and that 20th century
 > temperatures are generally cooler than during the medieval warmth."

>
 > Soon and his colleagues concluded that the 20th century is neither the
 > warmest century over the last 1000 years, nor is it the most extreme.
 > Their findings about the pattern of historical climate variations will
 > help make computer climate models simulate both natural and man-made chan
 > ges more accurately, and lead to better climate forecasts especially on
 > local and regional levels. This is especially true in simulations on
 > timescales ranging from several decades to a century.

>
 > Historical Cold, Warm Periods Verified

>
 > Studying climate change is challenging for a number of reasons, not the
 > least of which is the bewildering variety of climate indicators - all
 > sensitive to different climatic variables, and each operating on slightly
 > overlapping yet distinct scales of space and time. For example, tree ring
 > studies can yield yearly records of temperature and precipitation trends,
 > while glacier ice cores record those variables over longer time scales of
 > several decades to a century.

>
 > Soon, Baliunas and colleagues analyzed numerous climate indicators
 > including: borehole data; cultural data; glacier advances or retreats;
 > geomorphology; isotopic analysis from lake sediments or ice cores; tree or
 > peat celluloses (carbohydrates), corals, stalagmite or biological
 > fossils; net ice accumulation rate, including dust or chemical counts;
 > lake fossils and sediments; river sediments; melt layers in ice cores;
 > phenological (recurring natural phenomena in relation to climate) and
 > paleontological fossils; pollen; seafloor sediments; luminescent analysis;
 > tree ring growth, including either ring width or maximum late-wood
 > density; and shifting tree line positions plus tree stumps in lakes,
 > marshes and streams.

>
 > "Like forensic detectives, we assembled these series of clues in order to
 > answer a specific question about local and regional climate change: Is
 > there evidence for notable climatic anomalies during particular time
 > periods over the past 1000 years?" Soon says. "The cumulative evidence
 > showed that such anomalies did exist."

>
 > The worldwide range of climate records confirmed two significant climate
 > periods in the last thousand years, the Little Ice Age and the Medieval
 > Warm Period. The climatic notion of a Little Ice Age interval from 1300 to
 > 1900 A.D. and a Medieval Warm Period from 800 to 1300 A.D. appears to be
 > rather well-confirmed and wide-spread, despite some differences from one

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> region to another as measured by other climatic variables like
> precipitation, drought cycles, or glacier advances and retreats.
>
> "For a long time, researchers have possessed anecdotal evidence supporting
> the existence of these climate extremes," Baliunas says. "For example, the
> Vikings established colonies in Greenland at the beginning of the second
> millennium that died out several hundred years later when the climate
> turned colder. And in England, vineyards had flourished during the
> medieval warmth. Now, we have an accumulation of objective data to back up
> these cultural indicators."
>
> The different indicators provided clear evidence for a warm period in the
> Middle Ages. Tree ring summer temperatures showed a warm interval from 950
> A.D. to 1100 A.D. in the northern high latitude zones, which corresponds
> to the "Medieval Warm Period." Another database of tree growth from 14
> different locations over 30-70 degrees north latitude showed a similar
> early warm period. Many parts of the world show the medieval warmth to be
> greater than that of the 20th century.
>
> The study -- funded by NASA, the Air Force Office of Scientific Research,
> the National Oceanic and Atmospheric Administration, and the American
> Petroleum Institute -- will be published in the Energy and Environment
> journal. A shorter paper by Soon and Baliunas appeared in the January 31,
> 2003 issue of the Climate Research journal.
----- End of forwarded message -----
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--

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2973. 2003-04-08

date: Tue Apr 8 09:53:03 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: heat over Medieval warmth
to: simon.shackley@umist.ac.uk

Simon,
Not much validity. See the comments below from two very senior figures in the field, Keith Briffa here at UEA and Tom Crowley at Duke University in the States. The Soon/Baliunas/Idso/Idso/Legates paper is another contrived piece of hubris from convicted sceptics. There are no new data here that undermine the IPCC considered judgement.
Mike

From Keith Briffa
sorry to have been somewhat silent recently . I am now back at work after my operation and eager to state that what Tom says here is right on the nail.
I believe passionately that we have a long way to go to get realistic and accurate (absolute) measures of Hemispheric temperatures over the last millennium and earlier .
However, we must not lose sight of the fact that the "best evidence" is certainly in support of unprecedented (truly mean Hemispheric and annual) warming in the 20th century and recent decades. The modern (instrumental) indications of Hemispheric warmth are (almost literally) incomparably superior to those based on our high-resolution proxy records (with their narrow coverage and largely summer seasonal bias) . Even pushing the few individual records to their maximum warmth limit , the most sensible interpretation of the data does not provide much of a case for equivalent warmth in any "Medieval" period (or on any timescale). Those who prefer to believe in a globally warmer Medieval period largely fall back on poorly resolved , even more selective evidence that has real problems e.g. interpretable signal (temp. versus precip.) ; qualitative measurement ; non-deconvolved lagged responses, and geographical bias that is at least as poor as our high-resolution data. The science is not progressed without overcoming these problems. Our own desire to recognize and address the limitations of our own data in the search for accurate and absolute climate histories should not be confused with a clear expression that "as we stand" the evidence against unprecedented recent warming does not carry the day.
At 09:57 AM 4/4/03 -0500, Tom Crowley wrote:
Keith, forgot to cc you on this, Tom

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Date: Fri, 4 Apr 2003 09:56:40 -0500
To: Simon Tett <simon.tett@metoffice.com>
From: Tom Crowley <tcrowley@duke.edu>
Subject: Re: [simon.brown@metoffice.com: PRESS: 20th century is neither the warmest century nor the century with the most extreme weather of the past 1000 years]
Cc: "Jenkins, Geoff" <geoff.jenkins@metoffice.com>, "Parker, Dave" <deparker@metoffice.com>, "Tett, Simon" <sfbtett@metoffice.com>, "Folland, Chris" <chris.folland@metoffice.com>, "Stott, Peter" <pastott@metoffice.com>, "Jones, Gareth" <gsjones@metoffice.com>
Bcc: pnl_group.all
X-Attachments:
Simon,
along with others I was contacted by a New York Times reporter on the Soon and Baliunas paper - I know that Phil is chagrined by the Soon and Baliunas paper. Some of us are thinking about writing some type of rebuttal. at least three main problems in that paper are:
1) they show no data - only report what others state (sort of a pseudo-Bayesian expert assessment).
2) they report various multi-decadal warmings from different places, totally ignoring that they occur at different times - this was the point I made earlier in a paper I wrote in Ambio (others too have made the same point).
3) the reporting is suspect - in the description of my ambio paper they state that the data coverage was worldwide - it was not - all the data points were from the mid-high latitudes northern hemisphere, but the composite was compared against the northern hemisphere instrumental record.
They also state that I conclude that there was no Medieval Warm Period. Yet the title of my paper was "How warm was the Medieval Warm Period?" I do state that there was such a thing as a period in the Middle Ages warmer than the Little Ice Age - just that peak composite warming was no greater than the mid-20th century warming.
the reason that Soon and Baliunas have gotten a lot of attention about this is that the conservative publicity machine in the U.S. has contacts in high places - the rest of us could write the most eloquent, rigorous rebuttal and proof in the world and it would at best wind up in the trash bin of some Congressional committee.

Regards, Tom

At 15:38 07/04/2003 +0100, you wrote:

Mike - did you see this? has it any validity?

Simon

----- Forwarded message follows -----

Date sent: Mon, 07 Apr 2003 12:59:32 -0700
From: "Dr. Dennis Bray" <Dennis.Bray@gkss.de>
Subject: climate change wrong again
To: Simon.Shackley@umist.ac.uk, Hans.von.Storch@gkss.de, Eduardo.Zorita@gkss.de
Thought this might be of interest
D

Daily Telegraph

Middle Ages were warmer than today, say scientists

By Robert Matthews, Science Correspondent

(Filed: 06/04/2003)

Claims that man-made pollution is causing "unprecedented" global warming have been seriously undermined by new research which shows that the Earth was warmer during the Middle Ages.

>From the outset of the global warming debate in the late 1980s, environmentalists have said that temperatures are rising higher and faster than ever before, leading some scientists to conclude that greenhouse gases from cars and power stations are causing these "record-breaking" global temperatures.

Last year, scientists working for the UK Climate Impacts Programme said that global temperatures were "the hottest since records began" and added: "We are pretty sure that climate change due to human activity is here and it's accelerating."

This announcement followed research published in 1998, when scientists at the Climatic Research Unit at the University of East Anglia declared that the 1990s had been hotter than any other period for 1,000 years. Such claims have now been sharply contradicted by the most comprehensive study yet of global temperature over the past 1,000 years. A review of more than 240 scientific studies has shown that today's temperatures are neither the warmest over the past millennium, nor are they producing the most extreme weather - in stark contrast to the claims of the environmentalists.

The review, carried out by a team from Harvard University, examined the findings of studies of so-called "temperature proxies" such as tree rings, ice cores and historical accounts which allow scientists to estimate temperatures prevailing at sites around the world.

The findings prove that the world experienced a Medieval Warm Period between the ninth and 14th centuries with global temperatures significantly higher even than today.

They also confirm claims that a Little Ice Age set in around 1300, during which the world cooled dramatically. Since 1900, the world has begun to warm up again - but has still to reach the balmy temperatures of the Middle Ages.

The timing of the end of the Little Ice Age is especially significant, as it implies that the records used by climate scientists date from a time when the Earth was relatively cold, thereby exaggerating the significance of today's temperature rise.

According to the researchers, the evidence confirms suspicions that today's "unprecedented" temperatures are simply the result of examining temperature change over too short a period of time.

The study, about to be published in the journal *Energy and Environment*, has been welcomed by sceptics of global warming, who say it puts the claims of environmentalists in proper context. Until now, suggestions that the Middle Ages were as warm as the 21st century had been largely anecdotal and were often challenged by believers in man-made global warming.

Dr Philip Stott, the professor emeritus of bio-geography at the University of London, told *The Telegraph*: "What has been forgotten in all the discussion about global warming is a proper sense of history."

According to Prof Stott, the evidence also undermines doom-laden predictions about the effect of higher global temperatures. "During the Medieval Warm Period, the world was warmer even than today, and history shows that it was a wonderful period of plenty for everyone."

In contrast, said Prof Stott, severe famines and economic collapse followed the onset of the Little Ice Age around 1300. He said: "When the temperature started to drop, harvests failed and England's vine industry died. It makes one wonder why there is so much fear of warmth."

The United Nations Intergovernmental Panel on Climate Change (IPCC), the official voice of global warming research, has conceded the possibility that today's "record-breaking" temperatures may be at least

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partly caused by the Earth recovering from a relatively cold period in recent history. While the evidence for entirely natural changes in the Earth's temperature continues to grow, its causes still remain mysterious.

Dr Simon Brown, the climate extremes research manager at the Meteorological Office at Bracknell, said that the present consensus among scientists on the IPCC was that the Medieval Warm Period could not be used to judge the significance of existing warming.

Dr Brown said: "The conclusion that 20th century warming is not unusual relies on the assertion that the Medieval Warm Period was a global phenomenon. This is not the conclusion of IPCC."

He added that there were also doubts about the reliability of temperature proxies such as tree rings: "They are not able to capture the recent warming of the last 50 years," he said.

4 April 2003: English strawberries on shelves in record time

20 February 2003: Britain faces drier summers and flooding

4 February 2003: Climate change plagues hay fever sufferers

30 November 2002: Growth in flights will wreck climate, says commission

Previous story: 'Designer baby' mother will go to US if plea fails

Next story: Have you got a licence for that exotic minnow?

External links

UK Climate Impacts Program

Climatic Research Unit - University of East Anglia

Intergovernmental Panel on Climate Change - United Nations

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----- End of forwarded message -----

5226. 2003-04-09

date: Wed Apr 9 14:58:15 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: CRU interpolated climate

to: Tim Mitchell <t.mitchell@uea.ac.uk>

Tim - see my comments at the end

At 16:59 07/04/2003 +0100, you wrote:

Sarah,

Many questions!

I'll answer as best I can, but please do not quote these answers, as I ought to collaborate with co-authors before giving any quotable comments.

> I am now thoroughly confused and would be very grateful if you could

> sort me out! I have read your guidelines on the web-site, and need

> help with interpreting the following:

>

> "These choices mean that while this data-set is suitable for using as

> an input to environmental modelling, it is NOT suitable for use in

> detecting climate change. It is NOT a legitimate use of this data-set

> to attempt to prove or disprove the existence of climate change at an

> individual grid-box."

>

> My questions are:

> 1. Is the 1960-2000 climate time series really not to be used at all

> to detect climate change, even over aggregated, regional areas?

It depends on the region, period, and climatic variable! For 1961-1990, say, and for the European mainland, there will probably not be a problem. The density of stations is sufficient that individual stations coming in and out are not likely to substantially affect the values over this large area.

However, over central Africa this is probably not true.

Climate change detection is a specialised subject. It demands either individual station time-series, or carefully assembled (usually

low-resolution) grids. See Q4 of the FAQ.

> How

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> can it be used as input for environmental modelling if it is not
> accurate enough to show real phenomena of change?
The high-resolution grids do show "real phenomena of change". However, it is not the long-term changes for which the grids are optimised; the grids are optimised for high-resolution 'snapshots', month by month. Perhaps it would help if I gave an example of how the data-sets can be best used in data-sparse regions.

1. Constructing a trend at a grid-box is not a good idea, as we explained in the Nature paper.

2. It would be legitimate to use linear regression to compare (say) April precip over a few grid-boxes (or perhaps even one grid-box if the data at that box seems to warrant it) with some comparable areal (not point!!) environmental index from 1981-2000, to derive an estimate of the relationship between interannual variability in precip and interannual variability in the environmental index.

> Where does this

> leave all the previous publications from CRU on regional climate

> change?

Largely unchanged, as I see it. These high-resolution grids are our 'best estimate' of the climate, at a high spatial resolution, in each month in 1901-2000. Perhaps the risks of temporal inhomogeneities at the level of individual grid-boxes could have been made clearer in the past - that is a matter of judgement I guess. The coarser-scale grids produced by CRU, such as Phil Jones' work, are not affected because they use different methods.

> 2. Over what scale do you consider it legitimate to make spatial

> comparisons? Again, some of the publications show, for example, maps

> of Africa with different climate anomalies over about 1000km. With

> greater densities of met stations in Europe, is the spatial

> resolution any better there?

I find it hard to give a definitive answer, because the spatial scale over which the climate information is temporally homogenous varies with region, period, and climatic variable. My answer above provides some hints.

> I absolutely appreciate the problem of the changing input from met

> stations through time - we face the same sorts of irregular

> sequential data input from satellite sensors. And I equally

> appreciate that interpolation must blur the differences between

> neighbouring grid-boxes - but over what distance relative to the

> spatial distribution of input met stations?

This depends on the spatial scales over which different variables vary. See the New et al (2000) paper for the precise values used.

> We are being asked again and again to analyse patterns and causes of

> "emerging" diseases in many parts of the world, and we are really

> concerned to make real sense of the subject, which involves having an

> accurate idea of the degree of climate change within land masses the

> size of Europe. I am myself about to send off a paper for a

> conference proceedings concerned with tick-borne diseases in Europe.

> I have no agenda at all - I am as happy to discover that there has

> been, or has not been, any relevant climate change to account for the

> variety of temporal and spatial patterns of disease change across

> Europe, but I am desperately keen to get it right as a basis for

> further work.

>

> Looking forward to a fruitful dialogue with you.

>

> Regards

> Sarah

Regards

Tim

PS if any co-authors cc'd want to comment, please feel free!

Tim - worth distinguishing between two types of problems with the New et al. data set:

(a) it is specifically *not* designed for climate change detection/attribution in the

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classic IPCC anthropogenic GHG context because for environmental simulation we wish to capture *all* the changes in regional/local climate whether or not an artefact of urban development or land use change (this is the exact *opposite* of data sets for GHG detection since all such datasets should remove such influences - there is a string of papers going back 10 years or more criticising CRU/Phil's work on these very grounds - urban heat/desertification influences, etc.).
(b) a largely unrelated weakness in the dataset is the inhomogeneity introduced due to changing station coverage over time. And here you are right to point out that the "accuracy" depends on place, season, variable and scale of aggregation. Mark has some error grids I believe and publishing maps of # stations in interpolation range would help, but in the end the data set relaxes to 1961-90 in the absence of actual station anomalies.
This is what you mean by space-optimised, but space-optimised inevitably implies it becomes inhomogenous over time (increasingly so as scales become smaller in data sparse areas).
The other point worth advising people is if they really want to look at very local scale (certainly sub-grid-scale, but maybe even supra-grid scale in data poor areas) issues - whether trends or environmental modelling - then they would be best advised to approach GHCN (or CRU) for access to the underlying station data. Then of course, people need to pay attention to the credibility and homogeneity of individual station series, in itself not a trivial task and one that dozens of papers have been written about.
Hope this helps - share these comments with Phil or whoever else is appropriate.
Mike

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phone: +44 (0)1603 59 3904
fax: +44 (0)1603 59 3901
post: Tyndall, ENV, UEA, Norwich, NR4 7TJ, UK

2683. 2003-04-12

cc: n.nicholls@bom.gov.au, Peter.Whetton@csiro.au, Roger.Francey@csiro.au, David.Etheridge@csiro.au, Ian.Smith@csiro.au, Simon.Torok@csiro.au, Willem.Bouma@csiro.au, j.salinger@niwa.com, pachauri@teri.res.in, Greg.Ayers@csiro.au, Rick.Bailey@csiro.au, Graeme.Pearman@csiro.au, p.jones@uea.act.csiro.au, k.briffa@uea.act.csiro.au, d.wratt@niwa.co.nz, andy.reisinger@mfe.govt.nz
date: Sat, 12 Apr 2003 12:41:38 +1000
from: Barrie.Pittock@csiro.au
subject: RE: Recent climate sceptic research and the journal Climate Rese
to: j.salinger@niwa.co.nz, Barrie.Pittock@csiro.au, m.hulme@uea.ac.uk
Dear Jim,
Thanks for your comments and suggestions. I hope the co-editors of 'Climate Research' can agree on some joint action. I know that Peter Whetton is one who is concerned. Any action must of course be effective and also not give

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the sceptics an excuse for making de Freitas appear as a martyr - the charge should surely be not following scientific standards of review, rather than publishing contrarian views as such. If a paper is contested by referees that should at least be stated in any publication, and minimal standards of statistical treatment, honesty and clarity should be insisted on. Bringing the journal and publisher into disrepute may be one reasonable charge.

'Energy and Environment' is another journal with low standards for sceptics, but if my recollection is correct this is implicit in their stated policy of stirring different points of view - the real test for both journals may be whether they are prepared to publish refutations, especially simultaneously with the sceptics' papers so that readers are not deceived.

On that score you might consider whether it is possible to find who de Freitas got to review various papers and how their comments were dealt with. I heard second hand that Tom Wigley was very annoyed about a paper which gave very low projections of future warmings (I forget which paper, but it was in a recent issue) got through despite strong criticism from him as a reviewer.

Cheers,

Barrie Pittock.

-----Original Message-----

From: j.salinger@niwa.co.nz [mailto:j.salinger@niwa.co.nz]

Sent: Saturday, 12 April 2003 3:40 AM

To: Barrie.Pittock@csiro.au; Mike Hulme

Cc: n.nicholls@bom.gov.au; Peter.Whetton@csiro.au;

Roger.Francey@csiro.au; David.Etheridge@csiro.au; Ian.Smith@csiro.au;

Simon.Torok@csiro.au; Willem.Bouma@csiro.au; j.salinger@niwa.com;

pachauri@teri.res.in; Greg.Ayers@csiro.au; Rick.Bailey@csiro.au;

Graeme.Pearman@csiro.au; p.jones@uea.act.csiro.au;

k.briffa@uea.act.csiro.au; d.wratt@niwa.co.nz;

andy.reisinger@mfe.govt.nz

Subject: Re: Recent climate sceptic research and the journal Climate Research

Dear Mike, Barrie, Neville et al

Saturday morning here and thanks for all your efforts. I note the reference to Chris de Freitas. Chris writes very voluminously to the NZ media and right wing business community often recycling the arguments of sceptics run overseas, which have been put to bed.

I, personally would support any of these actions you are proposing particularly if CR continues to publish dishonest or biased science. This introduces a new facet to the publication of science and we should maybe have a panel that 'reviews the editors'. Otherwise we have the development of shonkey editors who then manipulate the editing to get papers with specific views published. Note the immediacy that the right wing media (probably planned) used the opportunity!

Your views appreciated - but I can certainly provide a dossier on the writings of Chris in the media in New Zealand.

Your views appreciated

Jim

On 11 Apr 2003, at 16:27, Mike Hulme wrote:

> Dear Barrie,

>

> Yes, this paper has hit the streets here also through the London Sunday Telegraph.

>

> Phil Jones and Keith Briffa are pretty annoyed, and there has been correspondence across the Atlantic with Tom Crowley and Ray Bradley. There has been some talk of a formal response but not sure where it has got to. Phil and Keith are really the experts here so I would leave that to them.

>

> Your blow by blow account of what they have done prompts me again to consider my position with Climate Research, the journal for whom I

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> remain a review editor. So are people like Tim Carter, Nigel Arnell,
> Simon Shackley, Rob Wilby and Clare Goodess, colleagues whom I know
> well and who might also be horrified at this latest piece of primary
> school science that Chris de Freitas from New Zealand has let through
> (there are a good number of other examples in recent years and
> Wolfgang Cramer resigned from Climate Research 4 years ago because of
> it).
>
> I might well alert these other colleagues to the crap science CR
> continues to publish because of de Freitas and see whether a
> collective mass resignation is appropriate. Phil Jones, I believe, is
> already boycotting reviews for that journal.
>
> Mike
>
>
> At 14:36 11/04/2003 +1000, you wrote:
> >Hi Neville,
> >
> >You are quite right. My mental process when I read that bit about
> >"warming, wetness or dryness..." was "You must be joking. Surely you
> >didn't really take wetness or dryness into account", so I forgot that
> >maybe they did! MAYBE it is explained in their longer paper?!
> >
> >So, who is going to take up the gauntlet?
> >
> >Cheers,
> >
> >Barrie.
> >
> >-----Original Message-----
> >From: n.nicholls@bom.gov.au [mailto:n.nicholls@bom.gov.au]
> >Sent: Friday, 11 April 2003 2:00 PM
> >To: Peter.Whetton@csiro.au; Roger.Francey@csiro.au;
> >David.Etheridge@csiro.au; Ian.Smith@csiro.au; Simon.Torok@csiro.au;
> >Willem.Bouma@csiro.au; Barrie.Pittock@csiro.au Cc: m.hulme@uea.ac.uk;
> >j.salinger@niwa.com; N.Nicholls@bom.gov.au; pachauri@teri.res.in;
> >Greg.Ayers@csiro.au Subject: RE: Recent climate sceptic research re
> >proxy data
> >
> >
> >Hi Barrie:
> >
> >You forgot to mention the most amusing aspect of the Soon & Baliunas
> >study. They decided that a proxy record showed evidence of a Medieval
> >Warm Period if it "showed a period of 50yr or longer for warming,
> >WETNESS or DRYNESS" between 800 and 1300. (I added the emphasis
> >here). So, almost any 50 yr climate anomaly means, to them, that
> >there has been a local Medieval Warm Period. I guess we should be
> >grateful that they didn't include a 50 yr cool period as evidence of
> >a medieval warm period!
> >
> >It is also worth pointing out that the important claim in The Age
> >newspaper this week, that during the medieval period "world
> >temperatures" were "significantly higher than today's" cannot be
> >based on the Soon & Baliunas paper - they never asked that question,
> >or any question that relates to this. They didn't even ask the
> >question whether the proxy record LOCALLY was warmer during the
> >medieval period than today.
> >
> >And nothing they did can be translated to an estimate of the relative
> >warmth of GLOBAL temperatures.
> >

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> >Neville

>

>

Dr Jim Salinger, CRSNZ

NIWA

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Newmarket, Auckland

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2597. 2003-04-14

date: Mon Apr 14 16:33:35 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: edp

to: Annie Ogden <a.ogden@uea.ac.uk>

Thanks Annie - I know shorter is always better. I'll send it off.

Mike

At 15:49 14/04/2003 +0100, you wrote:

Hello. I think I would try to make it a bit shorter if I were you - and depersonalise it

a bit (though I know HE started it...) . I think that the point about the Ice Ages is

very interesting but it gets a bit lost here. I had no idea the temperature swing was so

small at that time. It really brings home the difference a few degrees can make.

eg: Following is 179 words, rather than 240

Sir

Mr Tim Lenton is clearly an arch sceptic of the idea that humans are already changing world climate ("Doing nothing, Mr Hulme, actually might be best", 13 April).

For the many thousands of EDP readers, such cynicism and ignorance deserves a reply.

The Ice Ages he mentions occurred when global temperature swung just 4-5 degC in a

colder direction; the prospect ahead of us now is a 2-6degC movement to warmer conditions on the time-scale of no more than a century or so. This is indeed

a more radical change than the 6 billion (and rising) people on the planet have

previously had to cope with.

Whether we should do something about this is a matter of judgement rather than of

science, but there are many, many well-informed people who recognise that action to

reduce our emissions of greenhouse gases is at least desirable if not essential (witness

the front-page headline of the same issue of the EDP concerning incentives for biofuels).

This issue is too important to be left as amusing entertainment on an unanswered opinion

page.

Yours sincerely,

Annie,

I had drafted a letter this morning, but have not sent it yet. Here it is.

Normally,

I let Mr Lenton's comments pass me by, but I feel since he is using my name explicitly

that on this occasion I would respond. What do you think?

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Mike

Sir,
For Mr Tim Lenton, an evangelical arch-sceptic of the idea that humans are already changing world climate, a reply to his commentary ("Doing nothing, Mr Hulme, actually might be best", 13 April) is wasting both my time and his. But for the many thousands of EDP readers, such cynicism and ignorance deserves a reply. The Ice Ages cited occurred when global temperature swung 4-5 degC in a colder direction; the prospect ahead of us now is a 2-6degC movement to warmer conditions, on the time-scale of no more than a century or so. We are indeed suggesting something more radical than the 6 billion (and rising) people on the planet have previously had to cope with. Whether we should do something about this is a matter of judgement rather than of science, but there are many, many well-informed people who recognise that action to reduce our emissions of greenhouse gases is at least desirable if not essential (witness the front-page headline of the same issue of the EDP concerning incentives for biofuels). Whether or not I have to be concerned about global warming seems irrelevant. Tim Lenton is clearly unconcerned, I am clearly concerned. Let others make their own mind up, but let's make sure we at least present things the way they are, not the way Mr Lenton would like them to be. The issue is too important to be left as amusing entertainment on an unanswered opinion page.
Yours sincerely,
Professor Mike Hulme
Tyndall Centre
UEA, Norwich
(tel: 01603 593162)
(email: m.hulme@uea.ac.uk)
At 10:07 14/04/2003 +0100, you wrote:
Hi Mike. Just to alert you - if you hadn't already seen it - to the Tim Lenton column in today's EDP. I suspect that the best approach to this might be to follow the advice of the headline:
Doing nothing, Mr Hulme, actually might be best
- but I'll leave that decision to you! Regards, Annie
--

.....
Annie Ogden, Press & PR Manager
University of East Anglia,
Norwich, NR4 7TJ.
Tel:+44 (0)1603 592764
[1]<http://comm.uea.ac.uk/press>
--

.....
Annie Ogden, Press & PR Manager
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Norwich, NR4 7TJ.
Tel:+44 (0)1603 592764
[2]<http://comm.uea.ac.uk/press>

4857. 2003-04-14

cc: a.minns,e.l.jones
 date: Mon Apr 14 18:44:01 2003
 from: Mike Hulme <m.hulme@uea.ac.uk>
 subject: Re: Lenton, EDP and CRed
 to: "Trevor Davies" <T.D.Davies@uea.ac.uk>

Thanks Trevor - I had already drafted a letter to the EDP which has gone off.

It is attached below. I don't usually respond to Lenton's jibes, but I did to this one since he

(ab)used my name specifically.

Glad to know EDP are on board for CRed.

The brochure looks good. A couple of suggestions:

- the text by the graph of temperature could usefully have inserted "contained" between

"carbon" and "in fossil fuels"

- the graph could be a little bit better captioned/explained

- the blurb cutting from 5 balloons to 2 balloons - this could be improved; when it says

"released" what do you mean? this is for UK average - what about the world?

you have used

CO2 mass rather than C mass (the latter is the more common unit in formal statistics).

And a final point - I know Marcus is working on CRed and Simon G has a role; who else in

ENV is currently employed on CRed? Just useful to know who the right contact points are.

And as Tyndall's rep on the Advisory Group for CRed, will Elaine be involved in any formal

planning and development as the project takes off?

And a final, final point - we (Laura) could still do with a bit of text about the 6

September Saturday CRed event as part of SD3 and ZICER opening - or maybe she has it

already.

Thanks,

Mike

Sir

Mr Tim Lenton is clearly an arch sceptic of the idea that humans are already changing world

climate ("Doing nothing, Mr Hulme, actually might be best", 13 April). For the many

thousands of EDP readers, however, such cynicism and ignorance deserves a reply.

The Ice Ages he mentions occurred when global temperature swung just 4-5 degC in a colder

direction; the prospect ahead of us now is a 2-6degC movement to warmer conditions on the

time-scale of no more than a century or so. This is indeed a more radical change than the 6

billion (and rising) people on the planet have previously had to cope with.

Whether we should do something about this is a matter of judgement rather than of science,

but there are many, many well-informed people who recognise that action to reduce our

emissions of greenhouse gases is at least desirable, if not essential (witness the

front-page headline of the same issue of the EDP concerning incentives for biofuels).

This issue is too important to be left as amusing entertainment on an unanswered opinion page.

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Yours sincerely,

At 10:44 14/04/2003 +0100, you wrote:

Mike,

Before/if you respond to Lenton, you should know that I spoke with the Editors of both the EDP & the EEN last week. They are both fully behind CRed. The EDP will make the launch their top story & will include supplements etc. They will joins as "partners" and have agreed to support Cred actively (as newspaper "campaigns- with regular features, challenges etc et). UEA Communications Division is involved. I attach the latest version of the popular brochure for your info (still a few glitches).
Trevor

Professor Trevor D. Davies
Dean
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
Tel +44 (0)1603 592836
Fax +44 (0)1603 593792

4951. 2003-04-14

date: Mon Apr 14 17:15:21 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: NEED HELP WITH 2 REVIEWS
to: m.kelly@uea.ac.uk

I need you to review a couple of papers for me as soon as possible (to get me out of a muddle) . I believe I gave you one some time ago (by Ogurtsov et al (on solar influence on climate) which I think will be a rejection but I need hard justification . The other is a short paper on sea ice around Svalbard. PLEASE come in and get them off me and do then straight away. I will not ask you to double mark a load of Climate Change essays in exchange! Seriously though - this will be a big help . I am trying to dig myself out of a hole (backlog) with theses things so your help would be much appreciated.

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

2562. 2003-04-15

cc: <m.hulme@uea>
date: Tue, 15 Apr 2003 09:33:39 +0100
from: "Trevor Davies" <T.D.Davies@uea.ac.uk>
subject: Re: UK Energy Research Centre CONFIDENTIAL
to: "Waddams, Catherine Prof \ (MGT\)" <C.Waddams@uea.ac.uk>

Dear Catherine,
I was delighted to hear of your success in the ESRC competition. Well done - that is excellent - good for MGT & for UEA.

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Given our particular mix at UEA, we did not see ourselves leading up a bid, but are anticipating some involvement via the Tyndall Centre - hence I am cc'ing this to Mike Hulme who is the Exec Director of TYN. Mike- anyone going to the 2 May meeting? (CCR is something which would enhance UEA's involvement in the national bid- Catherine has done much work with energy companies - starting off at Warwick before she came to UEA a couple of years ago). Yes- looks like Powergen have an upper limit of 150K for the survey - Rob is going down tomorrow. CONFIDENTIAL- CRed now has an "Alliance" with Powergen & we have been talking with them & the City about a long-term plan for energy. The first step is the installation of 3 x 1.5 MW turbines- 2 on the Campus & one at Harford Bridge (where the Cambridge/London railway lines intersect). Powergen definitely want to do this & will be recommending it to their Board at the end of May & want to apply for planning in June. I am about to get together with main players in the City/County etc to develop a strategy before we go public after Easter. Later plans may include a large biomass plant in the middle of Norwich. PLEASE DO NOT

FORWARD THIS TO ANYONE.

Trevor

----- Original Message -----

From: [1]Waddams, Catherine Prof (MGT) mg519

To: [2]'t.d.davies@uea.ac.uk'

Cc: [3]'t.baldwin@uea.ac.uk' ; [4]Diaz-Rainey, Ivan Mr (MGT) mg284254 ;

[5]'laurence.wild@uea.ac.uk'

Sent: Tuesday, April 15, 2003 12:54 AM

Subject: RE: UK Energy Research Centre

Dear Trevor

I wondered whether you would have any interest in this. As you may have heard, we have been successful in our bid to the ESRC for Centre funding for the Centre for Competition and Regulation, so it seems greedy to go for too much else so soon, not to mention the danger of biting off more than we can chew. But if our success or my background contribution would help any case you would want to make, I hope we could help and I'd be glad to talk it over with you when I get back at the end of this month. Should UEA be represented at the 2nd May meeting? I might be able to go, but since I only get back that week it could be difficult, and if ENV is interested it's probably best to send someone from your School who could contribute 'across the piece'. I have also just replied to an ESRC consultation on the future of social science research in energy - obviously all part of the same programme. I gather from Ivan that Powergen are interested in the proposal we made to them but want to cut the cost down. For my part I would rather retain good data (or at least a substantial say in the questionnaire so it will be useful for research) than necessarily defend the full payments for my time - data are even harder to get hold of

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than money,
though both are of course welcome.
I'll respond to Rodney in rather vaguer terms about this, but wanted to let
you know

where I stood on this and potential Powergen changes.
Hope all is well and you will get a good break over Easter
best wishes

Catherine

-----Original Message-----

From: RodnBrk@aol.com

To: c.waddams@uea.ac.uk

Cc: t.d.davies@uea.ac.uk

Sent: 4/14/2003 8:57 AM

Subject: UK Energy Research Centre

Dear Catherine,

Please note I have moved away from hotmail and am using an aol address
until we get the Sohn Associates website up and running. I start trading
with the new venture on the 1st May, so not long now before I "retire"
from Powergen.

I hope you are enjoying the remainder of your stay in the US.

In case you are unaware of developments, I am writing to bring to your
attention the opportunity for UEA to bid to establish the UK Energy
Centre, as-described in the Energy Policy White Paper.

The Natural Environment Research Council have published information on
the process for establishing the Research Centre, and there is a meeting
on the 2nd May for which interested parties can register.

I thought that this may be of interest to UEA due to the reputation of
the University in energy-related and environment-related studies. I
would imagine that the winning bid will be led by a University, and
probably in collaboration with others. My thoughts are that Sohn

Associates, my new venture would be a suitable party to support a bid,
as we can offer the views of the Utilities in the Centre. Also we at
Sohn have some good, senior-management contacts in the Utilities, so we
may be able to attract funding and other involvement in the
development.

I have copied Prof. Trevor Davies into this e-mail. I had noted Prof.
Davies' name on a list of Renewables Stakeholder Group addresses which I
received in the course of interest in the recent EEDA invitation to
tender to build a renewables operating plan for Eastern England.
Further details of the Opportunity to submit Expressions of Interest in
the Energy Research Centre are available at the following url:

[6]<http://www.nerc.ac.uk/funding/programmes/sustenergy/>

A list of those currently registered for the meeting (including me!) can
be seen at:

[7]<http://badc.nerc.ac.uk/ckbmtg/view.cgi.pl>

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5095. 2003-04-15

date: Tue, 15 Apr 2003 14:23:07 +0100

from: "Mick Kelly" <m.kelly@uea.ac.uk>

subject: Review

to: k.briffa@uea.ac.uk

Keith

Could have said a lot more but I'm not going to rewrite this for them.
I think I'd reject but say you would consider a major rewrite? Unless

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other referees disagree.
Mick

Mick Kelly
University of East Anglia
United Kingdom
Tel: 44-1603-592091
Email: m.kelly@uea.ac.uk
web: <http://www.cru.uea.ac.uk/tiempo/>

Climatic Research Unit
Norwich NR4 7TJ
Fax: 44-1603-507784

Attachment Converted: "c:\eudora\attach\Evidence from millennial temperature proxies for solar influence on terrestrial climate.doc"

116. 2003-04-16

date: wed, 16 Apr 2003 21:05:09 +0100
from: "Max Beran" <maxberan@oldboot.demon.co.uk>
subject: RE: The Alexander technique
to: "Keith Briffa" <k.briffa@uea.ac.uk>
Keith
Yes but what about the substantive dendro queries.
Max

-----Original Message-----
From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
Sent: 16 April 2003 10:10
To: Max Beran
Subject: The Alexander technique

Dear Max
nice to hear from you and thanks for taking the trouble to get in touch with that advice. It so happens that I am booked for a session sponsored by the University in June , and ironically my wife Sarah , on reading a book someone else lent me with the same advice, has become a devotee. I have still to read it! I am having physio once a fortnight , but I have to say I am a little disappointed that I am being to get more frequent back aches and some pain again , particularly when I sit for a while. Anyway , I will go to that course and perhaps even find time to read the book. Thanks again
Keith

At 03:03 AM 4/16/03 +0100, you wrote:

>Keith

 \succ

>Did you have an opportunity to ponder the following.

>

>I hope your back is fully mended. You ought to take up the Alexander
>technique. I mention it because my wife is an Alexander technique teacher
>and our principal breadwinner now that I'm retired.

 \succ

>All the best

>

>Max

 \succ

>-----Original Message-----

>From: Max Beran [mailto:maxberan@oldboot.demon.co.uk]

>Sent: 25 February 2003 15:10

>To: k.briffa@uea.ac.uk

>Subject: Tree rings and the Mann hockey stick

 \succ γ

>Dear Keith

 \succ

>I deliver courses on global change in Oxford and area and one of the matters

>that comes up is the Mann hockey stick and its implications (Mann-made

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>climate change:-). It has been given enormous prominence both in terms of
>its message about the recent and "deep" past, and in terms of its portents.
>Its use as the take-home message from the policymakers summary of the
>IPCC-TAR demonstrates this clearly.

>
>I am aware that the detailed form of the curve conflicts with what is known
>about well attested features of the millennial climate (weak, if any,
>signatures of medieval warming and the little ice age), but what is
>exercising me more is what it says about trees themselves (I know it is
>multi-proxy but as I understand it, dendrochronology rules).

>
>That tree-ring contribution to the temperature reconstruction obviously
uses
>a numerical expression of the sensitivity between temperature and tree ring
>width/density. I don't have any numbers but if these are anything like the
>ones you show in Figure 2 of your 1998 Nature paper, then there is an
>approximate one-to-one between the standardised departures of
>April-September temperature and tree ring width or density.

>
>Two areas of concern are (a) the situation up to the present, and (b) the
>future.

The present

>
>Given that the standard deviation of the yearly values of summer average
>temperature is of the order of 0.5 degree, this is coincidentally about the
>same difference as between pre-industrial times and now. This implies that
>there ought to have been a similar one standard deviation growth in tree
>rings. (Again I've no access to real numbers but I guess we are talking a
mm
>or 3). At an individual site and year this is doubtless well within the
>noise level, but would be expected to shine through when maintained over a
>number of years and sites. I tried to compare this with Figure 7 of your
>1998 Royal Society paper but got mixed up with whether this shows the
annual
>values of the BAI (as implied by the text) or the annual values of the
>year-on-year "change-in-BAI" as in the caption. If the former, one might
>have expected some sort of compound interest pattern, if the latter an even
>faster growing pattern (compound compound). Perhaps the modesty of the rise
>is indicative of the reversal of the sensitivity between tree rings and
>temperature that is visible in the post-1940 data on Figure 6 of the Royal
>Society paper.

>
>How do you reconcile this reversal in the sign of the relationship between
>tree growth and temperature, and Figure 6 in general, with the statements
>elsewhere in the paper saying there has been a non-climatic "enhancement"
of
>tree growth?

>
>If there has indeed been a reversal in the sign of the sensitivity this
>would imply a very large reduction in NPP as a result of the conspiracy
>between ring width and wood density. One might then ask why this post-1940
>sensitivity is not a more reliable basis for backward reconstruction? I
know
>you tend towards non-climatic explanations (notwithstanding my confusion
>over the direction) but for my money this explanation could be at least as
>legitimately aimed at the period from 1880 to 1940. Huge proportional
>changes in land use and industrial pollution in that era make current
>incursions look relatively speaking benign. Just look at population,
>agricultural area, industrial outputs and emissions data to see this.

The future

>The climate models, bless'em, indicate a temperature increase of the order

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>of less than 5 to more than 10 standard deviations by the 2080s. Accepting
>the robustness of the sensitivities implicit in the Hockey stick
>reconstruction (much used to tune and confirm GCM behaviour), that suggests
>to me that we can anticipate a similar order of growth in tree ring width
>and density? I can't picture what the standard deviation of the density
>series might be in relation to the mean, but I would hazard a guess that
>applying this to the tree ring width alone would lead to a more than
>doubling of today's BAI. The overall effect on NPP of such a dramatic shift
>in growth behaviour would surely turn the current 60-ish Gt to well over
100

>Gt. If only a modest fraction was turned into NBU this could make a mighty
>hole in emissions and would be good news at least over the lifetimes of the
>trees. And all this is would put the benefit of CO2 fertilisation
completely
>in the shade.

>
>Seems to me we have a classic checks-and-balances situation here. The
>climate modellers (and the policy makers) implicitly accept the tree-ring
to
>climate sensitivity as far as the past is concerned. This bolsters their
>belief in the forward projections of temperature with all that implies
>for impacts and policy. By their own logic, they should then also accept
>that trees (far and away the dominant living carbon pool) would continue in
>their positive temperature-driven response and provide a hefty negative
>feedback acting via the land carbon cycle. In all seriousness though, does
>anyone really believe trees would respond so dramatically. We'd know about
>it from physiology and see some signal in latitude clines - as far as I
know
>they don't exist, but you'd know for sure.

>
>So at what point does the tree ring to temperature sensitivity break down?
>And what might its impacts be on the hockey stick and through that the GCM
>tuning? Have there been other periods when your post-1940 reversal occurred
>perhaps due to macroclimatic forces? Could these also account for the
>discrepancy between the hockey stick and what we thought we used to know
>about the climate since 1000 AD?

>
>Any thoughts on any of the above would be delightedly received. You may
even
>save a soul from falling into the embrace of the sainted Lomborg!

>
>Max Beran
>
>1 The Croft
>East Hagbourne
>Didcot OX11 9LS
>Tel: 01235 812493
>Fax: 0870 054 7384
--

Professor Keith Briffa,
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Phone: +44-1603-593909
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<http://www.cru.uea.ac.uk/cru/people/briffa/>

2272. 2003-04-16

cc:
p.jones@uea,pittock,a.minns,cramer_wolfgang,<j.salinger@niwa.co.nz>,<mann@virginia.
edu>,torok_simon
date: Wed Apr 16 18:47:35 2003

cg2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Climate Research and adequate peer review

to:

shackley_Simon,wilby_Rob,carter_Tim,arnell_Nigel,martens_Pim,whetton_Peter,c.goodes
s@uea

Dear Co-Review Editor,

You may or may not have seen/read the article by Soon and Baliunas (from the Harvard Smithsonian Astrophysics Lab) in the Jan 31 2003 issue of CR (vol.23,2). A variant of this analysis has just been published in the journal Energy and Environment. The authors/editor made a big media campaign to publicise this work, claiming it showed clearly the Medieval Warm Period was warmer than the 20th century and that the IPCC (and other) analysis claiming the 20th century was the warmest in the last millennium was plain wrong. In the UK, the Sunday Telegraph ran the story.

I have followed some email discussion about this amongst concerned paleoclimate experts here at UEA, in the USA and in Oz and NZ and there is overwhelming consensus that the Soon and Baliunas work is just crap science that should never be passed peer review (for a flavour see Mike Mann, Phil Jones and Barrie Pittock below). These paleo-experts have decided it is not worth a formal scientific response since the story has not run that widely in the mass media (although is now used by sceptics of course to undermine good science) and that the science is so poor it is not worth a reply.

The CR editor concerned is Chris de Freitas and I have followed over the years papers in CR that he has been responsible for reviewing. [Wolfgang Cramer resigned from CR a few years ago over a similar concern over the way de Freitas managed the peer review process for a manuscript Wolfgang reviewed].

Whilst we do not know who reviewed the Soon/Baliunas manuscript, there is sufficient evidence in my view to justify a "loss of confidence" in the peer review process operated by the journal and hence a mass resignation of review editors may be warranted. This is by no means a one-off - I could do the analysis of de Freitas's manuscripts if needbe.

I am contacting the seven of you since I know you well and believe you may also have similar concerns to me about the quality of climate change science and how that science is communicated to the public. I would be interested in your views on this course of action - which was suggested in the first place by me, once I knew the strength of feeling amongst people like Phil Jones, Keith Briffa, Mike Mann, Ray Bradley, Tom Crowley, etc. CSIRO and Tyndall communication managers would then think that a mass resignation would draw attention to the way such poor science gets into mainstream journals.

Of course, we would need to be sure of our case and to argue on grounds of poor conduct of peer review (I can forward a devastating critique of the Soon/Baliunas method from Barrie Pittock if you wish) rather than on disagreeable content of one manuscript. CR does of course publish some good science, but the journal is not doing anyone a service by allowing crap science also to be published.

Thoughts please,

Mike

FROM MIKE MANN

Dear all,

Phil relayed this message to me--this echoes discussions that others of us here have had as well, and at Phil's request, I'm forwarding some of these (Phil seems to have deleted them). I am encouraged at the prospect of some sort of action being taken.

The "Energy and Environment" piece is an ad hominem attack against the work of several of us, and could be legally actionable, though I don't think it's worth the effort. But more problematic, in my mind, is the "Climate Research" piece which is a real challenge to the integrity of the peer-review processes in our field. I believe that a boycott against publishing, reviewing for, or even citing articles from "Climate Research" is certainly warranted, but perhaps the minimum action that should be taken. A paper published there last year by a University of Virginia "colleague" of mine who shall remain nameless contained, to my amazement, an ad hominem attack against the climate modeling community, and the offending statement never should have seen the light of day (nor should have any of the several papers

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of his which have been published there in recent years, based on quality and honesty standards alone).

A formal statement of "loss of confidence" in the journal seems like an excellent idea. It may or may not be useful for me to be directly involved in this, given that I am a primary object of attack by these folks. However, I'm happy to help in any way that I can, and please keep me in the loop.

best regards,

Mike Mann

FROM PHIL JONES

Dear All,

There have been a number of emails on these two papers. They are bad. I'll be seeing

Hans von Storch next week and I'll be telling him in person what a disservice he's doing

to the science and the status of Climate Research.

I've already told Hans I want nothing more to do with the journal. Tom Crowley may be

writing something - find out also next week, but at the EGS last week Ray Bradley, Mike

Mann, Malcolm Hughes and others decided it would be best to do nothing. Papers that respond to work like this never get cited - a point I'm trying to get across to Hans.

We all have better papers to write than waste our time responding to drive like this.

Cheers

Phil Jones

FROM BARRIE PITTOCK

Dear Jim,

Thanks for your comments and suggestions. I hope the co-editors of 'Climate Research' can agree on some joint action. I know that Peter Whetton is one who is concerned. Any action must of course be effective and also not give the sceptics an excuse for making de Freitas appear as a martyr - the charge should surely be not following scientific standards of review, rather than publishing contrarian views as such. If a paper is contested by referees that should at least be stated in any publication, and minimal standards of statistical treatment, honesty and clarity should be insisted on. Bringing the journal and publisher into disrepute may be one reasonable charge.

'Energy and Environment' is another journal with low standards for sceptics, but if my recollection is correct this is implicit in their stated policy of stirring different points of view - the real test for both journals may be whether they are prepared to publish refutations, especially simultaneously with the sceptics' papers so that readers are not deceived.

On that score you might consider whether it is possible to find who de Freitas got to review various papers and how their comments were dealt with. I heard second hand that Tom Wigley was very annoyed about a paper which gave very low projections of future warmings (I forget which paper, but it was in a recent issue) got through despite strong criticism from him as a reviewer.

Cheers,

Barrie Pittock.

2563. 2003-04-17

cc: n.nicholls@bom.gov.au, Peter.Whetton@csiro.au, Roger.Francey@csiro.au, David.Etheridge@csiro.au, Ian.Smith@csiro.au, Simon.Torok@csiro.au, Willem.Bouma@csiro.au, j.salinger@niwa.com, pachauri@teri.res.in, Greg.Ayers@csiro.au, Rick.Bailey@csiro.au, Graeme.Pearman@csiro.au, mmaccrac@comcast.net, tcrowley@duke.edu, rbradley@geo.umass.edu

date: Thu, 17 Apr 2003 08:47:24 +0100

from: Phil Jones <p.jones@uea.ac.uk>

subject: RE: Recent climate sceptic research and the journal Climate

to: Barrie.Pittock@csiro.au, m.hulme@uea.ac.uk, Barrie.Pittock@csiro.au, mann@virginia.edu

<x-flowed>

Dear Barrie,

My earlier email reply to Neville gives the details of a paper already out there and two more

planned. It is clear when these come out we have to be more active in gaining more

widespread publicity for them (much more than we normally do). At the moment Ray's

extensive paper (with others) in the PAGES volume could be a starting point.

Mike Hulme is moving towards your 3b course of action and I'll talk to Hans von Storch,

who although he says he's not the Chief Editor is thought of by many to be this de facto.

3c is possible through contacts we all have with editors at Science and Nature. I realise

the issues with lobbying groups and I'm sure this has been discussed at the IPCC planning

meeting in Marrakesh this week.

Let's see how Mike gets on and my talks with Hans (and Tom Crowley) next week.

Have a good Easter break - yesterday was the warmest April day for many locations

in England since records began, the long daily ones (1890s).

Cheers

Phil

At 16:19 17/04/03 +1000, Barrie.Pittock@csiro.au wrote:

>Dear all,

>

>I just want to throw in some thoughts re appropriate responses to all this -

>probably obvious to some of you, but clearly different from some views

>expressed. This is not solely a reply to Phil Jones, as I have read lots of

>other emails today including all those interesting ones from Michael Mann.

>

>1. I completely understand the frustration by some at having to consider a

>reply to these nonsense papers, and I agree that such replies will not get

>cited much and may in fact draw attention to papers which deserve to be

>ignored.

>

>2. However, ignoring them can be interpreted as not having an answer, and

>whether we ignore them or not, there are people and lobby groups which will

>push these papers as 'refereed science' which WILL be persuasive to many

>small or large decision-makers who are NOT competent to make their own

>scientific judgements, and some of whom wish the enhanced GH effect would

>turn out to be a myth. In our Australian backwater for example, such papers

>WILL/ARE being copied to business executives and politicians to bolster

>anti-FCCC decisions, and these people do matter. There has to be a

>well-argued and authoritative response, at least for private circulation,

>and as a basis for advice to these decision-makers.

>

>3. I see several possible courses of action that would be useful.

>(a) Prepare a background briefing document for wide private circulation,

>which refutes the claims and lists competent authorities who might be

>consulted for advice on this issue.

>(b) Ensure that such misleading papers do not continue to appear in the

>offending journals by getting proper scientific standards applied to

>refereeing and editing. Whether that is done publicly or privately may not

>matter so much, as long as it happens. It could be through boycotting the

>journals, but that might leave them even freer to promulgate misinformation.

>To my mind that is not as good as getting the offending editors removed and

>proper processes in place. Pressure or ultimatums to the publishers might

>work, or concerted lobbying by other co-editors or leading authors.

>(c) A journalistic expose of the unscientific practices might work and

>embarrass the sceptics/industry lobbies (if they are capable of being

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>embarrassed) e.g., through a reliable lead reporter for Science or Nature.
>Offending editors could be labelled as "rogue editors", in line with current
>international practice? Or is that defamatory?
>(d) Legal action might be useful for authors who consider themselves
>libelled, and there could be financial support for such actions (Jim
>Salinger might have contacts here). However, we would need to be very
>careful to be moderate and reasonable in our responses to avoid counter legal
>actions.
>
>4. I thoroughly agree that just entering in to a public slanging match with
>the offending authors (or editors for that matter) on a one-to-one basis is
>not the way to go. We need some more concerted action.
>
>5. One other thought is that it may be worthwhile for some authors to do a
>serious further study to bring out some statistical tests for the likelihood
>of numerous proxy records showing unprecedented synchronous warming in the
>last 30+ years. This could be, somewhat along the lines of the tests used in
>the studies of observed changes in biological and physical systems in the
>TAR WGII report (SPM figure 1 and related text in Chapter 19, and recent
>papers by Parmesan and Yohe (2003) and Root et al. (2003) in Nature 421,
>37-42 and 57-60). Someone may already have this in hand. I am sure the
>evidence is even stronger than for the critters. That is of course what has
>already been done in fingerprinting the actual temperature record.
>
>Anyway, I am not one of the authors, and too busy (for a retired person), so
>I hope you can collectively get something going which I can support.
>
>Best regards to all,
>
>Barrie.
>
>Dr. A. Barrie Pittock
>Post-Retirement Fellow, Climate Impact Group
>CSIRO Atmospheric Research, PMB 1, Aspendale 3195, Australia
>Tel: +613 9239 4527, Fax: +61 3 9239 4688, email: <barrie.pittock@csiro.au>
>WWW: <http://www.dar.csiro.au/res/cm/impact.htm>
>
>Please Note: Use above address. The old <abp or barrie.pittock@dar.csiro.au>
>is no longer supported.
>
>Currently I am working on a couple of books and other writing re climate
>change and science issues. Please refer any matters re the Climate Impact
>Group to Dr. Peter Whetton, Group Leader, at <peter.whetton@csiro.au>, tel.:
>+61 3 9239 4535. Normally I am in the lab Tuesdays and Thursdays.
>
>"Far better and approximate answer to the right question which is often
>vague, than an exact answer to the wrong question which can always be made
>precise." J. W. Tukey
>
>
>-----Original Message-----
>From: Phil Jones [mailto:p.jones@uea.ac.uk]
>Sent: Wednesday, 16 April 2003 6:23 PM
>To: Mike Hulme; Barrie.Pittock@csiro.au
>Cc: n.nicholls@bom.gov.au; Peter.Whetton@csiro.au;
>Roger.Francey@csiro.au; David.Etheridge@csiro.au; Ian.Smith@csiro.au;
>Simon.Torok@csiro.au; Willem.Bouma@csiro.au; j.salinger@niwa.com;
>pachauri@teri.res.in; Greg.Ayers@csiro.au; Rick.Bailey@csiro.au;
>Graeme.Pearman@csiro.au
>Subject: Re: Recent climate sceptic research and the journal Climate
>Research
>
>

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> Dear All,
> There have been a number of emails on these two papers. They are bad.
> I'll be seeing
> Hans von Storch next week and I'll be telling him in person what a
> disservice he's doing
> to the science and the status of Climate Research.
> I've already told Hans I want nothing more to do with the journal. Tom
> Crowley may be
> writing something - find out also next week, but at the EGS last week Ray
> Bradley, Mike
> Mann, Malcolm Hughes and others decided it would be best to do nothing.
> Papers
> that respond to work like this never get cited - a point I'm trying to
> get across to Hans.
> We all have better papers to write than waste our time responding to
> drivel like this.

>
> Cheers
> Phil
Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
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</x-flowed>

3039. 2003-04-17

date: Thu, 17 Apr 2003 08:49:05 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Climate Research and adequate peer review
to: Mike Hulme <m.hulme@uea.ac.uk>
<x-flowed>

Mike,
See the other emails I've sent today. Came in to do some work ! Keep
me informed of the
results and I'll talk to Hans. Nice try to shut Tim Lenton up - he'll
continue though !

Cheers
Phil
At 18:47 16/04/03 +0100, you wrote:

> Dear Co-Review Editor,
>
> You may or may not have seen/read the article by Soon and Baliunas (from
> the Harvard Smithsonian Astrophysics Lab) in the Jan 31 2003 issue of CR
> (vol.23,2). A variant of this analysis has just been published in the
> journal Energy and Environment. The authors/editor made a big media
> campaign to publicise this work, claiming it showed clearly the Medieval
> Warm Period was warmer than the 20th century and that the IPCC (and other)
> analysis claiming the 20th century was the warmest in the last millennium
> was plain wrong. In the UK, the Sunday Telegraph ran the story.
>
> I have followed some email discussion about this amongst concerned
> paleoclimate experts here at UEA, in the USA and in Oz and NZ and their is
> overwhelming consensus that the Soon and Baliunas work is just crap
> science that should never be passed peer review (for a flavour see Mike
> Mann, Phil Jones and Barrie Pittock below). These paleo-experts have
> decided it is not worth a formal scientific response since the story has

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>not run that widely in the mass media (although is now used by sceptics of
>course to undermine good science) and that the science is so poor it is
>not worth a reply.

>

>The CR editor concerned is Chris de Freitas and I have followed over the
>years papers in CR that he has been responsible for reviewing. [Wolfgang
>Cramer resigned from CR a few years ago over a similar concern over the
>way de Freitas managed the peer review process for a manuscript Wolfgang
>reviewd].

>

>Whilst we do not know who reviewed the Soon/Baliunas manuscript, there is
>sufficient evidence in my view to justify a "loss of confidence" in the
>peer review process operated by the journal and hence a mass resignation
>of review editors may be warranted. This is by no means a one-off - I
>could do the analysis of de Freitas's manuscripts if needbe.

>

>I am contacting the seven of you since I know you well and believe you may
>also have similar concerns to me about the quality of climate change
>science and how that science is communicated to the public. I would be
>interested in your views on this course of action - which was suggested in
>the first place by me, once I knew the strength of feeling amongst people
>like Phil Jones, Keith Briffa, Mike Mann, Ray Bradley, Tom Crowley,
>etc. CSIRO and Tyndall communication managers would then think that a
>mass resignation would draw attention to the way such poor science gets
>into mainstream journals.

>

>Of course, we would need to be sure of our case and to argue on grounds of
>poor conduct of peer review (I can forward a devastating critique of the
>Soon/Baliunas method from Barrie Pittock if you wish) rather than on
>disagreeable content of one manuscript. CR does of course publish some
>good science, but the journal is not doing anyone a service by allowing
>crap science also to be published.

>

>Thoughts please,

>

>Mike

>

>

>

>

>FROM MIKE MANN

>

>Dear all,

>

>Phil relayed this message to me--this echos discussions that others of us
>here have had as well, and at Phil's request, I'm forwarding some of these
>(Phil seems to have deleted them). I am encouraged at the prospect of some
>sort of action being taken.

>

>The "Energy and Environment" piece is an ad hominem attack against the
>work of several of us, and could be legally actionable, though I don't
>think its worth the effort. But more problematic, in my mind, is the
>"Climate Research" piece which is a real challenge to the integrity of the
>peer-review processes in our field.

>

>I believe that a boycott against publishing, reviewing for, or even citing
>articles from "Climate Research" is certainly warranted, but perhaps the
>minimum action that should be taken. A paper published there last year by
>a University of Virginia "colleague" of mine who shall remain nameless
>contained, to my amazement, an ad hominem attack against the climate
>modeling community, and the offending statement never should have seen the
>light of day (nor should have any of the several papers of his which have
>been published there in recent years, based on quality and honesty

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>standards alone).

>

>A formal statement of "loss of confidence" in the journal seems like an
>excellent idea. It may or may not be useful for me to be directly involved
>in this, given that I am a primary object of attack by these folks.
>However, I'm happy to help in any way that I can, and please keep me in
>the loop.

>

>best regards,

>

>Mike Mann

>

>

>FROM PHIL JONES

>

>Dear All,

>

>There have been a number of emails on these two papers. They are bad. I'll
>be seeing
>Hans von Storch next week and I'll be telling him in person what a
>disservice he's doing
>to the science and the status of Climate Research.

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>I've already told Hans I want nothing more to do with the journal. Tom
>Crowley may be
>writing something - find out also next week, but at the EGS last week Ray
>Bradley, Mike
>Mann, Malcolm Hughes and others decided it would be best to do nothing. Papers
>that respond to work like this never get cited - a point I'm trying to get
>across to Hans.
>We all have better papers to write than waste our time responding to
>drivel like this.

>

>Cheers

>

>Phil Jones

>

>

>

>FROM BARRIE PITTOCK

>

>Dear Jim,
>Thanks for your comments and suggestions. I hope the co-editors of 'Climate
>Research' can agree on some joint action. I know that Peter Whetton is one
>who is concerned. Any action must of course be effective and also not give
>the sceptics an excuse for making de Freitas appear as a martyr - the charge
>should surely be not following scientific standards of review, rather than
>publishing contrarian views as such. If a paper is contested by referees
>that should at least be stated in any publication, and minimal standards of
>statistical treatment, honesty and clarity should be insisted on. Bringing
>the journal and publisher into disrepute may be one reasonable charge.
>'Energy and Environment' is another journal with low standards for sceptics,
>but if my recollection is correct this is implicit in their stated policy of
>stirring different points of view - the real test for both journals may be
>whether they are prepared to publish refutations, especially simultaneously
>with the sceptics' papers so that readers are not deceived.

>

>On that score you might consider whether it is possible to find who de
>Freitas got to review various papers and how their comments were dealt with.
>I heard second hand that Tom Wigley was very annoyed about a paper which
>gave very low projections of future warmings (I forget which paper, but it
>was in a recent issue) got through despite strong criticism from him as a
>reviewer.

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>
>Cheers,
>Barrie Pittock.
Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

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332. 2003-04-22

cc: n.nicholls@bom.gov.au, Peter.Whetton@csiro.au, Roger.Francey@csiro.au,
David.Etheridge@csiro.au, Ian.Smith@csiro.au, Simon.Torok@csiro.au,
Willem.Bouma@csiro.au, j.salinger@niwa.com, pachauri@teri.res.in,
Greg.Ayers@csiro.au, Rick.Bailey@csiro.au, Graeme.Pearman@csiro.au,
mmaccrac@comcast.net, tcrowley@duke.edu, rbradley@geo.umass.edu
date: Tue, 22 Apr 2003 22:28:22 +1200
from: j.salinger@niwa.co.nz
subject: RE: Recent climate sceptic research and the journal Climate
to: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Barrie.Pittock@csiro.au,
m.hulme@uea.ac.uk, Barrie.Pittock@csiro.au, mann@virginia.edu, Phil Jones
<p.jones@uea.ac.uk>
Dear All

Good to see some action - and I applaud your initiatives. As a
backgrounder I have attached various pieces that have been in the
NZ Herald which have either involved Chris de Freitas - or are his
'opinions'. He publishes as 'associate professor in geography'. The
NZ Herald is NZ's largest daily metropolitan newspaper.

These will show you exactly where he is coming from - and our
attempts locally in New Zealand to rebut these.
Any actions you do that produce results would be greatly
appreciated here, and I will ensure that the appropriate sources get
to know!

Look forward to updates.

Regards to all

Jim

On 17 Apr 2003, at 13:48, Phil Jones wrote:

>
> Mike,
> I'm in here along with Keith and Tim. Mike Hulme probably as
> well.
> We're all away here
> now until next Wednesday.

>
> Cheers
> Phil

>
> At 08:34 17/04/03 -0400, Michael E. Mann wrote:

> >Dear Phil et al,

> >

> >I'm going to try to get ahold of Dick Kerr today to see if I can get
> >his interest in doing a story. My guess is that Dick will go for it.
> >If so, I'd like to give him a list of names of people to contact for
> >comments.

> >

> >Who is game?

cg2003

> >
> >thanks,
> >
> >mike
> >
> >At 08:47 AM 4/17/03 +0100, Phil Jones wrote:
> >
> >> Dear Barrie,
> >> My earlier email reply to Neville gives the details of a paper
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> >> Cheers
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> >>>I just want to throw in some thoughts re appropriate responses to
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> >>>
> >>>1. I completely understand the frustration by some at having to
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> >>>2. However, ignoring them can be interpreted as not having an
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> >>>(a) Prepare a background briefing document for wide private
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> >>>Best regards to all,
> >>>
> >>>Barrie.
> >>>
> >>>Dr. A. Barrie Pittock
> >>>Post-Retirement Fellow, Climate Impact Group
> >>>CSIRO Atmospheric Research, PMB 1, Aspendale 3195, Australia
> >>>Tel: +613 9239 4527, Fax: +61 3 9239 4688, email:

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> >>><barrie.pittock@csiro.au> WWW:
> >>><http://www.dar.csiro.au/res/cm/impact.htm>
> >>>
> >>>Please Note: Use above address. The old <abp or
> >>>barrie.pittock@dar.csiro.au> is no longer supported.
> >>>
> >>>Currently I am working on a couple of books and other writing re
> >>>climate change and science issues. Please refer any matters re the
> >>>Climate Impact Group to Dr. Peter Whetton, Group Leader, at
> >>><peter.whetton@csiro.au>, tel.:
> >>>+61 3 9239 4535. Normally I am in the lab Tuesdays and Thursdays.
> >>>
> >>>"Far better and approximate answer to the right question which is
> >>>often vague, than an exact answer to the wrong question which can
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> >>>
> >>>-----Original Message-----
> >>>From: Phil Jones [mailto:p.jones@uea.ac.uk]
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> >>>j.salinger@niwa.com; pachauri@teri.res.in; Greg.Ayers@csiro.au;
> >>>Rick.Bailey@csiro.au; Graeme.Pearman@csiro.au Subject: Re: Recent
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> >>> Cheers
> >>> Phil
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> >>Prof. Phil Jones
> >>Climatic Research Unit Telephone +44 (0) 1603 592090
> >>School of Environmental Sciences Fax +44 (0) 1603 507784
> >>University of East Anglia
> >>Norwich Email p.jones@uea.ac.uk
> >>NR4 7TJ

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> >>UK  
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> >  
> >  
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> >  
> > Professor Michael E. Mann  
> > Department of Environmental Sciences, Clark Hall  
> > University of Virginia  
> > Charlottesville, VA 22903  
_____  
> > e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434)  
> > 982-2137  
> > http://www.evsc.virginia.edu/faculty/people/mann.shtml  
>  
> Prof. Phil Jones  
> Climatic Research Unit Telephone +44 (0) 1603 592090  
> School of Environmental Sciences Fax +44 (0) 1603 507784  
> University of East Anglia  
> Norwich Email p.jones@uea.ac.uk  
> NR4 7TJ  
> UK  
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Dr Jim Salinger, CRSNZ
NIWA
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Newmarket, Auckland
New Zealand
Tel + 64 9 375 2053 Fax + 64 9 375 2051
e-mail: j.salinger@niwa.co.nz

The following section of this message contains a file attachment prepared for transmission using the Internet MIME message format. If you are using Pegasus Mail, or any another MIME-compliant system, you should be able to save it or view it from within your mailer. If you cannot, please ask your system administrator for assistance.

```
File: New Zealand Herald pieces with Chris de Freitas.doc
Date: 22 Apr 2003, 22:15
Size: 57344 bytes.
Type: Unknown
```

5013. 2003-04-22

Hi Roland,
the largest change in global-mean sea-level in the naturally forced experiment is about 2 cm over 100 years (from circa 1850 to 1950) I think that this would make it hard to tell from proxy data if the model is right or not. If reconstruction errors are truly random then averaging over lots of data might help.... I'll send you a plot of sea-level changes from the naturally forced experiment.

cg2003

I would be happy to lead Hadley Centre collaboration with your work.

Simon

>>>>> "Roland" == Roland Gehrels <W.R.Gehrels@plymouth.ac.uk> writes:

Roland> Dear Simon

Roland> Thanks for your message and your interest.

Roland> I have just been reading the SOAP web site. I wasn't familiar with the project but I see that my data from the HOLSMEER project are actually written into the proposal (see below). I have a long-standing working relationship with Orson van de Plassche (partner 8) and we have recently done some work with modellers at the KNMI. I just spoke to him on the phone about SOAP and he's filled me in. As you probably know, Keith Briffa is also involved in HOLSMEER so he is familiar with some of the work on the European coasts.

Roland> Just to give you some idea, the Nova Scotia sea-level record has a precision of +/- 6 cm and a resolution of 56 data points over the past 500 years. The site is exposed to the open Atlantic coast (Labrador Current) so should have some palaeoceanographic relevance. Other records are somewhat less precise, +/- 15 cm at worst. (I assume the signal you're looking for is 20 cm per 100 yrs, not 2 cm?)

Roland> If it is useful for your project I would be quite happy to share palaeo data. If you think it's appropriate I could add your name/group as a collaborator on my next NERC submission.

Roland> Best wishes

Roland> Roland

Roland> Palaeo sea level variations will be estimated by partner 8 for north-western Europe and eastern USA and Canada, with a resolution of 50-200 years. Sea level records, based on foraminiferal analysis of tidal marsh cores, from six existing USA sites, augmented by records from more USA sites, and UK and German sites by early 2003, and further augmented by sites sampled during the current EC-funded HOLSMEER project in Iceland, Ireland, Denmark and Portugal that will become available by 2003-4, will be critically assessed for age control (with special focus on the onset of the current high rate of sea level rise), completeness and geographical representativeness, and combined to yield estimates of palaeo sea level for the two Atlantic regions. Changes over the past 2000-4000 years will be used to identify background trends related to vertical land movement (also simulated by existing isostatic earth models) and thus obtain absolute sea level. Comparison and combination with tide gauge records of 70 or more

--

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1185. 2003-04-24

cc: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>, Danny Harvey <harvey@cirque.geog.utoronto.ca>, Ben Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby <rob.wilby@kcl.ac.uk>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider <shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>, "simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton" <peter.whetton@dar.csiro.au>, "c.goodess" <c.goodess@uea.ac.uk>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Scott Rutherford <srutherford@deschutes.gso.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmaccrac@comcast.net>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>, wuebbles@atmos.uiuc.edu, christopher.d.miller@noaa.gov, mann@virginia.edu

cg2003

date: Thu, 24 Apr 2003 12:39:14 -0400
from: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
subject: Re: My turn
to: mark.eakin@noaa.gov
<x-flowed>

HI Mark,

Thanks for your comments, and sorry to any of you who don't wish to receive these correspondances...

Indeed, I have provided David Halpern with a written set of comments on the offending paper(s) for internal use, so that he was armed w/ specifics as he confronts the issue within OSTP. He may have gotten additional comments from other individuals as well--I'm not sure. I believe that the matter is in good hands with Dave, but we have to wait and see what happens. In any case, I'd be happy to provide my comments to anyone who is interested. I think that a response to "Climate Research" is not a good idea. Phil and I discussed this, and agreed that it would be largely unread, and would tend to legitimize a paper which many of us don't view as having passed peer review in a legitimate manner. On the other hand, the in prep. review articles by Jones and Mann (Rev. Geophys.), and Bradley/Hughes/Diaz (Science) should go along way towards clarification of the issues (and, at least tangentially, refutation of the worst of the claims of Baliunas and co). Both should be good resources for the FAR as well...

cheers,

mike

p.s. note the corrections to some of the emails in the original distribution list.

At 09:27 AM 4/24/03 -0600, Mark Eakin wrote:

>At this point the question is what to do about the Soon and Baliunas
>paper. Would Bradley, Mann, Hughes et al. be willing to develop and
>appropriate rebuttal? If so, the question at hand is where it would be
>best to direct such a response. Some options are:

- >
- >1) A rebuttal in Climate Research
- >2) A rebuttal article in a journal of higher reputation
- >3) A letter to OSTP
- >

>The first is a good approach, as it keeps the argument to the level of the
>current publication. The second would be appropriate if the Soon and
>Baliunas paper were gaining attention at a more general level, but it is
>not. Therefore, a rebuttal someplace like Science or Nature would
>probably do the opposite of what is desired here by raising the attention
>to the paper. The best way to take care of getting better science out in a
>widely read journal is the piece that Bradley et al. are preparing for
>Nature. This leaves the idea of a rebuttal in Climate Research as the
>best published approach.

>

>A letter to OSTP is probably in order here. Since the white House has
>shown interest in this paper, OSTP really does need to receive a measured,
>critical discussion of flaws in Soon and Baliunas' methods. I agree with
>Tom that a noted group from the detection and attribution effort such as
>Mann, Crowley, Briffa, Bradley, Jones and Hughes should spearhead such a
>letter. Many others of us could sign on in support.
>This would provide Dave Halpern with the ammunition he needs to provide
>the white House with the needed documentation that hopefully will dismiss
>this paper for the slipshod work that it is. Such a letter could be
>developed in parallel with a rebuttal article.

>

>I have not received all of the earlier e-mails, so my apologies if I am
>rehashing parts of the discussion that might have taken place elsewhere.

>

>Cheers,

>Mark

>

>
 >
 >Michael E. Mann wrote:
 >
 >>Dear Tom et al,
 >>
 >>Thanks for comments--I see we've built up an impressive distribution list
 >>here!
 >>
 >>This seemed like an appropriate point for me to chime in here. By in
 >>large, I agree w/ Tom's comments (and those of Barrie's as well). A
 >>number of us have written reviews and overviews of this topic during the
 >>past couple years. There has been a lot of significant scientific process
 >>in this area (both with regard to empirical "climate reconstruction" and
 >>in the area of model/data comparison), including, in fact, detection
 >>studies along the lines of what Barrie Pittock asked about in a previous
 >>email (see. e.g. Tom Crowley's Science article from 2000). Phil Jones and
 >>I are in the process of writing a review article for /Reviews of
 >>Geophysics/ which will, among other things, dispel the most severe of the
 >>myths that some of these folks are perpetuating regarding past climate
 >>change in past centuries. My understanding is that Ray Bradley, Malcolm
 >>Hughes, and Henry Diaz are working, independently, on a solicited piece
 >>for /Science/ on the "Medieval Warm Period".
 >>Many have simply dismissed the Baliunas et al pieces because, from a
 >>scientific point of view, they are awful--that is certainly true. For
 >>example, Neville has pointed out in a previous email, that the standard
 >>they applied for finding "a Medieval Warm Period" was that a particular
 >>proxy record exhibit a 50 year interval during the period AD 800-1300
 >>that was anomalously *warm*, *wet*, or *dry* relative to the "20th
 >>century" (many of the proxy records don't really even resolve the late
 >>20th century!) could be used to define an "MWP" anywhere one might like
 >>to find one. This was the basis for their press release arguing for a
 >>"MWP" that was "warmer than the 20th century" (a non-sequitur even from
 >>their awful paper!) and for their bashing of IPCC and scientists who
 >>contributed to IPCC (which, I understand, has been particularly viscious
 >>and ad hominem inside closed rooms in Washington DC where their words
 >>don't make it into the public record). This might all seem laughable, it
 >>weren't the case that they've gotten the (Bush) White House Office of
 >>Science & Technology taking it as a serious matter (fortunately, Dave
 >>Halpern is in charge of this project, and he is likely to handle this
 >>appropriately, but without some external pressure).
 >>
 >>So while our careful efforts to debunk the myths perpetuated by these
 >>folks may be useful in the FAR, they will be of limited use in fighting
 >>the disinformation campaign that is already underway in Washington DC.
 >>Here, I tend to concur at least in spirit w/ Jim Salinger, that other
 >>approaches may be necessary. I would emphasize that there are indeed, as
 >>Tom notes, some unique aspects of this latest assault by the skeptics
 >>which are cause for special concern. This latest assault uses a
 >>compromised peer-review process as a vehicle for launching a scientific
 >>disinformation campaign (often viscious and ad hominem) under the guise
 >>of apparently legitimately reviewed science, allowing them to make use of
 >>the "Harvard" moniker in the process. Fortunately, the mainstream media
 >>never touched the story (mostly it has appeared in papers owned by
 >>Murdoch and his crowd, and dubious fringe on-line outlets). Much like a
 >>server which has been compromised as a launching point for computer
 >>viruses, I fear that "Climate Research" has become a hopelessly
 >>compromised vehicle in the skeptics' (can we find a better word?)
 >>disinformation campaign, and some of the discussion that I've seen (e.g.
 >>a potential threat of mass resignation among the legitimate members of
 >>the CR editorial board) seems, in my opinion, to have some potential merit.
 >>
 >>This should be justified not on the basis of the publication of science

cg2003

>>>we may not like of course, but based on the evidence (e.g. as provided by
>>>Tom and Danny Harvey and I'm sure there is much more) that a legitimate
>>>peer-review process has not been followed by at least one particular
>>>editor. Incidentally, the problems alluded to at GRL are of a different
>>>nature--there are simply too many papers, and too few editors w/
>>>appropriate disciplinary expertise, to get many of the papers submitted
>>>there properly reviewed. Its simply hit or miss with respect to whom the
>>>chosen editor is. While it was easy to make sure that the worst papers,
>>>perhaps including certain ones Tom refers to, didn't see the light of the
>>>day at /J. Climate/, it was inevitable that such papers might slip
>>>through the cracks at e.g. GRL--there is probably little that can be done
>>>here, other than making sure that some qualified and responsible climate
>>>scientists step up to the plate and take on editorial positions at GRL.

>>>
>>>best regards,

>>>
>>>Mike

>>>
>>>At 11:53 PM 4/23/2003 -0600, Tom Wigley wrote:

>>>
>>>>>Dear friends,

>>>>>
>>>>>[Apologies to those I have missed who have been part of this email
>>>>>exchange -- although they may be glad to have been missed]

>>>>>
>>>>>I think Barrie Pittock has the right idea -- although there are some
>>>>>unique things about this situation. Barrie says

>>>>>
>>>>>(1) There are lots of bad papers out there
>>>>>(2) The best response is probably to write a 'rebuttal'

>>>>>
>>>>>to which I add

>>>>>
>>>>>(3) A published rebuttal will help IPCC authors in the 4AR.

>>>>>
>>>>>_____
>>>>>
>>>>>Let me give you an example. There was a paper a few years ago by Legates
>>>>>and Davis in GRL (vol. 24, pp. 2319-1222, 1997) that was nothing more
>>>>>than a direct
>>>>>and pointed criticism of some work by Santer and me -- yet neither of us
>>>>>was asked to review the paper. We complained, and GRL admitted it was
>>>>>poor judgment on the part of the editor. Eventually (> 2 years later)
>>>>>we wrote a response (GRL 27, 2973-2976, 2000). However, our response was
>>>>>more that just a rebuttal, it was an attempt to clarify some issues on
>>>>>detection. In doing things this way we tried to make it clear that the
>>>>>original Legates/Davis paper was an example of bad science (more
>>>>>bluntly, either sophomoric ignorance or deliberate misrepresentation).

>>>>>
>>>>>Any rebuttal must point out very clearly the flaws in the original
>>>>>paper. If some new science (or explanations) can be added -- as we did
>>>>>in the above example -- then this is an advantage.

>>>>>
>>>>>_____
>>>>>

>>>>>
>>>>>There is some personal judgment involved in deciding whether to rebut.
>>>>>Correcting bad science is the first concern. Responding to unfair
>>>>>personal criticisms is next. Third is the possible misrepresentation of
>>>>>the results by persons with ideological or political agendas. On the
>>>>>basis of these I think the Baliunas paper should be rebutted by persons
>>>>>with appropriate expertise. Names like Mann, Crowley, Briffa, Bradley,
>>>>>Jones, Hughes come to mind. Are these people willing to spend time on
>>>>>>this?

>>>
>>>_____

>>>
>>>There are two other examples that I know of where I will probably be
>>>involved in writing a response.

>>>
>>>The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16,
>>>10.1029/2002GL015345, 2002). I refereed a virtually identical paper for
>>>J. Climate, recommending rejection. All the other referees recommended
>>>rejection too. The paper is truly appalling -- but somehow it must have
>>>been poorly reviewed by GRL and slipped through the net. I have no
>>>reason to believe that this was anything more than chance. Nevertheless,
>>>my judgment is that the science is so bad that a response is necessary.

>>>
>>>The second is the paper by Michaels et al. that was in Climate Research
>>>(vol. 23, pp. 19, 2002). Danny Harvey and I refereed this and said it
>>>should be rejected. We questioned the editor (deFreitas again!) and he
>>>responded saying

>>>
>>>The MS was reviewed initially by five referees. ... The other three
>>>referees, all reputable atmospheric scientists, agreed it should be
>>>published subject to minor revision. Even then I used a sixth person
>>>to help me decide. I took his advice and that of the three other
>>>referees and sent the MS back for revision. It was later accepted for
>>>publication. The refereeing process was more rigorous than usual.

>>>
>>>On the surface this looks to be above board -- although, as referees who
>>>advised rejection it is clear that Danny and I should have been kept in
>>>the loop and seen how our criticisms were responded to.

>>>
>>>It is possible that Danny and I might write a response to this paper --
>>>deFreitas has offered us this possibility.

>>>
>>>_____

>>>
>>>This second case gets to the crux of the matter. I suspect that
>>>deFreitas deliberately chose other referees who are members of the
>>>skeptics camp. I also suspect that he has done this on other occasions.
>>>How to deal with this is unclear, since there are a number of
>>>individuals with bona fide scientific credentials who could be used by
>>>an unscrupulous editor to ensure that 'anti-greenhouse' science can get
>>>through the peer review process (Legates, Balling, Lindzen, Baliunas,
>>>Soon, and so on).

>>>
>>>The peer review process is being abused, but proving this would be
>>>difficult.

>>>
>>>The best response is, I strongly believe, to rebut the bad science that
>>>does get through.

>>>
>>>_____

>>>
>>>Jim Salinger raises the more personal issue of deFreitas. He is clearly
>>>giving good science a bad name, but I do not think a barrage of ad
>>>hominem attacks or letters is the best way to counter this.

>>>
>>>If Jim wishes to write a letter with multiple authors, I may be willing
>>>to sign it, but I would not write such a letter myself.

>>>
>>>In this case, deFreitas is such a poor scientist that he may simply
>>>disappear. I saw some work from his PhD, and it was awful (Pat Michaels'
>>>PhD is at the same level).

>>>

cg2003

>>>_____
>>>
>>>Best wishes to all,
>>>Tom.
>>
>>_____
>> Professor Michael E. Mann
>> Department of Environmental Sciences, Clark Hall
>> University of Virginia
>> Charlottesville, VA 22903
>>_____
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>
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</x-flowed>

1999. 2003-04-24

date: Thu, 24 Apr 2003 12:42:53 -0400
from: Mike MacCracken <mmaccrac@comcast.net>
subject: Re: My turn
to: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>, Danny Harvey <harvey@cirque.geog.utoronto.ca>, Ben Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby <rob.wilby@kcl.ac.uk>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider <shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>, "simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton" <peter.whetton@dar.csiro.au>, "c.goodess" <c.goodess@uea.ac.uk>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Mark Eakin <mark.eakin@noaa.gov>, Scott Rutherford <srutherford@deschutes.geo.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>
Tom, Michael, Neville, Jim, et al.--

I think there has been some quite insightful discussion and clarification of points in the last few emails. I echo Mike's concerns with the things being said behind closed

doors in

Washington--and sometimes it is expressed more in terms of threatened lawsuits, etc. The

"Skeptics"--and I put the term in quotes and capitalize it as it is a name a few have

absconded with when all good scientists are taught and practice a degree of skepticism--tend to have quite thin skins and at present seem to have ready access to legal

help on many fronts. As Neville suggests, we do need to keep our heads about us as we

consider how to respond.

In this regard, I do want to add a few thoughts for consideration:

a. I think we need to be very careful not to be implying that everything in the peer-reviewed literature is correct--even if the processes are followed meticulously. While

we strive for this, we must realize it can never be fully accomplished. In addition, Bob

Cess used to take pride in indicating he had disproven something that he had published a

decade or so earlier--understanding changes over time. What the peer review process can

strive to accomplish is that there is a well-argued and as complete an exposition as

possible, that criticisms of the explanation are addressed, that alternative explanations

are considered, etc. This does not always occur, and sometimes is subverted, but the

process is supposed to make sure the presentation is not in "Hyde Park speak" (if I may say

so), where virtually anything goes--as is pretty much the case in op-eds, various

newsletters, etc. "The Skeptics" typically rely on--and is instead thoughtful and measured,

and argued as an issue and not focused on personalities, etc. Where the process seems to be

being subverted, one would hope that the subscription base will lapse, the set of

submissions from leading authors will diminish, or the responsible party will learn about

the problems and concerns through letters and even surveys of scientists' views about the

journal and fix the situation.

b. In all of this, what we need to indicate is the strength of our efforts is the process.

As one example of where a problem can develop, we must be careful not to say that the

strength of the IPCC assessments is that they have involved 2500 people or something. A

number of us tried to discourage use of that measure as all it did was get "The Skeptics"

to put out a petition with 17,000 names and lead them to claim they had more on their side.

Science is not about voting--it is about having strong and clear explanations and

descriptions. What gives the IPCC its stature is the process that it uses to get to where

it gets--with a broadly based set of authors and very wide-ranging and careful reviews

involving experts from the scientific community around the world. And the IPCC then also

works to make sure that its results are clearly expressed by getting comments from

governmental and NGO/industrial experts/policy analysts so it is clear things

are both scientifically justified and effectively conveyed. However, for IPCC to climate change process leads to the most authoritative presentation of the issue, it is essential it consider not only the peer-reviewed literature, but also the various claims and perspectives of "The Skeptics"--basically, the IPCC has to be careful not to be seen as ignoring or hiding disagreements, but actually facing and explaining them. There have been a number of times where review comments I have been associated with have had to urge consideration of various views even when I did not agree with them--ignoring the issue just is not effective. I know the page limits sometimes make this seem a waste of space, but it is essential. c. What I think has been a bit unfortunate is that we (the scientific community) do not seem to really have an effective forum where all the various viewpoints can be published together on an ongoing basis and a really active (but civil) exchange of views can take place. While I may not think Lindzen's iris hypothesis is right, that it got published (and he himself called it speculative) has allowed a good active exchange of views on this. While this gave the idea a spurt of publicity and "The Skeptics" community gave him some DC forums to try and add to its exposure, ultimately it will stand or fall based on its ability to withstand the ongoing series of papers analyzing its suggestions. But this is pretty unique (Singer's analysis did get presented in EOS, and there was a nice response, and there are some other examples). However, as a number of us are trying to write an article responding to various of the criticisms of the National Assessment that have been going around, it has actually been a bit frustrating that these viewpoints have not been put out in forums where we can actually have a discussion about and cite them (the op-ed page of the Washington Times and the World Climate Report Pat Michaels puts out don't really provide a place for this, and the Congressional hearing involved took sworn testimony so does not allow other viewpoints to be submitted and published with it). I really think we need to find a place where these discussions can occur--where "The Skeptics" have to actually put their arguments forward and can expect focused responses to be published (it might be best if the publication of the article and first response occurred at the same time, of course, and then further rounds can take place). And where "The Skeptics" can put their comments on the works of the scientific community and get a response--so where each can take on the other side. By trying to keep the scientific literature too pure, we can really contribute to "The Skeptics" going to the back rooms where they can argue that there is not some forum where we will interact with

them. Because

I believe this has been a problem, I have, even against the recommendations of some, accepted invitations to present the IPCC case in a debate format with various of "The

Skeptics". I have done this even though I feel I am presenting the central consensus that

has already accounted for their views and that the actual debate should be between those

who are demanding certainty and those who are very concerned about the risk of what is

happening (so, about the meaning of the science and not just the explanation of the

science). It may not be much fun doing this (and I even got picketed by costumed picketers

one time), and it does distract from doing one's research (not a problem for me at this

point), but not being willing to respond or debate or provide a place for the debate seems

to me to lead to the backroom expounding and one-sided web sites (like John Daly's) that

have been so unfortunate. So let's not try to stamp it out--but to redirect the discussion

to somewhere where the exchanges can be documented.

d. If one is going to find some forum for a real exchange of views, it seems to me one

challenge will be to come up with a sponsoring entity, moderator and rules that might

attract both sides to it (and I realize I am likely overly naive in this as there are

also--maybe even dominantly--outside influences at work here--like some of those industries

that fund the contrarians, or as the contrarians might say, our agencies and their

political views). But, just because it might be difficult and not fully work is not, it

seems to me, reason to discard the notion of finding a forum where all can go at it on all

the various ideas and where the interested media can evaluate and compare explanations.

e. Meanwhile, rather than think about suing someone about seeming insults, I have taken the

suggestion of several people whom have been criticized before me, and have simply added to

my resume, for example, that ExxonMobil sent a letter to the Bush Admin in early 2001

urging my dismissal (along with getting rid of Bob Watson from IPCC, Rosina Bierbaum from

OSTP, and Jeff Miotke, who was the honorable and blameless career foreign service officer

leading the US Govt delegations based on instructions from above), and to hold a prominent

spot on my wall hoping that someday Pat Michaels will actually send me the "2002 Lump of

Coal Award" he honored me with (earlier recipients were VP Gore and Eileen Claussen--I'll

be happy to be in their company). Apparently, however, rather than letting me sequester the

carbon on my wall, Pat used it to generate some hot air--should I be surprised? Plus,

having survived the longest of the ExxonMobil Four before departing govt service, I have

had the reward of having gotten through the USG review process and into the

UNFCCC chapter

6 in the US 2002 Climate Action Report that later led to some biting editorial cartoons on

the issue and caused Rush Limbaugh to refer to the president as "George W. Al Gore." What

more can one ask as a going-away present?

f. That those of you being attacked are being attacked should be seen as a recognition of

the importance of your work--were it not important they would be ignoring it.

And if your

papers are sound (as you all argue they are--and seems the case to me), the misdirected and

false claims of "The Skeptics" will ultimately have no lasting effect, even if in the short

term some politicians pay them too much attention and induce some short-term harm and

delay. Near as i can tell, the public, including in the US, is not being fooled by the

misleading arguments, even if they are not yet responding as vigorously as would seem

justified from our perspectives. So, I would say, respond with clear statements rather than

think about suing.

Best to all--Mike

From: "Michael E. Mann" <mann@virginia.edu>

Date: Thu, 24 Apr 2003 09:23:22 -0400

To: Tom Wigley <wigley@ucar.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa

<k.briffa@uea.ac.uk>,

James Hansen <jhansen@giss.nasa.gov>, Danny Harvey

<harvey@cirque.geog.utoronto.ca>, Ben

Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby

<rob.wilby@kcl.ac.uk>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider

<shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>,

"simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter"

<tim.carter@vyh.fi>,

"p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton"

<peter.whetton@dar.csiro.au>,

"c.goodess" <c.goodess@uea.ucar.edu>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang

Cramer

<Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>,

"simon.torok"

<simon.torok@csiro.au>, Mark Eakin <mark.eakin@noaa.gov>, Scott Rutherford

<srutherford@deschutes.geo.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>,

Ray

Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmacrac@comcast.net>,

Barrie Pittock

<Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson4@osu.edu>,

"pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers"

<Greg.Ayers@csiro.au>,

mann@virginia.edu

Subject: Re: My turn

Dear Tom et al,

Thanks for comments--I see we've built up an impressive distribution list here!

This seemed like an appropriate point for me to chime in here. By in large, I agree w/

Tom's comments (and those of Barrie's as well). A number of us have written reviews and

overviews of this topic during the past couple years. There has been a lot of significant scientific process in this area (both with regard to empirical

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studies along the lines of what Barrie Pittock asked about in a previous email (see.

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writing a review article for Reviews of Geophysics which will, among other things,

dispel the most severe of the myths that some of these folks are perpetuating regarding

past climate change in past centuries. My understanding is that Ray Bradley, Malcolm

Hughes, and Henry Diaz are working, independently, on a solicited piece for Science on

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view, they are awful--that is certainly true. For example, Neville has pointed out in a

previous email, that the standard they applied for finding "a Medieval Warm Period" was

that a particular proxy record exhibit a 50 year interval during the period AD 800-1300

that was anomalously *warm*, *wet*, or *dry* relative to the "20th century" (many of the

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an "MWP" anywhere one might like to find one. This was the basis for their press

release arguing for a "MWP" that was "warmer than the 20th century" (a non-sequitur even

from their awful paper!) and for their bashing of IPCC and scientists who contributed

to IPCC (which, I understand, has been particularly vicious and ad hominem inside

closed rooms in Washington DC where their words don't make it into the public record).

This might all seem laughable, it weren't the case that they've gotten the (Bush) White

House Office of Science & Technology taking it as a serious matter (fortunately, Dave

Halpern is in charge of this project, and he is likely to handle this appropriately, but

without some external pressure).

So while our careful efforts to debunk the myths perpetuated by these folks may be

useful in the FAR, they will be of limited use in fighting the disinformation campaign

that is already underway in Washington DC. Here, I tend to concur at least in spirit w/

Jim Salinger, that other approaches may be necessary. I would emphasize that there are

indeed, as Tom notes, some unique aspects of this latest assault by the skeptics which

are cause for special concern. This latest assault uses a compromised peer-review

process as a vehicle for launching a scientific disinformation campaign (often vicious

and ad hominem) under the guise of apparently legitimately reviewed science, allowing

them to make use of the "Harvard" moniker in the process. Fortunately, the mainstream

media never touched the story (mostly it has appeared in papers owned by Murdoch and his

crowd, and dubious fringe on-line outlets). Much like a server which has been compromised as a launching point for computer viruses, I fear that "Climate Research" has become a hopelessly compromised vehicle in the skeptics' (can we find a better word?) disinformation campaign, and some of the discussion that I've seen (e.g. a potential threat of mass resignation among the legitimate members of the CR editorial board) seems, in my opinion, to have some potential merit. This should be justified not on the basis of the publication of science we may not like of course, but based on the evidence (e.g. as provided by Tom and Danny Harvey and I'm sure there is much more) that a legitimate peer-review process has not been followed by at least one particular editor. Incidentally, the problems alluded to at GRL are of a different nature--there are simply too many papers, and too few editors w/ appropriate disciplinary expertise, to get many of the papers submitted there properly reviewed. Its simply hit or miss with respect to whom the chosen editor is. While it was easy to make sure that the worst papers, perhaps including certain ones Tom refers to, didn't see the light of the day at J. Climate, it was inevitable that such papers might slip through the cracks at e.g. GRL--there is probably little that can be done here, other than making sure that some qualified and responsible climate scientists step up to the plate and take on editorial positions at GRL.

best regards,
Mike

At 11:53 PM 4/23/2003 -0600, Tom Wigley wrote:

Dear friends,

[Apologies to those I have missed who have been part of this email exchange -- although they may be glad to have been missed]

I think Barrie Pittock has the right idea -- although there are some unique things about this situation. Barrie says

(1) There are lots of bad papers out there

(2) The best response is probably to write a 'rebuttal' to which I add

(3) A published rebuttal will help IPCC authors in the 4AR.

Let me give you an example. There was a paper a few years ago by Legates and Davis in GRL (vol. 24, pp. 2319-2322, 1997) that was nothing more than a direct

and pointed criticism of some work by Santer and me -- yet neither of us was asked to review the paper. We complained, and GRL admitted it was poor judgment on the part of the editor. Eventually (> 2 years later) we wrote a response (GRL 27, 2973-2976, 2000). However, our response was more than just a rebuttal, it was an attempt to clarify some issues on detection. In doing things this way we tried to make it clear that the original Legates/Davis paper was an example of bad science (more bluntly, either sophomoric ignorance or deliberate misrepresentation). Any rebuttal must point out very clearly the flaws in the original paper. If some new science (or explanations) can be added -- as we did in the above example -- then this is an advantage.

There is some personal judgment involved in deciding whether to rebut.

Correcting bad science is the first concern. Responding to unfair personal criticisms is next. Third is the possible misrepresentation of the results by persons with ideological or political agendas. On the basis of these I think the Baliunas paper should be rebutted by persons with appropriate expertise. Names like Mann, Crowley, Briffa, Bradley, Jones, Hughes come to mind. Are these people willing to spend time on this?

There are two other examples that I know of where I will probably be involved in writing a response. The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16, 10.1029/2002GL015345, 2002). I refereed a virtually identical paper for J. Climate, recommending rejection. All the other referees recommended rejection too. The paper is truly appalling -- but somehow it must have been poorly reviewed by GRL and slipped through the net. I have no reason to believe that this was anything more than chance. Nevertheless, my judgment is that the science is so bad that a response is necessary. The second is the paper by Michaels et al. that was in Climate Research (vol. 23, pp. 19, 2002). Danny Harvey and I refereed this and said it should be rejected. We questioned the editor (deFreitas again!) and he responded saying The MS was reviewed initially by five referees. ... The other three referees, all reputable atmospheric scientists, agreed it should be published subject to minor revision. Even then I used a sixth person to help me decide. I took his advice and that of the three other referees and sent the MS back for revision. It was later accepted for publication. The refereeing process was more rigorous than usual. On the surface this looks to be above board -- although, as referees who advised rejection it is clear that Danny and I should have been kept in the loop and seen how our criticisms were responded to. It is possible that Danny and I might write a response to this paper -- deFreitas has offered us this possibility.

This second case gets to the crux of the matter. I suspect that deFreitas deliberately chose other referees who are members of the skeptics camp. I also suspect that he has done this on other occasions. How to deal with this is unclear, since there are a number of individuals with bona fide scientific credentials who could be used by an unscrupulous editor to ensure that 'anti-greenhouse' science can get through the peer review process (Legates, Balling, Lindzen, Baliunas, Soon, and so on). The peer review process is being abused, but proving this would be difficult. The best response is, I strongly believe, to rebut the bad science that does get through.

Jim Salinger raises the more personal issue of deFreitas. He is clearly giving good science a bad name, but I do not think a barrage of ad hominem attacks or letters is the best way to counter this. If Jim wishes to write a letter with multiple authors, I may be willing to sign it, but I would not write such a letter myself. In this case, deFreitas is such a poor scientist that he may simply disappear. I saw some work from his PhD, and it was awful (Pat Michaels' PhD is at the same level).

Best wishes to all,
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Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

cg2003

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
http://www.evsc.virginia.edu/faculty/people/mann.shtml

2469. 2003-04-24

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2690. 2003-04-24

cc: <bhare@ams.greenpeace.org>, <baldur.eliasson@ch.abb.com>,
<klaus.hasselmann@dkrz.de>, <gberz@munichre.com>,
<juergen.engelhard@rwerheinbraun.com>, <ccarraro@unive.it>

date: Thu, 24 Apr 2003 14:11:07 +0100

from: "Stephan Singer" <SSinger@wwfepo.org>
subject: Re: ECF 2003 autumn conference - proposal

to: <hourcade@boukha.centre-cired.fr>, <Carlo.Jaeger@pik-potsdam.de>,
<martin.welp@pik-potsdam.de>, <e.l.jones@uea.ac.uk>, <m.hulme@uea.ac.uk>

dear mike,

dear all

thanks for the papers.
two short remarks:

a) i can't do the teleconference next Tuesday - i am in berlin on my way to the airport by then unfortunately

b) can we extend the focus of the ecf conference slightly towards use of biomass generally and not only biofuels? solid biomass is from many reasons a much better renewable energy for power and heat than liquid biofuels for transport. We are just to finalise a study on solid biomass potentials in the oecd (by Imperail Colledge, UK) and that would very likely give a much more positive response to the need for biomass energy to replace fossil fuels.

best regards

stephan

Stephan Singer

cg2003

Head of European Climate and Energy Policy Unit
WWF, the conservation organization
E-mail: ssinger@wwfepo.org

www.panda.org/epo - Stay up-to-date with WWF's policy work in the capital of Europe
www.passport.panda.org - take action on global conservation issues -
have you got your Passport yet?

WWF European Policy Office
36 avenue de Tervuren Box 12
1040 Brussels, Belgium
Tel: +32-2-743-8817
Fax: +32-2-743-8819

>>> Mike Hulme <m.hulme@uea.ac.uk> 04/24/03 01:14pm >>>

Dear Martin, Stefan, Jean-Charles (and Carlo),
Please find attached two documents - a one-page proposed itinerary for the two-day ECF 03 Autumn Conference and a two-page more detailed proposal for the Biofuels day, the centrepiece of the Conference. We - Elaine and I - are circulating these documents to the small ECF planning team (ourselves plus Martin, Stefan and Jean-Charles, plus Carlo as ECF Chair) and also to the ECF extended Board members for their information and also for their comment. You will see the following:
- we are proposing an ECF general assembly on the Monday afternoon (perhaps this should start at 1530 hrs if business is lengthy?). We assume the Chair and secretary will take care of this business. How long for this business meeting?
- those additional guests arriving in time are then invited to the Inaugural Zuckerman Institute Reception and Lecture from 1800 onwards (this will be a much bigger event), followed by a dedicated dinner for ECF members on the Monday evening.
- the Tuesday is given over entirely to the Biofuels debate and we have suggested a detailed programme and speakers to cover the important and interesting issues. Comments please are welcome on this IMMEDIATELY before we send out invitations by the beginning of May.
- the Tuesday evening dinner is a joint one with another Tyndall/SD3 event and Steve Schneider will be the guest speaker.
- we propose then that the Wednesday morning be given over to discussion and planning of the ECF role in the AMS-Europe project (i.e., a more specifically in-house, and probably smaller, ECF session) and would ask Carlo and Klaus to lead this. Those attending the previous day would be welcome to stay for this, but also this is the first and perhaps best opportunity to invite AMS-Europe partners (who we would wish to become members of ECF) to attend both the Tuesday and the Wednesday sessions. By early Sept. we hope to have some reasonable indications from the Commission about the success of AMS-Europe.
We would welcome comments on this proposal and would suggest a telephone conference call next Tuesday (29th April) at 1700 hrs (CET) for the planning committee, plus Carlo, to make any final changes/adjustments. Please let me know if Martin, Carlo, Stefan and Jean-Charles can make this call and what numbers to reach you on. [Please note the next ECF Board tele-call - 5 May - occurs on an English Bank Holiday, so we will not be here for it].
Following invitations to speakers at the beginning of May, we would send out general invitations to participants before the end of May, still 3 months ahead of the event.

With best wishes,

Mike

At 17:22 17/04/2003 +0200, you wrote:

Dear Mike,

I just noticed that you had sent an email to Armin Haas concerning the AMS-Europe email-lists. (Armin is at the moment on holiday and he asked me to check his emails). Indeed the first list was incomplete and we have updated it. You should have received an email with the subject-header "AMS-Europe" a couple of hours ago. This email contains a link to the AMS-Europe website, where the final proposal can be downloaded.

Concerning the ECF conference in Norwich on 8-10 September 2003: I sent the list of speakers, participants and invitees who could not attend the

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conference to Laura Middleton and cc: to you too. The planning committee (you, Elaine Jones, Stephan Singer, Jean-Charles Hourcade and me) should probably have a teleconference quite soon to discuss further steps. As the board agreed in the teleconference on March 3, ECF has budgeted up to € 30.000,- for travel costs, accomodation and meals. I will try to contact you on Tuesday to discuss how to get forward with the preparations of the conference.

Best regards,
Martin

3052. 2003-04-24

cc: n.nicholls@bom.gov.au, Peter.Whetton@csiro.au, Roger.Francey@csiro.au, David.Etheridge@csiro.au, Ian.Smith@csiro.au, Simon.Torok@csiro.au, willem.Bouma@csiro.au, j.salinger@niwa.com, pachauri@teri.res.in, Greg.Ayers@csiro.au, Rick.Bailey@csiro.au, Graeme.Pearman@csiro.au, mmaccrac@comcast.net, tcrowley@duke.edu, rbradley@geo.umass.edu

date: Thu, 24 Apr 2003 11:21:50 +1200

from: j.salinger@niwa.co.nz

subject: Another course of Action - Recent climate sceptic research and the to: Barrie.Pittock@csiro.au, m.hulme@uea.ac.uk, Barrie.Pittock@csiro.au, mann@virginia.edu, Phil Jones <p.jones@uea.ac.uk>, harvey@geog.utoronto.ca, wigley@ucar.edu, n.nicholls@bom.gov.au

Dear All

For information, De Freitas has finally put all his arguments together in a paper published in the Canadian Bulletin of Petroleum Geology, 2002 (on holiday at the moment, and the reference is at work!)

I have had thoughts also on a further course of action. The present Vice Chancellor of the University of Auckland, Professor John Hood (comes from an engineering background) is very concerned that Auckland should be seen as New Zealand's premier research university, and one with an excellent reputation internationally. He is concerned to the extent that he is monitoring the performance of ALL his senior staff, from Associate Professor upwards, including interviews with them. My suggestion is that a band of you review editors write directly to Professor Hood with your concerns. In it you should point out that you are all globally recognized top climate scientist. It is best that such a letter come from outside NZ and is signed by more than one person. His address is:

Professor John Hood

Vice Chancellor

University of Auckland

Private Bag 92019

Auckland, New Zealand

Let me know what you think! See suggested text below.

Regards

Jim

Some suggested text below:

We write to you as the editorial board(review editors??) of the leading international journal Climate Research for climate scientists

....

We are very concerned at the poor standards and personal biases shown by a member of your staff.

When we originally appointed ... to the editorial board we were under the impression that they would carry out their duties in an objective manner as is expected of scientists world wide. We were also given to understand that this person has been honoured with science communicator of the year award, several times by your ... organisation.

Instead we have discovered that this person has been using his position to promote 'fringe' views of various groups with which

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they are associated around the world. It perhaps would have been less disturbing if the 'science' that was being passed through the system was sound. However, a recent incident has alerted us to the fact that poorly constructed and uncritical work has been allowed to enter the pages of the journal. A recent example has caused outrage amongst leading climate scientists around the world and has resulted in the journal dismissing (??).. from the editorial board.

We bring this to your attention since we consider it brings the name of your university and New Zealand into some disrepute. We leave it to your discretion what use you make of this information.

The journal itself cannot be considered completely blameless in this situation and we clearly need to tighten some of our editorial processes; however, up until now we have relied on the honour and professionalism of our editors. Sadly this incident has damaged our faith in some of our fellow scientists.

Regrettably it will reflect on your institution as this person is a relatively senior staff member.

>

>

> At 16:19 17/04/03 +1000, Barrie.Pittock@csiro.au wrote:

> >Dear all,

> >

> >I just want to throw in some thoughts re appropriate responses to all

> >this - probably obvious to some of you, but clearly different from

> >some views expressed. This is not solely a reply to Phil Jones, as I

> >have read lots of other emails today including all those interesting

> >ones from Michael Mann.

> >

> >1. I completely understand the frustration by some at having to

> >consider a reply to these nonsense papers, and I agree that such

> >replies will not get cited much and may in fact draw attention to

> >papers which deserve to be ignored.

> >

> >2. However, ignoring them can be interpreted as not having an answer,

> >and whether we ignore them or not, there are people and lobby groups

> >which will push these papers as 'refereed science' which WILL be

> >persuasive to many small or large decision-makers who are NOT

> >competent to make their own scientific judgements, and some of whom

> >wish the enhanced GH effect would turn out to be a myth. In our

> >Australian backwater for example, such papers WILL/ARE being copied

> >to business executives and politicians to bolster anti-FCCC

> >decisions, and these people do matter. There has to be a well-argued

> >and authoritative response, at least for private circulation, and as

> >a basis for advice to these decision-makers.

> >

> >3. I see several possible courses of action that would be useful. (a)

> >Prepare a background briefing document for wide private circulation,

> >which refutes the claims and lists competent authorities who might be

> >consulted for advice on this issue. (b) Ensure that such misleading

> >papers do not continue to appear in the offending journals by getting

> >proper scientific standards applied to refereeing and editing.

> >whether that is done publicly or privately may not matter so much, as

> >long as it happens. It could be through boycotting the journals, but

> >that might leave them even freer to promulgate misinformation. To my

> >mind that is not as good as getting the offending editors removed and

> >proper processes in place. Pressure or ultimatums to the publishers

> >might work, or concerted lobbying by other co-editors or leading

> >authors. (c) A journalistic expose of the unscientific practices

> >might work and embarrass the sceptics/industry lobbies (if they are

> >capable of being embarrassed) e.g., through a reliable lead reporter

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> >for Science or Nature. Offending editors could be labelled as "rogue
> >editors", in line with current international practice? Or is that
> >defamatory? (d) Legal action might be useful for authors who consider
> >themselves libelled, and there could be financial support for such
> >actions (Jim Salinger might have contacts here). However, we would
> >need to be very careful to be moderate and reasonable in our responses
> >to avoid counter legal actions.

> >
> >4. I thoroughly agree that just entering in to a public slanging
> >match with the offending authors (or editors for that matter) on a
> >one-to-one basis is not the way to go. We need some more concerted
> >action.

> >
> >5. One other thought is that it may be worthwhile for some authors to
> >do a serious further study to bring out some statistical tests for
> >the likelihood of numerous proxy records showing unprecedented
> >synchronous warming in the last 30+ years. This could be, somewhat
> >along the lines of the tests used in the studies of observed changes
> >in biological and physical systems in the TAR WGII report (SPM figure
> >1 and related text in Chapter 19, and recent papers by Parmesan and
> >Yohe (2003) and Root et al. (2003) in Nature 421, 37-42 and 57-60).
> >Someone may already have this in hand. I am sure the evidence is even
> >stronger than for the critters. That is of course what has already
> >been done in fingerprinting the actual temperature record.

> >
> >Anyway, I am not one of the authors, and too busy (for a retired
> >person), so I hope you can collectively get something going which I
> >can support.

> >
> >Best regards to all,

> >
> >Barrie.

> >
> >Dr. A. Barrie Pittock
> >Post-Retirement Fellow, Climate Impact Group
> >CSIRO Atmospheric Research, PMB 1, Aspendale 3195, Australia
> >Tel: +613 9239 4527, Fax: +61 3 9239 4688, email:
> ><barrie.pittock@csiro.au> WWW:
> ><http://www.dar.csiro.au/res/cm/impact.htm>

> >
> >Please Note: Use above address. The old <abp or
> >barrie.pittock@dar.csiro.au> is no longer supported.

> >
> >Currently I am working on a couple of books and other writing re
> >climate change and science issues. Please refer any matters re the
> >Climate Impact Group to Dr. Peter Whetton, Group Leader, at
> ><peter.whetton@csiro.au>, tel.:

> >+61 3 9239 4535. Normally I am in the lab Tuesdays and Thursdays.

> >
> >"Far better and approximate answer to the right question which is
> >often vague, than an exact answer to the wrong question which can
> >always be made precise." J. W. Tukey

> >
> >

> >-----Original Message-----

> >From: Phil Jones [mailto:p.jones@uea.ac.uk]
> >Sent: Wednesday, 16 April 2003 6:23 PM
> >To: Mike Hulme; Barrie.Pittock@csiro.au
> >Cc: n.nicholls@bom.gov.au; Peter.Whetton@csiro.au;
> >Roger.Francey@csiro.au; David.Etheridge@csiro.au; Ian.Smith@csiro.au;
> >Simon.Torok@csiro.au; Willem.Bouma@csiro.au; j.salinger@niwa.com;
> >pachauri@teri.res.in; Greg.Ayers@csiro.au; Rick.Bailey@csiro.au;
> >Graeme.Pearman@csiro.au Subject: Re: Recent climate sceptic research

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> >and the journal Climate Research
> >
> >
> >
> > Dear All,
> > There have been a number of emails on these two papers. They
> > are bad.
> >I'll be seeing
> > Hans von Storch next week and I'll be telling him in person what a
> >disservice he's doing
> > to the science and the status of Climate Research.
> > I've already told Hans I want nothing more to do with the
> > journal. Tom
> >Crowley may be
> > writing something - find out also next week, but at the EGS last
> > week Ray
> >Bradley, Mike
> > Mann, Malcolm Hughes and others decided it would be best to do
> > nothing.
> >Papers
> > that respond to work like this never get cited - a point I'm
> > trying to
> >get across to Hans.
> > We all have better papers to write than waste our time responding
> > to
> >driveI like this.
> >
> > Cheers
> > Phil

> Prof. Phil Jones
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> School of Environmental Sciences Fax +44 (0) 1603 507784
> University of East Anglia
> Norwich Email p.jones@uea.ac.uk
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> -----
>
>

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4393. 2003-04-24

cc: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones
<p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa
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<Thomas.R.Karl@noaa.gov>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>,
"simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>,
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<a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger"

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<j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Scott Rutherford <srutherford@deschutes.geo.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmaccrac@comcast.net>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>
date: Thu, 24 Apr 2003 22:03:53 -0700 (PDT)

from: Stephen H Schneider <shs@stanford.edu>

subject: Re: My turn

to: Mark Eakin <Mark.Eakin@noaa.gov>

Hello all. If some want to write an editorial in CLimatic Change about the peer review system in general and use some of these articles as examples, I would be happy to entertain such a draft. I would put it~--after the usual editorial review--on as fast a track as I could. If it were strictly a commentary on Baliunas/Soon polemics, then it would have to be a commentary and they would get a reply--but you might get a counter reply. Better to make it peripheral to that paper and a think piece motivated by it so it can stand alone. At least think about it. In case anyone is interested, Social Historian Paul EDWARDS NOW AT u/MICHIGAN AND i DID A PIECE ON PEER REVIEW USING THE SIETZ /SINGER PHONEY PEER REVIEW EXCUSE FOR CHARACTER ATTACKS ON ipcc AND Ben Santer. It might be useful for a backgrounder. I attach it for convenience in case a few of you are interested in peer review/social construction issues. Cheers, Steve
PS Don't expect much from the Administration, their ignorance and gullibility are studied.

On Thu, 24 Apr 2003, Mark Eakin wrote:

> At this point the question is what to do about the Soon and Baliunas
> paper. Would Bradley, Mann, Hughes et al. be willing to develop and
> appropriate rebuttal? If so, the question at hand is where it would be
> best to direct such a response. Some options are:

- >
- > 1) A rebuttal in Climate Research
- > 2) A rebuttal article in a journal of higher reputation
- > 3) A letter to OSTP

>

> The first is a good approach, as it keeps the argument to the level of
> the current publication. The second would be appropriate if the Soon
> and Baliunas paper were gaining attention at a more general level, but
> it is not. Therefore, a rebuttal someplace like Science or Nature would
> probably do the opposite of what is desired here by raising the
> attention to the paper. The best way to take care of getting better
> science out in a widely read journal is the piece that Bradley et al.
> are preparing for Nature. This leaves the idea of a rebuttal in Climate
> Research as the best published approach.

>

> A letter to OSTP is probably in order here. Since the White House has
> shown interest in this paper, OSTP really does need to receive a
> measured, critical discussion of flaws in Soon and Baliunas' methods. I
> agree with Tom that a noted group from the detection and attribution
> effort such as Mann, Crowley, Briffa, Bradley, Jones and Hughes should
> spearhead such a letter. Many others of us could sign on in support.
> This would provide Dave Halpern with the ammunition he needs to provide
> the White House with the needed documentation that hopefully will
> dismiss this paper for the slipshod work that it is. Such a letter
> could be developed in parallel with a rebuttal article.

>

> I have not received all of the earlier e-mails, so my apologies if I am
> rehashing parts of the discussion that might have taken place elsewhere.

>

> Cheers,
> Mark

>

>

>

> Michael E. Mann wrote:

>
 > > Dear Tom et al,
 > >
 > > Thanks for comments--I see we've built up an impressive distribution
 > > list here!
 > >
 > > This seemed like an appropriate point for me to chime in here. By in
 > > large, I agree w/ Tom's comments (and those of Barrie's as well). A
 > > number of us have written reviews and overviews of this topic during
 > > the past couple years. There has been a lot of significant scientific
 > > process in this area (both with regard to empirical "climate
 > > reconstruction" and in the area of model/data comparison), including,
 > > in fact, detection studies along the lines of what Barrie Pittock
 > > asked about in a previous email (see. e.g. Tom Crowley's Science
 > > article from 2000). Phil Jones and I are in the process of writing a
 > > review article for /Reviews of Geophysics/ which will, among other
 > > things, dispel the most severe of the myths that some of these folks
 > > are perpetuating regarding past climate change in past centuries. My
 > > understanding is that Ray Bradley, Malcolm Hughes, and Henry Diaz are
 > > working, independently, on a solicited piece for /Science/ on the
 > > "Medieval Warm Period".
 > >
 > > Many have simply dismissed the Baliunas et al pieces because, from a
 > > scientific point of view, they are awful--that is certainly true. For
 > > example, Neville has pointed out in a previous email, that the
 > > standard they applied for finding "a Medieval warm Period" was that a
 > > particular proxy record exhibit a 50 year interval during the period
 > > AD 800-1300 that was anomalously *warm*, *wet*, or *dry* relative to
 > > the "20th century" (many of the proxy records don't really even
 > > resolve the late 20th century!) could be used to define an "MWP"
 > > anywhere one might like to find one. This was the basis for their
 > > press release arguing for a "MWP" that was "warmer than the 20th
 > > century" (a non-sequitur even from their awful paper!) and for their
 > > bashing of IPCC and scientists who contributed to IPCC (which, I
 > > understand, has been particularly viscious and ad hominem inside
 > > closed rooms in Washington DC where their words don't make it into the
 > > public record). This might all seem laughable, it weren't the case
 > > that they've gotten the (Bush) White House Office of Science &
 > > Technology taking it as a serious matter (fortunately, Dave Halpern is
 > > in charge of this project, and he is likely to handle this
 > > appropriately, but without some external pressure).
 > >
 > > So while our careful efforts to debunk the myths perpetuated by these
 > > folks may be useful in the FAR, they will be of limited use in
 > > fighting the disinformation campaign that is already underway in
 > > Washington DC. Here, I tend to concur at least in spirit w/ Jim
 > > Salinger, that other approaches may be necessary. I would emphasize
 > > that there are indeed, as Tom notes, some unique aspects of this
 > > latest assault by the skeptics which are cause for special concern.
 > > This latest assault uses a compromised peer-review process as a
 > > vehicle for launching a scientific disinformation campaign (often
 > > viscious and ad hominem) under the guise of apparently legitimately
 > > reviewed science, allowing them to make use of the "Harvard" moniker
 > > in the process. Fortunately, the mainstream media never touched the
 > > story (mostly it has appeared in papers owned by Murdoch and his
 > > crowd, and dubious fringe on-line outlets). Much like a server which
 > > has been compromised as a launching point for computer viruses, I fear
 > > that "Climate Research" has become a hopelessly compromised vehicle in
 > > the skeptics' (can we find a better word?) disinformation campaign,
 > > and some of the discussion that I've seen (e.g. a potential threat of
 > > mass resignation among the legitimate members of the CR editorial
 > > board) seems, in my opinion, to have some potential merit.

> >
> > This should be justified not on the basis of the publication of
> > science we may not like of course, but based on the evidence (e.g. as
> > provided by Tom and Danny Harvey and I'm sure there is much more) that
> > a legitimate peer-review process has not been followed by at least one
> > particular editor. Incidentally, the problems alluded to at GRL are of
> > a different nature--there are simply too many papers, and too few
> > editors w/ appropriate disciplinary expertise, to get many of the
> > papers submitted there properly reviewed. Its simply hit or miss with
> > respect to whom the chosen editor is. While it was easy to make sure
> > that the worst papers, perhaps including certain ones Tom refers to,
> > didn't see the light of the day at /J. Climate/, it was inevitable
> > that such papers might slip through the cracks at e.g. GRL--there is
> > probably little that can be done here, other than making sure that
> > some qualified and responsible climate scientists step up to the plate
> > and take on editorial positions at GRL.
> >
> > best regards,
> >
> > Mike
> >
> > At 11:53 PM 4/23/2003 -0600, Tom Wigley wrote:
> >
> >> Dear friends,
> >>
> >> [Apologies to those I have missed who have been part of this email
> >> exchange -- although they may be glad to have been missed]
> >>
> >> I think Barrie Pittock has the right idea -- although there are some
> >> unique things about this situation. Barrie says
> >>
> >> (1) There are lots of bad papers out there
> >> (2) The best response is probably to write a 'rebuttal'
> >>
> >> to which I add
> >>
> >> (3) A published rebuttal will help IPCC authors in the 4AR.
> >>
> >> _____
> >>
> >> Let me give you an example. There was a paper a few years ago by Legates
> >> and Davis in GRL (vol. 24, pp. 2319-1222, 1997) that was nothing more
> >> than a direct
> >> and pointed criticism of some work by Santer and me -- yet neither of us
> >> was asked to review the paper. We complained, and GRL admitted it was
> >> poor judgment on the part of the editor. Eventually (> 2 years later)
> >> we wrote a response (GRL 27, 2973-2976, 2000). However, our response was
> >> more that just a rebuttal, it was an attempt to clarify some issues on
> >> detection. In doing things this way we tried to make it clear that the
> >> original Legates/Davis paper was an example of bad science (more
> >> bluntly, either sophomoric ignorance or deliberate misrepresentation).
> >>
> >> Any rebuttal must point out very clearly the flaws in the original
> >> paper. If some new science (or explanations) can be added -- as we did
> >> in the above example -- then this is an advantage.
> >>
> >> _____
> >>
> >> There is some personal judgment involved in deciding whether to rebut.
> >> Correcting bad science is the first concern. Responding to unfair
> >> personal criticisms is next. Third is the possible misrepresentation of
> >> the results by persons with ideological or political agendas. On the
> >> basis of these I think the Baliunas paper should be rebutted by persons

> >> with appropriate expertise. Names like Mann, Crowley, Briffa, Bradley,
> >> Jones, Hughes come to mind. Are these people willing to spend time on
> >> this?
> >>
> >> _____
> >>
> >> There are two other examples that I know of where I will probably be
> >> involved in writing a response.
> >>
> >> The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16,
> >> 10.1029/2002GL015345, 2002). I refereed a virtually identical paper for
> >> J. Climate, recommending rejection. All the other referees recommended
> >> rejection too. The paper is truly appalling -- but somehow it must have
> >> been poorly reviewed by GRL and slipped through the net. I have no
> >> reason to believe that this was anything more than chance. Nevertheless,
> >> my judgment is that the science is so bad that a response is necessary.
> >>
> >> The second is the paper by Michaels et al. that was in Climate Research
> >> (vol. 23, pp. 1-9, 2002). Danny Harvey and I refereed this and said it
> >> should be rejected. We questioned the editor (deFreitas again!) and he
> >> responded saying

> >> The MS was reviewed initially by five referees. ... The other three
> >> referees, all reputable atmospheric scientists, agreed it should be
> >> published subject to minor revision. Even then I used a sixth person
> >> to help me decide. I took his advice and that of the three other
> >> referees and sent the MS back for revision. It was later accepted for
> >> publication. The refereeing process was more rigorous than usual.
> >>
> >> On the surface this looks to be above board -- although, as referees who
> >> advised rejection it is clear that Danny and I should have been kept in
> >> the loop and seen how our criticisms were responded to.
> >>
> >> It is possible that Danny and I might write a response to this paper --
> >> deFreitas has offered us this possibility.
> >>
> >> _____
> >>
> >> This second case gets to the crux of the matter. I suspect that
> >> deFreitas deliberately chose other referees who are members of the
> >> skeptics camp. I also suspect that he has done this on other occasions.
> >> How to deal with this is unclear, since there are a number of
> >> individuals with bona fide scientific credentials who could be used by
> >> an unscrupulous editor to ensure that 'anti-greenhouse' science can get
> >> through the peer review process (Legates, Balling, Lindzen, Baliunas,
> >> Soon, and so on).
> >>
> >> The peer review process is being abused, but proving this would be
> >> difficult.
> >>
> >> The best response is, I strongly believe, to rebut the bad science that
> >> does get through.
> >>
> >> _____
> >>
> >> Jim Salinger raises the more personal issue of deFreitas. He is clearly
> >> giving good science a bad name, but I do not think a barrage of ad
> >> hominem attacks or letters is the best way to counter this.
> >>
> >> If Jim wishes to write a letter with multiple authors, I may be willing
> >> to sign it, but I would not write such a letter myself.
> >>
> >> In this case, deFreitas is such a poor scientist that he may simply

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> >> disappear. I saw some work from his PhD, and it was awful (Pat Michaels'
> >> PhD is at the same level).

> >>

> >>

> >>

> >> Best wishes to all,
> >> Tom.

> >

> >

> >

> >

> >

> >

> >

> > e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
> > <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

> >

>

>

> --

> C. Mark Eakin, Ph.D.
> Chief of NOAA Paleoclimatology Program and
> Director of the World Data Center for Paleoclimatology

>

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> Internet: mark.eakin@noaa.gov
> <http://www.ngdc.noaa.gov/paleo/paleo.html>

>

>

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Attachment Converted: "c:\eudora\attach\Ed-SHSIPCCpeer.pdf"

4482. 2003-04-24

date: Thu, 24 Apr 2003 20:28:20 +1200

from: j.salinger@niwa.co.nz

subject: And again from the south!

to: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme
<m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen
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<simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens"
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cg2003

"Greg.Ayers" <Greg.Ayers@csiro.au>, Tom Wigley <wigley@ucar.edu>

Dear friends and colleagues

This will be the last from me for the moment and I believe we are all arriving at a consensus voiced by Tom, Barrie, Neville et al., from excellent discussions.

Firstly both Danny and Tom have complained to de Freitas about his editorial decision, which does not uphold the principles of good science. Tom has shared the response. I would be curious to find out who the other four cited are - but a rebuttal would be excellent. Ignoring bad science eventually reinforces the apparent 'truth' of that bad science in the public mind, if it is not corrected. As importantly, the 'bad science' published by CR is used by the sceptics' lobbies to 'prove' that there is no need for concern over climate change. Since the IPCC makes it quite clear that there are substantial grounds for concern about climate change, is it not partially the responsibility of climate science to make sure only satisfactorily peer-reviewed science appears in scientific publications? - and to refute any inadequately reviewed and wrong articles that do make their way through the peer review process? I can understand the weariness which the ongoing sceptics' onslaught would induce in anyone, scientist or not. But that's no excuse for ignoring bad science. It won't go away, and the more we ignore it the more traction it will gain in the minds of the general public, and the UNFCCC negotiators. If science doesn't uphold the purity of science, who will?

We Australasians (including Tom as an ex pat) have suggested some courses of action. Over to you now in the north to assess the success of your initiatives, the various discussions and suggestions and arrive on a path ahead. I am happy to be part of it. Warm wishes to all

Jim

On 23 Apr 2003, at 23:53, Tom Wigley wrote:

> Dear friends,

>

> [Apologies to those I have missed who have been part of this email
> exchange -- although they may be glad to have been missed]

>

> I think Barrie Pittock has the right idea -- although there are some
> unique things about this situation. Barrie says

>

> (1) There are lots of bad papers out there
> (2) The best response is probably to write a 'rebuttal'

>

> to which I add

>

> (3) A published rebuttal will help IPCC authors in the 4AR.

>

>

>

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> Legates and Davis in GRL (vol. 24, pp. 2319-2322, 1997) that was
> nothing more than a direct and pointed criticism of some work by
> Santer and me -- yet neither of us was asked to review the paper. We
> complained, and GRL admitted it was poor judgment on the part of the
> editor. Eventually (> 2 years later) we wrote a response (GRL 27,
> 2973-2976, 2000). However, our response was more than just a rebuttal,
> it was an attempt to clarify some issues on detection. In doing things
> this way we tried to make it clear that the original Legates/Davis
> paper was an example of bad science (more bluntly, either sophomoric
> ignorance or deliberate misrepresentation).

>

> Any rebuttal must point out very clearly the flaws in the original

> paper. If some new science (or explanations) can be added -- as we did
> in the above example -- then this is an advantage.

>

>

> _____
>
> There is some personal judgment involved in deciding whether to rebut.
> Correcting bad science is the first concern. Responding to unfair
> personal criticisms is next. Third is the possible misrepresentation
> of the results by persons with ideological or political agendas. On
> the basis of these I think the Baliunas paper should be rebutted by
> persons with appropriate expertise. Names like Mann, Crowley, Briffa,
> Bradley, Jones, Hughes come to mind. Are these people willing to spend
> time on this?

>

>

> _____
>
> There are two other examples that I know of where I will probably be
> involved in writing a response.

>

> The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16,
> 10.1029/2002GL015345, 2002). I refereed a virtually identical paper
> for J. Climate, recommending rejection. All the other referees
> recommended rejection too. The paper is truly appalling -- but somehow
> it must have been poorly reviewed by GRL and slipped through the net.
> I have no reason to believe that this was anything more than chance.
> Nevertheless, my judgment is that the science is so bad that a
> response is necessary.

>

> The second is the paper by Michaels et al. that was in Climate
> Research (vol. 23, pp. 1-9, 2002). Danny Harvey and I refereed this
> and said it should be rejected. We questioned the editor (deFreitas
> again!) and he responded saying

>

> The MS was reviewed initially by five referees. ... The other three
> referees, all reputable atmospheric scientists, agreed it should be
> published subject to minor revision. Even then I used a sixth person
> to help me decide. I took his advice and that of the three other
> referees and sent the MS back for revision. It was later accepted for
> publication. The refereeing process was more rigorous than usual.

>

> On the surface this looks to be above board -- although, as referees
> who advised rejection it is clear that Danny and I should have been
> kept in the loop and seen how our criticisms were responded to.

>

> It is possible that Danny and I might write a response to this paper
> -- deFreitas has offered us this possibility.

>

>

> _____
>
> This second case gets to the crux of the matter. I suspect that
> deFreitas deliberately chose other referees who are members of the
> skeptics camp. I also suspect that he has done this on other
> occasions. How to deal with this is unclear, since there are a number
> of individuals with bona fide scientific credentials who could be used
> by an unscrupulous editor to ensure that 'anti-greenhouse' science can
> get through the peer review process (Legates, Balling, Lindzen,
> Baliunas, Soon, and so on).

>

> The peer review process is being abused, but proving this would be
> difficult.

>

> The best response is, I strongly believe, to rebut the bad science
> that does get through.

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>
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> _____
> Jim Salinger raises the more personal issue of deFreitas. He is
> clearly giving good science a bad name, but I do not think a barrage
> of ad hominem attacks or letters is the best way to counter this.
>
> If Jim wishes to write a letter with multiple authors, I may be
> willing to sign it, but I would not write such a letter myself.
>
> In this case, deFreitas is such a poor scientist that he may simply
> disappear. I saw some work from his PhD, and it was awful (Pat
> Michaels' PhD is at the same level).
>
> _____

> Best wishes to all,
> Tom.

>

Dr Jim Salinger, CRSNZ
NIWA
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Newmarket, Auckland
New Zealand
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e-mail: j.salinger@niwa.co.nz

4921. 2003-04-24

date: Thu Apr 24 15:42:36 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Can you provide a brief comment if not full review?
to: Keith Alverson <keith.alverson@pages.unibe.ch>

Keith
Sorry, I do have a recollection of quickly skimming this paper before and rapidly dumping it on a pile labelled "probably not worth the effort of giving a thorough review". Basically, I think the paper has little if anything to recommend publication. It adds little, except confusion, to the science. The main problem is a lack of focus and clear experimental design. The reducing sensitivity of tree growth to temperature forcing is unarguably a difficult and complex problem because the phenomenon is largely dependent on what trees/areas/variables/processing methods are used to make the comparison with "temperature". The temperature variable is itself a potentially ill-defined (compromise in effect) choice. Briffa et al have published a specific manifestation of this phenomenon - based on one highly selective set of data, for which they describe the local and regional associations with one optimum "summer average" for density data and another for ring width. The present paper, by not adequately defining the rules upon which they based their regionalisation of the tree-ring data or the basis for specifying a particular temperature season(s) to be used in the comparisons, serves to

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confuse a number of potential factors that contribute to the possible time-dependence in the correlations they describe. The overriding criticism is that they examine the regional tree ring series correlations with only the one (Northern Hemisphere annual mean) temperature series. It is therefore not possible to know to what extent the results represent a shift in the association between that Hemisphere mean series and the regional climates of the areas represented by their regional chronologies. There are other problems (such as time-dependent changes in the structure of these chronologies, non-comparability in the simple correlations because of the different lengths of period - it would be better to calculate significance levels over a moving window compatible in length to the short recent period(s) and test whether the reduced values are significant in the context of the longer records uncertainty estimates) . Work has been published that documents how temperatures averaged over different areas of the Hemisphere correlate with the Northern Hemisphere mean and the associations are subtly time dependent and time-scale dependent and seasonally dependent . The association between Northern Sweden and The Hemisphere in summer is especially weak and one would not presumably base a reconstruction of the latter on only one Tornetrask series anyway as shown in 4. The paper does not offer much because it needs to be very much reworked after considerable work - and the conclusions are pretty much hand waving anyway. I do not know whether this is sufficient but it does give my "overall" opinion. cheers

Keith

At 02:24 PM 4/23/03 +0200, you wrote:

Hi Keith,

I hope you have recovered from your back surgery well. I am writing with regard to the sonenckin paper submitted for a special issue of paleo3 that Olga Solomina and I are editing that I sent you asking for a review some time ago.

The timeline for the issue is rapidly drawing to a close so I absolutely must send this back to the author with his reviews before the end of April. The paper deals with the recent decoupling of temperature and tree ring indices in high latitude Eurasia that you have pointed out in previous publications, so I feel it is rather key to have your thoughts. The other reviewer has provided a very thorough set of suggestions, so I don't really need a thorough review, but I would very much appreciate it if you could have a quick read of the paper and let me know your general thoughts, in particular if there are any glaring errors in it! If you cannot find the time, please also let me know so I can find another option. Thanks in advance.

Keith

on 02/18/2003 11:56 AM, Keith Briffa, cru (Climatic Research Unit) at K.Briffa@uea.ac.uk wrote:

> Unfortunately, I am forced to be away from the office for some weeks at least
> during February and early March, having surgery on my back and undertaking a
> period of recuperation. If you are contacting me regarding outstanding review

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> requests or queries regarding the status of manuscripts submitted to The
> Holocene, please note that I am dealing with these during my absence and I
> will contact you directly. If your request is of a different nature, I will
> try to respond in due course, but you may prefer to try one of my colleagues
> (see below).

>

>

>

> Questions with regard to our current research proposals should be directed

to:

>

>

>

> Tim Osborn (t.osborn@uea.ac.uk) - SOAP or RAPID;

>

> Phil Jones (p.jones@uea.ac.uk) - HOLSMEER, ALPIMP.

>

>

>

> Keith Briffa

>

> 29/1/03

>

--

Keith Alverson
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Professor Keith Briffa,
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Phone: +44-1603-593909
Fax: +44-1603-507784
[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

1626. 2003-04-25

date: Fri, 25 Apr 2003 21:02:41 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: Review- confidential
to: Keith Briffa <k.briffa@uea.ac.uk>

Hi Keith,

I just got back from a meeting at Duke with Crowley, with Phil there and a bunch
of

modelers/stats types like Miles Allen, Tim Barnett, and Francis Zwiers. I was
asked to

specifically discuss the Esper series and how it was created. Overall it went
well I think.

I suspect that the paper Phil and Mike are going to put together is mainly in
response to

the Soon and Baliunas paper in Climate Research, although Mike will undoubtedly
do what he

can to discredit the Esper series. Phil didn't mention anything about this paper
to me, but

upon my return there was an email from Mike asking for all of the Esper data,

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including the

long Mongolia record. Fat chance on the latter. I will give him the former if Jan agrees,

but not out of any respect to him. Interesting too what you say about Ray, Malcolm, and

Henry. Not surprising, but interesting. I have already sent Ray the Esper data minus

Mongolia. I think he presented it in part of his talk in Nice on the MWP.

I will be happy to work on your suggested paper with you and Tim. Where do you think it

would be published. Let me know how you want to proceed.

Cheers,

Ed

I presume you are not there at the moment - but this can't wait

I have been mulling over the idea of a review along the lines of "Late Holocene History

of Northern Hemisphere Temperatures - the contribution of Tree-Ring Data" . In part this

is stimulated again recently by the news that Mike Mann and Phil Jones are writing a

review of the Northern Hemisphere series in which (according to Mike Mann) they will

" among other things, dispel the most severe of the myths that some of these folks are

perpetuating regarding past climate change in past centuries."

Also I understand that Ray Bradley, Malcolm Hughes, and Henry Diaz are working,

independently, on a solicited piece for Science on the "Medieval Warm Period".

I truly believe that for the good of the Science , this requires an informed contribution regarding the implications of the tree-ring input to this work.

It would encompass a review of the role of ring-width and density data , and implications of how they are used (standardised/ built into

chronologies/calibrated) in

the various series. It could be used to say a lot more as well about the apparent extent

of 20th century warming? I think Tim could write it - but with our help. What do you say

(in principal) and then we can think about organising a plan for the next couple of

months and a trip for us to come to you ?

--

Professor Keith Briffa,

Climatic Research Unit

University of East Anglia

Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

<http://www.cru.uea.ac.uk/cru/people/briffa/>

--

=====

Dr. Edward R. Cook

Doherty Senior Scholar and

Director, Tree-Ring Laboratory

Lamont-Doherty Earth Observatory

Palisades, New York 10964 USA

Email: drdendro@ldeo.columbia.edu

Phone: 845-365-8618

Fax: 845-365-8152

=====

1430. 2003-04-28

date: Mon Apr 28 15:03:41 2003

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from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: CR plus a fax

to: Phil Jones <p.jones@uea.ac.uk>

Thanks Phil. After my one email about possible resignations from CR a whole flood of

emails seems to have been released. I will wait to see what happens re. Hans and Clare,

and will just let my fellow review editors know the score. I might independently write to

the publishers voicing my own concern about losing faith in the peer review process in CR.

As an ex-Editor of CR I perhaps also carry some weight with them.

Mike

At 10:17 28/04/2003 +0100, you wrote:

Mike,

I've just talked to Clare about discussions I had with Hans last week in the US.

I think

he is now convinced about de Freitas and is drafting a letter with Clare to go to the

publishers

and to de Freitas. Basically trying to get the reviewer's names etc and their reports

in the

first instance, with maybe sending some of the background emails to the publishers.

Also

assessing copyright as the 'other' Soon/Baliunas paper in Energy and Env. is essentially

the same as that in CR.

Hans wanted to try this first, but didn't want to tell all what he was doing. Fears

a backlash

if de Freitas gets removed without due cause. So let's all try and keep the emails

down, and

hope we can report something to all once the correspondence Hans initiates gets

replies.

Cheers

Phil

PS There is a fax for you in CRU - Julie is away at the moment - from an Energy group.

Fax is an article in the Washington Times by Pat Michaels saying that to get Blair's

support

on Iraq the price was Kyoto ! Goes on to say that all UK climatologists are spineless

for not

going against the UK Govt ! The person who sent it would like you to reply - I would

check this

with DEFRA first. Sorry for reading your fax - I was in early and trying to sort out

one of mine

and yours was more interesting !

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

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NR4 7TJ

UK

4132. 2003-04-28

date: Mon, 28 Apr 2003 14:46:11 +0100
 from: Phil Jones <p.jones@uea.ac.uk>
 subject: Fwd: Re: Fwd: RE: Rog Outline
 to: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

Somewhere in this message is Mike's review of the seasonal cycle paper.

Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu
 X-Mailer: QUALCOMM Windows Eudora Version 5.1.1
 Date: Mon, 28 Apr 2003 09:02:43 -0400
 To: Phil Jones <p.jones@uea.ac.uk>
 From: "Michael E. Mann" <mann@virginia.edu>
 Subject: Re: Fwd: RE: Rog Outline
 Cc: mann@virginia.edu

Hi Phil,

Re, DeFreitas--good to hear. That piece that Jim Salinger just forwarded is especially damning...

Thanks for the message. I just got the record from Cronin before your email, so we're in pretty good shape. It would be nice if we can get the Briffa/Osborn, Cook, and D'Arrigo et al series, but already we can do a reasonable 2K composite. I've mostly been trying to seek out the long (2K) series so we can do the longer composite, but I suppose it would be useful to show a few key new records (especially tropical ones) that are shorter...

I'm also working on filling in some details and preparing rough drafts of the various sections, so perhaps within a week we can merge what we have...

Review on the JGR paper appended below. As you might imagine, my main sensitivity was w/ conclusions about implications for e.g. Mann et al which I didn't think necessarily followed from this analysis. The revisions requested are mostly changes in wording, and

it should be straightforward to address them in a final version...

mike

Comments:

General Comments:

This is an interesting manuscript, raising some important issues regarding seasonality of past temperature trends that are interesting in their own right, and may have potential implications for certain paleoclimate reconstructions. These issues are worthy of discussion in the literature, and JGR is an appropriate venue. The authors, as is typical, have done a careful job with their analysis, and it appears sound, as do the primary conclusions, although I have some specific reservations. The primary criticism is that the authors imply a greater generality to their conclusions than can actually be justified, given the limitations of the available data series. There are a number of important caveats that need to be invoked in the interpretation of the results, and the

limitations in drawing large-scale conclusions from the limited data need to be acknowledged up front. There are a number of underlying issues regarding the nature of the seasonal and spatial details of past climate change (in particular, forced climate change) which likely impact the interpretation of the results, which are not given adequate discussion in the manuscript at present. Given the space available in a JGR paper (vs. e.g. a GRL article), there is no excuse for not providing more detailed discussion where appropriate. I provide several specific comments below along these lines which should be addressed in a revised version of the manuscript.

Specific Comments

1) Abstract--the generality of the conclusions are overstated in the abstract. The evidence is only from Europe and China (i.e., only the fringes of the Eurasian continent alone) but the wording argues that implications apply to other regions. It isn't even clear that the conclusions apply to the interior of the Eurasian continent, let alone any of North America (see comments below). It is a leap of faith, then, to assume that the results generalize to extratropical hemispheric (let alone, full hemispheric) trends, and the authors need to be more cautious in drawing general conclusions.

2) Introduction, first sentence: There is a potential "straw man" argument being introduced here. Precisely which "annual temperature" reconstructions are being referred to here? The statement made could arguably apply to Crowley and Lowery (2000), which is based on scaling a composite of largely extratropical (and mostly summer-sensitive) proxy records against the annual mean Northern Hemisphere mean instrumental series. It is far more difficult, however, to argue that the authors' statements fairly characterize the Mann et al (1998;1999) annual mean temperature reconstruction. In the latter case, half of the area of the hemispheric mean surface temperature reconstruction comes from tropical latitudes (i.e., latitudes below 30N), and the proxy indicators primarily used to calibrate the tropical annual-mean patterns of variance are almost certainly not boreal warm-season in nature (for the example, the ENSO-scale patterns of tropical SST variance in the reconstruction are calibrated, in large part, by a combination of cold-season drought sensitive tree-ring data from Mexico, tropical tree-ring data, and tropical corals and ice cores--none of which could be argued to exhibit a boreal warm-season sensitivity bias!). The authors arguments cannot be argued to apply to these reconstructions (as seems to be implied by later comments--see below).

3) Discussion of Figures 1 and 2 on pages 5-6: the authors should compare a single

long-term composite series based on averaging the various (potentially, standardized) station JJA-DJF series with that which is available for the full NH back through the mid 19th century. The point here is to see how well they compare in terms of the general trends during the interval (back through the mid 19th century) of overlap--in fact, based on inspection of e.g. Figure 1, I don't think that there will be much similarity, and, if that is the case, then it demands extreme caution in generalizing about the true large-scale or hemispheric nature of inferred trends in summer-winter temperature differences based on the sparse long series available to the authors.

4) Related to point #3 above, recent studies (see e.g. the discussion in the Mann, 2002 piece which is in the reference list but not actually cited in the text, and also the results of Shindell et al, 2003) have shown that large seasonal differences in temperature trends are expected in past centuries because of the seasonally-specific response, in particular, to volcanic forcing (see Kirchner et al, 1999). The largest seasonal differences are likely to occur in the continental centers, where volcanic forcing tends to impart a large summer cooling but also typically a sizeable dynamically-induced warming (related to the response of the Northern Annual Mode, or 'AO' or 'NAO' to volcanic stratospheric aerosol forcing) in the following winter. The large differences, however, are observed over the continental centers, and in fringe regions such as Europe or China, the response may not even be of the same sign as the continental mean response, which is dominated by the behavior of the continental centers. Thus, any spatial network (proxy or instrumental) which exhibits a bias with respect to the sampling of the continents is likely to exhibit a bias in terms of the estimate of summer-winter temperature differences (Mann, 2002). Since the authors instrumental network only samples the fringes of the Eurasian continent, it is very unlikely to capture the true winter-summer difference in Eurasian continental mean temperature, let alone Northern Hemisphere extratropical continental (Eurasia and North America) temperature, let alone Northern Hemisphere extratropical mean (land and ocean) temperature, let alone true Northern Hemisphere (tropical and extratropical, land and ocean) temperature! Once again, this calls for caveats in the interpretation of the present results with regard to hemisphere-scale implications.

5) Related to the above, why don't the authors show, in Figure 1, the results for some of the long available North American series (which includes several long east coast series, but also a series in Minnesota back to the early 19th century) to establish the similarity of the longer-term summer-winter trends in the two continents

(this too

should be included in the composite discussed in point #3 above).

6) End of first paragraph on page 6, the authors might note that certain modeling studies (Shindell et al, 2003) have indeed already looked at potential seasonally-distinct temperature changes in past centuries, that are associated with the seasonally-distinct signature of the response to known natural climate forcings.

7) Figure 3 indicates a relationship that holds during the latter 20th century, presumably somewhat specific to the mix of internal and forced variability that dominates over that period. This may not be representative of the situation in earlier centuries, where the primary pattern of forced variability is by volcanic and solar forcing which impart distinct regional and seasonal signatures in the temperature field

(see Shindell et al, 2001;2003) that are likely to be quite different from those associated with anthropogenic forcing (GHG and aerosol) which dominate during the interval examined by the authors. Related to this, have the series been detrended before calculating the correlations shown in Figure 3? This has a bearing on the interpretation.

8) 3rd paragraph on page 7, the discussion of previous work (e.g. Mann et al, 1998;1999)

here is misleading for the reasons spelled out in point #2 above. The arguments assuming a warm-season sensitivity bias do not apply to the full hemispheric reconstruction but, at most, the extratropical component of the reconstruction. The

statement (2 sentences up from bottom of paragraph) "Their implicit assumption that the

relative trends..." is not a fair statement in reference to the Mann et al multiproxy

reconstructions, and the discussion needs to be revised here. An analysis (Rutherford et al, to be submitted) shows, using a common statistical method, but distinct data sets,

that the multiproxy network of Mann et al calibrates and cross-validates cold-season

variability more skillfully than the tree-ring maximum latewood density ('MXD') density

network of Briffa and coworkers, while the Briffa et al MXD network, in turn, calibrates

warm-season variance more skillfully than the multiproxy network. In short, the

conclusions drawn here don't apply to reconstructions of tropical surface temperature

variability, nor to multiproxy data used to reconstruct that variability, so the

implications of the authors results for multiproxy reconstructions of full Northern

Hemisphere annual mean temperature are not clear. The authors need to downplay their

conclusions in this regard.

9) The authors and this reviewer are in common agreement that seasonally-specific

biases are likely to be present in most climate proxy data, and that these biases need

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to closely considered in the process of climate reconstruction. This is a fair point, and one worth emphasizing in the conclusions But the specific conclusions of the authors in this study regarding summer-winter differences based on the series analyzed do not clearly generalize to other proxy-based surface temperature reconstructions (particularly multiproxy reconstructions with an equal tropical and extratropical emphasis) for the reasons spelled out above, and this point, in fairness, should be made.

REFERENCES:

Kirchner, I., G.L. Stenchikov, H.-F. Graf, A. Robock, and J.C. Antuna, Climate model simulation of winter warming and summer cooling following the 1991 Mount Pinatubo volcanic eruption, Journal of Geophysical Research, 104 (D16), 19039-19055, 1999.
Shindell, D.T., Schmidt, G.A., Mann, M.E., Rind, D., Waple, A., Solar forcing of regional climate change during the Maunder Minimum, Science, 294, 2149-2152, 2001.
Shindell, D.T., Schmidt, G.A., Miller, R., Mann, M.E., Volcanic and Solar forcing of "Little Ice Age" Surface Temperature Changes, Journal of Climate, in press, 2003.

At 01:44 PM 4/28/2003 +0100, you wrote:

Mike,

Now had a chance to catch up a little. On de Freitas I hope something is going to happen, but I don't to say anything yet. Hans and Clare will write to the publishers and try to get the reviews from de Freitas. Hans is now convinced he should go, but wants to do on a due cause basis and by the book so any backlash can be dealt with in a fair manner.

I think I might have mentioned this to you in an email from Duke, but I must have done something wrong as I've lost some emails. I can't find the one from you saying you'd reviewed

the recent JGR paper on the annual cycle, for example. I was bleary eyed at times at Duke,

but I'm sure I read it ! Can you send the review if it's easy to locate ?

On RoG all the series you've mentioned would be good to get. Tim is away here so I

can't ask him if he's sent the Eurasian one, but I'll check when he's here. All the

others seem good ones to go for. I'll email Dahl-Jenssen to see if I can get anything.

As for the title, why don't we go for 'Climate during the past two millennia',

still with the emphasis on the last one. This way it won't be too different from the one we gave to RoG.

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The first millennia will be semi quantitative and would just be smoothed versions - simple averages of what we can get, scaled against NH extended summers. We should probably put less emphasis on the MWP as Ray/Henry/Malcolm are working on that and more on the LIA in discussion - thinking aloud here. We could ask Ray for a draft in a couple of months and exchange bits of text. I did think of Climate during the Christian Era !!! but that was going too far ! So, we will use AD and BC dates if needed, remembering AD goes before.

At the EGS there was a 300 year coral series from Malindi, Kenya from Rob Dunbar that we should get. I'll email Rob if I can find his email address.

Finally, I've written two sections on instrumental and documentary for section 2.

Getting someone to type these in here and I'll work on them a bit before sending. I need to get Astrid's views on a few sentences on the Norse. Also I'll start a reference list as this might be a good way to start - who we must reference and also acknowledge. When I began the writing I realised it wouldn't take too long as there isn't that much space. So

Figures, Refs, Captions, Acknowledgements are crucial.

Ed also has some Nepalese reconstructions - he's just got a paper in proof stage.

They are not that long though, late 1500s. When he comes through you could ask him for those also.

Cheers

Phil

At 16:59 26/04/03 -0400, you wrote:

Hi Phil,

I've managed to get my hands on the long chinese reconstruction, and have sent out

requests for data to Ed Cook (the long RCS series from Esper et al), Tim (their long

Eurasian reconstruction as published in the '99 Science piece--haven't hear from Tim

yet, can you look into this?), D'Arrigo/Jacoby (Sol Dav Mongolia record) , and Cronin

(Chesapeake Bay spring temperature reconstruction). Ray apparently has been trying to

get the Dye3/GRIP borehole data from Dahl-Jensen for some time, but without success--perhaps you could also try to get ahold of these?

I'm going to make a preliminary attempt based on the few long (2K) records I already

have (western U.S., China, Quelccaya 018, Fennoscandia) to use as a placeholder in the

paper if nothing else, and we can improve on this as we get more data. Since we'll

probably only want to form a composite at decadal resolution, we can probably

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scan many
of the records if we haven't received them (I'm supporting an undergrad on a
grant who,
among other things, will be able to scan in series for us--they start in less
than a
month).

let me know what you think. thanks,

mike

Date: Thu, 24 Apr 2003 13:59:28 -0400

To: t.osborn@uea.ac.uk

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Fwd: RE: Rog Outline

Cc: p.jones@uea.ac.uk, mann@virginia.edu

Tim,

Can I get from you the Eurasian composite that you and Keith published in the
Science

perspective in '99? Phil and I are working on trying to do a simple-minded
composite of

a few of the 2K length temperature proxies for a piece we're working on
together.

thanks in advance for any help you can provide,

mike

X-WebMail-UserID: f028@uea.ac.uk

Date: Thu, 24 Apr 2003 11:14:26 +0100

Sender: f028 <f028@uea.ac.uk>

From: f028 <P.Jones@uea.ac.uk>

To: "Michael E. Mann" <mann@virginia.edu>

X-EXP32-SerialNo: 00104935

Subject: RE: Rog Outline

X-Mailer: InterChange (Hydra) SMTP v3.61.08

Mike,

Let's try and do this. I'll get back to you with more ideas next week.

So for the moment, let's go with the last few or two millennia. I'll

talk to a few who are here at Duke.

Send revisions then assuming last few millennia, but the main emphasis
will still be the last one.

Cheers

Phil

Professor Michael E. Mann
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Professor Michael E. Mann
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cg2003
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1238. 2003-04-29

date: Tue Apr 29 13:55:38 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Review- confidential
to: Edward Cook <drdendro@ldeo.columbia.edu>

Thanks Ed

Can I just say that I am not in the MBH camp - if that be characterized by an unshakable

"belief" one way or the other , regarding the absolute magnitude of the global MWP. I

certainly believe the " medieval" period was warmer than the 18th century - the equivalence

of the warmth in the post 1900 period, and the post 1980s ,compared to the circa Medieval

times is very much still an area for much better resolution. I think that the geographic /

seasonal biases and dating/response time issues still cloud the picture of when and how

warm the Medieval period was . On present evidence , even with such uncertainties I would

still come out favouring the "likely unprecedented recent warmth" opinion - but our

motivation is to further explore the degree of certainty in this belief - based on the

realistic interpretation of available data. Point re Jan well taken and I will inform him

At 07:59 AM 4/29/03 -0400, you wrote:

Hi Keith,

I will start out by sending you the chronologies that I sent Bradley, i.e. all but

Mongolia. If you can talk Gordon out of the latter, you'll be the first from outside

this lab. The chronologies are in tabbed column format and Tucson index format. The

latter have sample size included. It doesn't take a rocket scientist (or even Bradley

after I warned him about small sample size problems) to realize that some of the

chronologies are down to only 1 series in their earliest parts. Perhaps I should have

truncated them before using them, but I just took what Jan gave me and worked with the

chronologies as best I could. My suspicion is that most of the pre-1200 divergence is

due to low replication and a reduced number of available chronologies. I should also say

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that the column data have had their means normalized to approximately 1.0, which is not the case for the chronologies straight out of ARSTAN. That is because the site-level RCS-detrended data were simply averaged to produce these chronologies, without concern for their long-term means. Hence the "RAW" tag at the end of each line of indices. Bradley still regards the MWP as "mysterious" and "very incoherent" (his latest pronouncement to me) based on the available data. Of course he and other members of the MBH camp have a fundamental dislike for the very concept of the MWP, so I tend to view their evaluations as starting out from a somewhat biased perspective, i.e. the cup is not only "half-empty"; it is demonstrably "broken". I come more from the "cup half-full" camp when it comes to the MWP, maybe yes, maybe no, but it is too early to say what it is. Being a natural skeptic, I guess you might lean more towards the MBH camp, which is fine as long as one is honest and open about evaluating the evidence (I have my doubts about the MBH camp). We can always politely(?) disagree given the same admittedly equivocal evidence. I should say that Jan should at least be made aware of this reanalysis of his data. Admittedly, all of the Schweingruber data are in the public domain I believe, so that should not be an issue with those data. I just don't want to get into an open critique of the Esper data because it would just add fuel to the MBH attack squad. They tend to work in their own somewhat agenda-filled ways. We should also work on this stuff on our own, but I do not think that we have an agenda per se, other than trying to objectively understand what is going on. Cheers, Ed Ed thanks for this - and it is intriguing , not least because of the degree of coherence in these series between 1200 and 1900 - more than can be accounted for by either replication of data between the series (of which there is still some) or artifact of the standardisation method (with the use of RCS curves which are possibly inappropriate for all the data to which each is applied) . Having then got some not insubstantial confidence in the likelihood of a real temperature signal in this period - the question of why the extreme divergence in the series pre-1200 and post 1900? A real geographic difference in the forcing , replication and standardisation problems? - both are likely. We would like the raw cores for each site: the RCS indices upon which you base the chronologies ; the site chronologies (which I think you sent to Ray?). At first we will simply plot the site chronologies , correlate each with local climate and come

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back to
you again. We will also plot each "set" of indices and compare site RCS curves
and
reconsider the validity of the classification into linear and non-linear
growth
patterns. I know you have done all this but we need to get a feel for these
data and do
some comparisons with my early produce ring-width RCS chronologies for ceratin
sites and
compare the TRW series with the same site MXD chronologies - all a bit suck
and see at
first. I am talking with Tim later today about the review idea and I will
email/phone
before 16.00 my time today.

Thanks

Keith

At 10:01 AM 4/28/03 -0400, you wrote:

Hi Keith,

Here is the new Esper plot with three different forms of regionalization:
linear vs.

nonlinear (as in the original paper), north vs. south as defined in the
legend, and east
vs. west (i.e. eastern hemisphere vs. western hemisphere). All of the series
have been

smoothed with a 50-yr spline after first averaging the annual values. The
number of

cores/chronologies are given in the legend in parentheses. Not surprisingly,
the north

and south chronologies deviate most in the post-1950 period. Before 1950 and
back to

about 1200 the series are remarkably similar (to me anyway). Prior to 1200
there is more

chaos, perhaps because the number of chronologies have declined along with the
within-chronology replication. However, there is still some evidence for

spatially
coherent above-average growth. I showed this plot at the Duke meeting. Karl
Taylor

actually told me that he thought it looked fairly convincing, i.e. that the
low-frequency structure in the Esper series was not an artefact of the RCS

method.

Cheers,

Ed

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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=====
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cg2003

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3238. 2003-04-29

date: Tue, 29 Apr 2003 15:40:44 +0100
from: "R Warren" <R.Warren@uea.ac.uk>
subject: ESF proposal
to: "'Mike Hulme'" <m.hulme@uea.ac.uk>

Mike,

I have to list three referees : since most of the people we'd choose are actually IN the proposal or are proposed participants ... may need to choose others. However it doesn't actually SAY that they can't be participants or even that they must be independent. But I was thinking of listing Steve Schneider and Hadi Dowlatabadi. I need a third. Might they feel excluded from the workshop though? There wasn't budget to fly in people from the US. Perhaps a third referee could be Jan since he is not actually involved? (Although his insitution is involved). Also there is an opportunity to list someone NOT to referee the proposal, in strict confidence! Richard Tol?!

Your thoughts?

Rachel

Dr. Rachel Warren
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Tyndall Centre (HQ)
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ

Tel: 01603 593912
Fax: 01603 593901

4205. 2003-04-29

date: Tue Apr 29 12:51:19 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: fax to Andrew Warren
to: v.mcgregor

Vanessa,

Please send this fax to Andrew Warren at the Association for the Conservation of Energy. And then leave a copy for me and for Asher.

Fax #: 020 7359 0863

Dear Andrew,

Thanks for your fax of 17 April. Unfortunately it only reached me today, since it went to the Climatic Research Unit here at UEA instead of the Tyndall Centre (please update your records accordingly - see above).

There is nothing new about Pat Michaels causing mischief, although this particularly spin on Blair's foreign policy and the cowardly UK climate scientists is indeed new. This is a constant battle to be waged not only in newspapers, but also recently in peer-reviewed academic journals, where scientists on both sides of the Atlantic are particularly active at present in rebutting the claims of spurious and primary school science that appear to undermine what we know is happening to world climate. I will look into the Washington Times and see whether I can get a letter submitted - although time has now lapsed.

About the Scientific Alliance meeting - we discussed at some length whether Tyndall Centre should provide a speaker for this event (I am aware of the dubious nature of the organisation) and decided on balance that we should. It at least means that those who turn up for the event will get to hear a credible view from science about

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the overwhelming evidence. I appreciate that one tactical position is ever to refuse to appear on the same platform as people who flatly contradict what we know about global climate, but that is not a tactic I have generally adopted in my professional life (although I am careful and selective about which people and organisations I debate with, and where). Thanks for your interest in these issues - and thanks for your monthly column that you send me. I also read it with interest and it helps me follow the twists and turns of energy policy in the UK. With best wishes,
Mike

244. 2003-04-30

date: Wed Apr 30 09:43:25 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: FW: CRU05 current status
to: Tim Mitchell <t.mitchell@uea.ac.uk>

Depends on status of meeting. If "official" IPCC invite, then DEFRA would (should) agree

to pay your costs. If only an unofficial scoping meeting then perhaps not.

Either way, we can sort something out re. costs if (a) you are invited and (b) you are keen to go.

Mike

At 15:36 29/04/2003 +0100, you wrote:

Mike,

Phil has been pushing Martin Parry to get me an invite to the IPCC WG1/2 interface meeting on climate change *impact* detection (New York, mid-June).

The other UK invitees are Tim Sparks and Myles Allen.

If I get an official invite, I will need travel funds from some budget or other. I presume that the co-op budget (50% of my time at present) is the most likely candidate, but are there sufficient travel funds in there?

Tim

Dr. Tim Mitchell
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----- Forwarded Message

From: Phil Jones <p.jones@uea.ac.uk>
Date: Tue, 29 Apr 2003 13:57:06 +0100
To: Tim Mitchell <t.mitchell@uea.ac.uk>
Subject: Re: CRU05 current status

Tim,

You sent me this paper earlier. I said at the time to myself that I hoped Sarah wasn't

being asked too much about patterns and causes, as she could do with some education

on some issues. Hopefully your email has helped.

The web site ought to contain a slightly modified version of what Mike is getting at at the

end of this email. We certainly need this.

I was invited to an IPCC WG1/2 interface meeting on climate change detection in

New York in the week of June 16. As I'm unable to go I've talked to Kathy Maskell and to

Martin Parry (the organiser as the initiative comes from WG2) and suggested they invited

you. If you are able to go then can you represent CRU as well as

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Tyndall. I'll forward the email invitation and I hope they contact you. If you can't make it suggest Tim Osborn.

The issue of which datasets to use for different applications is an important aspect to get across to those going.

Cheers

Phil

At 11:47 28/04/03 +0100, you wrote:

> > When you do a new draft of the paper I would suggest you add in that
> > this data should
> > not be used for climate change detection studies - not this bluntly, but
> > you know what I'm
> > getting at.

>

>Will do.

>

> > Maybe this just needs to go on the web site.

>

>What do you think of the Q and A below? Mike's added his comments at the bottom. I'm thinking of adding an edited version of these Q and A to the website. I guess that another version could go in the paper.

>

>Tim

>

>----- Forwarded Message

>From: Mike Hulme <m.hulme@uea.ac.uk>

>Date: Wed, 09 Apr 2003 14:58:15 +0100

>To: Tim Mitchell <t.mitchell@uea.ac.uk>

>Subject: Re: CRU interpolated climate

>

>Tim - see my comments at the end

>

>

>

>At 16:59 07/04/2003 +0100, you wrote:

> > Sarah,

> >

> > Many questions!

> >

> > I'll answer as best I can, but please do not quote these answers, as I ought

> > to collaborate with co-authors before giving any quotable comments.

> >

> >> I am now thoroughly confused and would be very grateful if you could
> >> sort me out! I have read your guidelines on the web-site, and need
> >> help with interpreting the following:

> >>

> >> "These choices mean that while this data-set is suitable for using as
> >> an input to environmental modelling, it is NOT suitable for use in
> >> detecting climate change. It is NOT a legitimate use of this data-set
> >> to attempt to prove or disprove the existence of climate change at an
> >> individual grid-box."

> >>

> >> My questions are:

> >> > 1. Is the 1960-2000 climate time series really not to be used at all
> >> > to detect climate change, even over aggregated, regional areas?

> >>

> > It depends on the region, period, and climatic variable! For 1961-1990, say,

> > and for the European mainland, there will probably not be a problem. The
> > density of stations is sufficient that individual stations coming in

> and out

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> > are not likely to substantially affect the values over this large area.
> > However, over central Africa this is probably not true.
> >
> > Climate change detection is a specialised subject. It demands either
> > individual station time-series, or carefully assembled (usually
> > low-resolution) grids. See Q4 of the FAQ.
> >
> >> > How
> >> > can it be used as input for environmental modelling if it is not
> >> > accurate enough to show real phenomena of change?
> >
> > The high-resolution grids do show "real phenomena of change". However,
> > it is
> > not the long-term changes for which the grids are optimised; the grids are
> > optimised for high-resolution 'snapshots', month by month.
> >
> > Perhaps it would help if I gave an example of how the data-sets can be

best

> > used in data-sparse regions.
> > 1. Constructing a trend at a grid-box is not a good idea, as we
> > explained in
> > the Nature paper.
> > 2. It would be legitimate to use linear regression to compare (say) April
> > precip over a few grid-boxes (or perhaps even one grid-box if the data at
> > that box seems to warrant it) with some comparable areal (not point!!)
> > environmental index from 1981-2000, to derive an estimate of the
> > relationship between interannual variability in precip and interannual
> > variability in the environmental index.
> >
> >> > where does this
> >> > leave all the previous publications from CRU on regional climate
> >> > change?
> >
> > Largely unchanged, as I see it. These high-resolution grids are our 'best
> > estimate' of the climate, at a high spatial resolution, in each month in
> > 1901-2000. Perhaps the risks of temporal inhomogeneities at the level of
> > individual grid-boxes could have been made clearer in the past - that is a
> > matter of judgement I guess. The coarser-scale grids produced by CRU, such
> > as Phil Jones' work, are not affected because they use different methods.
> >
> >> > 2. Over what scale do you consider it legitimate to make spatial
> >> > comparisons? Again, some of the publications show, for example, maps
> >> > of Africa with different climate anomalies over about 1000km. With
> >> > greater densities of met stations in Europe, is the spatial
> >> > resolution any better there?
> >
> > I find it hard to give a definitive answer, because the spatial scale over
> > which the climate information is temporally homogenous varies with region,
> > period, and climatic variable. My answer above provides some hints.
> >
> >> > I absolutely appreciate the problem of the changing input from met
> >> > stations through time - we face the same sorts of irregular
> >> > sequential data input from satellite sensors. And I equally
> >> > appreciate that interpolation must blur the differences between
> >> > neighbouring grid-boxes - but over what distance relative to the
> >> > spatial distribution of input met stations?
> >
> > This depends on the spatial scales over which different variables vary.

See

> > the New et al (2000) paper for the precise values used.
> >
> >> > We are being asked again and again to analyse patterns and causes of
> >> > "emerging" diseases in many parts of the world, and we are really

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> >> > concerned to make real sense of the subject, which involves having an
> >> > accurate idea of the degree of climate change within land masses the
> >> > size of Europe. I am myself about to send off a paper for a
> >> > conference proceedings concerned with tick-borne diseases in Europe.
> >> > I have no agenda at all - I am as happy to discover that there has
> >> > been, or has not been, any relevant climate change to account for the
> >> > variety of temporal and spatial patterns of disease change across
> >> > Europe, but I am desperately keen to get it right as a basis for
> >> > further work.
> >> >
> >> > Looking forward to a fruitful dialogue with you.
> >> >
> >> > Regards
> >> > Sarah
> >
> > Regards
> >
> > Tim
> >
> > PS if any co-authors cc'd want to comment, please feel free!
>
> Tim - worth distinguishing between two types of problems with the New et al.
> data set:
>
> (a) it is specifically *not* designed for climate change
> detection/attribution in the classic IPCC anthropogenic GHG context because
> for environmental simulation we wish to capture *all* the changes in
> regional/local climate whether or not an artefact of urban development or
> land use change (this is the exact *opposite* of data sets for GHG detection
> since all such datasets should remove such influences - there is a string of
> papers going back 10 years or more criticising CRU/Phil's work on these very
> grounds - urban heat/desertification influences, etc.).
>
> (b) a largely unrelated weakness in the dataset is the inhomogeneity
> introduced due to changing station coverage over time. And here you are
> right to point out that the "accuracy" depends on place, season, variable
> and scale of aggregation. Mark has some error grids I believe and
> publishing maps of # stations in interpolation range would help, but in the
> end the data set relaxes to 1961-90 in the absence of actual station
> anomalies. This is what you mean by space-optimised, but space-optimised
> inevitably implies it becomes inhomogenous over time (increasingly so as
> scales become smaller in data sparse areas).
>
> The other point worth advising people is if they really want to look at very
> local scale (certainly sub-grid-scale, but maybe even supra-grid scale in
> data poor areas) issues - whether trends or environmental modelling - then
> they would be best advised to approach GHCN (or CRU) for access to the
> underlying station data. Then of course, people need to pay attention to
> the credibility and homogeneity of individual station series, in itself not
> a trivial task and one that dozens of papers have been written about.
>
> Hope this helps - share these comments with Phil or whoever else is
> appropriate.
>
> Mike
>
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>
>
> >
> >

Dr. Tim Mitchell
> > Tyndall Centre for Climate Change Research
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> > web: [2]www.cru.uea.ac.uk/~timm/ <[3]http://www.cru.uea.ac.uk/~timm/>
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> >

>----- End of Forwarded Message
Prof. Phil Jones
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NR4 7TJ
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----- End of Forwarded Message

2769. 2003-05-01

date: Thu, 01 May 2003 11:58:28 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Fwd: Re: [Fwd: Re: Greenland series]
to: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk
Date: Thu, 01 May 2003 12:37:23 +0200
From: Bo Vinther <bo@gfy.ku.dk>
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.0.1)
Gecko/20020823
Netscape/7.0
X-Accept-Language: en-us, en
To: Phil Jones <p.jones@uea.ac.uk>, "Sigfus J. Johnsen"
<sigfus@gfy.ku.dk>
Subject: Re: [Fwd: Re: Greenland series]
Dear Phil
I have attached the winter delta-018 ice core data we presented in the article
(Ice_cores.txt). Before we used these time series for reconstructing Southern
Greenland
winter temperatures we did however detrend them (in order to eliminate the
effect of
diffusional dampning of the annual delta-018 cycle).
I have attaced the PC1 time series (PC1.txt) as well - this is the best
temperature
proxy for Southern Greenland winter.
Unfortunately we have not yet looked into the summer season (we focused on
winter
because of the strong connection to NAO) so I am not able to supply you with
summer data
at this point.
Finally Sigfús has told me, that he will mail you long time series of annual
data from
GRIP and NGRIP (just to say we haven't completely forgotten what you
originally asked
for...)
Cheers
Bo
Phil Jones wrote:
Dear Bo,

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Thanks for the pdf file. I have replied to Sigfus giving him some details of the

series I would like to use. I'll be looking at temperature proxies (and mostly avoiding the NAO), but the

paper looks very interesting.

Cheers

Phil

At 10:03 29/04/03 +0200, Møllsøe / Vinther wrote:

Dear Phil Jones

This is my final version of the very recent paper I have done in cooperation with Sigfús (and three others from our department). I would like to mention that I have been informed by Jürg Luterbacher, that he will use the time series we present in the paper

for an upcoming NAO-reconstruction... I believe this is our best ice core based

seasonal resolved Greenland temperature proxy at present.

Best Regards,

Bo Vinther

----- Original Message -----

Subject: Re: Greenland series

Date: Tue, 29 Apr 2003 08:10:04 +0200

From: "Sigfus J. Johnsen" [1]<sigfus@mail.gfy.ku.dk>

To: Phil Jones [2]<p.jones@uea.ac.uk>

CC: Møllsøe / Vinther [3]<marievej@image.dk>, Dorte Dahl-Jensen

[4]<ddj@gfy.ku.dk>,

Katrine Krogh Andersen [5]<kka@gfy.ku.dk>

References: [6]<5.1.1.6.0.20030428135159.02289c50@pop.uea.ac.uk>

Dear Phil,

how much resolution do you really need?

We have been separating the isotopic winter and summer signal to compare with the NOAA, did you see the paper by Bo Vinther in GRL?

[7]<http://www.agu.org/pubs/crossref/2003/2002GL016193.shtml>

This paper also uses old data like Milcent, Crete and Dye-3.

I am asking Bo to send you the paper as he must have the correct version.

Cheers.....Sigfus

PS. I feel extremely honored but hardly qualified to receive the Hans Oeschger medal.

> Dear Dorte and Sigfus,

> I had intended asking you about some Greenland data at the

> Nice EGS, but there just

> didn't seem to be time with all the people who were there.

> Congratulations on the Hans

> Oeschger Medal, Sigfus. It was nice to see the presentation on the

> day and good to know

> that the medal I got last year is in good hands.

> I have managed to persuade Reviews of Geophysics that they

> need a review of climate of the

> past couple of millennia, so I'm trying to locate series to include

> in figures and discuss in the

> text. I would like to include some Greenland series and the ones I

> have in mind are

>

> 1. The borehole type/paleothermometry series that appeared in

> Science a few years ago.

> 2. The latest (and presumably best) series from GRIP and N.GRIP

> that have annually-resolved

> series (from delta 0-18 ?) for the last 3000 years. I'll probably

> only plot from 2000 years ago

> but I wanted the other 1000 for context.

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>
> Also, I have some of the earlier series from Crete, Milcent and
> others. Is there a review of
> Greenland Ice Core work that I should be referencing, helping me to
> decide which series
> to include.

>
> Thanks for any series and help you can give me. I will have an
> extensive reference list and
> acknowledgements where everyone will be thanked for their help.

> Best Regards

> Phil

>
> Prof. Phil Jones
> Climatic Research Unit Telephone +44 (0) 1603 592090
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> Norwich Email [8]p.jones@uea.ac.uk
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>-----
>
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Content-Type: text/plain;
name="Ice_cores.txt"
Content-Disposition: inline;
filename="Ice_cores.txt"
X-MIME-Autoconverted: from 8bit to quoted-printable by smtp230.tiscali.dk id
h41AcOxC056545

Greenland ice core data used to derive the PC1 time series presented in the
paper:

Vinther, B. M., S. J. Johnsen, K. K. Andersen, H. B. Clausen and A. W. Hansen,
NAO signal recorded in the stable isotopes of Greenland ice cores,
Geophysical Research Letters 30(7), 1387, doi:10.1029/2002GL016193, 2003.

The ice core delta-018 data are for the winter season only. All winters are
dated

according to
the year of January. Further details on the ice cores can be found in the

paper.
*) Delta-018 data, which are corrected for diffusion by S. J. Johnsen in line
with the
methods outlined in the paper:

Johnsen, S. J., H. B. Clausen, J. Jouzel, J. Schwander, A. E.
Sveinbjörnsdóttir and J.

White,
Stable Isotope Records from Greenland Deep Ice Cores: The Climate Signal and
the Role of
Diffusion, NATO ASI Series, Vol 156, 89-107, 1999.

Year	Crete*	Dye 3	71/4B	Dye 3	79	GRIP 89-1*	GRIP
93*	Milcent*		Renland				
1970	-36.453	-28.373	-27.726	-34.312	-40.080	-30.510	-30.960
1969	-35.244	-29.087	-29.299	-38.942	-36.524	-31.702	-27.829
1968	-38.080	-32.044	-32.503	-40.922	-42.231	-33.765	-29.357

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1967	-39.848	-30.577	-29.650	-42.226	-39.525	-33.509	-30.051
1966	-37.529	-29.784	-27.822	-38.776	-40.218	-32.003	-30.651
1965	-36.692	-28.704	-29.927	-40.309	-37.143	-32.675	-28.646
1964	-35.647	-29.628	-29.059	-36.892	-39.222	-30.334	-30.190
1963	-37.194	-28.716	-27.419	-35.353	-40.027	-31.191	-27.865
1962	-36.022	-30.983	-29.782	-37.659	-42.388	-31.370	-29.990
1961	-36.019	-28.966	-28.013	-41.315	-40.177	-32.131	-28.250
1960	-37.750	-28.415	-28.623	-38.546	-39.312	-31.192	-26.238
1959	-35.501	-29.516	-29.336	-36.082	-39.955	-33.898	-27.572
1958	-35.805	-29.062	-27.976	-38.863	-40.967	-29.959	-31.519
1957	-36.898	-30.449	-29.395	-45.059	-39.701	-32.192	-29.040
1956	-36.903	-30.783	-29.553	-35.432	-38.242	-30.997	-28.452
1955	-36.060	-29.649	-29.097	-39.281	-40.239	-34.131	-27.619
1954	-36.856	-30.699	-27.899	-36.509	-39.265	-32.352	-28.090
1953	-36.445	-28.099	-28.064	-41.492	-39.298	-32.405	-27.808
1952	-39.393	-28.856	-30.089	-39.331	-39.774	-32.983	-31.244
1951	-36.726	-29.426	-29.790	-39.362	-38.018	-32.097	-28.215
1950	-38.346	-28.994	-29.350	-40.132	-43.598	-32.462	-25.845
1949	-36.349	-29.163	-29.536	-42.901	-41.439	-32.633	-32.350
1948	-36.212	-30.306	-28.730	-40.262	-41.362	-32.203	-25.982
1947	-35.332	-27.519	-26.724	-37.001	-38.192	-29.480	-26.058
1946	-36.497	-29.096	-28.091	-35.434	-38.428	-31.305	-26.294
1945	-37.066	-30.119	-30.236	-38.475	-38.539	-32.113	-28.676
1944	-39.634	-30.229	-29.240	-38.649	-42.075	-30.999	-29.057
1943	-39.474	-29.267	-29.561	-38.830	-40.142	-32.690	-30.725
1942	-34.281	-30.312	-29.111	-39.536	-41.796	-32.611	-26.623
1941	-40.600	-29.361	-30.259	-40.737	-36.225	-31.681	-26.986
1940	-35.156	-26.900	-27.630	-34.770	-38.259	-32.283	-26.420
1939	-37.659	-28.959	-30.950	-37.274	-37.861	-33.478	-27.432
1938	-36.820	-27.850	-30.711	-42.795	-39.139	-31.511	-30.039
1937	-39.507	-28.106	-29.796	-41.478	-41.055	-34.582	-25.517
1936	-38.647	-30.103	-28.675	-36.754	-40.917	-31.753	-29.204
1935	-36.781	-28.786	-28.790	-36.239	-37.155	-33.223	-28.501
1934	-37.636	-27.873	-29.661	-39.430	-38.627	-34.859	-28.008
1933	-36.417	-29.689	-30.452	-39.892	-41.523	-33.668	-26.393
1932	-38.212	-29.103	-29.067	-38.790	-37.211	-31.952	-29.691
1931	-35.676	-30.243	-29.199	-40.237	-41.530	-32.461	-28.196
1930	-39.021	-30.314	-29.550	-40.842	-39.096	-31.803	-28.216
1929	-35.466	-28.861	-29.257	-35.029	-35.848	-33.115	-29.062
1928	-40.345	-28.715	-28.289	-39.303	-39.047	-34.862	-28.489
1927	-37.381	-30.617	-31.059	-39.830	-46.357	-34.234	-29.816
1926	-35.792	-29.595	-28.706	-37.000	-37.557	-31.526	-28.386
1925	-36.352	-30.776	-30.376	-41.370	-39.797	-32.536	-28.439
1924	-36.796	-31.389	-29.165	-38.560	-41.377	-33.512	-29.508
1923	-37.221	-28.459	-28.342	-36.920	-35.314	-31.669	-28.748
1922	-37.330	-29.231	-28.892	-38.759	-41.274	-35.035	-29.126
1921	-38.546	-29.971	-30.174	-41.990	-40.160	-38.017	-29.494
1920	-41.330	-29.899	-29.553	-36.783	-40.421	-33.180	-29.456
1919	-38.574	-30.188	-32.908	-42.648	-43.127	-35.442	-30.766
1918	-38.068	-30.358	-31.509	-38.907	-44.077	-35.020	-28.962
1917	-35.780	-26.191	-26.760	-36.142	-38.980	-28.787	-25.947
1916	-36.259	-29.049	-28.698	-37.663	-39.862	-31.429	-28.050
1915	-37.743	-29.163	-30.486	-40.944	-41.813	-35.095	-30.327
1914	-40.638	-28.762	-29.820	-40.091	-40.102	-34.633	-29.072
1913	-41.048	-30.574	-29.709	-40.872	-39.730	-33.715	-28.573
1912	-34.978	-29.176	-29.418	-37.188	-38.302	-32.692	-28.464
1911	-36.537	-29.914	-29.374	-38.962	-42.510	-34.316	-29.035
1910	-39.113	-30.971	-30.444	-37.312	-39.086	-32.783	-29.260
1909	-37.939	-29.543	-28.970	-39.089	-40.909	-31.683	-25.777
1908	-33.831	-29.757	-29.807	-36.540	-39.463	-36.938	-30.043
1907	-41.469	-32.855	-31.139	-42.095	-40.984	-32.995	-31.316
1906	-42.421	-30.156	-30.494	-39.955	-45.987	-34.322	-29.617
1905	-42.609	-29.291	-29.716	-40.494	-44.803	-34.972	-28.430

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1904	-37.571	-27.271	-28.591	-35.557	-42.081	-31.431	-28.762
1903	-40.042	-31.901	-29.828	-39.316	-40.715	-34.182	-29.309
1902	-36.287	-28.981	-28.575	-40.456	-39.824	-34.450	-27.959
1901	-36.760	-30.752	-29.779	-38.928	-36.004	-30.893	-28.441
1900	-40.380	-29.876	-31.250	-39.776	-42.693	-34.675	-29.727
1899	-36.697	-30.171	-30.031	-37.066	-39.776	-35.379	-28.047
1898	-39.923	-31.316	-30.527	-37.390	-41.991	-36.223	-30.552
1897	-37.608	-31.557	-30.472	-36.733	-38.344	-31.792	-27.350
1896	-37.249	-29.129	-30.110	-40.220	-38.396	-33.125	-28.552
1895	-36.106	-27.624	-26.443	-39.973	-38.929	-31.364	-28.398
1894	-40.154	-33.471	-30.010	-38.992	-40.894	-33.546	-30.090
1893	-33.924	-29.214	-28.150	-37.717	-38.290	-29.067	-28.223
1892	-37.367	-29.186	-29.410	-37.646	-37.131	-34.925	-27.878
1891	-38.456	-30.956	-30.266	-41.735	-40.951	-33.302	-26.723
1890	-39.395	-30.215	-28.831	-48.160	-32.772	-35.895	-30.636
1889	-40.270	-27.026	-28.842	-46.247	-37.347	-33.599	-28.158
1888	-36.555	-26.600	-29.267	-37.132	-28.030	-29.731	-31.503
1887	-39.122	-28.471	-26.246	-35.854	-40.106	-34.229	-29.600
1886	-41.940	-30.300	-29.490	-39.077	-39.517	-36.221	-29.008
1885	-42.310	-29.162	-28.930	-42.245	-41.596	-35.918	-27.633
1884	-39.493	-29.291	-29.594	-37.332	-40.262	-35.333	-29.293
1883	-38.649	-29.543	-30.698	-36.695	-35.602	-35.562	-29.095
1882	-39.488	-30.351	-30.357	-42.267	-35.264	-35.951	-30.764
1881	-38.978	-27.681	-27.263	-37.235	-39.615	-31.277	-28.486
1880	-36.192	-31.171	-31.026	-37.713	-36.786	-30.842	-30.173
1879	-37.018	-29.871	-27.892	-36.446	-33.473	-32.071	-29.697
1878	-37.930	-27.386	-29.049	-37.093	-38.150	-35.530	-31.111
1877	-38.399	-30.489	-29.003	-39.476	-36.193	-35.469	-27.564
1876	-37.294	-27.780	-28.633	-35.001	-35.536	-31.701	-28.359
1875	-38.109	-28.803	-28.585	-35.716	-37.257	-31.202	-28.859
1874	-41.614	-29.152	-31.496	-39.547	-39.091	-34.678	-26.807
1873	-34.817	-29.780	-30.088	-39.182	-40.000	-31.160	-29.454
1872	-37.418	-30.686	-29.621	-37.070	-36.682	-33.934	-27.862
1871	-37.795	-28.644	-27.882	-38.626	-38.983	-33.077	-28.648
1870	-38.293	-29.597	-27.921	-40.154	-43.603	-33.440	-28.309
1869	-34.562	-29.391	-30.293	-42.453	-40.263	-33.893	-28.871
1868	-34.820	-31.148	-29.892	-38.207	-37.238	-35.400	-28.281
1867	-38.781	-28.857	-30.356	-36.105	-41.016	-32.102	-27.817
1866	-37.433	-30.729	-30.387	-41.199	-43.130	-33.125	-28.936
1865	-36.190	-29.586	-30.236	-42.958	-41.268	-34.494	-27.270
1864	-38.949	-31.881	-31.521	-36.257	-38.326	-37.446	-27.754
1863	-41.514	-32.514	-30.854	-39.350	-39.942	-37.773	-26.440
1862	-37.640	-28.286	-28.842	-38.050	-39.048	-33.768	-27.743
1861	-37.678	-29.855	-28.847	-36.829	-39.711	-32.445	-27.378
1860	-38.287	-28.486	-30.413	-35.976	-36.857	-34.320	-27.846
1859	-37.320	-30.295	-30.871	-38.715	-38.134	-34.027	-29.420
1858	-39.246	-30.943	-30.554	-42.018	-39.395	-32.773	-29.044
1857	-38.820	-30.416	-30.733	-38.699	-41.800	-36.406	-30.340
1856	-35.883	-28.000	-27.366	-38.386	-39.047	-30.725	-27.497
1855	-36.788	-29.057	-29.778	-39.946	-36.759	-34.715	-29.981
1854	-37.644	-29.129	-29.530	-35.192	-39.387	-32.763	-30.643
1853	-35.793	-27.867	-28.499	-37.952	-33.753	-34.311	-29.300
1852	-33.970	-28.477	-28.564	-35.314	-36.198	-30.941	-28.314
1851	-36.029	-28.084	-29.069	-38.621	-37.312	-31.198	-28.363
1850	-38.370	-27.490	-28.085	-32.565	-38.225	-31.512	-28.726
1849	-39.563	-28.605	-29.201	-38.589	-38.860	-33.904	-27.683
1848	-38.038	-30.466	-30.509	-37.698	-41.130	-33.230	-28.757
1847	-35.690	-26.641	-26.469	-36.579	-37.054	-31.558	-27.777
1846	-37.911	-30.442	-29.397	-41.379	-37.709	-30.672	-29.546
1845	-40.308	-28.622	-29.300	-34.655	-38.760	-35.539	-28.881
1844	-36.699	-31.443	-31.804	-40.886	-41.318	-32.366	-30.046
1843	-34.054	-27.357	-29.830	-32.122	-38.702	-32.957	-28.685
1842	-39.708	-29.841	-30.573	-35.932	-41.084	-34.128	-28.171

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1841	-34.757	-27.854	-29.818	-38.770	-40.169	-31.367	-29.342
1840	-34.848	-30.500	-29.503	-34.720	-33.867	-33.345	-29.705
1839	-37.184	-30.519	-28.771	-37.719	-38.996	-39.031	-29.840
1838	-36.622	-27.981	-28.882	-40.592	-39.996	-31.158	-27.140
1837	-36.290	-30.300	-31.476	-28.233	-41.775	-34.302	-30.270
1836	-38.132	-30.510	-29.103	-42.120	-39.570	-34.510	-29.110
1835	-36.640	-32.657	-32.364	-38.949	-43.914	-35.336	-30.450
1834	-39.126	-30.762	-30.684	-39.110	-42.908	-34.215	-28.650
1833	-36.320	-29.532	-31.761	-41.506	-40.060	-34.930	-28.837
1832	-38.017	-27.881	-28.641	-36.232	-36.591	-34.427	-29.231
1831	-34.330	-29.543	-29.115	-40.250	-36.918	-30.214	-26.464
1830	-36.665	-29.120	-27.970	-33.538	-40.524	-32.077	-28.645
1829	-37.728	-28.340	-29.043	-31.853	-37.950	-29.325	-27.496
1828	-37.391	-28.442	-27.934	-38.917	-37.520	-34.644	-25.147
1827	-38.599	-28.194	-28.897	-37.916	-40.428	-34.710	-29.050
1826	-40.909	-28.610	-30.116	-42.341	-40.931	-36.797	-26.957
1825	-37.237	-30.743	-29.031	-39.878	-42.019	-35.774	-31.774
1824	-35.103	-29.651	-29.312	-35.679	-37.627	-28.527	-28.822
1823	-38.765	-30.123	-30.231	-36.738	-38.038	-33.647	-29.407
1822	-42.038	-29.180	-29.415	-42.252	-38.200	-35.217	-27.127
1821	-37.405	-29.571	-30.343	-40.622	-34.570	-31.218	-31.817
1820	-37.027	-29.529	-29.329	-39.348	-40.750	-31.188	-29.200
1819	-41.499	-29.671	-30.713	-36.381	-35.010	-38.234	-29.170
1818	-39.561	-32.291	-32.658	-38.431	-39.601	-38.451	-28.136
1817	-39.726	-31.221	-30.031	-41.504	-42.761	-36.240	-30.790
1816	-38.039	-29.217	-28.185	-33.797	-38.871	-32.084	-25.429
1815	-36.082	-29.338	-29.018	-39.680	-40.727	-33.941	-29.015
1814	-40.777	-30.304	-30.775	-37.627	-39.652	-36.640	-29.496
1813	-39.433	-29.138	-33.651	-39.086	-40.018	-33.282	-28.469
1812	-38.107	-28.605	-28.344	-36.211	-39.341	-31.450	-27.940
1811	-40.881	-30.357	-28.867	-37.591	-40.083	-32.200	-28.591
1810	-38.175	-29.898	-29.746	-37.014	-37.249	-34.298	-28.231
1809	-37.218	-27.262	-27.621	-35.801	-36.831	-30.930	-27.977
1808	-36.117	-27.157	-28.157	-36.653	-35.180	-29.822	-28.699
1807	-38.471	-26.686	-27.657	-34.674	-36.127	-31.459	-29.309
1806	-37.743	-27.071	-27.755	-38.329	-37.911	-31.119	-29.020
1805	-35.024	-28.483	-28.679	-35.264	-39.752	-35.432	-26.387
1804	-37.836	-29.143	-28.514	-38.894	-36.434	-31.208	-29.993
1803	-39.223	-30.286	-30.233	-35.125	-34.981	-33.971	-28.717
1802	-36.333	-28.386	-27.884	-39.265	-41.477	-34.287	-30.047
1801	-38.494	-28.971	-28.932	-34.866	-36.664	-30.646	-26.737
1800	-35.698	-27.846	-27.366	-39.924	-36.584	-31.500	-27.606
1799	-35.396	-27.796	-28.922	-39.372	-38.823	-32.250	-28.746
1798	-35.774	-30.821	-30.967	-39.541	-44.918	-30.520	-28.876
1797	-38.174	-27.317	-27.721	-32.258	-37.642	-32.210	-29.319
1796	-38.125	-29.483	-29.116	-38.533	-38.918	-31.858	-28.734
1795	-38.584	-26.614	-28.744	-38.256	-39.799	-28.377	-27.291
1794	-36.714	-31.676	-31.647	-40.752	-43.069	-37.275	-28.254
1793	-39.368	-30.262	-31.814	-36.700	-37.904	-33.903	-27.981
1792	-36.656	-27.557	-27.986	-37.470	-37.929	-34.235	-25.923
1791	-41.869	-30.531	-28.911	-32.825	-44.225	-34.431	-32.083
1790	-36.540	-28.949	-27.685	-38.357	-37.610	-36.799	-27.744
1789	-33.967	-28.214	-28.578	-33.145	-40.981	-30.057	-27.727
1788	-37.761	-28.871	-29.089	-35.672	-38.357	-33.878	-29.377
1787	-34.777	-29.461	-28.103	-29.565	-37.002	-32.255	-29.016
1786	-37.115	-27.664	-28.372	-35.684	-36.530	-33.999	-27.014
1785	-36.978	-28.625	-28.254	-35.224	-34.958	-30.631	-27.017
1784	-38.126	-26.929	-27.259	-37.622	-36.873	-31.474	-31.354
1783	-39.063	-32.571	-30.287	-36.110	-40.584	-31.755	-28.206
1782	-36.719	-29.714	-30.491	-36.725	-37.461	-35.774	-27.379
1781	-38.868	-29.429	-32.790	-39.445	-39.857	-32.507	-29.131
1780	-34.714	-28.429	-28.760	-35.841	-37.389	-33.037	-28.174
1779	-38.031	-28.514	-29.576	-36.966	-41.193	-34.115	-29.041

				cg2003			
1778	-39.864	-30.307	-31.035	-40.243	-39.950	-31.784	-30.366
1777	-37.008	-29.010	-29.572	-39.690	-38.449	-34.453	-28.614
1776	-38.351	-28.076	-28.824	-36.779	-35.390	-31.654	-27.146
1775	-34.797	-30.570	-28.163	-35.847	-34.233	-30.523	-28.331
1774	-35.483	-29.800	-31.087	-39.078	-35.750	-33.457	-29.230
1773	-38.139	-29.586	-28.857	-38.922	-37.706	-30.066	-28.923
1772	-33.295	-29.301	-28.817	-39.148	-38.675	-30.186	-26.490
1771	-38.925	-31.057	-30.690	-37.401	-40.169	-31.208	-27.330
1770	-40.864	-27.971	-28.728	-33.516	-38.881	-35.226	-28.246
1769	-35.176	-28.733	-27.981	-39.152	-38.422	-34.018	-27.046
1768	-36.809	-27.538	-28.893	-38.444	-40.411	-34.008	-30.150
1767	-36.031	-28.743	-29.800	-38.752	-36.357	-34.300	-27.823
1766	-37.246	-29.343	-29.314	-35.116	-36.887	-33.683	-29.370
1765	-36.284	-30.524	-30.113	-36.679	-41.368	-29.056	-27.840
1764	-37.479	-30.495	-31.343	-38.597	-42.377	-30.424	-27.769
1763	-38.232	-27.738	-29.352	-38.639	-37.302	-33.891	-26.270
1762	-37.883	-28.195	-28.781	-36.941	-39.614	-32.241	-28.431
1761	-40.596	-34.574	-32.429	-41.201	-43.303	-34.395	-29.342
1760	-35.609	-30.166	-29.094	-34.915	-38.672	-30.640	-28.995
1759	-39.284	-28.823	-30.160	-39.599	-39.985	-34.882	-28.213
1758	-37.520	-30.280	-29.964	-34.880	-41.066	-31.648	-28.330
1757	-38.904	-30.111	-29.529	-39.029	-42.093	-34.398	-28.664
1756	-40.035	-29.925	-29.682	-43.907	-43.944	-35.076	-29.504
1755	-36.623	-28.733	-30.979	-33.538	-41.330	-30.349	-29.324
1754	-37.920	-30.457	-31.485	-39.639	-43.101	-32.502	-29.810
1753	-35.740	-30.671	-27.199	-35.279	-40.318	-32.744	-28.778
1752	-37.213	-26.814	-28.653	-38.745	-40.215	-33.403	-29.020
1751	-37.438	-29.571	-28.349	-39.112	-36.222	-34.029	-27.347
1750	-39.306	-27.371	-28.553	-42.316	-41.115	-32.598	-30.089
1749	-39.303	-28.800	-28.613	-38.516	-35.809	-32.967	-27.647
1748	-34.640	-28.729	-27.096	-38.655	-38.921	-33.678	-28.003
1747	-38.797	-29.790	-31.019	-38.457	-38.870	-30.034	-29.508
1746	-37.785	-31.652	-30.775	-36.217	-39.479	-34.369	-28.777
1745	-35.842	-28.729	-28.643	-37.351	-39.584	-31.694	-30.104
1744	-38.316	-28.397	-29.112	-36.468	-39.826	-32.778	-28.364
1743	-38.024	-28.631	-29.876	-37.467	-37.747	-36.420	-29.027
1742	-36.982	-29.310	-29.005	-34.098	-38.576	-32.688	-27.731
1741	-40.319	-27.088	-28.576	-34.895	-38.431	-31.575	-30.110
1740	-35.451	-27.217	-27.115	-37.074	-35.444	-33.557	-30.923
1739	-37.537	-29.678	-29.553	-38.722	-37.747	-30.904	-29.966
1738	-40.700	-32.686	-30.984	-39.074	-41.628	-36.157	-26.244
1737	-40.675	-32.306	-31.580	-40.498	-42.416	-35.877	-28.794
1736	-36.885	-29.629	-29.010	-37.802	-39.769	-32.952	-27.798
1735	-41.925	-29.957	-30.028	-42.738	-42.350	-36.387	-25.949
1734	-35.985	-31.329	-31.160	-38.324	-40.575	-32.738	-30.949
1733	-36.960	-28.433	-28.454	-35.623	-40.574	-34.837	-27.667
1732	-35.563	-29.219	-27.089	-34.759	-38.293	-29.608	-27.223
1731	-33.367	-28.429	-28.147	-35.147	-38.072	-31.183	-28.908
1730	-34.970	-27.343	-27.134	-36.798	-34.773	-32.544	-27.784
1729	-35.179	-27.600	-29.183	-38.002	-38.798	-31.376	-30.746
1728	-35.776	-29.061	-29.063	-36.492	-39.536	-31.180	-29.954
1727	-36.233	-29.284	-28.421	-37.234	-36.838	-30.505	-29.134
1726	-37.797	-28.471	-29.236	-37.353	-37.758	-33.645	-29.676
1725	-37.745	-29.359	-29.533	-39.600	-39.730	-32.551	-29.903
1724	-41.643	-29.110	-29.526	-39.973	-38.034	-33.196	-29.223
1723	-39.591	-30.671	-32.023	-42.064	-39.855	-35.612	-29.207
1722	-38.270	-29.548	-30.297	-41.464	-42.757	-34.595	-27.701
1721	-38.457	-29.981	-30.490	-40.045	-41.456	-34.240	-28.297
1720	-38.561	-30.227	-29.846	-36.759	-40.126	-35.714	-30.211
1719	-37.250	-29.152	-28.727	-36.304	-39.587	-35.285	-29.806
1718	-38.443	-29.900	-30.193	-29.456	-40.921	-35.949	-29.293
1717	-29.303	-30.214	-28.806	-38.204	-37.843	-32.845	-30.264
1716	-35.089	-29.837	-29.007	-31.188	-37.975	-32.350	-26.886

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1715	-38.062	-29.100	-28.963	-37.109	-37.996	-32.008	-28.783
1714	-37.590	-28.308	-29.682	-37.291	-40.021	-33.477	-27.833
1713	-37.835	-28.726	-29.303	-36.600	-41.183	-31.190	-29.499
1712	-36.723	-26.922	-28.314	-36.367	-42.343	-33.163	-29.223
1711	-37.320	-28.286	-28.484	-43.097	-36.435	-33.276	-29.324
1710	-33.538	-30.305	-27.781	-36.860	-36.141	-32.872	-28.274
1709	-37.902	-28.319	-29.651	-36.202	-38.758	-29.886	-29.614
1708	-37.062	-29.214	-29.761	-38.432	-39.420	-33.579	-30.653
1707	-38.520	-30.331	-29.603	-39.704	-39.987	-30.678	-28.059
1706	-37.175	-33.147	-30.428	-36.338	-42.103	-34.009	-28.171
1705	-41.525	-28.857	-30.671	-40.198	-41.059	-35.351	-27.897
1704	-39.460	-29.757	-28.999	-40.971	-38.965	-34.659	-28.627
1703	-41.031	-31.457	-29.044	-37.854	-42.929	-34.816	-28.716
1702	-37.469	-27.547	-28.206	-37.190	-42.156	-34.408	-30.596
1701	-38.584	-30.414	-29.773	-35.132	-44.353	-35.939	-28.812
1700	-36.638	-31.200	-31.489	-39.636	-42.298	-38.259	-28.076
1699	-37.027	-30.839	-30.950	-43.059	-40.624	-37.450	-28.207
1698	-35.977	-29.997	-29.638	-39.849	-38.501	-31.258	-27.787
1697	-37.296	-29.243	-28.839	-36.003	-35.383	-35.181	-26.487
1696	-37.400	-31.100	-29.177	-37.611	-37.700	-35.569	-28.154
1695	-36.685	-26.400	-27.696	-36.828	-39.698	-32.261	-27.287
1694	-37.032	-28.186	-29.758	-41.293	-36.118	-33.842	-28.963
1693	-38.985	-28.814	-28.904	-35.223	-34.850	-32.475	-27.729
1692	-37.518	-26.019	-25.176	-37.585	-38.762	-31.262	-28.911
1691	-38.439	-27.334	-27.761	-40.101	-41.639	-32.269	-30.150
1690	-36.855	-31.344	-30.801	-34.249	-36.075	-32.296	-28.307
1689	-41.444	-32.238	-29.723	-40.283	-40.210	-32.492	-27.665
1688	-39.211	-30.498	-28.812	-40.346	-38.101	-36.697	-29.019
1687	-36.985	-29.056	-29.203	-35.591	-38.415	-32.441	-26.411
1686	-36.417	-30.671	-31.676	-37.213	-36.932	-32.988	-28.448
1685	-37.756	-29.593	-29.941	-39.076	-38.194	-33.489	-27.401
1684	-33.451	-28.110	-27.262	-36.076	-38.398	-32.764	-28.569
1683	-36.747	-29.948	-28.907	-40.739	-41.150	-34.509	-29.781
1682	-39.039	-28.838	-31.258	-38.262	-36.862	-33.367	-28.847
1681	-38.562	-29.547	-28.814	-38.894	-37.882	-34.070	-26.501
1680	-39.862	-28.422	-27.587	-35.796	-40.602	-36.942	-26.651
1679	-39.034	-29.664	-28.726	-36.635	-39.220	-30.873	-26.453
1678	-37.369	-30.157	-28.373	-38.876	-38.059	-33.946	-27.110
1677	-39.342	-26.833	-28.564	-42.672	-38.644	-34.569	-29.020
1676	-36.457	-31.962	-29.616	-36.928	-37.636	-34.587	-26.283
1675	-34.952	-30.381	-29.181	-39.275	-37.624	-33.860	-26.716
1674	-33.545	-28.638	-27.954	-39.597	-40.389	-35.549	-26.948
1673	-38.029	-30.671	-28.899	-37.010	-40.840	-30.814	-32.152
1672	-36.870	-29.922	-29.596	-43.131	-40.087	-34.962	-26.791
1671	-40.185	-34.095	-29.646	-37.984	-43.147	-34.546	-28.890
1670	-36.428	-28.462	-28.569	-33.129	-36.764	-32.558	-27.494
1669	-38.427	-30.129	-28.427	-39.502	-39.860	-31.432	-28.702
1668	-40.851	-29.543	-29.877	-35.138	-37.382	-34.636	-27.525
1667	-34.029	-25.131	-26.824	-39.505	-39.469	-34.869	-28.067
1666	-35.451	-27.943	-27.903	-35.463	-35.863	-33.100	-29.347
1665	-34.559	-32.160	-28.476	-36.504	-36.757	-31.400	-26.177
1664	-38.425	-27.791	-27.750	-38.597	-35.999	-32.493	-29.183
1663	-34.395	-29.629	-28.611	-34.228	-38.132	-31.913	-29.226
1662	-38.829	-27.800	-28.889	-43.763	-40.968	-35.788	-29.526
1661	-37.892	-30.438	-28.794	-38.134	-39.294	-33.943	-29.320
1660	-35.537	-27.633	-28.271	-35.766	-38.044	-32.571	-28.991
1659	-39.734	-30.524	-30.585	-39.654	-40.955	-34.448	-27.457
1658	-36.879	-30.600	-28.857	-37.795	-39.574	-30.176	-27.293
1657	-32.685	-26.143	-27.595	-37.443	-37.010	-32.663	-30.184
1656	-41.977	-29.100	-27.639	-41.651	-41.773	-35.294	-26.490
1655	-36.118	-28.562	-28.489	-38.325	-40.989	-30.575	-30.089
1654	-38.555	-27.705	-29.040	-38.532	-39.771	-34.357	-29.513
1653	-39.469	-26.600	-28.803	-40.339	-42.506	-36.602	-29.863

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1652	-37.232	-27.043	-26.917	-35.873	-40.432	-33.839	-29.919
1651	-37.253	-31.267	-31.047	-36.557	-37.145	-31.787	-30.461
1650	-35.741	-27.629	-29.483	-35.902	-38.313	-31.257	-27.295
1649	-33.911	-29.375	-27.889	-37.774	-38.660	-28.356	-28.374
1648	-35.053	-31.363	-29.259	-36.269	-38.305	-33.354	-28.059
1647	-35.297	-28.169	-29.489	-38.620	-40.268	-28.659	-28.614
1646	-37.265	-28.286	-29.676	-38.247	-40.568	-33.104	-29.738
1645	-36.670	-30.992	-30.496	-39.736	-41.508	-37.534	-26.619
1644	-39.465	-28.514	-29.949	-37.304	-38.868	-33.306	-28.300
1643	-38.589	-29.814	-30.905	-38.027	-40.158	-33.676	-28.262
1642	-42.038	-34.029	-33.238	-39.525	-41.982	-31.223	-28.139
1641	-36.635	-29.142	-28.497	-40.356	-39.993	-30.962	-27.987
1640	-37.379	-29.648	-28.746	-34.664	-36.681	-33.893	-32.266
1639	-35.357	-27.414	-28.499	-35.968	-38.800	-33.030	-30.323
1638	-40.325	-28.114	-27.627	-36.244	-39.104	-37.124	-27.946
1637	-39.840	-30.536	-30.227	-39.732	-41.300	-32.585	-29.391
1636	-41.198	-31.614	-30.793	-37.872	-40.125	-34.268	-26.740
1635	-37.297	-29.671	-30.961	-36.746	-38.945	-32.826	-26.283
1634	-37.573	-30.484	-28.880	-38.104	-41.231	-34.982	-29.086
1633	-38.936	-29.229	-30.234	-38.064	-41.437	-34.036	-32.250
1632	-37.184	-32.467	-30.422	-38.231	-41.256	-36.089	-28.140
1631	-40.429	-29.208	-30.020	-31.866	-36.945	-34.934	-26.514
1630	-35.871	-32.043	-30.266	-37.963	-37.809	-34.215	-28.250
1629	-38.912	-33.331	-29.667	-37.657	-37.301	-33.159	-27.690
1628	-36.058	-30.811	-30.746	-35.679	-35.783	-32.672	-29.294
1627	-35.881	-30.542	-30.763	-41.276	-41.422	-34.543	-28.018
1626	-37.700	-30.608	-31.173	-34.113	-40.856	-31.916	-28.633
1625	-39.777	-29.790	-29.041	-39.756	-39.735	-33.647	-29.164
1624	-37.537	-27.815	-27.295	-33.811	-38.281	-31.410	-28.587
1623	-37.602	-28.552	-27.950	-38.631	-38.815	-33.880	-29.183
1622	-37.106	-30.574	-31.323	-38.826	-40.044	-33.400	-26.869
1621	-35.376	-27.657	-28.923	-29.905	-38.489	-33.360	-28.647
1620	-37.743	-30.357	-29.930	-36.482	-36.101	-30.815	-27.003
1619	-36.396	-30.433	-30.217	-36.789	-38.276	-32.036	-28.302
1618	-34.796	-30.281	-30.581	-36.532	-39.824	-30.888	-27.303
1617	-37.226	-31.735	-29.958	-37.013	-37.818	-33.122	-27.562
1616	-39.669	-28.755	-29.883	-40.142	-37.016	-31.509	-29.816
1615	-35.675	-28.280	-28.406	-40.829	-35.478	-30.039	-28.811
1614	-36.214	-30.505	-31.047	-38.618	-38.204	-32.143	-28.657
1613	-35.627	-29.024	-29.681	-38.382	-38.135	-33.171	-27.771
1612	-35.348	-31.048	-28.104	-36.856	-36.247	-33.465	-27.199
1611	-37.367	-30.905	-30.820	-38.913	-38.614	-30.989	-28.654
1610	-36.192	-32.556	-29.122	-38.284	-38.770	-31.507	-28.869
1609	-36.948	-28.986	-30.660	-37.313	-40.061	-33.392	-30.791
1608	-33.952	-27.700	-29.961	-39.500	-38.678	-29.465	-29.182
1607	-37.879	-32.214	-30.068	-39.952	-41.785	-33.334	-28.370
1606	-36.478	-29.403	-29.218	-36.099	-40.472	-30.078	-30.900
1605	-38.897	-33.431	-28.993	-39.083	-40.733	-34.340	-27.774
1604	-39.125	-29.167	-29.182	-38.839	-36.810	-31.575	-29.904
1603	-36.681	-31.229	-30.501	-38.738	-36.503	-33.802	-26.715
1602	-39.771	-29.863	-29.114	-40.135	-38.560	-33.742	-28.381
1601	-33.973	-30.757	-28.436	-38.332	-38.721	-30.969	-26.603
1600	-37.716	-29.763	-29.882	-36.004	-38.183	-33.661	-29.455
1599	-37.517	-28.160	-27.873	-40.039	-39.156	-31.637	-29.038
1598	-41.768	-31.529	-30.099	-38.734	-36.995	-34.982	-29.370
1597	-36.372	-29.757	-29.389	-35.551	-37.599	-31.308	-28.543
1596	-38.584	-31.145	-29.317	-36.769	-37.439	-31.908	-27.378
1595	-37.488	-29.352	-27.271	-38.058	-39.091	-32.191	-26.773
1594	-37.611	-29.504	-27.330	-38.855	-35.747	-32.543	-28.659
1593	-40.939	-27.796	-27.890	-36.281	-40.032	-35.264	-28.398
1592	-37.120	-30.716	-30.913	-39.419	-37.970	-33.769	-28.426
1591	-38.203	-29.940	-30.258	-34.578	-38.865	-32.811	-28.488
1590	-35.341	-31.219	-30.357	-39.412	-35.966	-32.967	-27.766

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1589	-38.218	-31.024	-29.662	-39.618	-39.850	-32.287	-29.304
1588	-37.242	-28.900	-29.116	-38.040	-37.409	-34.827	-27.142
1587	-34.028	-28.029	-28.264	-37.485	-36.341	-31.945	-29.021
1586	-35.324	-28.443	-26.497	-37.102	-40.476	-31.362	-29.294
1585	-36.298	-30.792	-30.440	-39.529	-40.363	-34.249	-30.133
1584	-40.597	-27.214	-27.909	-39.853	-39.206	-35.548	-28.119
1583	-37.583	-27.514	-28.436	-35.453	-38.594	-31.540	-29.151
1582	-35.018	-29.686	-29.049	-35.041	-35.954	-30.335	-25.013
1581	-35.632	-29.857	-31.154	-31.948	-41.049	-34.393	-28.699
1580	-37.618	-28.257	-29.213	-40.562	-37.723	-30.793	-28.183
1579	-38.960	-28.457	-28.151	-36.226	-39.452	-31.254	-28.883
1578	-35.025	-28.760	-28.079	-41.324	-40.782	-33.019	-27.423
1577	-38.252	-31.683	-29.163	-40.157	-40.437	-33.953	-28.838
1576	-39.779	-28.745	-29.109	-34.562	-40.113	-35.677	-29.078
1575	-39.521	-27.957	-28.569	-39.082	-39.232	-33.303	-28.321
1574	-38.016	-27.624	-29.838	-40.314	-37.445	-33.738	-29.853
1573	-34.222	-28.751	-29.926	-39.547	-37.202	-30.074	-29.439
1572	-37.549	-32.514	-32.737	-33.836	-40.747	-31.210	-27.759
1571	-37.051	-31.899	-31.814	-38.919	-37.700	-32.619	-29.961
1570	-33.440	-30.005	-30.456	-35.513	-41.648	-30.354	-31.164
1569	-34.682	-29.102	-27.751	-36.723	-37.331	-30.894	-28.796
1568	-40.341	-29.520	-28.643	-31.874	-38.340	-33.324	-28.397
1567	-34.401	-28.282	-28.246	-35.643	-39.628	-30.621	-27.526
1566	-39.014	-27.043	-28.258	-34.610	-41.286	-33.552	-28.353
1565	-34.782	-29.355	-29.536	-37.377	-38.711	-35.674	-28.970
1564	-37.474	-30.748	-29.820	-39.687	-41.437	-33.952	-28.470
1563	-37.660	-26.890	-29.369	-36.235	-40.429	-32.764	-30.007
1562	-33.731	-32.365	-29.947	-38.743	-38.187	-32.092	-27.692
1561	-41.444	-30.606	-28.755	-37.687	-38.296	-34.796	-27.977
1560	-38.191	-30.466	-30.463	-37.145	-39.293	-34.486	-28.867
1559	-36.160	-29.903	-30.269	-41.424	-40.379	-32.192	-29.566
1558	-38.778	-30.467	-29.279	-39.577	-41.437	-35.659	-25.735
1557	-34.945	-32.343	-32.694	-39.943	-36.780	-30.098	-28.269
1556	-37.311	-29.619	-30.696	-36.686	-35.477	-30.063	-27.919
1555	-38.063	-29.819	-30.265	-34.353	-39.051	-30.926	-30.054
1554	-38.046	-29.864	-31.336	-34.143	-38.565	-33.245	-29.231
1553	-36.111	-27.014	-27.262	-39.631	-38.909	-31.246	-25.192
1552	-33.465	-27.543	-27.164	-33.786	-40.858	-31.438	-28.553
1551	-37.440	-27.843	-28.276	-41.073	-37.924	-34.832	-31.079
1550	-36.689	-28.029	-27.827	-34.735	-35.643	-29.599	-29.215
1549	-34.386	-29.486	-28.531	-33.003	-38.923	-33.414	-28.481
1548	-37.462	-28.808	-31.728	-38.480	-40.195	-35.976	-26.401
1547	-37.293	-29.186	-28.412	-38.995	-39.924	-33.411	-25.237
1546	-37.100	-28.725	-29.195	-34.435	-39.357	-32.639	-29.224
1545	-37.582	-29.736	-29.026	-34.680	-37.739	-32.720	-27.349
1544	-37.850	-31.412	-31.244	-36.887	-39.990	-31.604	-28.657
1543	-39.967	-31.920	-32.098	-34.774	-39.445	-34.695	-27.870
1542	-37.454	-29.333	-29.627	-39.593	-39.282	-35.569	-28.014
1541	-37.978	-30.173	-29.239	-39.766	-39.240	-32.171	-31.596
1540	-37.167	-30.187	-29.247	-36.223	-42.062	-34.748	-25.594
1539	-34.827	-30.466	-30.650	-35.553	-39.531	-33.790	-26.984
1538	-35.319	-31.429	-31.366	-40.058	-37.558	-36.591	-29.671
1537	-36.006	-30.397	-30.149	-35.265	-37.693	-28.728	-26.884
1536	-41.180	-30.163	-28.756	-40.068	-42.193	-35.622	-27.722
1535	-38.623	-29.720	-29.655	-40.090	-37.222	-34.144	-30.229
1534	-35.368	-29.019	-31.700	-40.285	-41.901	-34.410	-28.614
1533	-36.748	-33.367	-31.280	-35.374	-34.483	-31.317	-29.498
1532	-34.961	-31.503	-30.449	-38.864	-40.651	-32.637	-27.968
1531	-37.548	-29.793	-29.092	-38.187	-37.281	-31.451	-28.693
1530	-35.374	-30.643	-29.597	-38.989	-36.723	-30.759	-28.194
1529	-37.885	-28.929	-29.053	-40.709	-38.986	-31.799	-27.416
1528	-35.946	-28.581	-29.233	-37.024	-37.603	-37.657	-26.929
1527	-35.097	-29.100	-28.901	-39.558	-39.026	-33.917	-28.548

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1526	-40.758	-30.373	-30.478	-33.908	-43.126	-33.848	-25.109
1525	-38.828	-29.916	-30.149	-41.759	-39.743	-34.074	-26.934
1524	-35.658	-31.047	-31.684	-34.800	-38.716	-33.429	-28.839
1523	-35.186	-32.529	-32.979	-36.059	-37.959	-29.924	-28.821
1522	-36.006	-28.700	-31.208	-32.585	-37.924	-29.885	-27.761
1521	-32.904	-31.400	-30.604	-37.409	-35.628	-38.527	-27.893
1520	-36.276	-28.786	-30.090	-34.956	-37.728	-32.530	-30.100
1519	-38.122	-29.396	-29.596	-38.189	-41.269	-33.571	-27.426
1518	-37.197	-31.514	-32.600	-41.381	-38.230	-31.939	-30.329
1517	-35.898	-29.238	-29.366	-35.447	-37.952	-32.894	-29.430
1516	-37.447	-28.400	-28.689	-37.943	-37.612	-29.754	-28.557
1515	-35.788	-30.047	-29.221	-34.650	-38.509	-33.510	-27.619
1514	-37.460	-27.629	-31.294	-35.465	-37.556	-26.699	-28.267
1513	-39.132	-28.580	-29.363	-40.946	-39.507	-32.905	-28.620
1512	-37.807	-30.111	-30.716	-36.380	-40.326	-34.105	-28.626
1511	-40.300	-27.916	-30.096	-37.864	-36.987	-33.173	-28.486
1510	-36.016	-29.208	-29.458	-33.530	-38.661	-29.405	-30.640
1509	-38.613	-30.414	-29.817	-38.457	-38.923	-32.341	-29.094
1508	-35.518	-30.376	-29.397	-39.972	-38.678	-33.445	-26.550
1507	-35.156	-29.710	-28.492	-37.094	-36.538	-31.417	-28.437
1506	-38.756	-30.160	-30.902	-39.363	-39.848	-33.114	-29.551
1505	-37.493	-30.451	-31.377	-37.472	-39.531	-32.265	-29.262
1504	-36.433	-29.668	-28.484	-35.970	-40.485	-33.337	-27.298
1503	-39.309	-29.702	-28.885	-40.181	-37.328	-33.690	-27.469
1502	-38.203	-27.811	-29.651	-35.601	-38.329	-31.542	-28.675
1501	-35.888	-27.354	-27.006	-33.704	-35.465	-30.193	-28.337
1500	-38.762	-29.871	-30.337	-42.503	-37.634	-35.012	-26.873
1499	-36.264	-29.306	-29.035	-33.255	-38.520	-33.566	-31.677
1498	-39.212	-28.571	-29.305	-36.043	-39.748	-37.552	-26.974
1497	-36.001	-29.143	-30.735	-35.526	-39.531	-35.820	-27.506
1496	-34.943	-27.043	-29.321	-39.939	-35.988	-29.488	-28.884
1495	-37.717	-30.000	-29.434	-40.627	-40.809	-36.345	-29.086
1494	-36.259	-26.973	-29.389	-36.699	-41.589	-34.590	-27.871
1493	-37.525	-28.489	-27.522	-38.933	-39.466	-33.999	-28.290
1492	-38.630	-27.569	-30.649	-35.896	-38.391	-33.030	-28.315
1491	-36.772	-28.416	-30.912	-36.716	-39.065	-30.854	-26.079
1490	-36.659	-30.459	-28.713	-37.945	-36.152	-31.877	-27.202
1489	-35.104	-28.343	-28.349	-35.014	-36.010	-30.329	-28.106
1488	-37.890	-29.286	-29.327	-38.851	-40.838	-31.923	-27.127
1487	-37.386	-29.257	-30.135	-35.917	-38.875	-31.561	-27.420
1486	-39.194	-28.416	-27.434	-35.647	-38.685	-32.488	-28.041
1485	-38.556	-26.675	-28.360	-40.312	-42.128	-36.364	-26.890
1484	-36.606	-28.864	-28.856	-39.039	-35.951	-30.921	-29.439
1483	-39.906	-30.117	-29.400	-36.781	-38.083	-34.163	-30.008
1482	-37.518	-32.029	-29.603	-40.550	-40.280	-34.968	-28.045
1481	-36.777	-29.924	-29.948	-36.722	-38.788	-31.215	-31.030
1480	-36.652	-30.763	-30.531	-35.538	-38.907	-33.673	-28.031
1479	-36.666	-27.743	-29.120	-33.445	-35.460	-31.312	-28.109
1478	-36.062	-26.700	-28.726	-38.682	-42.304	-33.846	-28.341
1477	-37.481	-30.590	-29.408	-34.648	-36.923	-34.923	-26.917
1476	-36.492	-30.927	-29.415	-35.202	-39.257	-31.353	-28.043
1475	-37.092	-30.106	-31.888	-36.590	-40.217	-30.790	-29.566
1474	-38.797	-29.343	-29.713	-37.633	-43.376	-34.448	-26.620
1473	-38.937	-29.314	-27.973	-35.843	-37.914	-31.173	-27.180
1472	-37.606	-30.600	-30.421	-36.438	-41.716	-34.094	-28.463
1471	-41.051	-33.531	-32.296	-38.489	-37.615	-33.137	-28.420
1470	-38.863	-29.886	-28.786	-32.221	-38.344	-33.465	-27.170
1469	-34.041	-27.486	-29.256	-34.886	-38.188	-30.426	-28.644
1468	-39.978	-28.810	-28.496	-34.978	-38.358	-34.039	-30.086
1467	-34.849	-30.367	-32.133	-39.376	-37.184	-34.944	-27.370
1466	-37.938	-29.820	-29.165	-37.725	-38.702	-34.200	-26.396
1465	-37.467	-31.490	-32.319	-37.783	-40.983	-32.941	-29.026
1464	-35.171	-31.138	-30.823	-38.906	-38.961	-31.615	-29.570

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1463	-38.787	-29.892	-29.891	-37.743	-36.752	-30.109	-30.089
1462	-37.049	-30.724	-31.007	-39.815	-39.377	-35.139	-30.328
1461	-40.505	-29.186	-30.241	-39.841	-41.219	-34.523	-28.454
1460	-38.742	-28.650	-29.461	-38.044	-41.639	-33.394	-29.081
1459	-37.734	-29.267	-31.962	-36.025	-37.643	-29.791	-27.645
1458	-34.453	-29.781	-30.220	-38.231	-38.251	-33.116	-25.931
1457	-34.188	-30.467	-30.114	-39.318	-40.455	-31.027	-27.140
1456	-35.584	-28.095	-28.239	-40.449	-39.684	-36.811	-26.486
1455	-40.762	-26.786	-28.455	-38.208	-36.568	-34.103	-27.354
1454	-36.491	-27.543	-29.340	-40.189	-39.085	-33.814	-26.043
1453	-38.466	-30.286	-31.293	-36.280	-37.488	-32.844	-27.340
1452	-33.047	-32.219	-31.227	-37.013	-40.192	-30.755	-27.711
1451	-38.297	-27.543	-30.112	-36.716	-39.770	-34.520	-26.928
1450	-36.289	-29.100	-30.713	-39.922	-36.320	-31.208	-28.616
1449	-36.547	-31.284	-29.136	-35.033	-39.858	-29.156	-30.009
1448	-36.780	-29.974	-31.974	-37.263	-36.739	-31.453	-27.274
1447	-38.003	-28.057	-29.332	-39.759	-37.874	-32.251	-28.146
1446	-35.652	-28.748	-29.343	-32.946	-37.854	-30.845	-28.653
1445	-36.808	-28.110	-28.900	-33.782	-40.260	-32.296	-28.811
1444	-38.226	-29.605	-29.690	-37.082	-38.518	-31.760	-28.463
1443	-36.722	-28.939	-29.854	-38.551	-37.896	-35.611	-27.052
1442	-36.929	-29.968	-28.787	-39.444	-39.324	-34.375	-27.794
1441	-41.373	-27.638	-28.018	-34.691	-39.467	-33.295	-29.087
1440	-39.020	-31.171	-30.184	-39.661	-38.418	-33.742	-26.379
1439	-35.543	-29.946	-30.544	-36.926	-37.372	-37.584	-26.523
1438	-38.399	-29.898	-31.491	-37.323	-43.360	-30.439	-29.313
1437	-33.005	-27.887	-28.469	-36.953	-40.839	-33.380	-29.010
1436	-38.906	-27.468	-28.748	-39.763	-40.554	-31.853	-27.764
1435	-33.220	-30.055	-28.033	-36.170	-41.274	-36.459	-28.886
1434	-37.036	-28.227	-29.139	-38.811	-37.607	-34.267	-31.099
1433	-39.150	-27.414	-28.997	-38.574	-41.004	-32.941	-26.220
1432	-35.449	-30.681	-30.660	-37.680	-36.867	-29.948	-28.387
1431	-35.756	-30.338	-30.796	-38.165	-41.377	-32.881	-28.669
1430	-35.642	-26.772	-26.459	-38.115	-42.105	-35.762	-29.251
1429	-40.348	-28.868	-30.663	-39.549	-40.579	-34.042	-28.579
1428	-41.354	-29.514	-29.315	-32.647	-37.097	-33.021	-28.016
1427	-36.608	-29.043	-30.023	-39.498	-38.317	-34.095	-27.843
1426	-34.759	-30.577	-30.957	-33.232	-38.021	-32.745	-28.226
1425	-39.189	-28.926	-29.481	-36.111	-40.513	-34.280	-29.111
1424	-35.635	-31.240	-31.279	-37.637	-38.096	-30.687	-26.991
1423	-37.143	-30.859	-29.671	-37.721	-38.275	-32.506	-29.331
1422	-36.742	-29.974	-29.725	-35.390	-38.479	-32.040	-28.711
1421	-36.911	-26.886	-28.399	-40.482	-40.616	-34.359	-28.300
1420	-37.844	-29.386	-30.625	-34.132	-38.694	-33.174	-28.140
1419	-34.034	-28.803	-29.736	-37.614	-37.085	-32.272	-28.070
1418	-33.228	-29.691	-29.607	-38.114	-37.606	-32.720	-24.353
1417	-38.918	-30.531	-28.463	-34.256	-36.345	-31.414	-27.434
1416	-37.601	-28.820	-28.127	-36.116	-37.955	-34.037	-26.146
1415	-38.884	-29.924	-30.344	-33.769	-33.975	-35.423	-28.629
1414	-34.166	-28.814	-28.952	-35.376	-36.765	-33.323	-26.753
1413	-38.415	-29.196	-31.225	-36.523	-37.001	-34.183	-27.326
1412	-37.365	-29.008	-28.524	-38.749	-37.371	-32.250	-26.416
1411	-36.765	-28.686	-30.131	-36.560	-38.149	-30.379	-27.510
1410	-33.265	-28.648	-30.283	-36.639	-38.039	-31.927	-27.037
1409	-35.641	-28.471	-28.547	-39.048	-36.567	-32.473	-26.898
1408	-37.142	-30.397	-30.087	-36.572	-39.405	-36.024	-27.374
1407	-34.643	-30.771	-30.975	-37.153	-37.592	-32.085	-28.839
1406	-35.462	-27.625	-29.052	-35.550	-39.577	-33.657	-27.851
1405	-39.174	-29.629	-31.188	-36.509	-35.445	-34.799	-27.493
1404	-37.386	-27.171	-28.866	-39.487	-43.589	-33.487	-28.677
1403	-35.903	-28.778	-30.989	-38.931	-39.657	-33.043	-28.454
1402	-42.401	-31.370	-30.483	-36.714	-36.303	-29.834	-27.124
1401	-36.892	-27.996	-29.384	-39.558	-38.710	-34.526	-29.160

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1400	-37.623	-30.156	-30.804	-34.254	-38.415	-32.182	-27.649
1399	-36.832	-29.429	-29.140	-40.035	-34.914	-31.342	-23.749
1398	-34.347	-31.342	-29.737	-35.511	-36.938	-34.593	-26.697
1397	-37.128	-28.434	-28.114	-38.597	-38.776	-29.486	-28.999
1396	-33.900	-29.819	-29.421	-38.938	-34.973	-31.486	-27.354
1395	-35.100	-29.205	-28.165	-32.692	-33.009	-31.590	-28.449
1394	-38.074	-26.197	-26.331	-36.671	-36.215	-36.071	-27.573
1393	-38.270	-29.600	-29.856	-35.764	-37.210	-34.334	-27.941
1392	-38.934	-29.586	-28.304	-38.054	-40.441	-33.262	-27.500
1391	-38.647	-27.648	-29.672	-36.908	-39.392	-29.465	-26.706
1390	-36.485	-29.096	-28.735	-36.342	-39.353	-32.853	-22.761
1389	-35.295	-28.508	-28.615	-36.028	-37.431	-30.476	-27.801
1388	-36.628	-28.657	-28.350	-36.198	-38.283	-31.395	-29.074
1387	-37.005	-31.023	-27.899	-37.987	-36.315	-35.062	-28.117
1386	-37.449	-28.764	-29.060	-39.501	-38.689	-31.275	-28.914
1385	-38.531	-27.000	-28.671	-36.349	-40.582	-32.436	-28.489
1384	-34.092	-27.986	-30.140	-37.787	-35.561	-33.850	-28.054
1383	-36.582	-30.562	-28.718	-36.494	-38.437	-32.982	-26.783
1382	-38.654	-27.905	-28.768	-33.943	-39.395	-29.722	-30.131
1381	-40.017	-28.057	-29.332	-37.427	-40.102	-31.602	-28.001
1380	-41.243	-31.300	-30.854	-37.590	-39.699	-35.808	-27.500
1379	-36.722	-28.800	-27.586	-42.818	-41.009	-32.020	-27.951
1378	-40.462	-28.300	-29.373	-37.596	-39.697	-32.006	-27.524
1377	-36.330	-29.629	-29.691	-38.706	-37.418	-34.713	-28.291
1376	-39.827	-29.500	-29.058	-39.251	-38.422	-34.832	-27.393
1375	-37.139	-29.814	-32.677	-35.675	-38.574	-37.048	-26.560
1374	-36.911	-29.384	-30.203	-39.780	-38.814	-35.964	-31.233
1373	-40.046	-29.529	-28.299	-37.262	-44.541	-33.179	-28.960
1372	-35.710	-30.405	-31.059	-35.148	-39.841	-32.447	-29.223
1371	-39.541	-29.000	-27.443	-35.588	-40.237	-32.208	-29.884
1370	-36.715	-29.289	-29.310	-38.080	-36.865	-34.245	-26.991
1369	-35.570	-28.504	-29.479	-35.005	-37.337	-31.319	-28.450
1368	-38.946	-28.463	-27.649	-39.031	-37.901	-34.712	-30.029
1367	-37.540	-32.586	-33.081	-35.732	-38.192	-34.327	-28.597
1366	-36.869	-29.943	-28.640	-38.656	-38.829	-32.130	-29.226
1365	-40.020	-29.157	-28.934	-37.245	-38.557	-33.093	-28.436
1364	-40.501	-28.486	-29.579	-40.236	-38.593	-34.479	-31.850
1363	-35.984	-27.900	-27.486	-36.657	-34.670	-34.855	-29.346
1362	-34.766	-31.429	-28.738	-38.893	-41.853	-30.560	-28.340
1361	-37.460	-30.549	-32.547	-34.991	-37.247	-30.737	-26.529
1360	-35.726	-29.291	-29.168	-36.288	-38.185	-34.116	-26.186
1359	-37.590	-27.429	-28.680	-38.180	-38.663	-32.307	-27.770
1358	-34.984	-29.030	-28.556	-36.451	-40.875	-30.895	-27.544
1357	-38.486	-28.246	-29.035	-36.967	-38.975	-37.490	-29.260
1356	-40.724	-25.437	-25.638	-40.590	-41.505	-34.399	-30.213
1355	-33.958	-29.610	-30.367	-32.536	-36.026	-30.567	-28.093
1354	-37.051	-27.635	-29.182	-36.446	-37.233	-32.451	-28.899
1353	-39.683	-28.427	-28.577	-39.959	-40.217	-34.248	-28.331
1352	-40.385	-31.283	-31.190	-42.209	-37.878	-34.299	-27.476
1351	-35.748	-30.598	-30.609	-35.566	-40.202	-35.591	-27.738
1350	-36.229	-27.324	-27.520	-37.721	-39.111	-32.449	-27.784
1349	-36.566	-28.400	-29.363	-41.116	-36.928	-33.378	-29.626
1348	-34.157	-29.471	-28.558	-32.550	-37.069	-32.740	-29.460
1347	-36.000	-28.044	-27.813	-37.650	-38.123	-31.935	-31.741
1346	-35.156	-29.644	-30.720	-37.598	-39.292	-33.636	-27.379
1345	-36.050	-27.800	-28.301	-38.377	-40.847	-34.169	-27.081
1344	-37.726	-28.903	-28.969	-39.059	-38.255	-32.447	-29.844
1343	-37.128	-29.819	-29.591	-35.610	-38.058	-33.308	-28.586
1342	-36.284	-29.258	-30.765	-34.171	-37.224	-35.519	-27.670
1341	-39.114	-26.217	-26.787	-39.799	-41.196	-32.269	-28.407
1340	-33.132	-30.755	-31.041	-37.850	-39.071	-33.432	-29.431
1339	-35.000	-28.666	-30.071	-35.900	-36.968	-29.741	-27.284
1338	-35.539	-28.006	-29.746	-36.946	-38.593	-35.714	-27.553

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1337	-40.103	-29.021	-30.598	-40.868	-42.264	-34.947	-28.500
1336	-38.860	-30.843	-29.113	-34.432	-39.750	-32.690	-27.496
1335	-35.509	-28.610	-26.767	-38.186	-39.197	-32.124	-28.807
1334	-42.333	-30.580	-30.130	-37.626	-38.058	-30.997	-27.803
1333	-36.517	-27.166	-28.252	-36.385	-37.712	-30.757	-28.494
1332	-39.924	-29.323	-29.181	-38.519	-38.994	-34.961	-28.886
1331	-36.954	-30.727	-30.981	-36.480	-40.110	-32.762	-26.640
1330	-36.460	-26.979	-30.503	-37.303	-39.654	-33.015	-26.509
1329	-35.287	-28.525	-29.257	-36.316	-37.970	-33.446	-26.647
1328	-40.456	-29.196	-27.724	-39.329	-42.587	-32.948	-26.739
1327	-37.184	-28.201	-27.740	-38.025	-41.258	-31.811	-28.712
1326	-35.001	-27.749	-26.336	-35.047	-35.146	-32.829	-27.620
1325	-33.580	-30.100	-28.936	-37.092	-36.543	-32.665	-27.601
1324	-35.329	-29.495	-27.678	-38.603	-39.320	-32.431	-30.377
1323	-36.828	-27.810	-28.917	-40.334	-40.130	-36.070	-27.589
1322	-41.051	-30.871	-31.245	-39.483	-39.732	-35.193	-27.874
1321	-37.427	-28.941	-29.587	-35.966	-39.601	-32.960	-27.981
1320	-39.259	-31.029	-30.291	-38.483	-36.118	-31.348	-28.306
1319	-35.037	-30.600	-33.057	-37.887	-39.106	-34.897	-28.413
1318	-35.424	-31.023	-31.097	-39.169	-37.740	-31.508	-28.730
1317	-42.428	-29.381	-29.717	-38.627	-37.458	-33.195	-26.237
1316	-36.489	-27.700	-29.334	-37.446	-39.363	-32.048	-26.819
1315	-38.189	-29.857	-28.640	-33.361	-37.576	-34.056	-25.994
1314	-39.599	-31.905	-31.400	-39.587	-37.005	-35.409	-26.137
1313	-39.179	-31.166	-29.088	-36.300	-43.063	-33.801	-30.469
1312	-33.554	-27.882	-28.645	-37.195	-39.024	-35.391	-29.104
1311	-36.147	-26.978	-28.264	-37.950	-38.739	-35.004	-27.936
1310	-36.281	-29.614	-29.777	-41.314	-38.342	-35.536	-27.551
1309	-37.147	-30.706	-29.132	-38.645	-37.210	-31.446	-27.090
1308	-35.066	-26.162	-26.335	-35.136	-37.320	-30.886	-27.540
1307	-34.885	-31.143	-30.369	-37.223	-39.734	-30.355	-26.047
1306	-34.764	-27.914	-30.288	-35.744	-37.198	-31.625	-28.443
1305	-39.435	-27.271	-28.226	-35.168	-39.660	-32.503	-27.063
1304	-37.185	-29.168	-31.591	-35.881	-36.384	-33.519	-27.327
1303	-36.394	-28.065	-28.051	-35.194	-37.664	-33.027	-30.091
1302	-38.496	-29.862	-28.338	-39.659	-37.954	-34.472	-28.116
1301	-34.737	-28.043	-27.659	-34.438	-38.046	-36.243	-28.634
1300	-37.656	-28.639	-28.262	-38.924	-38.822	-31.413	-28.669
1299	-36.271	-28.402	-28.689	-39.053	-36.339	-36.826	-27.904
1298	-41.876	-28.671	-28.786	-35.131	-41.864	-34.850	-28.964
1297	-32.950	-28.240	-29.661	-39.979	-40.474	-33.252	-28.976
1296	-39.408	-30.576	-28.830	-35.631	-40.425	-30.699	-27.946
1295	-34.095	-29.567	-30.803	-36.424	-36.785	-31.215	-29.177
1294	-38.129	-28.351	-28.277	-34.667	-38.621	-35.178	-30.814
1293	-41.345	-29.343	-29.534	-38.406	-42.971	-32.884	-27.949
1292	-38.437	-29.129	-27.709	-37.262	-40.980	-32.350	-29.349
1291	-37.697	-30.329	-30.016	-33.320	-40.396	-34.923	-27.511
1290	-37.566	-27.535	-27.988	-38.601	-38.816	-30.796	-26.713
1289	-38.713	-28.290	-28.699	-43.223	-39.113	-32.977	-29.876
1288	-42.108	-30.414	-31.157	-38.840	-37.106	-32.584	-29.101
1287	-36.632	-30.602	-32.170	-38.633	-36.941	-30.095	-27.181
1286	-34.257	-27.365	-29.267	-38.037	-35.971	-31.583	-27.341
1285	-38.858	-29.853	-29.368	-35.660	-36.898	-33.583	-29.440
1284	-41.071	-29.705	-30.306	-42.777	-40.474	-31.144	-28.476
1283	-35.543	-28.500	-29.463	-36.191	-36.330	-32.180	-29.565
1282	-35.552	-30.107	-31.613	-36.438	-40.429	-31.246	-29.476
1281	-36.708	-28.057	-28.160	-34.055	-35.853	-33.994	-28.019
1280	-38.304	-27.729	-29.433	-29.906	-37.513	-28.738	-26.616
1279	-37.371	-28.469	-28.480	-39.308	-39.359	-36.364	-27.311
1278	-37.856	-29.236	-29.236	-38.851	-37.091	-33.272	-29.567
1277	-40.679	-30.617	-30.213	-43.129	-39.913	-33.668	-28.476
1276	-35.727	-30.700	-30.297	-33.410	-37.735	-29.966	-25.711
1275	-36.316	-28.735	-27.551	-35.951	-37.243	-33.849	-26.113

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1274	-38.605	-28.400	-28.713	-37.995	-35.859	-32.954	-26.913
1273	-36.633	-28.100	-29.223	-35.916	-37.145	-33.624	-28.573
1272	-35.591	-27.540	-28.581	-33.960	-35.663	-35.545	-27.746
1271	-38.007	-29.429	-28.971	-37.611	-39.531	-35.458	-26.639
1270	-37.346	-29.100	-29.726	-37.390	-37.821	-31.812	-28.571
1269	-31.305	-25.971	-28.145	-39.624	-38.041	-34.450	-25.883
1268	-38.851	-28.757	-30.410	-34.522	-38.053	-36.924	-28.337
1267	-37.098	-28.871	-30.869	-37.703	-39.435	-33.325	-28.772
1266	-38.327	-28.038	-29.045	-35.183	-38.546	-34.738	-27.685
1265	-32.507	-31.329	-30.004	-41.733	-39.657	-35.031	-29.757
1264	-36.876	-29.633	-30.302	-41.722	-40.107	-33.126	-29.873
1263	-41.053	-29.041	-27.896	-36.716	-41.164	-34.613	-31.179
1262	-38.058	-30.986	-30.469	-38.003	-37.951	-34.124	-29.366
1261	-36.784	-29.055	-31.040	-37.986	-38.421	-33.965	-29.048
1260	-36.076	-30.800	-30.710	-36.039	-38.110	-36.373	-27.894
1259	-35.993	-29.114	-30.630	-38.325	-39.167	-33.989	-27.327
1258	-40.011	-28.371	-27.213	-36.690	-37.939	-32.219	-27.650
1257	-34.467	-27.357	-27.823	-40.247	-37.648	-33.600	-28.370
1256	-36.951	-28.686	-27.596	-34.247	-39.092	-33.774	-26.703
1255	-38.224	-29.729	-28.651	-34.256	-35.158	-31.841	-28.290
1254	-33.709	-26.657	-27.240	-33.234	-34.751	-34.107	-28.860
1253	-31.515	-30.108	-30.541	-37.571	-38.432	-33.494	-29.979
1252	-36.170	-30.229	-30.271	-36.573	-38.602	-31.928	-27.099
1251	-40.681	-29.657	-31.087	-34.760	-39.833	-32.211	-28.366
1250	-34.103	-29.294	-29.012	-35.859	-35.338	-27.040	-29.383
1249	-35.000	-29.486	-28.453	-39.009	-39.773	-33.128	-26.349
1248	-30.858	-30.971	-31.478	-38.806	-37.157	-34.417	-29.483
1247	-37.993	-26.929	-27.976	-38.655	-40.465	-33.862	-29.503
1246	-39.045	-29.357	-28.491	-37.773	-35.240	-32.067	-27.459
1245	-37.866	-29.031	-30.567	-33.183	-40.575	-30.873	-26.837

Southern Greenland winter temperature index 1245 AD - 1970 AD.

Please use the following reference:

Vinther, B. M., S. J. Johnsen, K. K. Andersen, H. B. Clausen and A. W. Hansen, NAO signal recorded in the stable isotopes of Greenland ice cores, *Geophysical Research Letters* 30(7), 1387, doi:10.1029/2002GL016193, 2003.

The following time series is referred to as the PC1 time series in the article. It is based on winter delta-O18 data from 7 Greenland ice cores.

All winters are dated according to the year of January. The time series has zero mean and standard deviation one.

1245	0.572
1246	0.450
1247	-0.284
1248	-0.380
1249	0.025
1250	2.373
1251	-0.485
1252	0.179
1253	0.031
1254	1.712
1255	0.910
1256	0.523
1257	0.389
1258	0.454
1259	-0.501
1260	-1.039
1261	-0.621
1262	-0.995
1263	-1.112
1264	-1.092
1265	-1.219
1266	-0.119

1267	-0.504
1268	-0.978
1269	0.910
1270	0.249
1271	-0.817
1272	0.647
1273	0.364
1274	0.343
1275	0.647
1276	1.250
1277	-1.880
1278	-0.279
1279	-0.965
1280	2.258
1281	0.854
1282	-0.094
1283	0.777
1284	-1.165
1285	-0.178
1286	1.259
1287	0.210
1288	-1.067
1289	-0.852
1290	0.872
1291	-0.577
1292	-0.105
1293	-1.204
1294	-0.261
1295	0.770
1296	0.153
1297	-0.085
1298	-1.085
1299	-0.604
1300	0.329
1301	0.257
1302	-0.716
1303	0.688
1304	-0.023
1305	0.510
1306	1.013
1307	0.471
1308	2.186
1309	0.250
1310	-1.118
1311	0.025
1312	0.018
1313	-1.300
1314	-1.643
1315	0.326
1316	0.524
1317	-0.689
1318	-0.142
1319	-1.205
1320	-0.130
1321	0.019
1322	-1.993
1323	-0.991
1324	0.191
1325	0.737
1326	1.720
1327	0.266
1328	-0.768
1329	0.512

1330	0.218
1331	-0.407
1332	-1.077
1333	1.343
1334	-0.402
1335	0.704
1336	-0.101
1337	-1.920
1338	-0.301
1339	1.491
1340	-0.343
1341	0.203
1342	-0.205
1343	0.032
1344	-0.091
1345	-0.110
1346	-0.276
1347	0.636
1348	1.190
1349	-0.245
1350	0.759
1351	-0.870
1352	-1.796
1353	-0.898
1354	0.660
1355	1.686
1356	-0.471
1357	-1.182
1358	0.838
1359	0.438
1360	0.207
1361	0.388
1362	0.085
1363	0.654
1364	-1.274
1365	-0.283
1366	0.016
1367	-1.285
1368	-0.493
1369	1.146
1370	-0.067
1371	0.174
1372	-0.077
1373	-1.016
1374	-1.393
1375	-1.344
1376	-0.936
1377	-0.474
1378	-0.102
1379	-0.265
1380	-1.884
1381	0.044
1382	1.179
1383	0.192
1384	0.561
1385	0.323
1386	0.236
1387	-0.345
1388	0.870
1389	1.453
1390	0.631
1391	1.008
1392	-0.411

cg2003

1393	-0.219
1394	0.486
1395	2.046
1396	1.004
1397	1.040
1398	0.074
1399	0.898
1400	0.238
1401	-0.482
1402	0.122
1403	-0.350
1404	-0.606
1405	-0.503
1406	0.489
1407	0.178
1408	-0.998
1409	0.723
1410	0.953
1411	0.917
1412	0.464
1413	-0.363
1414	0.993
1415	-0.101
1416	0.332
1417	0.866
1418	0.742
1419	0.772
1420	0.069
1421	-0.382
1422	0.385
1423	-0.209
1424	0.307
1425	-0.639
1426	0.499
1427	-0.447
1428	0.258
1429	-1.280
1430	-0.215
1431	-0.614
1432	0.767
1433	-0.141
1434	-0.305
1435	-0.511
1436	0.017
1437	0.536
1438	-0.514
1439	-0.932
1440	-0.939
1441	0.097
1442	-0.610
1443	-0.585
1444	0.165
1445	0.725
1446	1.469
1447	0.144
1448	0.269
1449	0.855
1450	0.377
1451	-0.303
1452	0.252
1453	-0.208
1454	-0.059
1455	0.072

1456	-0.768
1457	0.227
1458	0.219
1459	0.722
1460	-0.651
1461	-1.488
1462	-1.488
1463	0.492
1464	-0.179
1465	-1.164
1466	-0.336
1467	-0.789
1468	-0.132
1469	1.728
1470	0.451
1471	-1.602
1472	-0.924
1473	0.872
1474	-1.074
1475	-0.067
1476	0.500
1477	-0.069
1478	-0.118
1479	1.706
1480	-0.265
1481	0.265
1482	-1.524
1483	-0.674
1484	0.867
1485	-1.087
1486	0.614
1487	0.495
1488	-0.105
1489	1.927
1490	0.635
1491	0.730
1492	0.160
1493	-0.101
1494	-0.117
1495	-1.686
1496	1.582
1497	-0.546
1498	-1.118
1499	0.383
1500	-1.291
1501	2.454
1502	0.743
1503	-0.478
1504	0.167
1505	-0.453
1506	-0.987
1507	1.079
1508	-0.222
1509	-0.387
1510	1.460
1511	-0.110
1512	-0.728
1513	-0.571
1514	1.997
1515	0.407
1516	1.177
1517	0.517
1518	-1.066

1519	-0.589
1520	0.569
1521	-0.974
1522	1.532
1523	0.247
1524	-0.217
1525	-1.184
1526	-0.829
1527	-0.117
1528	-0.578
1529	0.037
1530	0.682
1531	0.455
1532	-0.466
1533	0.301
1534	-1.139
1535	-0.764
1536	-1.819
1537	1.501
1538	-1.539
1539	-0.017
1540	-0.577
1541	-0.467
1542	-0.970
1543	-1.276
1544	-0.376
1545	0.603
1546	0.533
1547	-0.005
1548	-1.196
1549	0.917
1550	2.109
1551	-0.529
1552	1.732
1553	1.274
1554	-0.120
1555	0.519
1556	1.080
1557	0.039
1558	-1.380
1559	-0.570
1560	-0.837
1561	-0.950
1562	0.115
1563	0.313
1564	-1.092
1565	-0.373
1566	0.296
1567	1.543
1568	0.425
1569	1.455
1570	0.652
1571	-0.740
1572	-0.296
1573	1.073
1574	-0.286
1575	-0.177
1576	-0.640
1577	-1.180
1578	-0.009
1579	0.693
1580	0.568
1581	-0.079

1582	1.876
1583	1.070
1584	-0.735
1585	-1.074
1586	1.116
1587	1.377
1588	-0.104
1589	-0.605
1590	0.013
1591	0.116
1592	-0.698
1593	-0.473
1594	0.640
1595	0.592
1596	0.242
1597	0.927
1598	-1.426
1599	0.439
1600	-0.102
1601	0.950
1602	-0.738
1603	-0.297
1604	0.272
1605	-1.427
1606	0.762
1607	-1.289
1608	1.292
1609	-0.401
1610	0.016
1611	-0.042
1612	0.592
1613	0.259
1614	-0.087
1615	1.344
1616	0.010
1617	-0.174
1618	0.661
1619	0.298
1620	0.890
1621	1.504
1622	-0.703
1623	-0.007
1624	1.542
1625	-0.820
1626	-0.011
1627	-1.286
1628	0.409
1629	-0.607
1630	-0.538
1631	0.161
1632	-1.711
1633	-1.108
1634	-0.954
1635	0.051
1636	-1.377
1637	-1.049
1638	-0.712
1639	0.838
1640	0.271
1641	0.423
1642	-1.964
1643	-0.756
1644	-0.169

1645	-1.908
1646	-0.179
1647	1.317
1648	0.219
1649	1.936
1650	1.325
1651	0.048
1652	0.562
1653	-1.432
1654	-0.385
1655	0.681
1656	-1.463
1657	1.658
1658	0.723
1659	-1.385
1660	1.150
1661	-0.469
1662	-1.547
1663	1.250
1664	0.777
1665	1.069
1666	1.314
1667	0.803
1668	-0.326
1669	0.035
1670	1.441
1671	-2.025
1672	-1.185
1673	0.050
1674	-0.094
1675	0.091
1676	-0.288
1677	-0.636
1678	-0.086
1679	0.702
1680	-0.667
1681	-0.175
1682	-0.273
1683	-0.968
1684	1.399
1685	-0.227
1686	-0.070
1687	0.795
1688	-1.436
1689	-1.198
1690	0.560
1691	0.003
1692	1.718
1693	1.026
1694	-0.088
1695	1.183
1696	-0.643
1697	0.394
1698	0.403
1699	-2.332
1700	-2.417
1701	-1.406
1702	-0.270
1703	-1.609
1704	-0.995
1705	-1.657
1706	-1.104
1707	0.028

1708	-0.336
1709	1.091
1710	1.145
1711	-0.056
1712	0.393
1713	0.423
1714	0.029
1715	0.547
1716	1.527
1717	1.028
1718	-0.300
1719	-0.329
1720	-1.089
1721	-1.193
1722	-1.452
1723	-2.141
1724	-0.652
1725	-0.257
1726	0.163
1727	1.323
1728	0.851
1729	0.943
1730	1.846
1731	1.791
1732	2.102
1733	0.085
1734	-0.670
1735	-2.363
1736	0.133
1737	-2.613
1738	-2.227
1739	0.490
1740	1.245
1741	0.916
1742	0.839
1743	-0.668
1744	0.309
1745	0.723
1746	-0.769
1747	0.245
1748	0.656
1749	0.442
1750	-0.475
1751	0.210
1752	0.247
1753	0.635
1754	-1.132
1755	0.912
1756	-2.277
1757	-1.108
1758	0.318
1759	-0.960
1760	1.208
1761	-2.921
1762	0.561
1763	0.285
1764	-0.178
1765	0.858
1766	0.491
1767	0.220
1768	-0.060
1769	0.438
1770	-0.008

1771	-0.098
1772	1.440
1773	0.829
1774	0.083
1775	1.893
1776	1.287
1777	-0.351
1778	-0.797
1779	-0.336
1780	1.132
1781	-0.816
1782	-0.358
1783	-0.373
1784	1.252
1785	1.973
1786	0.975
1787	2.021
1788	0.260
1789	1.981
1790	-0.268
1791	-1.142
1792	0.737
1793	-0.568
1794	-2.443
1795	1.646
1796	0.272
1797	1.610
1798	-0.454
1799	0.708
1800	1.379
1801	1.492
1802	-0.246
1803	0.179
1804	0.806
1805	0.394
1806	1.240
1807	1.778
1808	2.246
1809	1.845
1810	-0.105
1811	-0.254
1812	0.930
1813	-1.086
1814	-1.690
1815	-0.342
1816	1.210
1817	-2.406
1818	-2.626
1819	-1.347
1820	0.206
1821	0.412
1822	-1.309
1823	-0.246
1824	1.952
1825	-1.524
1826	-1.928
1827	-0.389
1828	0.360
1829	2.188
1830	1.067
1831	1.345
1832	0.500
1833	-1.207

1834	-1.425
1835	-2.195
1836	-1.080
1837	0.187
1838	0.660
1839	-1.529
1840	1.096
1841	0.766
1842	-0.672
1843	1.534
1844	-1.042
1845	-0.324
1846	0.196
1847	2.065
1848	-0.578
1849	-0.201
1850	1.650
1851	1.168
1852	2.035
1853	0.967
1854	0.353
1855	-0.290
1856	1.468
1857	-1.815
1858	-1.010
1859	-0.524
1860	0.225
1861	0.398
1862	0.220
1863	-2.450
1864	-1.577
1865	-1.045
1866	-1.161
1867	0.180
1868	-0.312
1869	-0.588
1870	-0.666
1871	0.364
1872	0.103
1873	0.451
1874	-1.168
1875	1.221
1876	1.670
1877	-0.491
1878	-0.137
1879	1.387
1880	0.523
1881	1.045
1882	-1.360
1883	-0.350
1884	-0.826
1885	-1.838
1886	-1.594
1887	0.342
1888	2.813
1889	-0.642
1890	-1.466
1891	-0.993
1892	0.014
1893	1.969
1894	-1.489
1895	1.275
1896	-0.109

1897	0.253
1898	-1.860
1899	-0.531
1900	-1.719
1901	0.776
1902	-0.229
1903	-1.325
1904	0.934
1905	-1.985
1906	-2.205
1907	-2.037
1908	-0.506
1909	0.312
1910	-0.357
1911	-0.703
1912	0.771
1913	-1.112
1914	-1.013
1915	-1.339
1916	0.861
1917	2.781
1918	-1.649
1919	-2.461
1920	-0.518
1921	-2.140
1922	-0.624
1923	1.430
1924	-0.562
1925	-0.431
1926	1.147
1927	-1.765
1928	-0.519
1929	1.207
1930	-0.193
1931	-0.113
1932	0.516
1933	-0.468
1934	-0.207
1935	0.844
1936	0.251
1937	-0.798
1938	0.004
1939	0.163
1940	1.941
1941	0.130
1942	0.178
1943	-0.345
1944	-0.134
1945	0.217
1946	1.525
1947	2.457
1948	0.122
1949	-0.677
1950	-0.336
1951	0.432
1952	-0.472
1953	0.485
1954	0.651
1955	-0.157
1956	0.952
1957	-0.648
1958	0.943
1959	0.378

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1960	0.960
1961	0.372
1962	0.035
1963	1.368
1964	1.139
1965	0.353
1966	0.278
1967	-1.098
1968	-1.759
1969	1.111
1970	1.569

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UK

2781. 2003-05-01

cc: e.l.jones@uea.ac.uk
date: Thu May 1 12:35:17 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: Growing the Future 11 Workshop on Non-Food Crops
to: "dee rawsthorne (JIC)" <dee.rawsthorne@bbsrc.ac.uk>

Dee,
Sounds interesting. I am in Swindon at an EPSRC meeting on 21 November, but free on 20 November. Could happily give a state-of-the-science talk about global climate change (with some side references to European agriculture, although I am not an expert on this) and also maybe 1 or 2 comments on the role of non-food crops in climate mitigation policy. If this fits, count me in - but I do not write papers for these events, i.e., I'll give a power-point based talk only and join in any discussion on the day. Otherwise my work load would be too great. What audience are you going for and how many? By the way, we are planning a day seminar in September here at UEA on biofuels and climate policy, using the European Climate Forum as a platform. Does this have any interest over at JIC?

Mike
At 16:12 29/04/2003 +0100, you wrote:

Dear Professor Hulme,
The John Innes Centre and Rothamsted Research are jointly organising a series of workshops called Growing the Future. The workshops were conceived to produce an authoritative, scientific analysis of a set of closely focussed topics that relate to the complex global problems. The first was on insect pests and their control and the second is on non-food crops which is to be held at the John Innes Centre on November 20/21, 2003.

The is an initial enquiry to find out if you would be available and willing to participate in this second workshop by giving an overview of global climatic

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change and

its potential effects on European agriculture?

The provisional plan and speakers for the meeting at the moment is as follows:

Introduction and scene setting

Global Climate Change

Economic Consequences

Current status of the Science, Overview

Industry perspective

Industrial applications of the science

Case Histories

Biosafety/Segregation

Landscape Preservation

Potential of New Science

Summing up

If possible, I would also like to ask for some advice, as we are not so familiar with

the economic aspects of these issues and would like to invite a speaker to cover this

area. Any suggestions you have would be much appreciated.

If there is any other information you would like before reaching a decision then please

do not hesitate to contact me.

Best wishes

Dee

Dee Rawsthorne

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Colney

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E-mail dee.rawsthorne@bbsrc.ac.uk

Fax: 44 (0)1603 450025

3864. 2003-05-01

date: Thu, 01 May 2003 10:27:12 +0100

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Re: Mike Mann's review

to: Phil Jones <p.jones@uea.ac.uk>, k.briffa@uea.ac.uk

<x-flowed>

Phil & Keith,

here are my suggestions for how to deal with Mike's comments - though we might want to wait for other review before doing anything.

(1) Valid point but easy to deal with by adding something like... "without more widespread data we don't know how general these changes are, but if they are applicable to other parts of the world then there are two principal implications..."

(2) Don't get into discussion right at the beginning on which reconstructions are results are applicable to; instead change meaning of our first sentence by "...Briffa and Osborn (2002) noted that IF reconstructions of annual temperature trends (for parts of the last millennium) ARE based on predictors that are strongly influenced by summer conditions, THEN THEY tacitly...." Then expand the discussion section (see point 8 below) to mention some particular reconstructions and perhaps that the potential bias is less for those that use more non-summer-sensitive proxies.

(3) Worth doing the composites as they're easy to do - (if the composite does look like the NH-mean, then we can call it yet another NH reconstruction!).

(4) Some discussion of forcing vs. internally generated influences on seasonal differences could go in the discussion section (as I suggested prior to submission!)

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(5) Do you have these N. American series Phil? Are they long enough to tell us more than is already in the NH network?

(6) Just cite Shindell et al. as an example where we say that seasonal differences are a good diagnostic for testing model performance, for model runs of the last millennium.

(7) Not sure whether we want to detrend or not, since the trend itself is part of the signal we're after reconstructing. But certainly we could discuss that fig. 3 is based on 20th century variations and the seasonal-annual relationships may well differ in other centuries which don't have such strong anthropogenic forcing. This is, of course, the whole point of the paper, that seasonal-annual relationships may not be stationary!

(8) In the discussion section I think we can expand things to make the caveat that not all proxies are summer sensitive and therefore some reconstructions may not be biased by seasonality changes so much (e.g., Mann et al., 1998). But then go on to note that this probably doesn't mitigate the bias early on when the corals and the TexMex tree-rings aren't available (e.g., Mann et al., 1999), and therefore are results are valid to that - and that early bit is the crucial bit when claiming that 20th century temperatures are warmest in the millennium!

(9) This will all have been covered by the above.

What do you think?

Cheers

Tim

Dr Timothy J Osborn

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</x-flowed>

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sunclock:

<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

4947. 2003-05-01

date: Thu May 1 16:11:50 2003

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Re: Writing up the "Long simulations"

to: Simon Tett <simon.tett@metoffice.com>, Keith Briffa <k.briffa@uea.ac.uk>

Hi Simon,

At 10:18 17/04/2003, Simon Tett wrote:

Dear Keith & Tim,

I've been thinking how I want to write up the long simulations. My current thought

on the title is

"Testing simulated climate change of the last 500 years".

Nice and snappy, but perhaps too ambiguous? Also see point below about single vs. multiple

papers...

What I would like to do is compare the simulations with:

1) Tree-ring timeseries.

2) Bore-hole data.

4) Instrumental & early instrumental data.

Was there a 3, or just 1, 2 and 4?

When we get into it there is a lot that can/should be done on each comparison.

For

example, for tree-rings there is comparisons along the lines of Collins et al.

(st dev,

spectra, EOF), but then the whole question about decline and shortwave signal -

which would

necessitate backing it up with some in depth consideration of known

tree-shortwave

relationships or at least some additional analysis to increase confidence. Then

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the borehole stuff should, at some point, consider soil-air temperature differences and the influence of land cover and snow cover on these differences, plus the comparison itself. Then instrumental & early instrumental data (and some documentary records) should also include seasonal-differences (winter/summer are quite different in the observations/documentary) as a test of model performance under forcings. Then, of course, there is the question of what we go just for HadCM3 and what we do just for ECHO-G and what we do for both models. But, are you suggesting that we do a first paper, just for HadCM3, that takes a quick look at each of the 3 comparisons that you list above, that is sufficient to make it an eye-catching (==well-cited) paper, but that leaves some more in-depth and comprehensive work for later papers. In which case we could fit it all in to one paper, though we may risk coming to incorrect conclusions if we haven't done the in-depth analysis yet. I have just been reading Mann's latest JGR paper on boreholes -- I am unclear what he is doing..... but I think the assumption that underlys it is flawed. To my mind he assumes that bias in the 20th century in the borehole data is a coherent pattern over the previous 400 years. What do you think? I read a draft version of this, but haven't yet got round to reading the final published version. I assume they're similar, though he had great trouble in getting it accepted so perhaps they're not. Anyway, from the draft version, I agree that he is assuming a 20th century bias has the same pattern as bias in the earlier part. Perhaps this can be explained/supported by arguments about the bias being related to an incorrect determination of the background (i.e., steady state, if climate had been constant) heat flux out of the ground, though I'm not fully convinced. What I found more worrying however was that I didn't really feel they could claim to have found the true, unbiased 20th century pattern. The statistics just didn't seem strong enough and one then wonders whether the authors were only happy with the result because it matched Mann et al. Anyway, I really ought to read the published version before saying anything more. Cheers
Tim

398. 2003-05-09

date: Fri, 9 May 2003 10:31:02 -0000
from: "LICC" <no-reply@licc.org.uk>
subject: LICC - Connecting with Culture - x2
to: m.hulme@uea.ac.uk
LICC connecting with culture
x2
directed by Bryan Singer

Humans have never been good at sharing the Planet, as a voiceover reminds us at the start of X2, the sequel to the 2001 blockbuster X-Men. We have enough territorial disputes between ourselves, so what happens when we come face-to-face with a new breed - not homo sapiens but homo superiors, who can read minds, walk through walls and control the weather?

This film isn't subtle as it explores its main theme, xenophobia; but it delivers a useful examination of racism and ignorance nevertheless, reminding us that when we put up such barriers we not only alienate ourselves, but we become less human and less like God leaving our true potential untapped.

As Spiderman taught us last year (as, no doubt, will Neo in the Matrix this summer, along with Ang Lees Hulk), it's not having the power, but what we do with it that counts. It is a point not missed by X2's most spiritual character, the demonic looking but deeply devout Nightcrawler. After Halle Berrys Storm informs him that anger is what helps her to make it through, he replies that faith does the same job. When sadly a colleague dies, he recites the 23rd psalm.

Power without character can be crippling. X2 joins a legion of stories such as H G Wells War of the Worlds and James Camerons Aliens that warn against the use of superior technology in subduing supposedly inferior races. Wells castigated the British Empire, while Cameron focused on the US in Vietnam. The most disturbing character in the X-Men story is Magneto, a holocaust victim who intends to duplicate the elitist savagery of the Nazis, citing evolutionary superiority as his mantra.

This is a timely story about the destructive power of power. Those who wield it would do well to learn from the way Jesus dealt with temptation in the desert. He sought to order his private world before heading into public ministry, understanding that power was not a talent to be abused but a gift to be used wisely in the service of others.

Jason Gardner

humans have never been particularly good at sharing the planet
power without character can be crippling

The rate of climate change is greater now than it has been for thousands of years - is there a way to tackle the issues of global warming and see it as a positive opportunity for change? Sir John Houghton is at LICC on June 10th and will explore the challenge to individuals and to business. Click here for more details:
[1]<http://www.licc.org.uk/events/event.php/id/63>
The London Institute for Contemporary Christianity
St Peter's, Vere St, London, W1G 0DQ (t) 020 7399 9555 (e) mail@licc.org.uk
Visit
[2]www.licc.org.uk for articles and events listings.
If you have received this email indirectly and would like to subscribe to our mailing list please send a request to [3]mail@licc.org.uk. To be removed please reply to [4]mail@licc.org.uk with the subject "unsubscribe".

Embedded Content: wftw93.gif: 00000001,00000001,00000000,7544c561

4573. 2003-05-09

date: Fri May 9 17:55:50 2003
 from: Mike Hulme <m.hulme@uea.ac.uk>
 subject: Fwd: RE: Climate Equities: please see attached items (the covering
 to: n.adger

Neil,

Some interesting contributions from Pete Betts at the Treasury,

Mike

From: "Betts, Peter" <Peter.Betts@hm-treasury.x.gsi.gov.uk>

To: 'John Ashton' <john@lead.org>, "Kate Hampton (GA) (E-mail)"

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"Richard Sandbrook (E-mail)" <RSandbrook@aol.com>, "Tom Roper

(E-mail)"

<roper@tomw.freemove.co.uk>, "Zen A Makuch (E-mail)"

<z.makuch@imperial.ac.uk>

Cc: "Elliot Diringer (E-mail)" <DiringerE@pewclimate.org>,

"Dan Bodansky (E-mail) (E-mail)" <bodansk@attglobal.net>,

"Xueman Wang (E-mail) (E-mail)" <xueman.wang@biodiv.org>

Subject: RE: Climate Equities: please see attached items (the covering let
 ter is "equitycommentsltr": open that first)

Date: Fri, 9 May 2003 09:59:39 +0100

X-Mailer: Internet Mail Service (5.5.2653.19)

Dear John (and Xueman)<?xml:namespace prefix = o ns =

"urn:schemas-microsoft-com:office:office" />

Many thanks for the opportunity to comment. Can I offer some prosaic
 thoughts?

I agree with your two main conclusions:

-

That any attempt to define equity, or to

agree a very

detailed approach on the basis of it, is bound to fail and risks diverting

negotiating energy from more productive terrain; and
 - that any lasting agreement will have to be seen as equitable by all (or nearly all) Parties
 If your paper can use informal contacts to push towards consensus on that, it will have performed a valuable role.
 Put simply: developed countries will need to act first and make the biggest contribution; the key developing countries will then progressively need to be brought within the system. There will need to be transfers to developing countries to assist them with adaptation in particular. But at the heart of a deal that makes a difference will be big cuts by some (relatively few) emitters. Voluntary cuts in standard of living are unlikely however. So we need to drive technology. That means putting a price on Carbon
 All of that is commonplace. But the equity debate in the negotiations is almost never about that. How is it raised?:
 i) most commonly in terms of the process with (mostly) developing country representatives feeling excluded by the complexity and bounced by the big players, or OPEC telling us they are. You address this by suggesting a need for greater capacity building of delegates. But this will not change the fundamentals: a workable climate regime will need a lot of detailed rules mostly worked up by specialists: key developed countries will not be in the room on everything. We need to separate the technical from the political. (If I wanted to be more controversial I would add that any political deal that will make a difference will be done by a very small group of people in a small room. Of course there will need to be processes for progressively bringing in others; but unless we recognise that fact of life we will not be serving the needs of future generations);
 ii) the next most common context is that no deal is acceptable unless it shows parallel progress on all issues, including OPEC issues. This gives OPEC big leverage. Clearly adaptation and technology transfer are much more worthy causes; but does it really make sense for deals on emissions cuts by developed countries to be held up by developing countries unless they get progress on all issues at every meeting?
 iii) The other common context is opposition to the Kyoto mechanisms (in themselves key to setting a global price for Carbon) on the basis that they amount to permanent ownership rights. Is it naive to think your paper could help to reassure people on that?
 Your paper hints at the weaknesses in the process (eg future generations are not

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present in the negotiating room) but arguably process is worth a paper on its own in Pew's batch for COP9.

Two points of detail:

i) in Section 5.2, you describe the flexibilities of the Kyoto Protocol. You do not mention what is arguably the biggest: that the setting of

overall limits other than (say) technology requirements enables Parties to decide where to focus their reduction efforts;

ii) in Section 5.8, you raise an idea from Tom Burke that we might score Carbon emissions at the point of consumption rather than production. I can see

where this is coming from, but it risks creating a new source of complexity in the negotiations. Once we have established a market price for carbon won't that be

reflected in the price of the product? Won't it be for producers of energy intensive

goods to make their case for this to be reflected in their emission limits?

Pete

-----Original Message-----

From: John Ashton [[1]mailto:john@lead.org]

Sent: 14 April 2003 17:23

To: Kate Hampton (GA) (E-mail); Kiyo Akasaka (E-mail); Benito Mueller (E-mail); Nick

Mabey (office) (E-mail); Tom Burke (Rio) (E-mail); Richard Macrory

(E-mail); Nancy

Kete (E-mail); John Topping (E-mail); John Lennox (E-mail); John Beale (E-mail);

Justin Mundy (E-mail); James Cameron (E-mail); Mike Mason (E-mail);

Rajendra

Pachauri (E-mail); Iain Orr (E-mail); Michael J Grubb (E-mail); Alex Evans (E-mail);

Mike Hulme (E-mail); Duncan Brack (E-mail); Geoff Jenkins (E-mail); Peter Betts

(E-mail); Peter Unwin (E-mail); Calestous Juma (E-mail); Crispin Tickell

Lowen (E-mail); Dan Esty (E-mail); David Fisk (E-mail); Derek Osborn (E-mail); James

(E-mail); Jennifer Morgan (E-mail); Jeremy Leggett (E-mail); Kirsty Hamilton

Lilia Abron (E-mail); Malini Mehra (E-mail); 'Michael Northrop' (E-mail);

Richard

Sandbrook (E-mail); Tom Roper (E-mail); Zen A Makuch (E-mail)

(E-mail) Cc: Elliot Diringer (E-mail); Dan Bodansky (E-mail) (E-mail); Xueman Wang

(E-mail)

Subject: Climate Equities: please see attached items (the covering letter is

"equitycommentsltr": open that first)

<<equitycommentsltr.doc>> <<equityreviewtext.doc>> <<Pewoverview.doc>>

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3923. 2003-05-12

date: Mon May 12 17:26:29 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Review- confidential
to: Edward Cook <drdendro@ldeo.columbia.edu>

Ed
just back from really sunny Austria and very pleasant south of France. Have talked at length with Jan and he says it is fine to send the raw and detrended cores series (segmented for each site if possible). Do you also have a convenient Table with the Lats and Longs you used to plot the sites map? This would mean I don't have to look them all up.
I will phone to report on our discussions and ask several things that arose from these.
Just have to do essential other stuff first - so probably tuesday afternoon (my time) Do you have that review yet?
love and kisses
Keith
At 07:59 AM 4/29/03 -0400, you wrote:
Hi Keith,
I will start out by sending you the chronologies that I sent Bradley, i.e. all but Mongolia. If you can talk Gordon out of the latter, you'll be the first from outside this lab. The chronologies are in tabbed column format and Tucson index format. The latter have sample size included. It doesn't take a rocket scientist (or even Bradley after I warned him about small sample size problems) to realize that some of the chronologies are down to only 1 series in their earliest parts. Perhaps I should have truncated them before using them, but I just took what Jan gave me and worked with the chronologies as best I could. My suspicion is that most of the pre-1200 divergence is due to low replication and a reduced number of available chronologies. I should also say that the column data have had their means normalized to approximately 1.0, which is not the case for the chronologies straight out of ARSTAN. That is because the site-level RCS-detrended data were simply averaged to produce these chronologies, without concern for their long-term means. Hence the "RAW" tag at the end of each line of indices.
Bradley still regards the MWP as "mysterious" and "very incoherent" (his latest pronouncement to me) based on the available data. Of course he and other

members of the

MBH camp have a fundamental dislike for the very concept of the MWP, so I tend to view their evaluations as starting out from a somewhat biased perspective, i.e. the cup is not only "half-empty"; it is demonstrably "broken". I come more from the "cup half-full" camp when it comes to the MWP, maybe yes, maybe no, but it is too early to say what it is. Being a natural skeptic, I guess you might lean more towards the MBH camp, which is fine as long as one is honest and open about evaluating the evidence (I have my doubts about the MBH camp). We can always politely(?) disagree given the same admittedly equivocal evidence.

I should say that Jan should at least be made aware of this reanalysis of his data.

Admittedly, all of the Schweingruber data are in the public domain I believe, so that should not be an issue with those data. I just don't want to get into an open critique of the Esper data because it would just add fuel to the MBH attack squad. They tend to work in their own somewhat agenda-filled ways. We should also work on this stuff on our own, but I do not think that we have an agenda per se, other than trying to objectively understand what is going on.

Cheers,

Ed

Ed

thanks for this - and it is intriguing , not least because of the degree of coherence in these series between 1200 and 1900 - more than can be accounted for by either replication of data between the series (of which there is still some) or artifact of the standardisation method (with the use of RCS curves which are possibly inappropriate for all the data to which each is applied) . Having then got some not insubstantial confidence in the likelihood of a real temperature signal in this period - the question of why the extreme divergence in the series pre-1200 and post 1900? A real geographic difference in the forcing , replication and standardisation problems? - both are likely.

We would like the raw cores for each site: the RCS indices upon which you base the chronologies ; the site chronologies (which I think you sent to Ray?). At first we will simply plot the site chronologies , correlate each with local climate and come back to you again. We will also plot each "set" of indices and compare site RCS curves and reconsider the validity of the classification into linear and non-linear growth patterns. I know you have done all this but we need to get a feel for these data and do some comparisons with my early produce ring-width RCS chronologies for ceratin sites and compare the TRW series with the same site MXD chronologies - all a bit suck and see at

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first. I am talking with Tim later today about the review idea and I will
email/phone before 16.00 my time today.

Thanks

Keith

At 10:01 AM 4/28/03 -0400, you wrote:

Hi Keith,

Here is the new Esper plot with three different forms of regionalization:
linear vs.

nonlinear (as in the original paper), north vs. south as defined in the
legend, and east

vs. west (i.e. eastern hemisphere vs. western hemisphere). All of the series
have been

smoothed with a 50-yr spline after first averaging the annual values. The
number of

cores/chronologies are given in the legend in parentheses. Not surprisingly,
the north

and south chronologies deviate most in the post-1950 period. Before 1950 and
back to

about 1200 the series are remarkably similar (to me anyway). Prior to 1200
there is more

chaos, perhaps because the number of chronologies have declined along with the
within-chronology replication. However, there is still some evidence for

spatially coherent above-average growth. I showed this plot at the Duke meeting. Karl
Taylor

actually told me that he thought it looked fairly convincing, i.e. that the
low-frequency structure in the Esper series was not an artefact of the RCS

method.

Cheers,

Ed

Professor Keith Briffa,

Climatic Research Unit

University of East Anglia

Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

--

=====

Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152

=====

--

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[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

847. 2003-05-13

date: Tue, 13 May 2003 14:18:51 -0600

from: Tom Wigley <wigley@ucar.edu>

subject: paleo & extremes

cg2003

to: Tim Osborn <t.osborn@uea.ac.uk>
<x-flowed>

Tim,
I thought you might be able to tell me how the Hadley paleo runs are going. We (Caspar Ammann et al.) have completed a run from 1000AD to the present using the paleo version of CSM (slightly poorer resolution than the standard CSM). The results are quite interesting. I believe we are using a better volcano record (Caspar's) than anyone else. Solar is tricky -- the astronomical basis for Judith Lean's low frequency irradiance reconstruction has been shot down as you probably know. One of the nice results is that we can use MAGICC to back out the signal from the noise. Sarah has some material that I sent her showing that MAGICC can simulate both volcanic responses (on a monthly to century time scale) and solar responses (annual to century) with extremely high fidelity. Ask her to show you.
On another matter, you showed me a paper some time back on extreme value distributions, which I think you were refereeing. There was an earlier paper on the same subject by the same author. Can you give me the references -- and any other relevant items dealing with the modeling of changes in the frequency of extremes?

Best wishes,
Tom.
</x-flowed>

249. 2003-05-14

date: wed, 14 May 2003 14:43:20 +0100
from: "Lamb, Angela" <A.Lamb@livjm.ac.uk>
subject: Climate Change Debate at Liverpool John Moores University
to: "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>

Dear Professor Hulme

we are holding a 6th form debate entitled "Climate Change: past present and future" here at Liverpool John Moores University on June 30th this year (please see attached outline). We have held two such debates in the last 9 months, one on Biodiversity and one on the Gaia Hypothesis (see links below for reports). The day consists of an opening address by an eminent scientist before the students break up into small discussion groups led by staff/research students. Each group formulates a question or two that is put to a panel of experts in the form of a debate, chaired by the eminent scientist. In the past students have got a lot out of this contact with scientists and we think it is a valuable way to introduce 6th formers to key environmental topics.

The experts on the panel represent a variety of aspects of the topic and we would appreciate it very much if yourself, (or if you are unable to make it, perhaps another representative from the Tyndall Centre)

could make a contribution to the proposed debate as an expert on future climate change

predictions. Currently on the panel we have Adrian Lister from UCL (discussing climate change in the past) and David Vaughan from the British Antarctic Survey (discussing the role of the major ice sheets).

We could cover expenses and provide a lunch, but there is no fee. If you want more information please contact me by one of the means shown below. I very much hope you can make it.

Best wishes

Angela Lamb

<http://cwis.livjm.ac.uk/jmunews/final/story.asp?ref=200302131234070700634200000000>

<http://cwis.livjm.ac.uk/jmunews/final/story.asp?ref=200207051055170565853300000000>

<<flyer.doc>>

Dr Angela Lamb

(Lecturer in Physical Geography)

School of Biological and Earth Sciences

Liverpool John Moores University
 Byrom Street
 Liverpool L3 3AF
 Tel 0151 231 2410
 Fax 0151 207 3224
 Attachment Converted: "c:\eudora\attach\flyer.doc"

2923. 2003-05-14

date: Wed, 14 May 2003 10:33:03 -0600
 from: Tom Wigley <wigley@ucar.edu>
 subject: Re: paleo & extremes
 to: Tim Osborn <t.osborn@uea.ac.uk>
 <x-flowed>

Tim,

Thanx. I will keep in touch on the paleo stuff. Comparisons of results is a good first goal. Different forcings and different climate sensitivities present a bit of a challenge, but I think we can get around that using MAGICC.

Tom.

+++++

Tim Osborn wrote:

> Hi Tom,

>

> (1) the extremes reference in question appeared last year:

>

> Extreme daily precipitation in Western Europe with climate change at appropriate spatial scales

> M. J. Booij

> Department of Civil Engineering, University of Twente, PO Box 217, 7500

> AE Enschede, The Netherlands

> email: M. J. Booij (m.j.booij@sms.utwente.nl)

>

> International Journal of Climatology

> Volume 22, Issue 1, 2002. Pages: 69-85

>

> I'm not sure which other references you would be particularly wanting - obviously this is a big subject area. We have a useful list of some of them on one of our webpages:

> <http://www.cru.uea.ac.uk/projects/mice/html/extremes.html#references>

>

> Also see these (need to make your font pretty big to read these!):

> <http://www.met.rdg.ac.uk/~han/Extremes/bib0.html>

> <http://www.met.rdg.ac.uk/~han/Extremes/bib1.html>

>

> (2) HadCM3 run for 1500-2000 under natural only forcings is complete.

> 1750-2000 under "all" forcings is almost complete (its into the 20th

> century). I did email Caspar in January asking whether there is

> interest in comparisons across the models (we also have

> ECHAM4/HOPE[==ECHO-G] runs in our EU project SO&P), but I didn't get a

> response. Multi-model comparisons might be interesting to do still

> (though SO&P partners were keen to keep the initial focus within our

> project just on HadCM3 and ECHO-G). Is that why you were asking,

> because of possible comparisons that might be made? As to the forcings,

> I agree that the volcanic forcings used could be better, but we were to

> some extent constrained by our wish to have very similar forcings in

> HadCM3 and ECHO-G and the ECHO-G runs were begun about 2 years ago I

> think! The solar forcing is also of concern - did you use the Lean et

> al. estimates for that? While there are clearly some big uncertainties,

> from what I understand there is qualitative (at least) agreement with

> 14C and 10Be records. Presumably MAGICC (or similar) could help to

> quantify the global/hemispheric scale differences due to forcing

> uncertainties? I'm keen to keep collaborative links open on these

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> issues (though subject to agreement with my SO&P partners, of course),
> so if you have any specific analyses/comparisons that we could work on
> together (or involving Sarah) then let me know.
>
> Cheers
>
> Tim
>
> At 21:18 13/05/2003, you wrote:
>
>> Tim,
>>
>> I thought you might be able to tell me how the Hadley paleo runs are
>> going. We (Caspar Ammann et al.) have completed a run from 1000AD to
>> the present using the paleo version of CSM (slightly poorer resolution
>> than the standard CSM). The results are quite interesting. I believe
>> we are using a better volcano record (Caspar's) than anyone else.
>> Solar is tricky -- the astronomical basis for Judith Lean's low
>> frequency irradiance reconstruction has been shot down as you probably
>> know.
>>
>> One of the nice results is that we can use MAGICC to back out the
>> signal from the noise. Sarah has some material that I sent her showing
>> that MAGICC can simulate both volcanic responses (on a monthly to
>> century time scale) and solar responses (annual to century) with
>> extremely high fidelity. Ask her to show you.
>>
>> On another matter, you showed me a paper some time back on extreme
>> value distributions, which I think you were refereeing. There was an
>> earlier paper on the same subject by the same author. Can you give me
>> the references -- and any other relevant items dealing with the
>> modeling of changes in the frequency of extremes?
>>
>> Best wishes,
>> Tom.
>
>
> Dr Timothy J Osborn | phone: +44 1603 592089
> Senior Research Associate | fax: +44 1603 507784
> Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
> School of Environmental Sciences | web-site:
> University of East Anglia | <http://www.cru.uea.ac.uk/~timo/>
> Norwich NR4 7TJ | sunclock:
> UK | <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>
>
>
</x-flowed>

4574. 2003-05-15

date: Thu May 15 13:55:40 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: paper/comment request
to: David Appell <appell@nasw.org>

David
I would be happy to discuss the background and this paper in general if you care
to phone
(see number below) . I will be here all friday
I agree with a lot of what Phil said in his message = but the complications
arise because
of the mis use of the results by the greenhouse sceptics - and paranoia of some
who believe

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in greenhouse warming.

I believe passionately that we have a long way to go to get realistic and accurate

(absolute) measures of Hemispheric temperatures over the last millennium and earlier.

However, we must not lose sight of the fact that the "best evidence" is certainly in

support of unprecedented (truly mean Hemispheric and annual) warming in the 20th century

and recent decades. The modern (instrumental) indications of Hemispheric warmth are

(almost literally) incomparably superior to those based on our high-resolution proxy

records (with their narrow coverage and largely summer seasonal bias). Even pushing the

few individual records to their maximum warmth limit, the most sensible interpretation of

the data does provide much of a case for equivalent warmth in any "Medieval" period (or on

any timescale). Those who prefer to believe in a globally warmer Medieval period largely

fall back on poorly resolved, even more selective evidence that has real problems e.g.

interpretable signal (temp. versus precip.); qualitative measurement; non-deconvolved

lagged responses, and geographical bias that is at least as poor as our high-resolution

data. The science is not progressed without overcoming these problems. Our own desire to

recognize and address the limitations of our own data in the search for accurate and

absolute climate histories should not be confused with a clear expression that "as we

stand" the evidence against unprecedented recent warming does not carry the day.

At 04:11 PM 5/13/03 -0400, you wrote:

Hi. I was wondering if I could get a copy of your 1998 paper:

Jones, P.D., Briffa, K.R., Barnett, T.P. and Tett, S.F.B., 1998

"High-resolution palaeoclimatic records for the last millennium:

interpretation, integration and comparison with General Circulation

Model control-run temperatures," The Holocene 8(4), 455-471 (1998).

As you may know, this paper has been cited by Soon and Baliunas as evidence for a worldwide "discernible climatic anomaly during the Little Ice Age, defined as 1300-1900." [Soon W, Baliunas S, "Proxy climatic and environmental changes of the past 1000 years," Climate Research, 23:89-110 (2003)] (attached) -- see question 1, p. 90.

I'm wondering whether you agree with Soon and Baliunas classification of your paper.

I'd be interested in any thoughts, by this coming Monday, May 19th --

I'm writing a news article for "Scientific American" magazine on these claims.. As well as any thoughts you have on the Soon & Baliunas paper

(as well as their longer paper,

"Reconstructing Climatic and Environmental Changes of the Past 1000

Years: A Reappraisal," Willie Soon, Sallie Baliunas, Sherwood B. Idso, Craig Idso and David R. Legates, Energy and Environment, vol. 14, issues 2 & 3, April 11, 2003.

Thank you,

David

--

David Appell, freelance science journalist

<http://www.nasw.org/users/appell>

p: 207-646-3080

f: 815-333-1486

e: appell@nasw.org

cg2003

m: 27 Beach Street Rear, P.O. Box 42, Ogunquit, ME 03907-0042

--

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Fax: +44-1603-507784
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1798. 2003-05-16

cc: Jerry Meehl <meehl@ucar.edu>, Caspar Ammann <ammann@ucar.edu>
date: Fri, 16 May 2003 14:18:36 -0600
from: Tom Wigley <wigley@ucar.edu>
subject: Soon et al. paper
to: "Michael E. Mann" <mann@virginia.edu>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Dear folks,

I have just read the Soon et al. paper in E&E. Here are some comments, and a request.

Mike said in an email that he thought the paper contained possibly 'legally actionable' ad hominem attacks on him and others. I do not agree that there are ad hominem attacks. There are numerous criticisms, usually justified (although not all the justifications are valid). I did not notice any intemperate language.

While many of the criticisms are invalid, and some are irrelevant, there are a number that seem to me to be quite valid. Probably, most of these can be rebutted, and perhaps some of these are already covered in the literature. In my view, however, there are a small number of points that are valid criticisms.

[Off the record, the most telling criticisms apply to Tom Crowley's work -- which I do not hold in very high regard.]

The real issue that the press (to a limited extent) and the politicians (to a greater extent) have taken up is the conclusions of the paper's original research.

First, Soon et al. come down clearly in favor of the existence of a MWE and a LIA. I think many of us would agree that there was a global-scale cool period that can be identified with a LIA. The MWE is more equivocal. There are real problems in identifying both of these 'events' with certainty due to (1) data coverage, (2) uncertainty in transfer functions, and (3) the noise of internally generated variability on the century time scale. [My paper on the latter point is continually ignored by the paleo community, but it is still valid.]

So, we would probably say: there was a LIA; but the case for *or against* a MWE is not proven. There is no strong disagreement with Soon et al. here.

The main disagreements are with the methods used by Soon et al. to draw their LIA/MWE conclusion, and their conclusion re the anomalousness/uniqueness of the 20th century (a conclusion that is based on the same methods).

So what is their method? I need to read the paper again carefully to check on this, but it seems that they say the MWE [LIA] was warm [cold] if at a particular site there is a 50+ year period that was warm, wet, dry [cold, dry, wet] somewhere in the interval 800-1300 [1300-1900], where warm/cold, wet, dry are defined relative to the 20th century.

The problems with this are

(1) Natural internally generated variability alone virtually guarantees that these criteria will be met at every site.

(2) As Nev Nicholls pointed out, almost any period would be identified as a MWE or LIA by these criteria -- and, as a corollary, their MWE period could equally well have been identified as a LIA (or vice versa)

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(3) If the identified warm blips in their MWE were at different times for different locations (as they are) then there would be no global-mean signal.

(4) The reason for including precip 'data' at all (let alone both wet and dry periods in both the MWE and LIA) is never stated -- and cannot be justified. [I suspect that if they found a wet period in the MWE, for example, they would search for a dry period in the LIA -- allowing both in both the MWE and LIA seems too stupid to be true.]

(5) For the uniqueness of the 20th century, item (1) also applies.

So, their methods are silly. They seem also to have ignored the fact that what we are searching is a signal in global-mean temperature.

The issue now is what to do about this. I do not think it is enough to bury criticisms of this work in other papers. The people who have noticed the Soon et al paper, or have had it pointed out to them, will never see or become aware of such rebuttals/responses. Furthermore, I do not think that a direct response will give the work credibility. It is already 'credible' since it is in the peer reviewed literature (and E&E, by the way, is peer reviewed). A response that says this paper is a load of crap for the following reasons is *not* going to give the original work credibility -- just the opposite.

How then does one comprehensively and concisely demolish this work?

There are two issues here. The first is the point by point response to their criticisms of the literature. To do this would be tedious, but straightforward. There will be at least some residual criticisms that must be accepted as valid, and this must be admitted. Cross-referencing to other review papers would be legitimate here.

The second is to demolish the method. I have done this qualitatively (following Nev mainly) above, but this is not enough. What is needed is a counter example that uses the method of reductio ad absurdum. This would be clear and would be appropriate since it avoids us having to point out in words that their methods are absurd. I have some ideas how to do this, but I will let you think about it more before going further. You will see from this email that I am urging you to produce a response. I am happy to join you in this, and perhaps a few others could add their weight too. I am copying this to Jerry since he has to give some congressional testimony next week and questions about the Soon et al work are definitely going to be raised. I am also copying this to Caspar, since the last millenium runs that he is doing with paleo-CSM are relevant.

Best wishes,

Tom.

</x-flowed>

2170. 2003-05-16

date: Fri May 16 10:39:23 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: Fwd: RE: HadRM3 paper and data

to: Phil Jones <p.jones@uea.ac.uk>

Thanks Phil - this seems a bit of a fiasco (what is the literal meaning of that word

anyway?)!

I guess it means Dave is even gladder BADC are now handling data downloads.

Mike

At 09:19 16/05/2003 +0100, you wrote:

Mike,

You might be interested in these emails as they relate to HadRM3 and UKCIP.

Also I see you are supervising an U/G on Scottish snow and the NAO - I'm the second

marker. We've completed the reworking of monthly temperature series for the Scottish

Mainland, NW/N islands and N. Ireland. There will be a paper submitted to IJC

cg2003

in the

next few months and a report to SNIFFER. Better for the student to use these data

than those in the old SNIFFER report. We've homogenized 8 long series (inc. Braemar)

to do this.

Cheers

Phil

Date: Thu, 15 May 2003 16:56:25 +0100

From: "Jenkins, Geoff" <geoff.jenkins@metoffice.com>

Subject: RE: HadRM3 paper and data

To: 'Phil Jones' <p.jones@uea.ac.uk>

Cc: "Jones, Richard" <rgjones@metoffice.com>,

"Murphy, James" <james.murphy@metoffice.com>,

"Mitchell, John FB (Chief Scientist)" <john.f.mitchell@metoffice.com>

X-Mailer: Internet Mail Service (5.5.2653.19)

Phil

I will leave Richard to answer most of tr email, but in item 5 you ask

5. You say UKCIP is OK, yet in a few months LINK will be distributing the

new HadRM3H data, which will be different from that available through

UKCIP02. We have meetings next week related to the EPSRC projects on

impacts of future climate change over the UK. Here, we are developing

tailored scenarios for different sectors. Should we

base these on what we have here (i.e the HadRM3H used in UKCIP02) or should

we use the new

HadRM3H runs ? Some quick advice here would be useful as there are meetings on Monday

and Thursday - both involving UKCIP/EPSRC and Stakeholders.

The answer is: use HadRM3H as in UKCIP02. New HadRM3H run data is within the

ensemble of UKCIP02 HadRM3H data over the UK, so there is no point using the

new RM3H run. Any old-new differences pale into insignificance compared to

the biggest uncertainty; that of the driving GCM.

Cheers

Geoff

> -----Original Message-----

> From: Phil Jones [SMTP:p.jones@uea.ac.uk]

> Sent: 15 May 2003 15:36

> To: Richard Jones; d.viner@uea.ac.uk; jhc@dmf.dk; c.goodess@uea.ac.uk;

> j.palutikof@uea.ac.uk

> Cc: john.f.mitchell@metoffice.com; dave.griggs@metoffice.com;

> geoff.jenkins@metoffice.com; james.murphy@metoffice.com;

> simon.brown@metoffice.com; richard.jones@metoffice.com;

> david.hassell@metoffice.com; dave.rowell@metoffice.com;

> erasmo.buonomo@metoffice.com; david.hein@metoffice.com;

> jonathan.gregory@metoffice.com; cath.senior@metoffice.com;

> a.moberg@uea.ac.uk

> Subject: Re: HadRM3 paper and data

>

>

> Dear Richard,

> Since calling you and Geoff we've had some more discussions in CRU,

>

> and a meeting

> might be necessary to sort a few points out. In this email I'm going to

> summarise briefly

> a few points from the phonecalls, but also the concerns we have in CRU

> and the implications

> for some of our EU projects.

>

> 1. Your reason for rerunning HadAM3H/HadRM3H over Europe is to have the

>

> integrations

> compatible with the PRECIS runs in other parts of the world.

>
> 2. I've talked to David Viner and he will get into contact with Bryan
> Lawrence at BADC to
> see how quickly they might be able to begin downloading the necessary
> new
> integrations.
> Some have already been completed and most of the rest will be by June
> 15,
> when one
> of T3Es is turned off.
>
> 3. I'm happy for Anders to rerun the programs for our specific paper.
> Anders probably won't
> be, but it seems the text will not have to changed that much as you
> expect the results to be
> little changed. I would appreciate comments on the current version when
> you have some time.
> with this in mind, though, it seems that you've not fully thought
> through
> the implications. If,
> as you say, you expect the results to change little, how then has this
> undermined our
> justification for doing the study ?
>
> 4. The paper with Anders isn't the only paper affected. A number of
> people have been working
> with HadRM3H data for upwards of a year and have drafts of papers which
> are near submittal
> stage. One is for a project that has only 2 weeks to run, with the final
>
> report near completion.
> Some in CRU read your email, as, all these studies need rerunning with
> the
> new HadRM3H
> data - except possibly if they relate just to the UK. Is this a correct
> reading of your (the HC)
> intentions? This particular project is looking at storm tracks over the
>
> UK and another is
> looking at extreme precipitation variation again across the UK. The
> latter is part of an EU
> project - can we go with our UK analyses ? - yet we have to tell our EU
> partners who have
> been doing similar work in Switzerland, Iberia and Germany that they
> must
> rerun their
> analyses? It doesn't seem consistent.
>
> 5. You say UKCIP is OK, yet in a few months LINK will be distributing
> the
> new HadRM3H
> data, which will be different from that available through UKCIP02. We
> have meetings next
> week related to the EPSRC projects on impacts of future climate change
> over the UK. Here,
> we are developing tailored scenarios for different sectors. Should we
> base these on what we
> have here (i.e the HadRM3H used in UKCIP02) or should we use the new
> HadRM3H runs ?
> Some quick advice here would be useful as there are meetings on Monday
> and Thursday -
> both involving UKCIP/EPSRC and Stakeholders.
>

> 6. The implications of rerunning much of the above work will be onerous,
>
> but they can be
> achieved. Much more important though is the implications for the
> PRUDENCE/STARDEX/MICE
> projects. Here, the aims of the projects were to intercompare RCMs,
> intercompare
> statistical and dynamical downscaling and intercompare various impacts
> sectors with
> RCM and statistically downscaled inputs - all also with RCM downscaling
> with
> various models forced with near-perfect boundary conditions from NCEP.
> Although you say
> the alterations to HadRM3H and HadAM3H will not have major influences in
>
> Europe, the
> whole experimental design of the many intercomparisons has been
> compromised. The
> modelling groups are supposed to continue with the boundary conditions
> they have yet the
> STARDEX and MICE groups and the impacts work in PRUDENCE are supposed to
>
> rerun
> all their work with the new integrations. This seems, to all of us in
> CRU, to have completely
> scuppered the whole set-up of the three projects. Jens, would probably
>
> add that this was
> already compromised by the different resolution of boundary conditions
> supplied to the other
> groups.
>
> 7. There is a simple way around all this - well to me anyway. Can the
> current HadAM3H
> and HadRM3H data we have within LINK and the various EU projects have be
>
> designated
> HadAM3HU/HadRM3HU (the U referring to as used in UKCIP02) and the newer
> versions
> HadAM3HP/HadRM3HP (the P referring to PRECIS)? As you've already looked
>
> at how the
> new runs compare with the old ones, can they for Europe be considered as
>
> a larger
> ensemble, going from 3 to 6 for the A2 scenario and 1 to 2 for B2. If
> the
> differences between
> the different integrations are within the ensemble noise this would seem
>
> possible. In the
> paper with Anders the inter-ensemble variability seemed much lower than
> I
> would expect from
> two independent 30-year observational periods. In other words, the
> combination of two
> versions of the models would give more realistic estimates of
> within-model noise (natural
> climate variability - call it what you like).
>
>
> I've gone on for far too long, but I hope you've got a feel for our
> concerns - and the need for

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> some quick responses on a few of the issues. There are a lot of people
> and a lot of groups
> around Europe involved in some form of HadRM3H analyses and a few less
> for HadAM3H.

> Cheers
> Phil

> At 12:10 15/05/03 +0100, Richard Jones wrote:

> > Dear Phil,
> > Sorry for the long delay in replying to this but it has
> > generated a lot of internal discussion on related issues which we have
> > now resolved. On the paper itself we agree that it is an interesting and
> > useful investigation into the performance of the RCM. However, we are
> > going to have to ask for a fundamental, though hopefully not
> > time-consuming, revision before submission. In brief, the reason for
> > this is that we have undermined one of your main justifications for the
> > paper in that we have upgraded HadAM3H/RM3H and are currently rerunning
> > all the experiments. As a result (and the reason for the wide
> > distribution of this email) we are going to withdraw all the data from
> > the old experiments currently resident at LINK (except those relevant to
> > UKCIP) and replace them. On the specific issue of your and Anders' paper
> > this implies that you will need to regenerate your figures though our
> > assessment of the new model is that little will change in respect of the
> > paper's message (hence my prediction that the revision will not take
> > long).

> > OK so those are the headlines, now for the detail. HadAM3H was
> > originally built as a model to overcome shortcomings in HadCM3 with
> > respect to generating RCM predictions for UKCIP though with the
> > important proviso that it was a model which performed globally as well
> > as or better than HadCM3 (so we did not undermine its credibility for
> > simulating climate or predicting climate change). Clearly, the European
> > dimension of the experiments was also important. Another dimension was
> > its use over other regions, specifically southern Africa and India and
> > then, somewhat less critical at the time, as a source of boundary
> > conditions globally (i.e. for PRECIS). In the meantime, our analysis of
> > the model and its use over different regions has encouraged us to
> > reformulate certain aspects of the cloud and precipitation physics which
> > provide further improvements in surface climatology globally. The main
> > motivation here is now PRECIS with current users in India, China and
> > Africa all using the new model (the configuration of HadRM3H follows
> > directly from that of HadAM3H). Thus, given that PRECIS is to provide
> > the functionality for producing consistent high resolution climate
> > scenarios globally we felt it necessary to regenerate our initial set of
> > European experiments using the PRECIS RCM (i.e. HadRM3H). These are the
> > new data which we will supply to LINK and thus will form the basis for
> > European climate scenario generation from the current Hadley Centre RCM.

> > Clearly there are a series of implications resulting from these
> > developments, not least for your paper, which hopefully I will address
> > below. These are mostly of common interest, hence the general email
> > which I felt was the best way of disseminating this information, but are
> > individually addressed to those most directly involved. I will start
> > with the simplest first (and try and be brief).

> >
> > 1) Dave, please ensure that no more of the existing HadRM3H data are
> > released, similarly for HadAM3H for which no more extraction from our
> > archives is necessary. We will need to discuss offline how to get the
> > new data to you.

> >
> > 2) Jens, the implications for Prudence I think can be split into

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> >categories. First is the use of HadAM3H to drive the other RCMS in
> >Prudence and second is the use of HadRM3H within Prudence. On the first
> >point there is no change, we are completing extraction of the third set
> >of A2 6 hourly data for distribution. On the second point, the existing
> >Hadley Centre RCM experiments in Prudence will be retained for the basic
> >intercomparison work within WP2. However, for the use of daily data in
> >the impacts areas of Prudence, we will provide only the new RCM data as
> >this will then be consistent with others uses of the data in Europe via
> >LINK and other uses of HadRM3H worldwide via PRECIS. All 50km
> >experiments will be completed next month so this should imply little
> >delay in getting data to the central archive.

> >
> >3) Clare and Jean, the implications for Stardex and Mice follow from the
> >comments to Jens I think. Clare, your recent conversation with Simon
> >Brown implied that little concerted use of HadAM3H/RM3H data has been
> >made in Stardex. Any initial set-up of software etc. to handle the data
> >will be immediately applicable to data from the new experiments and we
> >will do all we can to get the relevant data to you so as not to
> >compromise your milestones. As the new experiments are all being written
> >into MASS (our new mass-storage system) and the teething problems with
> >MASS have been resolved then this should not involve major delays.

> >
> >I am sorry if this comes as a bit of a shock to you and means
> >disruption to your work. I am sure there are implications which we have
> >not considered or are glossed over in my brief comments above. If you
> >would like to discuss this in more detail then please phone me, today
> >and next week I will only be available on my mobile (07855 822104)
> >though I am in the office tomorrow and after that back in on Tuesday
> >27th.

> >Best wishes,
> >Richard.

> >--
> >Dr. Richard Jones Regional climate change research manager
> >richard.jones@metoffice.com [1]http://www.metoffice.com
> >Telephone: +44 (0)1344 856418 Fax: +44 (0)1344 854898
> >Mail: Met office Hadley Centre, London Road, Bracknell, RG12 2SY, UK.

> >
> >Dear James and Richard,
> >Anders is keen to submit the paper and wants to move onto some
> >aspects of changes in extremes in the observational data, whilst he is
> >still a CRU employee. Will you be able to send any comments in the next
> >couple of weeks or are you happy with us submitting the paper? I gave
> >John
> >a copy in Nice but haven't heard anything from him.

> >
> >Cheers
> >Phil

> >
> >
> >Dear James and Richard,
> >I talked to John Mitchell at the EGS in Nice the other week and
> >gave
> >him a copy of this paper. He suggested I should send it to the two of
> >you
> >for any comments you might be able to make. This request stems from our
> >agreement to send you copies of papers before submission (our meeting
> >this
> >time last year here, when Richard, John and others came).
> >Anders and I are keen to submit this in the next month or so to
> >Climate Dynamics as their colour costs seem reasonable. Anders email is
> >a.moberg@uea.ac.uk, and if you have any comments can you cc them to him

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> as
> > well as me.
> > Have a good Easter - pity the hot spell isn't going to last !
> >
> > Cheers
> > Phil

> >
> >
> >
> >
> > Prof. Phil Jones
> > Climatic Research Unit Telephone +44 (0) 1603 592090
> > School of Environmental Sciences Fax +44 (0) 1603 507784
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> > UK

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> > --

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> > UK

> > -----

2524. 2003-05-16

date: Fri, 16 May 2003 16:11:38 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Fwd: Re: Fwd: Re: Harvard?
to: m.hulme@uea.ac.uk

X-Sender: paul_epstein@hms.harvard.edu
X-Mailer: QUALCOMM Windows Eudora Version 5.1
Date: Thu, 15 May 2003 17:37:36 -0400
To: "Michael E. Mann" <mann@virginia.edu>
From: Paul Epstein <paul_epstein@hms.harvard.edu>
Subject: Re: Fwd: Re: Harvard?
Cc: eric_chivian@hms.harvard.edu, trenbert@ncar.ucar.edu,
jmccarth@oeb.harvard.edu

Dear Michael Mann,

graph for the
our It is indeed a great pleasure to receive your message (showing your famous
last millennium so often in talks). My feelings -- and those of Eric Chivian,

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center's director -- are mutual. It is appalling what Baliunus et al. are doing, using the past 50 year window for example, to tell their distorted story. And of course the

story is used by those whose interests have become self evident.

Have you spoken with Mike McElroy and Jim McCarthy? I know that Dan S is steaming mad

and I do suggest a call to Mike to encourage a response from the Harvard University

Committee on the Environment. I have discussed this with both Mike and Jim, but a note

from you might help move things along.

I look forward to hearing back and would certainly be open to developing a response

based on climatology and the accumulating biological and health responses to climate change.

With best regards, Paul

mbm@io.harvard.edu

617-495-4359

jmccarth@oeb.harvard.edu

617-495-2330

At 10:49 AM 5/15/2003 -0400, Michael E. Mann wrote:

Dear Paul,

Kevin reminded me that you would be a good person to contact. I don't know if you have

followed this story. Baliunas and company have published these two terrible paper which

purport, without any credibility whatsoever, to undermine IPCC conclusions.

The papers

were published in "Energy and Environment" (an industry shell) and "Climate Research"

(with help from some dubious individuals on the editorial boards--there is an investigation now into the practices of the editor in question, Chris

DeFries of New

Zealand, who rights anti-IPCC and anti-Kyoto op ed pieces in New Zealand).

They are

making some headway within the beltway, though the mainstream media and scientific

community recognize the stuff for what it is [I'll resist using the appropriate words

here, because my message might then not make it through the email filters].

Any insights you might have into the goings on within the PR office at Harvard would be

of interest. It is disappointing to see Harvard's press office allow itself to be used

as a pawn in this transparently political, pseudo-scientific, and industry-backed

stunt...

thanks in advance for any help or insight you can provide,

mike

Date: Wed, 14 May 2003 18:00:06 -0600

From: Kevin Trenberth <trenbert@cgd.ucar.edu>

Organization: NCAR/CGD

User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.0.1)

Gecko/20020823

Netscape/7.0

X-Accept-Language: en-us, en

To: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Harvard?

My main contact there is Dan Schrag; also Paul Epstein.

Kevin

Michael E. Mann wrote:

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Dear Colleagues,
Baliunas and co. appear to have successfully hijacked Harvard's PR office on this. Any of you have contacts there you might be able to get some information from? Both of these appeared in the "Harvard Gazette":
[1]<http://www.news.harvard.edu/gazette/2003/04.24/04-sun.html>
[2]<http://www.news.harvard.edu/gazette/2003/04.24/01-weather.html>
That provides the appearance of Harvard's stamp of approval for unsound claims which have otherwise been ignored by any other mainstream media outlets (despite the repeated attempts of the authors and their promoters to get wider coverage, the story has generally only been picked up by right-wing online sites and Murdoch-owned newspapers). While the work is getting ignored in scientific circles, and by the mainstream media, it is nonetheless being heavily promoted within Washington DC by not just the usual suspects like the "Marshall Institute", which has sponsored multiple presentations on this by the authors on capitol hill, but by the administration and agencies directly under their control. From what I am told, they are beginning to make political inroads in their attempts to use this to attempt to undermine IPCC's credibility. Phil Jones and I are writing a review paper for "Reviews in Geophysics" which will include a debunking of much of what they say, and Ray Bradley and others have something in the works in Science along these lines, but these will both have a long residence time--something more immediate may be necessary in the meantime. Thoughts and suggestions as to how best to proceed would be appreciated.
mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: [3]mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Website. <[6]www.med.harvard.edu/chge>

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.....
The Mission of the Center for Health and the Global Environment is to study
and to promote a wider understanding
of the human health consequences of global environmental change. The Center
believes that people will protect the
natural environment when they realize its importance to their health, and to
the health and lives of their children.

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[7]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3189. 2003-05-16

date: Fri, 16 May 2003 15:45:47 +0100
from: Suraje Dessai <s.dessai@uea.ac.uk>
subject: Revised dangerous paper
to: Neil Adger <N.Adger@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, John Turnpenny
<j.turnpenny@uea.ac.uk>, Jonathan Kohler <J.Kohler@econ.cam.ac.uk>, R Warren
<R.Warren@uea.ac.uk>
<x-flowed>

Dear all,
As you might remember Steve Schneider's main concern about our paper was
that we "didn't go too far" so I've spent the last couple of months adding
substance to it with the help of several of you. The result is attached.
This draft is close to final so if you have any
comments/changes/suggestions send them to me asap. If you think we require
a meeting to discuss the paper let me know. My plan was to submit the paper
in 2 weeks time once we receive the review of some external people. On that
note, could I ask Neil to send the paper to Nick Pidgeon as discussed. I
will also send this to Irene, Alex Haxeltine and Simon Shackley to see if
they have any further comments.

Have a good weekend,

Suraje

</x-flowed>

Attachment Converted: "c:\eudora\attach\dangerousCC_revised5.doc"

4285. 2003-05-16

cc: Jerry Meehl <meehl@ucar.edu>, Caspar Ammann <ammann@ucar.edu>,
mann@virginia.edu
date: Fri, 16 May 2003 17:04:35 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Soon et al. paper
to: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>, rbradley@geo.umass.edu

Tom,

Thanks for your response, which I will maintain as confidential within the small
group of

the original recipients (other than Ray whom I've included in as well), given
the

sensitivity of some of the comments made.

Whether or not their comments are ad hominem or potentially libelous is probably
immaterial

here (some people who have read them think they might be--in certain places,
alterior

motives are implied on the part of individually named scientists in the discussion of scientific methodologies).

However, the real issue, as you point out, is whether or not their arguments and criticisms

are valid. I would argue that very few of them are--I have prepared (and have attached) a

draft of replies to some of the specifics in their two papers--this is rough, and I'm

working on preparing a refined version of this for use by those who are trying to combat

the disinformation that the Baliunas and co. supporters are working at spreading within the

beltway, with the full support of industry, and perhaps the administration. By necessity

this is brief and focus on the most salient points--a point-by-point rebuttal would take a

very long time.

In the meantime, Phil and I, and Ray/Malcolm/Henry D are independently working on review

pieces (ours for R.O.G., Ray et al's for Science) that will also correct in more detail

some of the most egregious untruths put forward by the Baliunas/Soon pieces (what one

colleague of mine aptly chooses to abbreviate as "BS").

The most fundamental criticism, of course, is that the hypothesis, methods, and assumptions

are absolutely nonsensical by construction--as you already pointed out. One could

demonstrate that with an example, but then again, why do so when it is self evident that

defining an anomaly of either wetter or dryer (what does that leave out?) relative to the

20th century (a comparison which is itself also ill-defined by the authors, since they

don't use a uniform 20th century reference period for defining their qualitative anomalies,

and discuss proxy records with variable resolution and temporal sampling of the 20th

century) was "warmer than the 20th century" is nonsense at the most fundamental level. It

defies the most elementary logic, and thus is difficult to reply to other than noting that

it is nonsense by its very nature.

Would we be compelled to provide a counterexample to disprove the authors if they had

asserted that "1=2"? What they have done isn't that much different...

So its one thing to throw out a bunch of criticisms, very few of which are valid. But to

then turn around and present a fundamentally ill-posed, supposed "analysis" which doesn't

even attempt to provide a quantitative "alternative" to past studies, to claim to have

disproven those past studies, and to supposedly support the non-sequitor conclusion that

the "MWP was warmer than the 20th century" is irresponsible, deceptive, dishonest, and a

violation of the very essence of the scientific approach in my view.

One or two people can't fight that alone, certainly not with the "artillery" (funding and

political organization) that has been lined up on the other side. In my view, it is the

responsibility of our entire community to fight this intentional disinformation

campaign,
 which represents an affront to everything we do and believe in. I'm doing
 everything I can
 to do so, but I can't do it alone--and if I'm left to, we'll lose this battle,
 mike

At 02:18 PM 5/16/2003 -0600, Tom Wigley wrote:

Dear folks,

I have just read the Soon et al. paper in E&E. Here are some comments, and a
 request.

Mike said in an email that he thought the paper contained possibly
 'legally actionable' ad hominem attacks on him and others. I do not
 agree that there are ad hominem attacks. There are numerous criticisms,
 usually
 justified (although not all the justifications are valid). I did not notice
 any

intemperate language.

While many of the criticisms are invalid, and some are irrelevant, there are a
 number

that seem to me to be quite valid. Probably, most of these can be rebutted,
 and perhaps

some of these are already covered in the literature. In my view, however,
 there a small

number of points that are valid criticisms.

[Off the record, the most telling criticisms apply to Tom Crowley's work --
 which I do

not hold in very high regard.]

The real issue that the press (to a limited extent) and the politicians (to a
 greater

extent) have taken up is the conclusions of the paper's original research.

First, Soon et al. come down clearly in favor of the existence of a MWE and a
 LIA. I

think many of us would agree that there was a global-scale cool period that
 can be

identified with a LIA. The MWE is more equivocal. There are real problems in
 identifying

both of these 'events' with certainty due to (1) data coverage, (2)
 uncertainty in

transfer functions, and (3) the noise of internally generated variability on
 the

century time scale. [My paper on the latter point is continually ignored by
 the paleo

community, but it is still valid.]

So, we would probably say: there was a LIA; but the case for *or against* a
 MWE is not

proven. There is no strong disagreement with Soon et al. here.

The main disagreements are with the methods used by Soon et al. to draw their
 LIA/MWE

conclusion, and their conclusion re the anomalousness/uniqueness of the 20th
 century (a

conclusion that is based on the same methods).

So what is their method? I need to read the paper again carefully to check on
 this, but

it seems that they say the MWE [LIA] was warm [cold] if at a particular site
 there is a

50+ year period that was warm, wet, dry [cold, dry, wet] somewhere in the
 interval

800-1300 [1300-1900], where warm/cold, wet, dry are defined relative to the
 20th

century.

The problems with this are

(1) Natural internally generated variability alone virtually guarantees that
 these

criteria will be met at every site.

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(2) As Nev Nicholls pointed out, almost any period would be identified as a MWE or LIA by these criteria -- and, as a corollary, their MWE period could equally well have been identified as a LIA (or vice versa)

(3) If the identified warm blips in their MWE were at different times for different locations (as they are) then there would be no global-mean signal.

(4) The reason for including precip 'data' at all (let alone both wet and dry periods in both the MWE and LIA) is never stated -- and cannot be justified. [I suspect that if they found a wet period in the MWE, for example, they would search for a dry period in the LIA -- allowing both in both the MWE and LIA seems too stupid to be true.]

(5) For the uniqueness of the 20th century, item (1) also applies. So, their methods are silly. They seem also to have ignored the fact that what we are searching is a signal in global-mean temperature.

The issue now is what to do about this. I do not think it is enough to bury criticisms of this work in other papers. The people who have noticed the Soon et al paper, or have had it pointed out to them, will never see or become aware of such rebuttals/responses.

Furthermore, I do not think that a direct response will give the work credibility. It is already 'credible' since it is in the peer reviewed literature (and E&E, by the way, is peer reviewed). A response that says this paper is a load of crap for the following reasons is *not* going to give the original work credibility -- just the opposite.

How then does one comprehensively and concisely demolish this work? There are two issues here. The first is the point by point response to their criticisms of the literature. To do this would be tedious, but straightforward. There will be at least some residual criticisms that must be accepted as valid, and this must be admitted.

Cross-referencing to other review papers would be legitimate here.

The second is to demolish the method. I have done this qualitatively (following Nev mainly) above, but this is not enough. What is needed is a counter example that uses the method of reductio ad absurdum. This would be clear and would be appropriate since it avoids us having to point out in words that their methods are absurd. I have some ideas how to do this, but I will let you think about it more before going further. You will see from this email that I am urging you to produce a response. I am happy to join you in this, and perhaps a few others could add their weight too. I am copying this to Jerry since he has to give some congressional testimony next week and questions about the Soon et al work are definitely going to be raised. I am also copying this to Caspar, since the last millenium runs that he is doing with paleo-CSM are relevant.

Best wishes,
Tom.

cg2003
Professor Michael E. Mann
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\BaliunasetalComments.doc"

4808. 2003-05-16

cc: p.jones@uea.ac.uk
date: Fri, 16 May 2003 16:11:04 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Climate Research and adequate peer review
to: Mike Hulme <m.hulme@uea.ac.uk>

Dear Mike
Did anything ever come of this?
Clare Goodness was in touch w/ me indicating that she had discussed the matter
w/ Von
Storch, and that DeFries would be relieved of his position.
However, I haven't heard anything. A large segment of the community I've been in
contact
with feels that this event has already done its damage, allowing Baliunas and
colleagues to
attempt to impact U.S. governmental policy, w/ this new weapon in hand--the
appearance of a
legitimate peer-reviewed document challenging some core assertions of IPCC to
wave in
congress. They appear to be making some headway in using this to influence U.S.
policy,
which makes our original discussions all the more pressing now.
In this context, it seems important that either Clare and Von Storch take
imminent action
on this, or else actions of the sort you had mentioned below should perhaps be
strongly
considered again. Non-action or slow action here could be extremely damaging.
I'll forward you some emails which will indicate the damage that the publication
has
already caused.
Thanks very much for all your help w/ this to date, and for anything additional
you may be
able to do in this regard to move this forward.
best regards,
mike

At 06:47 PM 4/16/2003 +0100, you wrote:

Dear Co-Review Editor,
You may or may not have seen/read the article by Soon and Baliunas (from the
Harvard
Smithsonian Astrophysics Lab) in the Jan 31 2003 issue of CR (vol.23,2). A
variant of
this analysis has just been published in the journal Energy and Environment.
The
authors/editor made a big media campaign to publicise this work, claiming it
showed
clearly the Medieval Warm Period was warmer than the 20th century and that the
IPCC (and
other) analysis claiming the 20th century was the warmest in the last
millennium was
plain wrong. In the UK, the Sunday Telegraph ran the story.
I have followed some email discussion about this amongst concerned
paleoclimate experts
here at UEA, in the USA and in Oz and NZ and there is overwhelming consensus

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that the

Soon and Baliunas work is just crap science that should never be passed peer review (for

a flavour see Mike Mann, Phil Jones and Barrie Pittock below). These paleo-experts have

decided it is not worth a formal scientific response since the story has not run that

widely in the mass media (although is now used by sceptics of course to undermine good

science) and that the science is so poor it is not worth a reply.

The CR editor concerned is Chris de Freitas and I have followed over the years papers in

CR that he has been responsible for reviewing. [Wolfgang Cramer resigned from CR a few

years ago over a similar concern over the way de Freitas managed the peer review process

for a manuscript Wolfgang reviewed].

Whilst we do not know who reviewed the Soon/Baliunas manuscript, there is sufficient

evidence in my view to justify a "loss of confidence" in the peer review process

operated by the journal and hence a mass resignation of review editors may be warranted. This is by no means a one-off - I could do the analysis of de

Freitas's manuscripts if needbe.

I am contacting the seven of you since I know you well and believe you may also have

similar concerns to me about the quality of climate change science and how that science

is communicated to the public. I would be interested in your views on this course of

action - which was suggested in the first place by me, once I knew the strength of

feeling amongst people like Phil Jones, Keith Briffa, Mike Mann, Ray Bradley, Tom

Crowley, etc. CSIRO and Tyndall communication managers would then think that a mass

resignation would draw attention to the way such poor science gets into mainstream

journals.

Of course, we would need to be sure of our case and to argue on grounds of poor conduct

of peer review (I can forward a devastating critique of the Soon/Baliunas method from

Barrie Pittock if you wish) rather than on disagreeable content of one manuscript. CR

does of course publish some good science, but the journal is not doing anyone a service

by allowing crap science also to be published.

Thoughts please,

Mike

FROM MIKE MANN

Dear all,

Phil relayed this message to me--this echos discussions that others of us here have had

as well, and at Phil's request, I'm forwarding some of these (Phil seems to have deleted

them). I am encouraged at the prospect of some sort of action being taken.

The "Energy and Environment" piece is an ad hominem attack against the work of several

of us, and could be legally actionable, though I don't think its worth the effort. But

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more problematic, in my mind, is the "Climate Research" piece which is a real challenge to the integrity of the peer-review processes in our field. I believe that a boycott against publishing, reviewing for, or even citing articles from "Climate Research" is certainly warranted, but perhaps the minimum action that should be taken. A paper published there last year by a University of Virginia "colleague" of mine who shall remain nameless contained, to my amazement, an ad hominem attack against the climate modeling community, and the offending statement never should have seen the light of day (nor should have any of the several papers of his which have been published there in recent years, based on quality and honesty standards alone). A formal statement of "loss of confidence" in the journal seems like an excellent idea.

It may or may not be useful for me to be directly involved in this, given that I am a primary object of attack by these folks. However, I'm happy to help in any way that I can, and please keep me in the loop.
best regards,
Mike Mann

FROM PHIL JONES

Dear All,

There have been a number of emails on these two papers. They are bad. I'll be seeing Hans von Storch next week and I'll be telling him in person what a disservice he's doing to the science and the status of Climate Research.

I've already told Hans I want nothing more to do with the journal. Tom Crowley may be writing something - find out also next week, but at the EGS last week Ray Bradley, Mike

Mann, Malcolm Hughes and others decided it would be best to do nothing. Papers that respond to work like this never get cited - a point I'm trying to get across to Hans.

We all have better papers to write than waste our time responding to drive1 like this.

Cheers

Phil Jones

FROM BARRIE PITTOCK

Dear Jim,

Thanks for your comments and suggestions. I hope the co-editors of 'Climate Research' can agree on some joint action. I know that Peter Whetton is one who is concerned. Any action must of course be effective and also not give the sceptics an excuse for making de Freitas appear as a martyr - the charge should surely be not following scientific standards of review, rather than publishing contrarian views as such. If a paper is contested by referees that should at least be stated in any publication, and minimal standards of statistical treatment, honesty and clarity should be insisted on. Bringing the journal and publisher into disrepute may be one reasonable charge. 'Energy and Environment' is another journal with low standards for sceptics, but if my recollection is correct this is implicit in their stated policy of stirring different points of view - the real test for both journals may be whether they are prepared to publish refutations, especially simultaneously with the sceptics' papers so that readers are not deceived.

On that score you might consider whether it is possible to find who de Freitas got to review various papers and how their comments were dealt with. I heard second hand that Tom Wigley was very annoyed about a paper which

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gave very low projections of future warmings (I forget which paper, but it was in a recent issue) got through despite strong criticism from him as a reviewer.

Cheers,
Barrie Pittock.

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4993. 2003-05-16

date: Fri, 16 May 2003 16:13:10 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Fwd: Re: [Fwd: Harvard-Smithsonian Climate study]
to: m.hulme@uea.ac.uk
Date: Mon, 12 May 2003 11:41:55 -0600
From: Jerry Meehl <meehl@ucar.edu>
X-Mailer: Mozilla 4.79 [en] (Windows NT 5.0; U)
X-Accept-Language: en
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: [Fwd: Harvard-Smithsonian Climate study]
X-Spam-Status: No, hits=-7.1 required=5.0
tests=FWD_MSG,QUOTED_EMAIL_TEXT,REFERENCES,SPAM_PHRASE_02_03,
USER_AGENT_MOZILLA_XM,X_ACCEPT_LANG
version=2.41

X-Spam-Level:

Mike,

Thanks! It never ceases to amaze me what tactics the naysayers come up with--this latest, using what would appear to be a quasi-legitimate "journal" to publish results that they then claim are peer-reviewed and mainstream to launch a disinformation campaign, is very devious. Plus it appears they have won--the current administration is on their side--but they keep it up anyway. Bizarre.

Johannes Loschnigg (the AMS congressional fellow I mentioned) may contact you directly if he needs more ammunition in his capacity of climate person assigned to deal with these issues in Liebermann's office.

Thanks again!

Jerry

"Michael E. Mann" wrote:

>

> HI Jerry,

>

> This is crap of the worst kind--it was written explicitly for
> political purposes; there is no science there at all--the mainstream
> media completely ignored it, having figured that out, but various
> right-wing groups (such as "Western Fuels Association") have continued
> to try to promote this in fringe media circles and through political
> channels within Washington DC (so the story continued to appear on web
> sites like "Techcentralstation" and Murdoch-supported newspapers).

>

> I'll forward a whole bunch of emails (in confidence) that should
> clarify the situation. We've all decided that this is so bad a direct
> response cannot even be made. Phil Jones and I, and Ray Bradley, Henry
> Diaz, and Malcolm Hughes are writing two review papers which will
> dismiss much of what they've said.

>

> please feel free to contact me for more information,

cg2003

>
> cheers,
>
> mike
>
> p.s. NYT, Scientific American, and a few other journalists are working
> on exposes of Baliunas and co., and those should appear soon. It
> looks like Chris Defrietas, the editor at "Climate Research"
> responsible for publishing the Baliunas et al piece, and numerous
> other dubious other awful articles by "skeptics" over the past couple
> years, will be dismissed as a result of this latest act.

>
> At 09:37 AM 5/12/2003 -0600, you wrote:

>
> > Hi Mike,
> >
> > I am starting to get media calls on this study:

> >
> > >
> > > [1]<http://cfa-www.harvard.edu/press/pr0310.html>
> > >

> >
> > Given the authors' political motivations, I have doubts about it,
> > but I
> > am sure you must be involved with the critique that will emerge from
> > the
> > scientific community, and may be getting calls yourself. They claim
> > they have surveyed many sources and have proved that the 20th
> > century
> > hasn't been as warm as the Medieval warm period, or something like
> > that.
> > The obvious direction they are taking this is that the warming we
> > have
> > seen in the 20th century is not such a big deal or even unique in
> > the
> > past 1000 years, in obvious contradiction to your work and the IPCC
> > conclusions. So I was curious how you have been responding to
> > people
> > calling you about this study. Also, the AMS congressional fellow in
> > Liebermann's office, Johannes Loschnigg, is getting questions about
> > it
> > and I may put him in contact with you to help him out.

> >
> > Thanks!

> >
> > Jerry

>

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> 982-2137

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1594. 2003-05-18

cc: <marshdz@bss1.bham.ac.uk>, <sustainable_technologies@sussex.ac.uk>,
<m.hulme@uea>
date: Sun, 18 May 2003 14:28:36 +0100
from: "Trevor Davies" <T.D.Davies@uea.ac.uk>
subject: wind turbines
to: <d.toke@bham.ac.uk>

Dear Dr Toke,
CONFIDENTIAL

I am Dean of the School of Environmental Sciences at UEA, and involved in a Community Carbon Reduction Project (CRed) - which is about to be officially launched & is aiming to build a community which will reduce CO2 emissions by 60% by 2025. CRed is partnered by the Norwich City, Norfolk County Council, District Councils, as well as businesses etc. An important objective is to get the "ordinary community" on board - & this is going very well. It is a real-world research experiment & - at the same time - a demonstration and awareness raising project. The research element is identifying, & how to overcome, obstacles to the adoption of low carbon practices. There are close links with the national Tyndall Centre for Climate Change Research (headquartered in this School, & which has the brief for identifying sustainable solutions for climate change). The Tyndall Centre is funded by ESRC/NERC/EPSC/DTI. As part of CRed activity we have been having discussions with a commercial organisation, & have - in principle - agreed the construction of a wind farm at the edge of the City (2 on the campus in District Council territory, and 1 in the City on City-owned ground - 4.5MW in total). If it goes ahead this will represent the first wind farm so close to the edge of a city- & testing the notion that turbines might be more acceptable where there are already built structures. If things go as expected a planning application will be made in the next few weeks. Given CRed, and the fact that all the relevant Councils as well as many others in the community are enthusiastic backers of CRed, & the particular character of UEA (a very strong record in environmental research, especially related to climate change) one might assume that conditions are more favourable for agreement over wind turbines than in many other cases. Yet even ahead of any public discussion, let alone a formal planning application, it is clear that all the underlying factors which you identify in your Project Brief note are at play. I, being a mere physical scientist, have been surprised & fascinated at the same time at the appearance of these factors, & how (as you indicate) they are wrapped round by the words "visual impact". Since we have just started on this

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possible wind farm venture (altho the "other factors" you identify have immediately kicked-in), & given the local circumstances (CRed, Tyndall Centre, etc), it occurs to us that this might be an excellent real-time case study for your programme. This would be able to provide the element of simultaneous observation, & indeed experimental participation, which would be lacking in the analysis of past cases. If you think this is a sensible idea, it would mean having to move quickly to get the relevant resources in place. Any argument you may wish to make to ESRC might be include an argument along the added value line because of the ESRC's existing substantial involvement in the Tyndall Centre & indeed in the School's Centre for Social and Economic Research on the Global Environment- which hosts the ESRC funded programme on environmental decision making. So we do have some considerable local presence which could help support your involvement from Birmingham. I have cc'd this to the STP Office at Sussex. Mike Hulme (Exec Director Tyndall Centre) alerted me to this possibility & so would, I'm sure, support any case to ESRC, as would Kerry Turner (Director of CSERGE). The major commercial organisation we are dealing with are piloting a number of things with us & so, I am sure, would provide the fullest information to this possible study.

Best Wishes
Trevor Davies

Professor Trevor D. Davies
Dean
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
Tel +44 (0)1603 592836
Fax +44 (0)1603 593792

2104. 2003-05-19

cc: "p.jones" <p.jones@uea.ac.uk>, mann@virginia.edu
date: Mon, 19 May 2003 16:35:04 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Fwd: Problem with the review process at Climate Research
to: f034 <C.Goodess@uea.ac.uk>, Clare Goodess <C.Goodess@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>
From: harvey@cirque.geog.utoronto.ca
To: mann@virginia.edu, trenberth@ucar.ncar.edu, wigley@ucar.edu, jhansen@giss.nasa.gov, jto@u.arizona.edu, simon.shackley@umist.ac.uk, Robert wilby <rob.wilby@kcl.ac.uk>, tim.carter@vyh.fi, p.martens@icis.unimaas.nl, peter.whetton@dar.csiro.au, c.goodess@uea, Mike Hulme <m.hulme@uea.ac.uk>, p.jones@uea, PITTOCK Barrie <abp@dar.csiro.au>, a.minns@uea.ac.uk, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, j.salinger@niwa.co.nz, simon.torok@csiro.au, harvey@geog.utoronto.ca
Illegal-Object: Syntax error in To: address found on bureau6.utcc.utoronto.ca:
To: N.W.Arnell<N.W.Arnell@soton.ac.uk>
^-missing end of address
Date: Tue, 22 Apr 2003 13:12:19 -0400

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Subject: Problem with the review process at Climate Research
Reply-to: harvey@cirque.geog.utoronto.ca
Priority: normal
X-mailer: Pegasus Mail for win32 (v3.01b)
X-MIME-Autoconverted: from Quoted-printable to 8bit by
multi-proxy.evsc.Virginia.EDU id
h3MHF3105779

Dear All:

Tom Wigley forwarded to me recent correspondence over concerns about another bad paper that was published in Climate Research (by Baliunus), and suggested that I forward a copy of an email that we (after some procrastination) have just sent to de Freitas (see below).

I might add that we were both independently informed that the reviewer's "were split". Since both Tom and I were strongly against publishing the paper, that implies that there were four reviewers, which of course is highly unusual. We have chosen not to raise questions about that at this time.

Regards

Danny Harvey

Dear Dr. de Freitas:

We have discovered that we were both reviewers of the paper

Revised 21st

century temperature projections by Michaels et al. recently published in

your journal (vol. 23, pp. 19, 2002). In our reviews, we both judged the

paper to be in category d (Publication not recommended) because of

numerous flaws in the arguments, which we carefully documented.

We now see that the paper has been published almost without alteration

from the original submission, except for a few added paragraphs that

either do not address or inadequately address the main objections that we

raised. The revised manuscript was apparently not subjected to re-review

at least not by us. We find this to be most unusual even if the authors presented a counter-argument to each of our objections, it is the

normal procedure among reputable journals for the authors reply to be

forwarded to the original reviewers for further comment.

We note in this regard that even under the less damning evaluation category c (Revise and re-submit for additional review), responses and

revisions should be sent back to the original referees. Your decision that

a paper judged totally unacceptable for publication should not require re-

review is unprecedented in our experience.

We therefore request that you forward to us copies of the authors responses to our criticisms, together with: (1) your reason for not sending these responses or the revised manuscript to us; (2) an explanation for your judgment that the revised paper should be published in the absence of our re-review; and (3) your reason for failing

to follow accepted editorial procedures.

Yours truly,

Danny Harvey and

Tom Wigley

cg2003
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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4159. 2003-05-19

cc: "p.jones" <p.jones@uea.ac.uk>, mann@virginia.edu
date: Mon, 19 May 2003 16:34:57 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: RE: FWD: S&B in E&E
to: f034 <C.Goodess@uea.ac.uk>, Clare Goodess <C.Goodess@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>

Dear Clare,

Thanks very much for the update, and for your efforts to do something about this.

De Freitas' argument seems to amount to "well the editor at 'Energy and Environment' was even worse than me", and that doesn't quite hold water.

As de Freitas apparently seeks to distance himself from culpability, please keep in mind

that this is only one of numerous past complaints of suspicious and apparently unethical

behavior on his part in association with his position at "Climate Research". I'm forwarding, under separate cover, an email describing a complaint from Danny

Harvey and Tom

wigley.

I, as well as many other of our colleagues, look forward to hearing what happens here.

thanks again for your help.

best regards,

mike

At 08:58 PM 5/19/2003 +0100, f034 wrote:

Mike

Hans and I have already raised this issue with Inter Research, but they havent taken it up yet.

Hans and I have have contacted de Freitas and InterResearch over the issues that you and others have raised before. One of the things de Freitas said in response, was that he had contacted the editor of Energy and Environment to see why it had been published. The editor told him that it deserved 'a less interferedwith version' , i.e., the original authors had complained about the changes required by the CR reviewers!

Hans, InterResearch and I are still discussing what action needs to be taken and how to respond to de Freitas' inititial responses. I will ensure that all those who have expressed concerns to me and/or Hans/Mike Hulme are informed of the outcome.

Best wishes, Clare

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

374. 2003-05-20

cc: mann@virginia.edu
date: Tue, 20 May 2003 10:28:26 -0400

cg2003

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Fwd: Clivar Conference 2004

to: Keith Briffa <k.briffa@uea.ac.uk>

Hi Keith,

Thanks for getting back to me on this.

I'm confused too--perhaps you can seek some clarification from Lennart, and cc me in on

this? Copying this message would be fine...

I like the idea of gearing the theme towards quantification of natural and anthropogenic

variability over e.g. past 1-2 millennia based on proxy data and model/data comparison.

We could incorporate perhaps a few number of the "usual suspects" (say just Phil and Ray),

and I would want to include Drew Shindell and/or Gavin Schmidt, along w/ Caspar Ammann

and/or Simon Tett--Tom Crowley would be logical to include too, unless you say any sort of

conflict here.

In my experience, Von Storch can be difficult, to say the least, and I would just as soon

avoid including him (having tried my best to jointly organize workshops and special

sessions with him in the past, and with limited success at best).

Will await further word from you,

mike

At 02:57 PM 5/20/2003 +0100, Keith Briffa wrote:

Mike

Lennart has managed to confuse me with his latest message. At one point he mentioned

that you and I would do a joint overview paper . Now he suggests we choose 5-10

co-authors but also refers to "other people in our section" who he has apparently

already informed , need "to consult with you (ie us) as required" (my emphasis).

As for my opinion of the theme or content of our section , I suggest it be "quantifying

Natural and Anthropogenic influences on the course of Global climate during recent

millennia" or some such . This allows for the review , redefinition of Global climate

history (Southern as well as Northern , and moisture as well as Temperature).

Importantly , it also incorporates the issue of forcing history(ies) and work quantifying the influence of these histories - using simple empirical

techniques or

using them in conjunction with models of different complexity to attribute causes of

this change.

I am happy to go with the "usual suspects" in the overview paper , but would be happy if

we considered others who are also running controlled model/data comparisons (examples

are Von Storch , Simon Tett , Caspar Ammann). We need first to clarify whether we will

present one large , multi-author presentation/paper or whether it is just me and you and

the others divided into other papers/presentations/posters. Should we copy this message

to Lennart or contact him directly with specific questions?

Keith

At 09:49 PM 5/18/03 -0400, you wrote:

Hi Keith,

cg2003

I hope all is well.

Apparently, we're supposed to choose 5-10 additional "co-authors"? I guess the obvious ones would be Phil, Tim, Ray, Malcolm, perhaps Ed Cook, Scott Rutherford,...any other suggestions?

As I understand it, the co-authors would be invited to attend and present in the poster session; I assume they are listed separately from you and I who will jointly present the oral overview. As for the theme, I'm assuming "climate changes of the past couple/few millennia" or something like that. As we have 45 minutes total between the two of us, I would suggest we each take about 20 minutes, and then we'll have 5 minutes left for questions.

Any suggestions, thoughts would be greatly appreciated.

thanks,

mike

X-Sender: m214001@regen.dkrz.de

Date: Sun, 18 May 2003 22:53:58 +0200

To: k.briffa@uea.ac.uk, mann@virginia.edu

From: "Prof. Dr. Lennart Bengtsson" <bengtsson@dkrz.de>

Subject: Clivar Conference 2004

Cc: bengtsson@dkrz.de, kornelia.mueller@dkrz.de

--

Dear Dr. Mann,

Dear Dr. Briffa,

The preparation of the Clivar conference is progressing well and all invited speakers

have now agreed (See attached draft program). As I have informed you previously Journal

of Climate will have a special issue devoted to the Conference and I expect you would be

willing to prepare a paper to be ready at the time of the conference. I have made

arrangements with the chief editor to make a flexible interpretation of the content of

the papers so to agree with the objective of the conference and the draft program.

We would now like you to come up with a suitable theme for your presentation at the

conference as well a list of names which you have selected as co-authors. As we

anticipate a broad and forward-looking contribution I believe some 5-10 people seems

appropriate. It was our intention that the first person listed should be the lead

author but you can arrange this otherwise if you prefer to do so. I have informed the other

speakers in your section to consult with you as required.

For the conference I expect a rather wide audience in addition to a broad scientific

community including representatives from different agencies such as the meteorological

services, as well as media representatives. For the media we intend to provide a special

set of information. In view of the societal importance of the CLIVAR program and the

considerable progress in extended range forecasts and climate change assessment and

prediction I believe there will be an excellent opportunity to bring the

cg2003

scientific

progress and associated applications of CLIVAR to the participants of the conference.

It would be very helpful if you could let me know the status of your arrangements not

later than June 15. If you see any particular difficulties please let me know as soon as possible.

As you can see from the attached program each part of the conference will have poster

sessions. The poster sessions will be an important part of the conference and I

anticipate that some of your co-authors will prepare such posters. We also plan to have

the poster contents on a CD ROM prior to the conference.

The practical planning of the conference as a whole is proceeding well. The arrangements

in Baltimore are quite excellent with the nearby Baltimore inner harbor as a particular

attractive focal point. There are all reasons that the conference will be a success both

scientifically and socially. See further the Clivar Conference website:

[1]<http://www.clivar2004.org>.

We are presently exploring the possibilities for financial support of selected participants. However, any support you may manage to obtain from national

funds would be most helpful.

With my very best regards
Lennart Bengtsson

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[3]<http://www.cru.uea.ac.uk/cru/people/briffa/>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2896. 2003-05-20

cc: "Mike Hulme" <m.hulme@uea.ac.uk>
date: Tue, 20 May 2003 12:22:03 +0100
from: "Asher Minns" <a.minns@uea.ac.uk>
subject: Re: Communications strategy
to: "Samantha Jones" <Samantha.Jones@uea.ac.uk>

Dear Sam,

Here are a few short paragraphs about progress on a new comms strategy.

cg2003

Basically, there has not been much progress to date by way of producing a draft, but I hope I have given you something that gives a suitable flavour. Flag-it for specific comments when you give the draft report to Mike. Of course, our main external audience is business people, who will be entirely missing from any communication strategy that I write!

Asher

----- Original Message -----

From: "Samantha Jones" <Samantha.Jones@uea.ac.uk>

To: "Asher Minns" <a.minns@uea>

Sent: Tuesday, May 20, 2003 9:27 AM

Subject: FW: Communications strategy

>

>

> -----Original Message-----

> From: Samantha Jones [mailto:Samantha.Jones@uea.ac.uk]

> Sent: 06 May 2003 16:06

> To: Mike Hulme; Asher Minns

> Subject: Communications strategy

>

>

> Dear Mike and Asher

>

> At its last meeting on 28 November 2002, the Advisory Board recommended that

> the comments made at the meeting should be used to develop a stronger, longer term communications strategy. This should address the material to be

> communicated, to whom and by what means. The strategy should be set out in

> a draft document, which could be circulated to Board members and other relevant people for comment, before a final version was produced.

>

> The Board also recommended that the Tyndall Centre should provide an interim

> report to the Advisory Board six months after its annual meeting (i.e in May

> 2003). This should outline the progress to date on the recommendations made

> at the meeting. I am now drafting this report.

>

> Please could you provide an update on implementing the recommendation on development of the communications strategy. I would say this should be half

> an A4 page maximum. If you could send it to me by 16 May 2003, it would be appreciated. I will then include this in the interim report.

>

> Thanks Sam

>

> Samantha Jones

>

> Administrator

> Tyndall Centre for Climate Change Research

> School of Environmental Sciences

> University of East Anglia

> Norwich NR4 7TJ

>

> Tel 01603 593903

>

> <http://www.tyndall.ac.uk>

>

>

>

>

Attachment Converted: "c:\eudora\attach\progresson communicationstrategy.doc"

5328. 2003-05-22

date: Thu, 22 May 2003 15:14:01 -0400
 from: Scott Rutherford <srutherford@gso.uri.edu>
 subject: Re: New tree-ring density data
 to: Tim Osborn <t.osborn@uea.ac.uk>
 <x-flowed>

Tim,
 When you have a moment can you send me the lat/long for the 115 MXD series? I've ended up with two datafiles that are slightly different and I just want to make sure I've got things straight. I'm almost done revising the manuscript. It took me much longer than I thought do to other commitments rearing their heads. Look for it in the next week or so.
 Hope all is well.

Regards,

Scott

On Wednesday, November 8, 2000, at 07:45 AM, Tim Osborn wrote:

> Scott & Mike,
 >
 > Keith and I have created a new gridded tree-ring density dataset, by
 > superimposing (as we discussed before I left, Mike) additional
 > low-frequency temperature variability from the age-banded regional
 > timeseries on to the existing gridded tree-ring density dataset that
 > had
 > been traditionally standardised and was therefore lacking in low
 > frequency
 > variance. I've put the new dataset onto holocene for you to pick up
 > (/users/tosborn/data/schweingruber_mxdabd_grid.dat.gz). Once you have
 > gunzip'd it, you'll see that the format is the same as before: columns
 > are
 > the 115 grid boxes, rows are the 595 years from 1400-1994. These data
 > are
 > actually our calibrated data (deg C anomalies wrt 1961-90), though you
 > should make them dimensionless by normalising with their 1900-1960
 > mean and
 > standard deviation prior to putting them through the Tapio Schneider
 > regularized EM process. And of course, set all post-1960 to missing.
 > The
 > missing code in the file is -9.99.
 >
 > Although the two Briffa et al. papers that I left with you are the main
 > references to use for the data set and for the regional-mean
 > reconstructions (the Holocene paper for the standardised ones and the
 > JGR
 > paper for the age banded ones), the gridding, the calibration of the
 > gridded data set and the incorporation of the low-frequencies into the
 > gridded data will all be written up in a different paper. The
 > provisional
 > reference for this is:
 > Osborn TJ, Briffa KR, Jones PD and Schweingruber FH (2000)
 > Reconstructing
 > summer temperature over the Northern Hemisphere since AD1400 from a
 > tree-ring network. In preparation.
 > So that's the one to use if you wish to cite the gridded datasets.
 >
 > How are the imputations going with the standardised gridded data?
 >
 > Best regards
 >
 > Tim

cg2003

>
>
> Dr Timothy J Osborn | phone: +44 1603 592089
> Senior Research Associate | fax: +44 1603 507784
> Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
> School of Environmental Sciences | web-site:
> University of East Anglia | http://www.cru.uea.ac.uk/~timo/
> Norwich NR4 7TJ | sunclock:
> UK |
> http://www.cru.uea.ac.uk/~timo/sunclock.htm
>
>
>

Scott Rutherford
Marine Research Scientist
Graduate School of Oceanography
University of Rhode Island
e-mail: srutherford@gso.uri.edu
phone: (401) 874-6599
fax: (401) 874-6811
snail mail:
South Ferry Road
Narragansett, RI 02882
</x-flowed>

2416. 2003-05-23

date: Fri, 23 May 2003 10:19:17 -0600
from: Dale Kellogg <dkellogg@ucar.edu>
subject: AR4 Cross-Cutting Theme on Uncertainty and Risk
to: m.hulme@uea.ac.uk
<x-rich><excerpt><color><param>0000,0000,0000</param>Dear Dr. Hulme:
As part of its planning for the Fourth Assessment (AR4) the IPCC has
agreed to consider a number of cross-cutting themes, each of which is
a possible cross-cutting focus for consideration by some of the
authors in more than one of the Working Group reports.

The themes being considered are:

1. Uncertainty and risk
2. Regional integration
3. Water
4. Key vulnerabilities (including issues relating to
Article 2 of the UNFCCC)
5. Adaptation and mitigation
6. Sustainable development
7. Technology

Scoping papers are now being prepared to elaborate how these themes
might be used in the assessment. These scoping papers are expected to
be from 5 to 20 pages in length and are to be reviewed by a small
number of experts before being considered further at the Second
Scoping meeting for the AR4 in September.

We would be very grateful if you would agree to act as a reviewer for
the scoping paper on the Uncertainty and risk theme. The review period
will be from the first week of June to June 27th.

Given the potential importance these themes to the assessment process
we believe it is important that they be subject to careful review. It
would be especially helpful if your review would help us to evaluate
the likelihood that such a theme would be useful in cutting across
Working Group reports, and provide any requirements that you may see
for ensuring a sound, scientific basis for such cross-cutting. We
believe that you can aid us in carrying out such a careful review and
in doing so you would be helping us to improve the quality of the
report.

cg2003

Could you please confirm by May 30 whether you are able to assist us with this review.

Regards

Susan Solomon and Qin Dahe, co-chairs, WGI</color>

</excerpt>

<fontfamily><param>Gill

Sans</param><color><param>0000,0000,8080</param>=====

IPCC WG1 TSU

303.497.7072

NOAA Aeronomy Laboratory

325 Broadway DSRC R/AL8

ipcc-wg1@a1.noaa.gov

Boulder, CO 80305

</color></fontfamily>

</x-rich>

Phone:

Fax: 303.497.5686

Email:

4800. 2003-05-23

date: Fri, 23 May 2003 11:32:11 +0100

from: "Peter Rawlinson" <prawlinson@btopenworld.com>

to: <m.hulme@uea.ac.uk>

Hi Mike. Sorry for slow response, only just read this message on the Toyota site, which they are closing down BTW.

There are 200 or less Prius sold before March 2001. Mine was number 188 and I bought it 3 weeks before the deadline. Why not join us on the uk group at:

http://autos.groups.yahoo.com/group/prius_uk/

Cheers

Peter Rawlinson

Reply from Mr M Hulme | Mon, 14 Apr 2003 19:43

Re: Pre March 2001 Prius's - read this !

I am a pre-March 2001 owner and am aware of this anomaly. How many of us are there? Only Toyota will know so I suggest we ask Toyota to lobby DVLC on our behalf.

Mike Hulme

m.hulme@uea.ac.uk

277. 2003-05-29

cc: geoff.jenkins@metoffice.com, "Wilkins, Diana (GA)"

<Diana.Wilkins@defra.gsi.gov.uk>

date: Thu May 29 10:24:52 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: RE: response to Hans Verolme

to: "Warrilow, David (GA)" <David.Warrilow@defra.gsi.gov.uk>,

"Hans.Verolme@fco.gsi.gov.uk" <Hans.Verolme@fco.gsi.gov.uk>,

simon.brown@metoffice.com, "Johnson, Cathy (GA)" <Cathy.Johnson@defra.gsi.gov.uk>,

"John Schellnhuber (E-mail)" <h.j.schellnhuber@uea.ac.uk>, "Martin Parry (E-mail)"

<parrym1@aol.com>

David (and others),

My quick answer to this would include the following:

- there is clear evidence that some types of extreme weather in some regions of the world

are increasing; this is the solid conclusion reached in Chapter 2 of TAW WGI -

so Jerry

Taylor from Cato Institute is wrong; (but this is not a mandate to say we are seeing

increases in all types of extreme weather everywhere);

- there is reasonably well founded basis for claiming that at least some of these extreme

weather changes are associated with planetary warming;

- whether emerging and future changes pose "catastrophic" risks for poor citizens is more

cg2003

of a value statement than the result of careful scientific analysis; poor citizens are currently exposed to what many people would regard as unacceptable climate hazards - destabilising world climate will certainly add to these risks unless adaptive measures are implemented;

- the argument about rising damages over the last 20-30 years (cf. M-R report and others) I think says more about the insurance industry than it does about climate change (i.e., I would not use these data as the primary basis for judging whether extreme weather was changing); it is very difficult to pull out the climate signal from such data and even harder to pull out the anthropogenic climate signal (and also to extrapolate such curves out to 2060 and claim, as some have done, that we then face climate damage of 50% of GWP is not wise);
- this issue is pertinent to questions of what is dangerous climate change - in a sense what is important about the exponentially rising damage curves from the insurance people is what it reveals about our exposure to climate risk and how we try to protect (insure) against that risk and hence our expectations about how climate (and hence climate change) impacts on our lives and well-being; this curve suggests therefore a different way of approaching dangerous climate change - not in a formal scientific sense of attributing cause and effect but in the sense that experience and expectation are powerful drivers of perception, things that are all wrapped-up in any definition of "danger".

Mike Hulme

At 11:33 28/05/2003 +0100, Warrilow, David (GA) wrote:

Hans

I don't think there is a quick answer. There is evidence that extremes are getting larger and Hadley Centre is working on this. Their quantification on the driver side would be useful. The damage end is undoubtedly more difficult as there are several factors to assess 1) increased frequency of extremes, 2) changes to planning policy or practice, leading to increased exposure and other physical changes 3) more expensive property (although M-R report is normalised at 1990 prices or some such). There may be other factors too.

I am sure Simon can help with factor (1)

I am also copying this to Martin Parry, co-chair of IPCC WG2 and Mike Hulme/John Schellenhuber at the Tyndall Centre for their comments on points (2) and (3). Their views on the M-R figures would be useful. Grateful for short replies by end of week if possible?

David

-----Original Message-----

From: Hans.Verolme@fco.gsi.gov.uk [[1]mailto:Hans.Verolme@fco.gsi.gov.uk]
Sent: 23 May 2003 20:48

cg2003

To: simon.brown@metoffice.com; Johnson, Cathy (GA)

Cc: Warrillow, David (GA); geoff.jenkins@metoffice.com

Subject: RE: Questions to ask Soon and Balianus

While causing trouble. Can you all give an authoritative view in response

to the

following quote?:

in

"It's false," said Jerry Taylor, a policy analyst at the Cato Institute

events are on

Washington, D.C. "There is absolutely no evidence that extreme weather

is a

the increase. None. The argument that more and more dollar damages accrue

reflection of the greater amount of wealth we've created."

2003 (see

This in response to the latest Worldwatch Institute report Vital Signs

there

below). How does that stack up relative to Munich and Swiss Re. views? Is

debate on

research on increased intensity and frequency of 'extreme events'? As the

issue we

adaptation v mitigation in the US becomes more alive this will be another

will be asked to comment on.

Cheers,

HANS

Poor to bear brunt of climate change -- Worldwatch

Lauren Miura, Greenwire reporter

associated with

Rising temperatures, extreme weather events and other consequences

citizens,

global climate change pose "catastrophic" risks to the world's poorest

positive side,

according to a Worldwatch Institute report released yesterday. On the

is expected

the report notes wind power generation has expanded in recent years and

increase 15-fold over the next two decades.

dozens of

The report, Vital Signs 2003, is Worldwatch Institute's annual summary of

in

economic, environmental and social trends. Researchers at the Institute,

to gauge

cooperation with the United Nations Environment Programme, use the report

year's

the health of societies around the world and the global environment. This

environmental

report focuses on poverty and its link to social, health and

have

As levels of carbon dioxide in the atmosphere climb, the report said, so

nations facing

average global temperatures -- leaving many of the world's poorest

from evenly

the brunt of the consequences. "The burdens of climate change are far

Worldwatch.

distributed," said Molly O'Meara Sheehan, a senior researcher with

scientists

For example, the report identifies erratic weather patterns -- what some

famine for

believe to be an effect of climate change -- as the primary cause of

weather-related

millions of Africans. Over the past two decades, floods and other

Bangladesh

natural disasters have prompted nearly 10 million people to migrate from

to India, creating immense population pressure.

In 2002, the report said economic damages from weather disasters were

estimated at \$53 billion, a 93 percent jump from 2001, partially because of the return of El Nino. Weather disasters were also blamed for nearly 8,000 deaths, according to the report. Such trends are likely to continue, the report says, as "scientists believe that rising global temperatures may increase the intensity and frequency of extreme weather events even more." Buildings and infrastructure in developing countries are also less likely to withstand extreme weather events, Sheehan said. Moreover, public health systems in poor countries are less able to handle emergencies, she said, meaning "those sorts of weather disasters are likely to hit them harder." Rising sea levels also pose serious threats to small island nations, the report says. Some island states that have compiled "worst-case scenarios" anticipate a 1-meter rise in sea level over the next 100 years. More immediate problems associated with rising seas include flooding, coastal erosion, coral bleaching and economic losses. "In terms of vulnerability, [island nations] are the most at risk," the report says, although "they account for less than 1 percent of global greenhouse gas emissions." Critics condemned Worldwatch's link between climate change and severe weather events. "It's false," said Jerry Taylor, a policy analyst at the Cato Institute in Washington, D.C. "There is absolutely no evidence that extreme weather events are on the increase. None. The argument that more and more dollar damages accrue is a reflection of the greater amount of wealth we've created." Wind power surges Wind power is the world's fastest-growing energy source, with an average growth rate of 33 percent between 1998 and 2002, according to the report. Natural gas, the fastest growing energy source among fossil fuels, grew at an annual rate of 2 percent. European countries led the push for wind power, particularly Germany, Spain and Denmark. Among the report's other findings: Roughly 25 percent of the world's armed conflicts in recent years have involved fights over natural resources, and virtually all of the conflicts have occurred in poor countries. There are approximately 50 million "environmental refugees" around the world, people driven from their homes by drought, floods and other environmental problems resulting from human and natural activities. World population growth has slowed, but the 49 poorest countries in the world are growing at an average of 2.4 percent per year -- nearly 10 times the

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annual growth of industrialized nations.
Worldwatch president Christopher Flavin expressed his concern that a
struggling global economy and efforts to restore peace in the Middle East will
overshadow the need to address the causes and consequences of poverty in developing
countries. "The human tragedies behind the statistics in Vital Signs 2003 are compelling
reminders that social and environmental progress are not luxuries that can be set
aside when the world is experiencing economic and political problems," he said.
See [2]<http://www.worldwatch.org/pubs/vs/2003/overview.html>
-----Original Message-----
From: Brown, Simon [[3]mailto:simon.brown@metoffice.com]
Sent: 23 May 2003 12:12
To: 'Johnson, Cathy (GA)'
Cc: 'Hans.Verolme@fco.gsi.gov.uk'; Jenkins, Geoff
Subject: RE: Questions to ask Soon and Balianus
Cathy,
re:
Myron Ebell of the Competitive Enterprise Institute in
closing intervened claiming the IPCC had recently refused to accept data
supporting Soon's argument and stating the raison d'etre of IPCC was to
prop up the FCCC. Does anyone have background on such recent exchanges with
sceptics and the IPCC?
I remember something along the lines that the National Academy of Science
was asked to determine how much the IPCC was swayed by politics and came
out with a fairly strong statement that it was clean. It might be on file...
Hans - regarding Ebell's comment on data - IPCC accepts all data which is
properly reviewed and published. If it wasn't accepted then there is a
reason.
Simon.
> -----Original Message-----
> From: Jenkins, Geoff
> Sent: 22 May 2003 09:56
> To: 'Johnson, Cathy (GA)'; 'Phil Jones'; 'Peter Stott';
> 'Hans.Verolme@fco.gsi.gov.uk'
> Cc: Brown, Simon; Tett, Simon
> Subject: RE: Questions to ask Soon and Balianus
>
> Hans
>
> Thanks for your comprehensive report of the meeting. I am glad that we
> were able to help - and that you were able to use the ammunition on the
> day.
>
> You ask a couple of questions - I have put them in red in your email so
> that others can add.
> I recall Enegry and Environment publishing un-peer-reviewed sceptical
stuff
> before. It was when David Everest (ex- Chief Scientist of DOE, who told
me
> off for being too green when I worked there!) was the editor. I wrote
to
> him to complain and he wrote back saying the editorial had made that
> plain; poor sceptics didnt get a voice etc etc. All copied to DoE, but
> maybe 5 or 6 years ago now.
>
> I would agree that the raison detre of IPCC is to support FCCC! but

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I > probably not in the way that was meant, ie it provides impartial
 > scientific evidence and doesn't support any particular policy eg Kyoto.

Phil > guess "exchanges between IPCC and sceptics" have been/will be within
 > individual chapters (eg that on detection and attribution).
 >

Simon > Re data: as far as I am aware all our data sets (and those joint with
 > Jones) are available to bona fide researchers, and can be got from
 > Tett here of Phil at UEA. Websites give info on this.
 > Phil/Simon - can you agree/disagree/expand please?
 >

estimate > Re funding: we took \$1M from a bunch of oil companies (inc EXXON) via
 > IPIECA about 10 years ago. We used it to come up with the first
 > of the second indirect cooling effect of aerosol on predictions. I have
 to > say that at no time did we come under any even slight pressure to get
 us > to say or omit anything in papers we wrote. Of course in Soon's case
 they > already knew where he stood, so I guess could be confident that he
 would > use their money to come up with more sceptical stuff.
 >

information > Peter, Simon (and Phil) - thank you for helping DEFRA/FCO with
 > and comments etc.
 >
 > Best wishes
 >
 > Geoff
 >
 >
 > -----Original Message-----
 > From: Johnson, Cathy (GA) [SMTP:Cathy.Johnson@defra.gsi.gov.uk]
 > Sent: 21 May 2003 09:20
 > To: 'Phil Jones'; 'Peter Stott'
 > Cc: 'Geoff Jenkins'
 > Subject: FW: Questions to ask Soon and Balianus
 >
 > Peter and Phil
 > see message from Hans - to which I add defra's thanks!
 > Cathy
 > -----Original Message-----
 > From: Hans.Verolme@fco.gsi.gov.uk
 > [[4]mailto:Hans.Verolme@fco.gsi.gov.uk]
 > Sent: Tuesday, May 20, 2003 10:59 PM
 > To: Johnson, Cathy (GA)
 > Cc: Warrilow, David (GA); Noguer, Maria (GA);
 > Christian.Turner@fco.gov.uk; Jonathan.Temple@fco.gov.uk
 > Subject: RE: Questions to ask Soon and Balianus
 >
 >
 >
 > Cathy, please pass to Hadley / UEA
 >
 > All,
 > Thank you, in particular to Peter Stott at the Hadley Centre and
 > Phil Jones at U. East Anglia, for the excellent speaking points for the
 > briefing by Dr. Willie Soon of the Harvard-Smithsonian Center for
 > Astrophysics organized by the climate sceptic Marshall Institute. The

event
 Senator
 came
 Marshall
 that
 record
 the
 O'Keefe,
 not
 retreating
 on
 concern,
 climate-closet
 the
 if
 provided
 had
 refused

> went well, if maybe not the way the organizers and their sponsor,
 > George Allan (R-Virginia), had expected.
 >
 > The audience if not already firmly in the sceptic camp likely
 > away with little confidence in the scientific credibility of the
 > Institute and the work of Dr. Soon.
 >
 > The presentation consisted of a jumble of over 40 transparencies
 > showing various temperature records from around the globe, most of them
 > pre-instrumental proxies. Dr. Soon presented them as evidence of the
 > occurrence of a medieval warm period and a little ice age and argued
 > the present-day instrumental record compared against the historical
 > provided no evidence of 20th century warming.
 >
 > During the Q&A that followed, Soon quickly conceded the
 > synchronicity point saying further research was needed.
 >
 > Greenpeace then challenged Soon on the issue of peer review and
 > Marshall Insitute on its sources of funding (which include ExxonMobil).
 > Soon responded the article had been published by the "Journal of Energy
 > and Environment." (Any views on the status of the Journal?). Bill
 > the president of the Institute, stated ExxonMobil's contribution had
 > influenced the research in any way.
 >
 > A question about present climate change impacts such as
 > glaciers and decreases in sea ice thickness was partly ignored, partly
 > portrayed as requiring significant further research. Even so, Soon went
 > to say, paleo-records show increased CO2 levels should not be of
 > double the present levels had occurred. He came out of the
 > and people perked up.
 >
 > I took a gentler initial approach, drawing people's attention to
 > endorsement by the National Academy of Sciences of the IPCC TAR and
 > President Bush' acceptance of that view. Soon responded by saying most
 > not all of his data were published post-TAR.
 >
 > Noting the IPCC acknowledged uncertainties and degrees of
 > confidence, I explained how these were not grounds for inaction. Soon
 > responded they were not uncertainties but unknowns and therefore
 > no basis for action. (A point lost on most of the audience from my
 > reading).
 >
 > Soon got nervous when I asked him about the manner in which he
 > chosen to represent other peoples data, such as Tom Crowley's. He
 > to answer the question and asked me to discuss it outside the meeting.

He
spot
presentation
Sherry
unnecessarily
concur.
instrumental
knowing
comments

> claimed he was "merely a synthesizer." You seem to have found a weak
> here, keep at it. He further said I was misunderstanding his
> of the data on the medieval warm period and little ice age. Some in the
> audience audibly disagreed. Your points were well taken.
>
> Myron Ebell of the Competitive Enterprise Institute in closing
> intervened claiming the IPCC had recently refused to accept data
> supporting Soon's argument and stating the raison d'etre of IPCC was to
> prop up the FCCC. Does anyone have background on such recent exchanges
> with sceptics and the IPCC?
>
> The meeting disbanded in a somewhat disorganized manner. Mission
> accomplished.
>
>
> Follow-up
>
> Jeff Nesmith of the Cox Newspapers group is working on a piece
> exposing the sceptics. We agreed to speak.
>
> Staff in Rep. Bart Gordon's office (D-Tennessee) told me Rep.
> Boehlert (R-NY chair of the Science Cie.) had persuaded Rep. Mark Udall
> (D-Colorado) not to add a climate amendment to recent legislation.
> Boehlert who is an ally and expert politician said it would
> antagonize the House leadership and stood no chance of passing. I
> We agreed to stay in touch.
>
> Bill O'Keefe was eager to gain access to further recent
> temperature data we hold. Would you consider his request for data
> they will likely be spun?
>
> Finally, Ian Murray, a former UK Dept. for Transport official is
> joining the Competitive Enterprise Institute.
>
>
> Thanks for enlivening up my Friday.
> HANS
>
> -----Original Message-----
> From: Johnson, Cathy (GA) [
<[5]mailto:Cathy.Johnson@defra.gsi.gov.uk>]
>
> Sent: 15 May 2003 04:36
> To: 'Hans Verolme'
> Cc: Warrilow, David (GA); Noguer, Maria (GA)
> Subject: Questions to ask Soon and Balianus
>
>
> Dear Hans
> I am in the branch of GA Division covering Climate Science, and I
> have
> received from Peter Stott at the Hadley Centre the attached
> on Soon
> and Balianus' "paper"; they include three questions you could

cg2003

ask.

>
> I hope all this makes sense to you, but I will be glad to discuss
> them with
> you if you wish. I will be in the office until 17.45 UK time
today,
> my
> direct line is 44 (0)20 7944 5226 and my colleague Maria will be
here
>
> tomorrow on ext. 5437.
> Alternatively Peter Stott's number is 44 (0)1344 854011
> Good luck, we'll be interested to know how you get on!
>
> best wishes
> Cathy
> <<Stott_Soon_comment.doc>>
>

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2231. 2003-06-02

cc: a.minns
date: Mon Jun 2 15:57:26 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Fwd: Winning Arguments event, 25 June 2003, Commonwealth Club
to: shackley_Simon

Simon,
Frans has put this event together during social science week. I've been
thinking of what
Tyndall examples we could give of either "success" or "failure" form Tyndall
work.

I did wonder whether your work on area-based decarbonisation for the SDC might
be seen as a

success - some of your ideas did seem to be taken forward by the SDC. Do you
think this is

valid? And if so, would you be prepared to join the event in London on the
evening of 25
June?

If you don't think the story adds up to what Frans is after, do you have other
ideas of
successes or failures?

Thanks,

Mike

To: "Mike Hulme" <m.hulme@uea.ac.uk>,
"John Schellnhuber" <h.j.schellnhuber@uea.ac.uk>,
"Asher Minns" <a.minns@uea.ac.uk>,
"Kerry Turner" <r.k.turner@uea.ac.uk>,
"Jouni Paavola" <j.paavola@uea.ac.uk>,
"Ken Peattie" <Peattie@cardiff.ac.uk>,
"Bob Lee" <LeeRG@Cardiff.ac.uk>,
"Paul Ekins" <p.ekins@psi.org.uk>

Cc: "Michelle Harris" <m.harris@sussex.ac.uk>

Subject: Winning Arguments event, 25 June 2003, Commonwealth Club

cg2003

Date: Tue, 27 May 2003 18:18:54 +0100

X-Mailer: Microsoft Outlook Express 6.00.2600.0000

From: F.Berkhout@sussex.ac.uk (Frans Berkhout)

Dear all

We have now put together an outline of the evening reception for ESRC Social Science Week.

Our aims have been to create an event that gives an impression of the scope of ESRC investments in the area of environment and sustainability and which produces some interaction between people. We wanted to avoid lengthy expositions of what programme and centres were doing, but also wanted to give research leaders a change to pick out some research highlights. We wanted a mixture of short, informal interventions at the start, followed by a relaxed exchange of views over wine and canapes. The overall objectives are to signal to policymakers that there is a substantial social science research effort in this general area, to encourage some debate about how research can speak to policy more effectively, and to help create a few new contacts between people.

The structure we have come up with is to have an exchange of topical 'war stories' about research interacting with and influencing policy at the start of the event (told by both researchers and policymakers), followed by an open discussion and then some informal and private networking. The capacity of the Commonwealth Club for this sort of event is 80 people, and I guess the ideal mix would be about 50 researchers and about 30 policy types.

The Chief Executive of the ESRC, Ian Diamond, will start the event off, and I am still trying to line up a couple of senior DTI/DEFRA speakers. I am hoping that you will all be able to come up with short remarks describing either a success in communicating with policymakers (whether in the UK or elsewhere) or a failure (good research which you felt never found an interested audience) and to draw one or two lessons from this experience.

Policymakers will be encouraged to talk about cases where they have drawn on research, or about others where the evidence base was missing. The idea is that the discussion that follows (probably moderated by Ian Diamond) will produce some ideas about what works and what doesn't.

A rough order of play would be:

5.30-6.00	Arrival
6.00-6.45	Ian Diamond, 5 Programme and Centre Directors and 2 policymakers
6.45-7.15	Open discussion
7.15-8.00	Networking
8.00	Close

The idea of the title is that it is eye-catching and has both pragmatic (how to?) and rhetorical (what is?) meanings.

I would appreciate your feedback on this. In particular, I would be interested

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to know

whether you are happy with the overall aims and programme, and whether you are willing

to do one of the short talks at the beginning. I think the aim should be to say

something pithy and fairly off-the-cuff to stimulate responses and discussion.

We are planning to send out invitations this week and are assuming that most of the

participants will be either London- or 'near London'-based. ESRC are being very helpful

in providing lists of possible invitees from Westminster departments, and a number of

you have provided lists of others. If you have lists of potential invitees to the event

- especially researchers involved in your Programmes and Centres, that would be

extremely helpful. We will be relying to some extent on the GEC database which is now a

several years out of date.

Hope to hear from you soon.

Frans

Dr Frans Berkhout

Director, ESRC Sustainable Technologies Programme

SPRU-Science and Technology Policy Research

Freeman Centre

University of Sussex

Brighton BN1 9QE

UK

PLEASE NOTE CHANGED ADDRESS DETAILS

t (direct): +44 1273 877 130

t (Michelle Harris): +44 1273 873 615

f: +44 1273 685 865

[1]www.sustainabletechnologies.ac.uk

[2]www.sussex.ac.uk/spru/environment

[3]www.sustainability-performance.org

2705. 2003-06-02

date: Mon Jun 2 13:49:07 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: IPCC WG2 AR4 draft outlines - WGII outline & Chapters 2 and 13

to: "Pritchard, Norah" <norah.pritchard@metoffice.com>

Dear Osvaldo and Martin,

It is very difficult to make considered input into this process at such short notice. I

received the emails Wednesday afternoon, just before being away from the office for 48

hours. I also am not fully aware of the process into which this is fitting and it is the

first time I have seen the WGII outline. I do however make some comments on the following:

The WGII outline

Chapter 2 on data etc.

Chapter 13 on critical damage etc.

WGII outline

Key Questions: there is, in analytical terms, very little difference between the 2nd and

4th key question you pose. The impacts under unmitigated CC (Q2) are not in any fundamental way different from the impacts under mitigated CC (Q4). 2degC

warming, for example, will give broadly the same impacts whether this occurs because of strong CC policy intervention or whether it occurs because of low carbon development paths. What matters more for impacts is the rate of CC and what matters more for how important those impacts are is the development path pursued. I think this distinction between mitigated and unmitigated CC is tenuous and unhelpful. This has a bearing on the later discussions about stabilisation (where "stabilisation" is usually assumed to be, indeed often synonymous with, the result of mitigative action; actually (quasi-) stabilisation, at different levels, can occur in a world with relatively little direct CC mitigation policy).

The progression through the sections follows a rather linear and reductionist model - observed impacts, future impacts, adaptation, regions. I would have liked to have seen an early opening chapter on the nature of the dynamic relationship between climate and society (before we even start talking about climate change), this being able to bring out notions of vulnerability and adaptation - both fundamental to put on the table before we start thinking about future climate change and how important it is. This could also point out that "critical" damage is already being caused by climate and climate variability.

Under your structure, the observed impacts section (II) should surely parallel the later future impacts section (III) in terms of sectors/themes. There are only 4 themes in section II, yet 6 (different) themes in section III. Why for example is nothing said about observed impacts on urban infrastructure or on coasts? The asymmetry between these section sub-themes is itself perhaps revealing.

It seems odd that adaptation is to be addressed in all the thematic chapters in Section III *as well as* in a separate later chapter on adaptation. This situation is ripe for overlap and redundancy. Our understanding of adaptation in any case should be brought in right at the beginning (see above).

The avoiding critical damage chapter suffers from the same problem identified above - what matters is whether and how such exceedance rates can be identified, not whether they result from either a mitigated or an unmitigated scenario - this academic distinction cannot be sustained in the real world.

The regional section is in danger of repeating the mistake in the TAR, again leading to dispersion of effort and redundancy. My suggestion would be *not* to assess all new regional knowledge (again; very turgid), but instead to produce a much more streamlined section focusing on a few regional/local case studies that illustrate sharply many of the (integrating) themes introduced earlier - vulnerability, adaptation,

criticality, impacts.

Deliberately seek to be selective and not comprehensive.

I also do not see how the WGII chapters will be co-ordinated with the 5 cross-cutting

papers identified here - again, there seems much scope for duplicitous effort and

redundancy or even contradiction. And since the cross-cutting papers are really the

interesting and useful ones, this suggests to me that the old traditional WG structure of

IPCC is now deeply flawed (as I have said more than once before in public).

Chapter 2 - Assumptions, etc.

First question to raise is what is WGI doing in this regard? I cannot comment sensibly

without knowing how WGI will tackle questions of scenarios and future projections.

In section 2.3, 4th bullet: how relevant really are these "Stabilisation scenarios

(mitigation)"? At the very least IPCC must clear up this issue about whether stabilisation

is a short-hand for mitigation (as implied here). This is potentially misleading, since

stabilisation can occur in many different worlds, by no means all of them worlds with

strong CC mitigation policies. Continuation of this thinking means reality is being forced

to accommodate the arbitrary thinking of the UNFCCC rather than UNFCCC being forced to take

account of reality.

Also in this bullet is "Impacts of extreme climate events". Why are impacts being looked

at here? Surely this is totally misplaced. What is important are scenarios - of whatever

origin and methodology - that embed within them changes in the character of "extreme"

weather and how we describe such changes. We should not separate this out as a separate

issue surely.

Section 2.4 (the second appearance) confuses me. Much of this material appears earlier in

2.3, thus characterisations of future conditions is what 2.3 is about and also the

projected changes in key drivers is what the scenarios part of 2.3 is all about.

Do you

mean to differentiate between methodology (2.3) and outcomes (2.4b)? And as always you

will run into the problem of summarising what scenarios actually *are* assumed in this

report - is there to be an IPCC 4AR standard scenario(s) that all should use? I suspect

not. Resolving this problem gets to the heart of the structural problem with IPCC.

Different people will use different assumptions.

Chapter 13 - Critical Damage ...

This outline was almost unintelligible to me! For example having read the opening aims and

scope statement several times, I am still not clear about the approach this chapter is

taking. Sections 13.2 and 13.3 are also extremely unclear as is section 13.4.

I think someone needs to do some clearer thinking about this chapter before sending it out

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for people to comment on. I have my own views on this, but at such short notice and

without knowing the agreed IPCC process I'm not going to write the chapter outline for you.

Inter alia, the chapter should address the following:

- different paradigms for defining "critical"; will vary by sector, culture, etc.
- distinction between external (pronounced) definitions of critical and internal (experienced/perceived) definitions
- relationship between adaptive capacity and "critical" rates of change
- dependence of critical thresholds on sector and spatial scale
- reversibility (or not) of critical damage
- ... and if the use of "critical" is a euphemism for "dangerous" then it is not very subtle
- people will see through this. What is the difference between critical and dangerous?

Professor Mike Hulme

Tyndall Centre

At 14:32 28/05/2003 +0100, you wrote:

Dear Mike

We are now developing chapter outlines for the Fourth Assessment Report of the IPCC and we write to ask if you will help us in this task. Enclosed is a one-page outline of the proposed chapter on Assumptions, Data and Scenarios, which we would like you to adjust and expand (but not to more than one and a half pages in all, please). The overall list of proposed topics to be covered in the assessment is also attached.

We would like to make the next revision to the outline in a few days so could you please return your outline to Norah Pritchard <<ipccwg2@metoffice.com>> at the WGII Technical Support Unit at the UK Met Office's Hadley Centre not later than 2nd June?

The process of designing the Fourth Assessment and selecting authors is different from previously. This time the authors will not be nominated by governments and then selected until *after* the outline has been approved by IPCC Plenary this November. The outlines are therefore being widely commented on between now and mid-September, when they will be finalised. We consider your input at this time to be most important.

We appreciate that you are busy, but urge that you give a few minutes to this crucial task.

In another message we will be writing for your suggestions regarding other experts to consult in the fields of Assumptions, Data and Scenarios.

We look forward to hearing from you

With thanks and kind regards,

Osvaldo Canziani and Mart in Parry

Co-Chairs, IPCC Working Group II (Vulnerability, Impacts and Adaptation)

Dr Martin Parry,

Co-Chair Working Group II (Impacts and Adaptation),

Intergovernmental Panel on Climate Change,

Hadley Centre,

UK Met Office,

London Road,

Bracknell RG12 2SY, UK.

Tel direct: +44 1986 781437

Tel switchboard: +44 1344 856888

direct e-mail: parryml@aol.com

e-mail for WGII Technical Support Unit: ipccwg2@metoffice.com

<<AR4_outline27May_2scen_v1.doc>> <<AR4 WG2 summary final.doc>>

4366. 2003-06-02

date: Mon, 2 Jun 2003 15:27:07 +0100

from: "Young G.M." <G.M.Young@Swansea.ac.uk>

subject: FW: Holocene manuscript

to: "'k.briffa@uea.ac.uk'" <k.briffa@uea.ac.uk>

cg2003

-----Original Message-----

From: Phil Camill [mailto:pcamill@carleton.edu]

Sent: 27 May 2003 02:00

To: J.A.Matthews@swansea.ac.uk; G.M.Young@Swansea.ac.uk

Cc: pcamill@carleton.edu

Subject: Holocene manuscript

Dear Gill and John,

I apologize for being somewhat persistent, but the following manuscript "Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western US and possible climatic forcing by the Pacific Decadal Oscillation" has now been in review at The Holocene for one year. I understand from previous conversations that Professor Briffa has been in the hospital, but it seems like a review should have been completed. I have not yet heard from Keith.

Can you provide me with any information on the status of the manuscript and help me draw the review process to a timely close?

All the best,

Phil

Dr. Phil Camill

Assistant Professor of Biology and Environmental Studies

Carleton College, Department of Biology

One North College St.

Northfield, MN 55057

phone: (507) 646-5643

fax: (507) 646-5757

4977. 2003-06-02

cc: shepherd_John,h.j.schellnhuber@uea.ac.uk

date: Mon Jun 2 15:26:40 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: idea for Royal Society meeting

to: F.Berkhout@sussex.ac.uk (Frans Berkhout)

Dear Frans,

Thanks for alerting me to this. I will bring this to the attention of John Schellnhuber

who will also have some views.

"Climate stabilisation" is certainly central to David Warrilow's interests, and has a

policy driver rather than a scientific one, but it can mean different things to different

people. To do justice to it also requires a widely inter-disciplinary thrust, including

economics and technology.

A variant on this theme might be to focus on critical thresholds for adaptation to climate

change in both human and natural systems - this could bring in some interesting non-standard scientific perspectives from anthropology and biological science,

anything to

get away from a repeat of the same old IPCC crowd (RS did a meeting on IPCC last December

anyway) and thus allowed to be a bit more creative.

Mike

At 18:06 23/05/2003 +0100, you wrote:

Mike

I will comment on this next week.

On another issue: I am on Brian Hoskyn's Royal Society Global Environmental Research

Committee which includes bods from various international programmes sponsored mainly by

cg2003

NERC. They are casting around for themes for a possible meeting at the Royal Society (international, high profile, bringing senior and junior researchers together), and something on climate stabilisation was mentioned - partly at the instigation of David Warriolow. Do you think this is sensible? Should there be a Tyndall presence? I agreed with John Shepherd (also at the meeting) that I'd raise this with you.

All the best
Frans

1413. 2003-06-03

cc: Mike Mann <mann@virginia.edu>
date: Tue, 3 Jun 2003 14:51:09 -0400
from: Scott Rutherford <srutherford@gso.uri.edu>
subject: revised NH comparison manuscript
to: Malcolm Hughes <mhughes@lrr.arizona.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>
<x-flowed>

Attached to this e-mail is a revision of the northern hemisphere comparison manuscript. First some general comments. I tried as best as possible to incorporate everyone's suggestions. Typically this meant adding/deleting or clarifying text. There were cases where we disagreed with the suggested changes and tried to clarify in the text why. In this next round of changes I encourage everyone to make specific suggestions in terms of wording and references (e.g. Rutherford et al. GRL 1967 instead of "see my GRL paper"). I also encourage everyone to make suggestions directly in the file in coloured text or by using Microsoft Word's "Track Changes" function (this will save me deciphering cryptic penmanship; although I confess, my writing is worse than anyone's). If you would prefer to use the editing functions in Adobe Acrobat let me know and I will send a PDF file. If you still feel strongly that I have not adequately addressed an issue please say so. I will incorporate the suggestions from this upcoming round into a manuscript to be submitted. After review, everyone will get a crack at it again.

I will not detail every change made (if anyone wants the file with the changes tracked I can send it). Here are the major changes:

- 1) removal of mixed-hybrid approach and revised discussions/figures
- 2) removal of CE scores from the verification tables
- 3) downscaling of the Esper comparison to a single figure panel and one paragraph.
- 4) revised discussion of spatial maps and revised figure (figure 8).
- 5) seasonal comparisons have been revised

Several suggestions have been made for where to submit. These are listed on page 1 of the manuscript. Please indicate your preference ASAP and I will tally the votes.

I would like to submit by late July, so if you could please get me comments by say July 15 that would be great. I will send out a reminder in early July. If I don't hear from you by July 15 I will assume that you are comfortable with the manuscript.

Please let me know if you have difficulty with the file or would prefer a different format.

Regards,
Scott
</x-flowed>

Attachment Converted: "c:\eudora\attach\nhcomparison_v7_1.doc"
<x-flowed>

Scott Rutherford

Marine Research Scientist
 Graduate School of Oceanography
 University of Rhode Island
 e-mail: srutherford@gso.uri.edu
 phone: (401) 874-6599
 fax: (401) 874-6811
 snail mail:
 South Ferry Road
 Narragansett, RI 02882
 </x-flowed>

680. 2003-06-04

date: Wed, 4 Jun 2003 11:16:10 -0400
 from: Edward Cook <drdendro@ldeo.columbia.edu>
 subject: Re: Review- confidential REALLY URGENT
 to: Keith Briffa <k.briffa@uea.ac.uk>
 <x-flowed>

Hi Keith,

Here is my review. I must admit to not being quite as negative about it as Stahle, but I do feel that it is marginal at best and could be justifiably rejected. Read my review. Of course, you will want to cut out the review and send it to the authors as a separate document.

Review of "Using a New 672-Year Tree-Ring Drought Reconstruction from West-Central Montana to Evaluate Severe Drought Teleconnections in the Western U.S. and Possible Climatic Forcing by the Pacific Decadal Oscillation" by D.A. Hunzicker and P. Camill

This paper is reasonably well written, but has some problems in it that bother me. The first issue relates to the tree-ring chronology that was developed at Lindberg Lake. Anytime less than half of the core samples (61 or 152) are used in developing a chronology, this is cause for concern. The fact that there are "unresolvable sections of missing rings" (p. 10) can mean a lot of things. However, ponderosa pine is known to cross-date well, which includes "locating" locally-absent rings during the cross-dating phase, so it is surprising that the authors have chosen not to work through these problems. Presumably, the trees with missing rings are also those most sensitive to drought, so isn't there a chance that the chronology being analyzed in this paper is less sensitive to drought than it ought to be? I also wonder how much their chronology is truly contributing to the overall stated goal of this paper, i.e. evaluating "Severe Drought Teleconnections in the Western U.S. and Possible Climatic Forcing by the Pacific Decadal Oscillation". The authors extensively use the PDSI reconstructions of Cook et al. (1999) in their analyses. Aside from the increased length of their new tree-ring chronology, what does it contribute that was not possible simply by using the Cook et al. reconstructions to test for teleconnections and forcing. None of the indices of forcing (ENSO, PDO, sunspots) extend back before the beginning of the Cook et al. reconstructions, so there is little to be gained in using one longer series from west-central Montana in this analysis. One could point to Fig. 3, which compares the MT reconstruction vs the SWDI series. But even this comparison is limited in its overall contribution to the paper. I also don't like the use of the FFT for estimating power spectra, even if the confidence limits are determined by bootstrapping. The power spectra calculated by the FFT are still inconsistent estimates. A more contemporary and consistent method of spectral estimation, like the Multi-Taper Method, should be used. For the reasons stated above, I do not consider this paper to be ready for publication as is. I will leave it to the Editor to decide how to proceed with it past this point.

--

=====

Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152

=====

</x-flowed>

3323. 2003-06-04

cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.edu>

date: Wed, 04 Jun 2003 10:17:57 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Prospective Eos piece?

to: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Tom Wigley

<wigley@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Keith Briffa

<k.briffa@uea.ac.uk>, trenbert@cgd.ucar.edu, Michael Oppenheimer

<omichael@princeton.edu>, Jonathan Overpeck <jto@u.arizona.edu>

Thanks Phil, and Thanks Tom W and Keith for your willingness to help/sign on.

This

certainly gives us a "quorum" pending even a few possible additional signatories
I'm

waiting to hear back from.

In response to the queries, I will work on a draft today w/ references and two
suggested

figures, and will try to send on by this evening (east coast USA). Tom W
indicated that he

wouldn't be able to look at a draft until Thursday anyway, so why doesn't everyone
just take

a day then to digest what I've provided and then get back to me with
comments/changes

(using word "track changes" if you like).

I'd like to tentatively propose to pass this along to Phil as the "official
keeper" of the

draft to finalize and submit IF it isn't in satisfactory shape by the time I
have to leave

(July 11--If I hadn't mentioned, I'm getting married, and then honeymoon, prior
to IUGG in

Sapporo--gone for about 1 month total). Phil, does that sound ok to you?

Re Figures, what I had in mind were the following two figures:

1) A plot of various of the most reliable (in terms of strength of temperature
signal and

reliability of millennial-scale variability) regional proxy temperature
reconstructions

around the Northern Hemisphere that are available over the past 1-2 thousand
years to

convey the important point that warm and cold periods were highly regionally
variable.

Phil and Ray are probably in the best position to prepare this (?). Phil and I
have

recently submitted a paper using about a dozen NH records that fit this
category, and many

of which are available nearly 2K back--I think that trying to adopt a timeframe
of 2K,

rather than the usual 1K, addresses a good earlier point that Peck made w/
regard to the

memo, that it would be nice to try to "contain" the putative "MWP", even if we
don't yet

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have a hemispheric mean reconstruction available that far back [Phil and I have one in review--not sure it is kosher to show that yet though--I've put in an inquiry to Judy Jacobs at AGU about this]. If we wanted to be fancy, we could do this the way certain plots were presented in one of the past IPCC reports (was it 1990?) in which a spatial map was provided in the center (this would show the locations of the proxies), with "rays" radiating out to the top, sides, and bottom attached to rectangles showing the different timeseries. Its a bit of work, but would be a great way to convey both the spatial and temporal information at the same time.

2) A version of the now-familiar "spaghetti plot" showing the various reconstructions as well as model simulations for the NH over the past 1 (or maybe 2K). To give you an idea of what I have in mind, I'm attaching a Science piece I wrote last year that contains the same sort of plot.

However, what I'd like to do different here is:

In addition to the "multiproxy" reconstructions, I'd like to Add Keith's maximum latewood density-based series, since it is entirely independent of the multiproxy series, but

conveys the same basic message. I would also like to try to extend the scope of the plot

back to nearly 2K. This would be either w/ the Mann and Jones extension (in review in GRL)

or, if that is deemed not kosher, the Briffa et al Eurasian tree-ring composite that

extends back about 2K, and, based on Phil and my results, appears alone to give a

reasonably accurate picture of the full hemispheric trend.

Thoughts, comments on any of this?

thanks all for the help,

mike

At 09:25 AM 6/4/2003 +0100, Phil Jones wrote:

Mike,

This is definitely worth doing and I hope you have the time before the 11th, or can

pass

it on to one of us at that time. As you know I'm away for a couple of days but back

Friday.

So count me in. I've forwarded you all the email comments I've sent to reporters/fellow

scientists, so you're fully aware of my views, which are essentially the same as all of

the list

and many others in paleo. EOS would get to most fellow scientists. As I said to you the

other

day, it is amazing how far and wide the SB pieces have managed to percolate. When it

comes

out I would hope that AGU/EOS 'publicity machine' will shout the message from rooftops

everywhere. As many of us need to be available when it comes out.

There is still no firm news on what Climate Research will do, although they will

cg2003

likely
have two editors for potentially controversial papers, and the editors will
consult
when papers
get different reviews. All standard practice I'd have thought. At present the
editors
get no
guidance whatsoever. It would seem that if they don't know what standard
practice is
then
they shouldn't be doing the job !
Cheers
Phil

At 22:34 03/06/03 -0400, Michael E. Mann wrote:

Dear Colleagues,
Eos has invited me (and prospective co-authors) to write a 'forum' piece (see
below).

This was at Ellen Mosely-Thompson's suggestion, upon my sending her a copy of
the
attached memo that Michael Oppenheimer and I jointly wrote. Michael and I
wrote this to
assist colleagues who had been requesting more background information to help
counter
the spurious claims (with which I believe you're all now familiar) of the
latest
Baliunas & Soon pieces.

The idea I have in mind would be to use what Michael and I have drafted as an
initial
starting point for a slightly expanded piece, that would address the same
basic issues
and, as indicated below, could include some references and figures. As
indicated in
Judy Jacobs' letter below, the piece would be rewritten in such a way as to
be less
explicitly (though perhaps not less implicitly) directed at the Baliunas/Soon
claims,
criticisms, and attacks.

Phil, Ray, and Peck have already indicated tentative interest in being
co-authors. I'm
sending this to the rest of you (Tom C, Keith, Tom W, Kevin) in the hopes of
broadening
the list of co-authors. I strongly believe that a piece of this sort

co-authored by 9
or so prominent members of the climate research community (with background
and/or
interest in paleoclimate) will go a long way in helping to counter these

attacks, which
are being used, in turn, to launch attacks against IPCC.

AGU has offered to expedite the process considerably, which is necessary
because I'll be

travelling for about a month beginning June 11th. So I'm going to work hard to
get

something together ASAP. I'd would therefore greatly appreciate a quick
response from

each of you as to whether or not you would potentially be willing to be
involved as a

co-author. If you're unable or unwilling given other current commitments, I'll
understand.

Thanks in advance for getting back to me on this,

mike

Date: Tue, 03 Jun 2003 20:19:08 -0400

From: Ellen Mosley-Thompson <thompson.4@osu.edu>

Subject: Re: position paper by Mann,

cg2003

Bradley et al that is a refutation to Soon et al

X-Sender: ethompso@pop.service.ohio-state.edu

To: Judy Jacobs <JJacobs@agu.org>, "Michael E. Mann" <mann@virginia.edu>

X-Mailer: QUALCOMM Windows Eudora Version 4.3

Judy and Mike -

This sounds outstanding.

Am I right in assuming that Fred reviews and approves the Forum pieces?

If so, can you hint about expediting this. Timing is very critical here.

Judy, thanks for taking the bull by the horns and getting the ball rolling.

Best regards,

Ellen

At 07:33 PM 06/03/2003 -0400, Judy Jacobs wrote:

Dear Dr. Mann,

Thanks for the prompt reply.

Based on what you have said, it sounds to me as if Mann, Bradley, et al. will not be in violation of AGU's prohibition on duplicate publication.

The attachment to your e-mail definitely has the look and feel of something that would be published in Eos under the "FORUM" column header. FORUM pieces are usually comments on articles of any description that have been published in previous issues of Eos; or they can be articles on purely scientific or science policy-related issues around which there is some controversy or difference of opinion; or articles on current public issues that are of interest to the geosciences; or on issues--science or broader policy

ones---On which there is an official AGU Position Statement. In this last category, I offer, for example, the teaching of creationism in public schools, either alongside evolution, or to the exclusion of evolution.

AGU has an official Position Statement, "Climate Change and Greenhouse Gases," which states, among other things, that there is a high probability that man-made gases primarily from the burning of fossil fuels is contributing to a gradual rise in mean global temperatures. In this context, your proto-article---in the form of the attachment

you sent me-- would seem right on target for a Forum piece. However, since the Soon et

al. article wasn't actually published in Eos, anything that you and Dr. Bradley craft

will have to minimize reference to the specific article or articles, and concentrate on

"the science" that is set forth in these papers. Presumably this problem could be solved by simply referencing these papers.

A Forum piece can be as long as 1500 words, or approximately 6 double-spaced pages. A

maximum of two figures is permitted. A maximum of 10 references is encouraged, but if

the number doesn't exceed 10 too outrageously, I don't make a fuss, and neither will

Ellen.

Authors are now asked to submit their manuscripts and figures electronically via AGU's

Internet-based Geophysical Electronic Manuscript System (GEMS), which makes it possible

for the entire submission-review process to be conducted online.

cg2003

If you have never used GEMS before, you can register for a login and password, and get

initial instructions, by going to
[1]<http://eos-submit.agu.org/>

If you would like to have a set of step-by-step instructions for first-time GEMS users, please ask me.

Ellen indicated that she/you would like to get something published sooner rather than

later. The Eos staff can certainly expedite the editorial process for anything you and your colleagues submit.

Don't hesitate to contact me with any further questions.

Best regards,

Judy Jacobs

Michael E. Mann wrote:

Dear Judy,

Thanks very much for getting back to me on this. Ellen had mentioned this possibility,

and I have been looking forward to hearing back about this.

Michael Oppenheimer and I drafted an informal memo that we passed along to colleagues

who needed some more background information so that they could comment on the Soon et al

papers in response to various inquiries they were receiving from the press, etc. I've

attached a copy of this memo.

It has not been our intention for this memo to appear in print, and it has not been

submitted anywhere for publication. On the other hand, when Ellen mentioned the

possibility of publishing something *like* this in e.g. the "Eos" forum, that seemed

like an excellent idea to me, and several of my colleagues that I have discussed the

possibility with.

What we had in mind was to produce a revised version of the basic memo that I've

attached, modifying it where necessary, and perhaps expanding it a bit, seeking broader

co-authorship by about 9 or so other leading climate scientists. So far, Phil Jones of

the University of East Anglia, Ray Bradley of the University of Massachusetts, and

Jonathan Overpeck of the University of Arizona, have all indicated their interest in

co-authoring such a piece. We suspect that a few other individuals would be interested

in being co-authors as well. I didn't want to pursue this further, however, until I

knew whether or not an Eos piece was a possibility.

So pending further word from you, I would indeed be interested in preparing a multi-authored "position" paper for Eos in collaboration with these

co-authors, based

loosely on the memo that I have attached.

I look forward to further word from you on this.

best regards,

mike mann

At 04:59 PM 6/3/2003 -0400, you wrote:

Dear Dr. Mann,

I am the managing editor for Eos, the weekly newspaper of the American Geophysical Union.

Late last week, the Eos editor for atmospheric sciences, Ellen

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Mosley-Thompson, asked me if Eos would publish what she called "a position paper" by you, Phillip Bradley, et al that would, in effect, be a refutation to a paper by Soon et al. that was published in a British journal, Energy & Environment a few weeks ago. This Energy & Environment article was subsequently picked up by the Discovery Channel and other print and electronic media that reach the general public.

Before I can answer this question, I need to ask if you and your colleagues intend for this position paper to be published simultaneously in outlets other than Eos. If this is the case, I'm afraid it being published in Eos is a moot point, because of AGU's no duplicate publication policy: if the material has been published elsewhere first, AGU will not publish it.

I look forward to your response.

Best regards,

Judy Jacobs

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\MannPersp20021.pdf"

1232. 2003-06-05

cc: Kevin Trenberth <trenbert@cgd.ucar.edu>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Caspar Ammann <ammann@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, mann@virginia.edu, jto@u.arizona.edu
date: Thu, 05 Jun 2003 22:48:03 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: EOS text
to: Tom Wigley <wigley@ucar.edu>

Hi Tom et al,

Wanted to comment briefly on some of the specifics below, just to make sure we

don't get too sidetracked. This is a very interesting and worthwhile discussion. In fact, these are precisely the kinds of issues that Phil and I are trying to sort out w/ the review paper we're writing for ROG [we'll probably be soliciting comments from many of you on different sections of that paper in the near future]. But I think its useful at this juncture to make a make a distinction between these sorts of scientifically interesting issues, and the nonsensical arguments that SB03 are actually making. We can quibble, for example, over the nature of the relationship between past variations in the surface temperature field, the atmospheric circulation, and the types of proxies that might inform our knowledge of each of these. I agree with Tom's point that in many case precipitation indicators don't tell us much at all about the surface temperature field, certainly in the 'local' sense. In a sort of 'state space' sense, however, they may in some instances be quite helpful. Winter drought-sensitive tree-ring chronologies provide us some of our best proxy information with regard to winter synoptic-scale variability in semi-arid regions like the desert southwest or the mediterannean. There appears to been some success (i.e., demonstrated statistical skill) in reconstructing patterns of anomalous atmospheric circulation related to the usual suspect sorts of indices (PNA, NAO, etc.) from those sorts of proxies. To the extent that much of the regional winter season variability in the extratropical surface temperature field is related to these sorts of atmospheric circulation anomalies, one expects some skill in using these predictors to reconstruct features of the cold-season atmospheric circulation and, thus, regional temperature anomalies related to those features. I think a good case has been made that we can, perhaps, understand a good detail of the structure of the extratropical winter temperature anomalies during parts of the 'LIA' in terms of, e.g., the behavior of the NAO--a lot of evidence now seems to be pointing in that direction. A similar argument can be made, for example, that a precipitation proxy in the western tropical Pacific may be an excellent predictor of SST variability in the eastern and central tropical Pacific, for the obvious reasons. So, in this larger-scale sense, there are some potentially useful relationships, and I agree with what Kevin says in this regard. Of course, it is also true that there are some obvious stationarity assumptions implicit in this sort of reasoning, and in the use of any proxy precip/drought/atmospheric circulation information to infer or help reconstruction features in the surface temperature field. There are, however, similar stationarity assumptions implicit in the idea that a modest network (say, of a

dozen) proxy surface temperatures over, say, the Northern Hemisphere, can be used to reconstruct hemispheric mean temperature. The implicit assumption is that the relative importance of each of a small number of locations in estimating the large-scale temperature field remains constant over time. As the number of regions sampled approaches the number of degrees of freedom in the surface temperature field, this because a better and better assumption. If we were only talking about a handful of locations, it may be a pretty bad assumption. This sort of stationarity assumption is potentially just as, or even more (depending on the size of the network used) suspect than the former stationarity assumption, but is much more rarely discussed or acknowledge. Of course, there are ways to test these sorts of assumptions in a modeling context, and there are several studies now published, and others in the works, , that suggest the situation probably isn't as bad as we might have feared (again, something Phil and I will touch on in our ROG paper). See for example, these:

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', *Geophysical Research Letters*, 29 (10), 1501, doi: 10.1029/2001GL014554, 2002.

Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., Climate Field Reconstruction Under Stationary and Nonstationary Forcing, *Journal of Climate*, 16, 462-479, 2003.

Zorita, E., Gonzalez-Rouco, F., and Legutke, S., Testing the Mann et al. (1998) Approach to Paleoclimate Reconstructions in the Context of a 1000-yr Control Simulation with the ECHO-G Coupled Climate Model, *J. Climate*, 16, 1378-1390, 2003.

But these are all legitimate caveats, and interesting points, that would be great to discuss over some beers sometime, and which will be given more than adequate treatment in e.g. the review paper mentioned above.

Unfortunately, that's not the task at hand. SB03 have no appreciation whatsoever for these sorts of subtle, legitimate considerations, which involve thinking in a much higher sphere than the one they are thinking in, and certainly, the one that they are playing to. Their logic is much more basic, and immensely less reasonable, than anything we're talking about here.

Their logic, in essence, literally EQUATES hydroclimatic and temperature anomalies, since they hold that the existence of a large extreme in precipitation/drought in a particular region is as good as evidence of anomalous warmth, in support of the proposition of e.g. a "medieval warm period". So, in a very roundabout way, what I'm saying is, lets definitely not give these bozos more credit than they deserve!

Unfortunately, we have precious little space in this Eos piece. Phil and I have a lot more

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space in our ROG article, and this sort of discussion will help us in making sure that these issues are adequately addressed there. I suspect that this longer review, and others that Ray and folks are working on, will be helpful in e.g. the next IPCC report. But for the time being, we have to keep things simple and to the point here. What we say of course needs to be rigorously defensible and we would like to educate the readers as much as we can in the short space available, but most of all we really have to do, in as simple terms as possible, is explain why the SB03 stuff is so fundamentally flawed. And, to boot, we have to do so in such a way that it seems more a casual consequence of what we say, than (as it is in fact) the central motivation of the article. So there is a real balancing act here, and that's what we're coming up against. Let me do my best to strike this balance, and see if I can come up with a revised version that strikes the right balance between everyone's concerns here. Again, I still need comments from several more people before I can attempt a revised draft. So responses (e.g. in the next day or so) would be greatly appreciated from those I haven't heard back from... thanks in advance,

mike

At 05:08 PM 6/5/2003 -0600, Tom Wigley wrote:

Dear all,

Re AGU's position, this is something I must have overlooked if it was in an earlier email. One way around this is to make the scientific error points and quote SB as an example of how not to do it (which one would have to do for at least three specific points).

Re Kevin's suggestion, his text could be misinterpreted. It implies that one might be able to use wet/dry as a T proxy if the right statistical analysis were done first. I agree with what Kevin says, but I have looked at these sort of physically meaningful relationships and they are invariably too weak to use in a paleo context. For example, if the paleo indicator explains 50% of the precip (seasonal) variance (and such a high, independently validated value is rare), and if the r^2 for precip vs temp were similar, then we are left with 25% (at most -- the above assumptions are very optimistic). This is weak. Worse still, this assumes no paleo atmos circulation changes, also doubtful.

The bottom line is that proxy precip data *cannot* be used as a T indicator except in the rarest of circumstances. Even in high latitudes there are problems -- see, e.g., Bradley and England, late 1970s report (Ray, I'm sure you will remember this about the rareness of precip events).

I think it is extremely dangerous to leave SB any loopholes here. In my view, what Kevin

says does just this.
Tom.

Michael E. Mann wrote:

Thanks Kevin,

I've already made some revisions in response to your earlier comment about explicitly discussing the spatial variability issue with regard to the LIA/MWP. The prospective Figure 2 should help in this regard--looking forward to hearing back from Ray/Phil on that...

I'll do my best to come up w/ a revised version that reflects everyone's suggestions and wishes once all the comments are in,
mike

At 02:53 PM 6/5/2003 -0600, Kevin Trenberth wrote:

Tom

I agree with Mike that it is not possible to directly confront their methods in this

way. It can be confronted by stating clearly that cold periods that are not contemporaneous at different locations do not make for a cold hemispheric value:

currently the article already makes this point to some extent but it can be made more directly relevant to SB. In fact it may be worthwhile pointing out that the LIA is defined by different authors to be in different periods precisely because they were

looking at a different part of the world (like blind men exploring the elephant).

And we can also say that it makes no sense to equate wet or dry period with cold or warm universally (ref SB).

In fact what is found generally in mid lats is that warm in winter goes with wet (through moist and warm advection) and with dry in summer (drought and heat waves). So

seasonality matters a lot. Maybe we can say something like this:

It is well established in current climate studies that warm conditions tend to accompany

wet conditions in the extratropics in winter owing to the dominant role of the atmospheric circulation so that southerlies are warm and moist in the northern hemisphere while northerlies are cold and dry. But in summer, the weaker atmospheric

circulation means that moist thermodynamics is more important so that dry conditions

favor warm spells and heat waves, as heat from the sun no longer evaporates moisture and

instead increase temperatures. In the Tropics, during El Nino events, droughts occur

in one part of the world (e.g. Australia) while wet conditions and floods occur in other

parts (e.g. Peru), and the wet spots tend to switch with the dry spots during La Nina.

Accordingly, there is no unique link between wet or dry with warm or cold conditions

(such as erroneously assumed by SB).

Not sure if this is useful but I offer it anyway.

Kevin

Tom Wigley wrote:

Mike et al.,

I will send tracked editorial suggestions later. In the meantime, what is

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lacking in my
view is a clear statement at the start of the SB method. At present, the
context of your
later comments is a bit unclear to those who have not read the papers -- which
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(1). What I
say here overlaps with some things you say later, so minor changes are needed
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We are using this to educate people about the good paleo work, but a key
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to demolish the bad stuff. I think, therefore, that the criticism of SB must
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This
suggested new material also provides a balance, and makes what we now have
appear less
self serving (which I know you are not trying to do, but there is still a hint
of this).

Tom.

--

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2191. 2003-06-05

cc: "Michael E. Mann" <mann@virginia.edu>, "Raymond S. Bradley"
<rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley
<tcrowley@duke.edu>, Caspar Ammann <ammann@ucar.edu>, Phil Jones
<p.jones@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Kevin Trenberth
<trenbert@ucar.edu>
date: Thu, 05 Jun 2003 14:53:59 -0600
from: Kevin Trenberth <trenbert@cgd.ucar.edu>
subject: Re: EOS text
to: Tom Wigley <wigley@ucar.edu>
<x-flowed>

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methods in this way. It can be confronted by stating clearly that cold
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cg2003

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> Mike et al.,

>

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> We are using this to educate people about the good paleo work, but a
> key motivation is to demolish the bad stuff. I think, therefore, that
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> a statement of their work is essential. This suggested new material
> also provides a balance, and makes what we now have appear less self
> serving (which I know you are not trying to do, but there is still a
> hint of this).

>

> Tom.

--

Kevin E. Trenberth
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</x-flowed>

4937. 2003-06-05

date: Thu, 5 Jun 2003 12:09:04 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>

subject: REALLY URGENT for you too!!!

to: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi Keith,

cg2003

This is not terribly kosher, but I am sending you the paper I am reviewing that attempts to destroy dendroclimatology as presently done, and my present review of it. This does not have to be sent in until next week sometime, so there is time for you to add any comments. Doing this is justified in my view because the authors use your Tornetrask reconstruction as the main whipping boy. The paper is rather mathematical in parts, but the bias they show in condemning the standard method of climate reconstruction is pretty apparent. I don't know if there is a hidden agenda or just an effort on their part to show us dumb asses how to do it right! Anyway, give me a call at home tomorrow if you wish, but certainly read what I have sent you and please recommend changes or additions.

Cheers,

Ed

P.S. Please keep this confidential for now since it is a paper under review.

--

=====
Dr. Edward R. Cook
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Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
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=====

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Attachment Converted: "c:\eudora\attach\Review of Specification.pdf"

682. 2003-06-06

date: Fri Jun 6 14:57:47 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: EOS text
to: "Michael E. Mann" <mann@virginia.edu>

Mike

there is often no benefit in bandying fine points of emphasis and implication-
Hence , I

think that what you have already drafted is fine. Do not start to dilute or
confuse the

issue with too much additional detail. The job , as you state , is to place on
record the

statement of disagreement with the "science(!)" and spin. To this end , it may
also be

worth stating in less couched terms that merely eyeballing the relative
magnitudes of

recent versus prior period(s) of large scale warmth, is in itself very limited
as a basis

for claiming the reality OR OTHERWISE of anthropogenic forcing of the recent
warming , if

this is done without reference to the uncertainty and causes of these
differences.

The points you make to Tom are of course very valid , but do not be tempted to
guild the

lily too much here - stick with your current content

Keith

At 09:15 AM 6/6/03 -0400, you wrote:

Thanks for the comments Tom,

I'm working on having a revised version by early this afternoon (in time for
Phil to

look at before nightfall in the UK).

Phil has kindly agreed to take over the lead role on this if we're not ready

cg2003

to submit

by the time I have to leave (Jun 11th). Will update on this when necessary.

More soon,

mike

At 10:03 PM 6/5/2003 -0600, Tom Wigley wrote:

Mike,

We'll put! By chance SB03 may have got some of these precip things right, but we don't

want to give them any way to claim credit.

Also, stationarity is the key. Let me tell you a story. A few years back, my son Eirik

did a tree ring science fair project using trees behind NCAR. He found that widths

correlated with both temp and precip. However, temp and precip also correlate. There is

much other evidence that it is precip that is the driver, and that the temp/width

correlation arises via the temp/precip correlation. Interestingly, the temp correlations

are much more ephemeral, so the complexities conspire to make this linkage nonstationary.

I have not seen any papers in the literature demonstrating this -- but, as you point out

Mike, it is a crucial issue.

Tom.

+++++

Michael E. Mann wrote:

Hi Tom et al,

Wanted to comment briefly on some of the specifics below, just to make sure we don't get

too sidetracked. This is a very interesting and worthwhile discussion. In fact, these

are precisely the kinds of issues that Phil and I are trying to sort out w/ the review

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We can quibble, for example, over the nature of the relationship between past variations in the surface temperature field, the atmospheric circulation, and the types

of proxies that might inform our knowledge of each of these. I agree with Tom's point

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the extratropical surface temperature field is related to these sorts of atmospheric circulation anomalies, one expects some skill in using these predictors to reconstruct features of the cold-season atmospheric circulation and, thus, regional temperature anomalies related to those features. I think a good case has been made that we can, perhaps, understand a good detail of the structure of the extratropical winter temperature anomalies during parts of the 'LIA' in terms of, e.g., the behavior of the NAO--a lot of evidence now seems to be pointing in that direction. A similar argument can be made, for example, that a precipitation proxy in the western tropical Pacific may be an excellent predictor of SST variability in the eastern and central tropical Pacific, for the obvious reasons. So, in this larger-scale sense, there are some potentially useful relationships, and I agree with what Kevin says in this regard. Of course, it is also true that there are some obvious stationarity assumptions implicit in this sort of reasoning, and in the use of any proxy precip/drought/atmospheric circulation information to infer or help reconstruction features in the surface temperature field. There are, however, similar stationarity assumptions implicit in the idea that a modest network (say, of a dozen) proxy surface temperatures over, say, the Northern Hemisphere, can be used to reconstruct hemispheric mean temperature. The implicit assumption is that the relative importance of each of a small number of locations in estimating the large-scale temperature field remains constant over time. As the number of regions sampled approaches the number of degrees of freedom in the surface temperature field, this becomes a better and better assumption. If we were only talking about a handful of locations, it may be a pretty bad assumption. This sort of stationarity assumption is potentially just as, or even more (depending on the size of the network used) suspect than the former stationarity assumption, but is much more rarely discussed or acknowledged. Of course, there are ways to test these sorts of assumptions in a modeling context, and there are several studies now published, and others in the works, that suggest the situation probably isn't as bad as we might have feared (again, something Phil and I will touch on in our ROG paper). See for example, these:

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', *Geophysical Research Letters*, 29 (10), 1501, doi: 10.1029/2001GL014554, 2002. Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., Climate Field Reconstruction Under Stationary and Nonstationary Forcing, *Journal of Climate*, 16, 462-479, 2003. Zorita, E., Gonzalez-Rouco, F., and Legutke, S., Testing the Mann et al. (1998) Approach to Paleoclimate Reconstructions in the Context of a 1000-Yr Control Simulation with the ECHO-G Coupled Climate

cg2003

Model, J. Climate, 16, 1378-1390, 2003.

But these are all legitimate caveats, and interesting points, that would be great to discuss over some beers sometime, and which will be given more than adequate treatment in e.g. the review paper mentioned above. Unfortunately, that's not the task at hand. SB03 have no appreciation whatsoever for these sorts of subtle, legitimate considerations, which involve thinking in a much higher sphere than the one they are thinking in, and certainly, the one that they are playing to. Their logic is much more basic, and immensely less reasonable, than anything we're talking about here. Their logic, in essence, literally EQUATES hydroclimatic and temperature anomalies, since they hold that the existence of a large extreme in precipitation/drought in a particular region is as good as evidence of anomalous warmth, in support of the proposition of e.g. a "medieval warm period". So, in a very roundabout way, what I'm saying is, lets definitely not give these bozos more credit than they deserve! Unfortunately, we have precious little space in this Eos piece. Phil and I have a lot more space in our ROG article, and this sort of discussion will help us in making sure that these issues are adequately addressed there. I suspect that this longer review, and others that Ray and folks are working on, will be helpful in e.g. the next IPCC report. But for the time being, we have to keep things simple and to the point here. What we say of course needs to be rigorously defensible and we would like to educate the readers as much as we can in the short space available, but most of all we really have to do, in as simple terms as possible, is explain why the SB03 stuff is so fundamentally flawed. And, to boot, we have to do so in such a way that it seems more a casual consequence of what we say, than (as it is in fact) the central motivation of the article. So there is a real balancing act here, and thats what we're coming up against. Let me do my best to strike this balance, and see if I can come up with a revised version that strikes the right balance between everyones concerns here. Again, I still need comments from several more people before I can attempt a revised draft. So responses (e.g. in the next day or so) would be greatly appreciated from those I haven't heard back from... thanks in advance, mike

At 05:08 PM 6/5/2003 -0600, Tom Wigley wrote:
Dear all,
Re AGU's position, this is something I must have overlooked if it was in an earlier email. One way around this is to make the scientific error points and quote SB as an example of how not to do it (which one would have to do for at least three specific

points).

Re Kevin's suggestion, his text could be misinterpreted. It implies that one might be able to use wet/dry as a T proxy if the right statistical analysis were done first. I agree with what Kevin says, but I have looked at these sort of physically meaningful relationships and they are invariably too weak to use in a paleo context. For example, if the paleo indicator explains 50% of the precip (seasonal) variance (and such a high, independently validated value is rare), and if the r^2 for precip vs temp were similar, then we are left with 25% (at most -- the above assumptions are very optimistic). This is weak. Worse still, this assumes no paleo atmos circulation changes, also doubtful. The bottom line is that proxy precip data *cannot* be used as a T indicator except in the rarest of circumstances. Even in high latitudes there are problems -- see, e.g., Bradley and England, late 1970s report (Ray, I'm sure you will remember this about the rareness of precip events). I think it is extremely dangerous to leave SB any loopholes here. In my view, what Kevin says does just this.
Tom.

Michael E. Mann wrote:

Thanks Kevin,
I've already made some revisions in response to your earlier comment about explicitly discussing the spatial variability issue with regard to the LIA/MWP. The prospective Figure 2 should help in this regard--looking forward to hearing back from Ray/Phil on that...

I'll do my best to come up w/ a revised version that reflects everyone's suggestions and wishes once all the comments are in,
mike

At 02:53 PM 6/5/2003 -0600, Kevin Trenberth wrote:
Tom

I agree with Mike that it is not possible to directly confront their methods in this way. It can be confronted by stating clearly that cold periods that are not contemporaneous at different locations do not make for a cold hemispheric value: currently the article already makes this point to some extent but it can be made more directly relevant to SB. In fact it may be worthwhile pointing out that the LIA is defined by different authors to be in different periods precisely because they were looking at a different part of the world (like blind men exploring the elephant). And we can also say that it makes no sense to equate wet or dry period with cold or warm universally (ref SB). In fact what is found generally in mid lats is that warm in winter goes with wet (through moist and warm advection) and with dry in summer (drought and heat

waves). So

seasonality matters a lot.

Maybe we can say something like this:

It is well established in current climate studies that warm conditions tend to accompany

wet conditions in the extratropics in winter owing to the dominant role of the atmospheric circulation so that southerlies are warm and moist in the northern hemisphere while northerlies are cold and dry. But in summer, the weaker

atmospheric

circulation means that moist thermodynamics is more important so that dry conditions

favor warm spells and heat waves, as heat from the sun no longer evaporates moisture and

instead increase temperatures. In the Tropics, during El Nino events, droughts occur

in one part of the world (e.g. Australia) while wet conditions and floods occur in other

parts (e.g. Peru), and the wet spots tend to switch with the dry spots during La Nina.

Accordingly, there is no unique link between wet or dry with warm or cold conditions

(such as erroneously assumed by SB).

Not sure if this is useful but I offer it anyway.

Kevin

Tom Wigley wrote:

Mike et al.,

I will send tracked editorial suggestions later. In the meantime, what is lacking in my

view is a clear statement at the start of the SB method. At present, the context of your

later comments is a bit unclear to those who have not read the papers -- which will be

the case for most readers. I suggest adding the attached before your point (1). What I

say here overlaps with some things you say later, so minor changes are needed (which I

will send later) to avoid clear duplication.

We are using this to educate people about the good paleo work, but a key motivation is

to demolish the bad stuff. I think, therefore, that the criticism of SB must be more

focussed and specific -- which is why a statement of their work is essential. This

suggested new material also provides a balance, and makes what we now have appear less

self serving (which I know you are not trying to do, but there is still a hint of this).

Tom.

--

Kevin E. Trenberth

e-mail: trenbert@ucar.edu

Climate Analysis Section, NCAR

[1]www.cgd.ucar.edu/cas/ <[2]http://www.cgd.ucar.edu/cas/>

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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[7]<http://www.cru.uea.ac.uk/cru/people/briffa>[8]/

1460. 2003-06-06

date: Fri, 6 Jun 2003 16:19:39 +0100
from: "Emma L. Tompkins" <e.tompkins@uea.ac.uk>
subject: your lunchtime presentation
to: "'Mike Hulme'" <m.hulme@uea.ac.uk>

Hi Mike

I just wanted to put into words what I was poorly expressing at lunch.

1) You argue that dangerous climate change is undefinable at the global scale as it is context-dependent.

2) The proponents of global measures or indicators of 'danger' all omit important elements and hence provided very biased results which generate different policy conclusions.

3) I would argue that it is important that people understand what the dangers of climate change are so that we can work towards solving the problem - but this is what the impacts work provides (I think).

4) If dangerous climate change cannot be defined at the global scale, we need to be very clear about this and suggest that better approaches might be to contextualise climate change for different groups and smaller scales - perhaps using your method...

Does there really have to be a chapter on this in AR4? Instead 1 chapter on sensitivity analysis of the impacts, 1 on perceptions of what dangerous mean, and 1 on communicating this information might be a better way to deal with this?

Hope this helps explain my thoughts a little better,

Emma

-----*

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web: <http://www.tyndall.ac.uk>

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2850. 2003-06-06

cc: Scott Rutherford <srutherford@gso.uri.edu>
 date: Fri, 06 Jun 2003 12:37:58 -0400
 from: "Michael E. Mann" <mann@virginia.edu>
 subject: Revised Version!
 to: "Raymond S. Bradley" <rbradley@geo.umass.edu>, Keith Briffa
 <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Caspar Ammann
 <ammann@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer
 <omichael@princeton.edu>, Kevin Trenberth <trenbert@ucar.edu>, mann@virginia.edu,
 Tom Wigley <wigley@ucar.edu>, jto@u.arizona.edu

Dear all,

Here is my best attempt to incorporate everyone's suggestions, views, etc. One major change you'll notice is that the final item (the one on co2 increase and recent warming) was eliminated, because it seemed to open a can of warms, and also distract from the central message. Note that, with the number of references we have, we are currently just about at the word limit for the piece. We shouldn't go over 1400 words, which puts some tight constraint on any additions, etc.

I hope to forward a draft of Figure 1 later on this afternoon. I'm assuming that Phil can take care of Figure 2 (Phil?--Scott has graciously indicated his willingness to help if necessary), but its pretty clear what this figure will show, so I don't think its that essential that we have that figure done to try to finalize the draft.

I'll attempt one final(?) revision of the text based on any remaining comments you may have--please try, if possible, to keep the suggested changes minimal at this point. I'll assume that anyone we haven't yet heard back from in the author list over the next day or so is unable to be a co-author, and will respectfully drop them from the author list any related future emailings.

Thanks all for your help. Its rare to have every single co-author make substantial contributions to improving the draft, and that was clearly the case here...

mike

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 [1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
 Attachment Converted: "c:\eudora\attach\EosForum2.doc"

3505. 2003-06-06

date: Fri, 6 Jun 2003 09:52:26 -0400
 from: Edward Cook <drdendro@ldeo.columbia.edu>
 subject: Fwd: Re: JABES manuscript MS03030 review request
 to: Keith Briffa <k.briffa@uea.ac.uk>
 Hi Keith,

cg2003

okay, here is what I just sent to Olsen.

Cheers,

Ed

Date: Fri, 6 Jun 2003 09:50:27 -0400

To: Olsen.Tony@epamail.epa.gov

From: Edward Cook <drdendro@ldeo.columbia.edu>

Subject: Re: JABES manuscript MS03030 review request

Cc:

Bcc:

X-Attachments: :Macintosh HD:27226:Review of Specification.pdf:

Hi Tony,

Here is my review of the Yoo and Wright paper. Frankly, it is very poor for reasons that

I describe in my review and really must not be published as is. It would do grossly

unfair harm to dendroclimatology because the authors have simply not made their case. If

anything, they have actually vindicated the "reverse regression" method based on what

they show in their Table 2, even if they don't care to admit it.

I also see that they used the same tree-ring data as Briffa to test their method. Yet,

there is not so much as the slightest acknowledgement of where the data were obtained.

>From Briffa? I assume so. If so, they should acknowledge it. Also, had you considered

Briffa as a reviewer as well? Since this paper is such a negative attack on his work,

that would have been proper.

Anyway, I appreciate the fact that you sent me this paper to review.

Cheers,

Ed

[cid:a05200f01bb064cb92f51@[10.0.1.4].1.0]

Dr. Cook,

I am not sure if you agreed to do the review (at least I can't find it).

Will you be able to complete the review in the next week? Two weeks?

Look forward to hearing from you.

Anthony (Tony) R. Olsen

USEPA NHEERL

Western Ecology Division

200 S.W. 35th Street

Corvallis, OR 97333

Voice: (541) 754-4790

Fax: (541) 754-4716

email: Olsen.Tony@epa.gov

--

=====

Dr. Edward R. Cook

Doherty Senior Scholar and

Director, Tree-Ring Laboratory

Lamont-Doherty Earth Observatory

Palisades, New York 10964 USA

Email: drdendro@ldeo.columbia.edu

Phone: 845-365-8618

Fax: 845-365-8152

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Fax: 845-365-8152

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00000001,57bd7b73,00000000,00000000
Attachment Converted: "c:\eudora\attach\Review of Specification1.pdf"

4407. 2003-06-06

cc: Tom Wigley <wigley@ucar.edu>, rbradley@geo.umass.edu, p.jones@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, ssolomon@a1.noaa.gov, Ellen Mosley-Thompson <thompson.4@osu.edu>
date: Fri, 06 Jun 2003 15:58:14 -0400
from: Michael Oppenheimer <omichael@Princeton.EDU>
subject: Re: Fwd: world's best scientists behind soon study
to: "Michael E. Mann" <mann@virginia.edu>

It can't hurt to set the record straight vs "240", obvious confusion with IPCC.
I would stay away from attacks on credentials.

"Michael E. Mann" wrote:

Tom,
In my opinion, it probably can't hurt anything. Wondering what others think.
I've forwarded the Inhofe story to Andy Revkin at NYT, who has been following
this story with interest. I've also let him know about the response we will be submitting
to Eos,

mike

At 01:16 PM 6/6/2003 -0600, Tom Wigley wrote:

Dear all,

I am happy to send a personal email to Inhofe. OK?

Tom.

Michael E. Mann wrote:

Dear co-authors,

Our Eos piece can't appear too soon, at this point,

mike

Subject: world's best scientists behind soon study

To: mann@virginia.edu

X-Mailer: Lotus Notes Release 5.0.9 November 16, 2001

From: Jeff Nesmith <jeffn@coxnews.com>

Date: Fri, 6 Jun 2003 14:11:09 -0400

X-MIMETrack: Serialize by Router on CN-ATL-NML01/Coxnews(Release 5.0.8 |June 18, 2001)

at

06/06/2003 02:10:43 PM

X-MIME-Autoconverted: from base64 to 8bit by multiproxy.evsc.Virginia.EDU id h56IGuD17232

I covered a hearing by the Senate Environment and Public Works Committee yesterday and Sen. Inhofe broke away from the agenda for a few minutes to lecture some White House guy on the importance of having sound science, yahyah, etc. As an example, he said, this new study about the medieval warming period casts a whole new light on the global warming issue. Then he said that this was the work of 240 of the best scientists in the world.
jeff n.

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

cg2003

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\omichael.vcf"

4765. 2003-06-06

cc: rbradley@geo.umass.edu, p.jones@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Michael Oppenheimer <omichael@princeton.edu>, ssolomon@a1.noaa.gov, Ellen Mosley-Thompson <thompson.4@osu.edu>
date: Fri, 06 Jun 2003 13:16:47 -0600
from: Tom Wigley <wigley@ucar.edu>
subject: Re: Fwd: world's best scientists behind soon study
to: "Michael E. Mann" <mann@virginia.edu>
<x-flowed>

Dear all,
I am happy to send a personal email to Inhofe. OK?
Tom.

Michael E. Mann wrote:

> Dear co-authors,
>
> Our Eos piece can't appear too soon, at this point,
>
> mike
>
>> Subject: world's best scientists behind soon study
>> To: mann@virginia.edu
>> X-Mailer: Lotus Notes Release 5.0.9 November 16, 2001
>> From: Jeff Nesmith <jeffn@coxnews.com>
>> Date: Fri, 6 Jun 2003 14:11:09 -0400
>> X-MIMETrack: Serialize by Router on CN-ATL-NML01/Coxnews(Release 5.0.8
>> |June 18, 2001) at
>> 06/06/2003 02:10:43 PM
>> X-MIME-Autoconverted: from base64 to 8bit by
>> multiproxy.evsc.virginia.EDU id h56IGuD17232
>>
>>
>>
>>
>> I covered a hearing by the Senate Environment and Public Works Committee
>> yesterday and Sen. Inhofe broke away from the agenda for a few minutes to
>> lecture some White House guy on the importance of having sound science,
>> yahyah, etc. As an example, he said, this new study about the medieval
>> warming period casts a whole new light on the global warming issue.
>> Then he
>> said that this was the work of 240 of the best scientists in the world.
>>
>> jeff n.

> Professor Michael E. Mann
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> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
 >
 </x-flowed>

2103. 2003-06-07

cc: Tom Wigley <wigley@ucar.edu>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Kevin Trenberth <trenberth@ucar.edu>, mann@virginia.edu, Tom Crowley <tcrowley@duke.edu>
 date: Sat, 07 Jun 2003 21:59:06 -0400
 from: tcrowley@duke.edu
 subject: Re: EOS text
 to: "Michael E. Mann" <mann@virginia.edu>
 Quoting "Michael E. Mann" : Mike, thank you for your comments; in principle I agree with them, but you cannot have it both ways - you cannot state that you are addressing a general problem of logical errors in alternate paleoclimate reconstructions, and then go out of your way to single out Soon and Baliunas with every point and a closing barb. I agree with you that it should be kept at a general level. That is why I would argue that you consistently keep the critiques at a general level as to why people run into trouble when looking at different data - the need for temperature proxies, different temporal and spatial patterns of warming, and failure to adequately define what is the "present", ie, late 20th century. stated from that viewpoint there is NO need to mention Soon and Baliunas after the original statement, and there is certainly not a need to make a charge of "flawed analyses with an apparently non-scientific agenda." why make a statement that is just going to turn off referees and AGU? I therefore argue that the rebuttal points be rewritten (shortened) to avoid singling out s+b, AND that the last part of the last paragraph be dropped. I also strongly feel that the Hegerl et al paper should be referenced - that paper compared a model with four different paleoclimate data sets - to my knowledge no one else has done that and concluded that for all the data the question of an anthropogenic signal in the 20th century is clear. I will be out of town and virtually incommunicado from Sunday to Wednesday so I am not sure if I will be able to respond before Thursday to any other thoughts you have. Regards, Tom > > Dear Tom and others...
 > Thanks Tom for the comments, several of which are quite helpful and > have > been incorporated into the attached, final (?) version. > Unfortunately, > you arrived very late in the game. Not your fault, but it does make > it > difficult to incorporate a number of your suggestions at this very > late > stage. I don't want to open up the "N body problem"
 > at > this point...
 > We've all already worked extremely hard to agree upon the > latest > wording (looks like the draft you were working on was actually > slightly out of date, so some of

cg2003

your

suggested changes are similar > to > ones that were already made in the most recent draft).

One thing > we've > been over several times now is the issue of why we need to > reference

the S&B papers as tangentially as we do--it has to do > with > the instructions that were

given to us by Judy Jones as to how we may > and > may not refer to the S&B work, given the

nature of the Eos > "forum" rules, since the S&B papers didn't appear in > Eos. I've

forwarded Judy's email to you, in case you missed it. > Tom > W is drafting a separate

piece, which more directly targets the > S&B > piece, to be sent to "Climate Research" and

you want to > discuss > this with him. This will be good opportunity for a more directed, >

point-by-point rebuttal of S&B. We don't have the mandate to do > that > in this piece. We

can only use S&B as examples of more general > points. That's why they are simply

referenced, parenthetically, in > the > context of broader points that we are making. Re

the last few > sentences, > I think they give the piece some critical impact, and I'm

averse to > removing them. AGU is the final arbiter here--I'm sure they will tell > us > if

they feel we're out of bounds, when we submit the piece.

> Adopting your suggested additions would also put us about 500 words > over > the limit. I

would really like to discuss a lot of the things you > mention. But we simply cannot with

the length restrictions. We > already > worked extremely hard to hit the key points in about

1400 words (we > need > to be about 100 under the 1500 word limit, given the > larger-than-normal > number of references). I'm sure there are alternative ways

we could > achieve this, but we've already worked very hard to arrived at the > particular version we

have.

> In summary, I apologize we can't incorporate several of your > comments at this point,

and I hope you feel comfortable enough with > the > attached, revised version that you're

willing to sign on.

> I want to close the text today or tomorrow, so I'm proposing the > attached > as a

tentative final version, pending Phil's finalization of Figure > #2 > and caption, and

only, please (!), the most minor of any additional > suggested changes in wording now!

> Thanks for understanding,

> mike

> At 04:18 PM 6/7/2003 -0400, Tom Crowley wrote:

>

Hi,

> I've been out of town and therefore out of the loop with respect to > the > recent

flurry of emails on the EOS piece. I have my own views > on > the writeup that are a

little different than what is presently > included - > I offer the changes for your

contemplation - note that my comments > are in > green and include Kevin's

cg2003

modifications

in red. there are some > places > where the two texts don't mesh because I did not want

to be changing > anything that Kevin wrote before everybody saw everything.

> Tom

> ps suggest that people date any future modifications and list > who > is doing the

modification (see my file name as an example).

>

> --

> Thomas J. Crowley

> Nicholas Professor of Earth Systems Science

> Dept. of Earth and Ocean Sciences

> Nicholas School of the Environment and Earth Sciences

> Box 90227

> 103 Old Chem Building Duke University

> Durham, NC 27708

> tcrowley@duke.edu

> 919-681-8228

> 919-684-5833 fax

>

>

>

> &nb> sp; > Professor Michael E. Mann
> > Department > of Environmental Sciences, Clark Hall
> &nbs> p; > University of Virginia
> &nbs> p; > Charlottesville, VA 22903

>

> e-mail: mann@virginia.edu Phone: (434) > 924-7770 > FAX: (434) 982-2137

> > [1]eudora=>

"autourl"><http://www.evsc.virginia.edu/faculty/people/mann.shtml>

>

285. 2003-06-10

cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

date: Tue, 10 Jun 2003 08:26:12 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Figure 1

to: Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford <srutherford@gso.uri.edu>

Hi Phil,

Still here, heading out tomorrow but put the "vacation" message up already.

Your/Keith's/Tim's suggestions all sound good to me. Re last sentence,

penultimate

paragraph: Re-reading it I can see that it does potentially could be interpreted in the

wrong way--its meant simply to say that you can't criticize the conclusions of relative

warmth of the 80s/90s w/ records that don't contain or resolve the latter 20th century, as

SB03 do--but it sounds like a criticism of such records, and that's not what we want it to

be. Feel free to agree on an appropriate re-wording that still conveys the point we are

trying to convey...

Re, Figure #1--all sounds reasonable too. I'd only differ on a few minor things. Scott and

I have experimented a lot w/ line types/thicknesses, etc.

I take it none of you are partially color blind? Out finding has been that using too thin

coloured lines makes them indistinguishable to many people. The thicker coloured lines are

easier to make out, for people who have trouble distinguishing fine colour

cg2003

differences. So

I'd lobby for the thicker lines, using thin lines in a few cases to draw further distinctions (with this many curves, we need to use colour, thickness, and line pattern

type as much as possible, to distinguish).

You guys should decide mutually what is best (I really will be off tomorrow), but I would

encourage sticking w/ the thick lines where possible, using a few thin lines to create

contrast when necessary.

I think we can change the colour of the thin gray line to make it more distinct against the

grey background--I didn't really like that choice either. I think a different colour would

fix this...

The scaling should be clarified in the caption. I believe (Scott?) that we've scaled the

1856-1980 trends to be equal to those of the instrumental annual full NH mean record,

after setting the means equal over the same interval 1856-1980. One can also scale the

variance (as you and I did in our submitted GRL article) and the result is basically the

same...

The only exception is Briffa et al MXD, where the 1856-1940 period is used instead

(because it starts to diverge downward about 1940 relative to the NH annual mean record).

We also don't show it after 1940.

I agree this has to be made very clear in the caption, and Scott should be able to help you

guys make sure the caption is accurate.

Thin black line to show reference period (zero) mean is a good idea too.

I'll be online through tomorrow morning in case you guys need any more feedback from me.

By the way, Phil: I told Peck to get in touch w/ you about signing on.

cheers,

mike

At 12:21 PM 6/10/2003 +0100, Phil Jones wrote:

Scott (and Mike if he's still there),

The three of us have been through the text, Fig 1 and decided what to put in Fig

2.

Tim is doing Fig 2 (9 long series - we'll send when we have it). I'm modifying the text

slightly - adding in refs that are missing (mostly with Fig 2) and generally tidying

up.

Keith is working on the final sentence of the penultimate para. We all agree with this,

but it could be misinterpreted - so trying to avoid this.

WRT Fig 1.

There are quite a few changes we think would improve things and make it more

consistent,

all to the labelling.

1. Add et al to Bauer and Gerber (twice).

2. Years only in for Mann et al., so this is the only one where refs would be ambiguous.

3. So, Briffa et al 2000 becomes Briffa and Osborn 1999

4. Briffa et al, 2001 becomes Briffa et al .

5 Remove Long instrumental - the orange line from the plot and key. It isn't explained

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in the
caption, nor in the text.

lines 6. As the grey line may not be seen under the grey shading, we think that all
should

same be as thin as the grey one. Some are thicker than others - can all be the
thinness.

2003 7. Back to key, change optimal borehole (Mann et al, 2003) to Mann et al.
(optimal borehole) for consistency with the others.

annual) 8. Most important is the SCALING. Needs to be clear which are scaled (to
and

which aren't. Text in caption is ambiguous. So can you tell us which is scaled (to
annual)

and which aren't. If they are scaled then key should say - scaled 1856-1980 as
with Jones et al.

Does this apply to Briffa and Osborn and to Briffa et al (the grey and
orange lines).

giving the 9. whilst on scaling are all scaled or regressed? Scaling we think of as
the same

mean and variance. Regression does this also but which has been used.

line from 0 10. Finally, Figure would look good with a thin black line along the zero
to 2000.

email Call me or Tim if anything you don't follow. Try Mike as well. I sent him an
earlier

today and he'd already put his reply message up for the next 4-5 weeks.

Cheers
Phil

At 12:25 09/06/03 -0400, Scott Rutherford wrote:

Mike and Phil,

You can view Attached is figure 1. The format is Adobe Illustrator with an embedded PDF.

it in Acrobat. Let me know if you have questions.

Regards,
Scott

Scott Rutherford

Marine Research Scientist
Graduate School of Oceanography
University of Rhode Island
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phone: (401) 874-6599
fax: (401) 874-6811
snail mail:

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Narragansett, RI 02882
Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
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Professor Michael E. Mann

Page 388

cg2003
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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

595. 2003-06-10

cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
date: Tue, 10 Jun 2003 10:01:17 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Figure 1
to: Scott Rutherford <srutherford@gso.uri.edu>, Phil Jones <p.jones@uea.ac.uk>

Note correction. Sorry,
mike
At 09:58 AM 6/10/2003 -0400, Michael E. Mann wrote:
HI Scott,
I wouldn't bother w/ a version with all thin lines--will be too difficult to
tell apart
the different colors (for me anyway, and I bet for lots of people).
Instead, why don't you try a scheme that uses a combination of thick and thin.
What about thick-dashed for models, thick-solid for, and thin for dendro only
(would include Esper).
What about thick-dashed for models, thick-solid for multiproxy estimates, and
thin for dendro only (would include Esper).
You could also try a version where the "thick" lines aren't quite as thick?
thanks,
mike
At 09:51 AM 6/10/2003 -0400, Scott Rutherford wrote:
Phil et al.,
I will work on the figure later today. I'll produce one with all
thinner lines and we can see how it looks. There is a substantial
portion of the male population that is red-green colorblind to various
degrees so we do need to be careful.
Scott
On Tuesday, June 10, 2003, at 07:21 AM, Phil Jones wrote:
Scott (and Mike if he's still there),
The three of us have been through the text, Fig 1 and decided
what to put in Fig 2.
Tim is doing Fig 2 (9 long series - we'll send when we have it). I'm
modifying the text
slightly - adding in refs that are missing (mostly with Fig 2) and
generally tidying up.
Keith is working on the final sentence of the penultimate para. We
all agree with this,
but it could be misinterpreted - so trying to avoid this.
WRT Fig 1.
There are quite a few changes we think would improve things and
make it more consistent,
all to the labelling.
1. Add et al to Bauer and Gerber (twice).
2. Years only in for Mann et al., so this is the only one where refs
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3. So, Briffa et al 2000 becomes Briffa and Osborn 1999
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5 Remove Long instrumental - the orange line from the plot and key.
It isn't explained in the
caption, nor in the text.
6. As the grey line may not be seen under the grey shading, we think
that all lines should

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be as thin as the grey one. Some are thicker than others - can all be the same thinness.

7. Back to key, change Optimal borehole (Mann et al, 2003) to Mann et al. 2003 (Optimal borehole) for consistency with the others.

8. Most important is the SCALING. Needs to be clear which are scaled (to annual) and which aren't. Text in caption is ambiguous. So can you tell us which is scaled (to annual) and which aren't. If they are scaled then key should say - scaled 1856-1980 as with Jones et al.

Does this apply to Briffa and Osborn and to Briffa et al (the grey and orange lines).

9. Whilst on scaling are all scaled or regressed? Scaling we think of as giving the same mean and variance. Regression does this also but which has been used.

10. Finally, Figure would look good with a thin black line along the zero line from 0 to 2000.

Call me or Tim if anything you don't follow. Try Mike as well. I sent him an email earlier

today and he'd already put his reply message up for the next 4-5 weeks.

Cheers

Phil

At 12:25 09/06/03 -0400, Scott Rutherford wrote:

Mike and Phil,

Attached is figure 1. The format is Adobe Illustrator with an embedded PDF. You can view it in Acrobat. Let me know if you have questions.

Regards,

Scott

Scott Rutherford

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2895. 2003-06-10

cc: p.jones@uea.ac.uk, t.osborn@uea.ac.uk
date: Tue Jun 10 14:53:21 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: possible rewording of section of letter?
to: "Michael E. Mann" <mann@virginia.edu>
thanks for that Mike - now the reference to "agree remarkably well with the proxy-based reconstructions (Figure 1) " [later part of paragraph] . Unfortunately , the Bauer et al curve clearly does not - at least from AD 1100 to 1400!
Again some qualify is needed - perhaps "for the most part , agree well " ?
and later [middle of the 6th paragraph],
"relative hemispheric warmth during the 10th to 12th centuries" is ambiguous and we prefer
"relative hemispheric warmth during the 10th,11th and 12th centuries"
At 08:53 AM 6/10/03 -0400, Michael E. Mann wrote:
Thanks Keith,
I agree w/ you entirely, and the revised wording seems better indeed.
It definitely has my blessing.
Thanks for the help,
mike
p.s. I'm available through tomorrow morning in case there are any other important last-minute issues that arise
At 01:15 PM 6/10/2003 +0100, Keith Briffa wrote:
Mike
I know you up to your neck in marital bliss , and I am sorry to bother you , but on the advice of Phil I thought it worth asking for your sanction of the following rewording of the end of the penultimate paragraph of the letter.
This is, we believe, important because the original phrasing is a large hostage to fortune, given that it seems to criticise (completely rubbish might be a better phrase) all work based on proxies that do not actually resolve the "climate trends of the last few decades" . As you know, many proxies used by you , us, and others, do not extend over this period of rapid warming and some that do (eg our MXD data) do not display an appropriate rapid response. What you have written could conceivably be twisted to imply that we (you) are criticising our (your) own work. How about changing the section with currently reads -

cg2003

The conclusions , for example, of theof temperatures during the most recent decades against reconstructions of past temperatures, taking into account the uncertainties in those reconstructions. As it is only the past few decades during which Northern Hemisphere temperatures have exceeded the bounds of natural variability, any analysis (SB03) that considers simply '20th century' mean conditions , or does not properly resolve the changes of the late 20th century (e.g. through the interpretation of evidence from proxy indicators which do not resolve the climate trends of the past few decades), cannot yield any insight into whether or not recent warming is anomalous in a long-term and large-scale context.

to -

The conclusions , for example, of theof temperatures during the late 20th century against reconstructions of past temperatures, taking into account the uncertainties in those reconstructions. As it is only the past few decades during which Northern Hemisphere temperatures have exceeded the bounds of natural variability, any analysis (SB03) that considers simply '20th century' mean conditions, or interprets past temperatures using the evidence from proxy indicators not capable of resolving decadal-timescale trends, can provide only very limited insight at best into whether or not recent warming is anomalous in a long-term and large-scale context.

--
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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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--
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[3][http://www.cru.uea.ac.uk/cru/people/briffa\[4\]/](http://www.cru.uea.ac.uk/cru/people/briffa[4]/)

2945. 2003-06-10

cc: phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>,
t.osborn@uea.ac.uk
date: Tue, 10 Jun 2003 14:26:07 -0400
from: "Michael E. Mann" <mann@virginia.edu>

cg2003

subject: Re: EOS text

to: Scott Rutherford <srutherford@gso.uri.edu>

HI Scott,

I concur w/ your assessment--keeping the figure the way it is now is preferable in my opinion...

mike

At 02:23 PM 6/10/2003 -0400, Scott Rutherford wrote:

Dear All,

I agree that figure 1 is very busy, but I'm not sure that is a bad thing in this case

because we aren't trying to highlight differences between reconstructions/models or

single out one or two from the rest. I think the current figure illustrates the range of

reconstructions, the range of models and how well they agree (similar to one of our

original ideas of a "cloud of reconstructions").

If we put the models into a separate panel we will need a curve common to both panels

that people can use as a reference. If we go with the two panel figure I suggest that

the second panel include the models, the Mann et al. 1999 reconstruction with uncertainties and the instrumental record.

I'll leave it to the group to decide.

-Scott

On Tuesday, June 10, 2003, at 01:16 PM, Michael E. Mann wrote:

I don't really like the idea of changing the figure dramatically at this point.

If we have to, I suggest the following options:

1) Take out one of the model simulation results--e.g. Gerber et al w/ the lower

sensitivity

2) If we want to adopt Kevin's two panel strategy, then show the model results along w/

the gray-shaded uncertainty region from the top (reconstructions) panel. And show the

instrumental record in both panels.

Anyway, up to you guys...

mike

At 10:59 AM 6/10/2003 -0600, you wrote:

Phil

Thanks for the great work.

Some reactions.

1) Fig. 1 is very busy and perhaps unduly crowded. My reaction is to take the model

results out and put them in a separate panel. The separate panel would fit along side

the key. But better below the main figure.

Can we change "gridded and arealy weighted" to "gridded, area-weighted..")

What is "optimal borehole",? Should "optimal" be in quotes?

2) Fig. 2: Can we please add a country to each name for those that don't have them?

Increased spacing between them would be nice.

Thanks

Kevin

Phil Jones wrote:

Dear All,

also Keith, Tim and I have been at this for part of the day. Scott has

redrawn Fig 1.

Attached is the latest draft, which includes Kevin's from about 1 hour ago, but not

cg2003

Ray's
latest email.

Fig 1 from Scott is OK to us here. Fig 2 is a draft. Tim needs to
space the series out a little. To use all these we've needed to add a load of references.
Getting these and making the captions OK has taken most time and the drawing of Fig 2.
Hopefully we can all agree to this in the next day or so, then I'll
submit on say Thursday UK morning time, so you've all got all day today and tomorrow.
We've been through the text carefully and all happy with it.
Apologies - no time to make Fig 2 pdf. Hope all can see postscript. We
still need to work on the captions and tidy the refs a little more.
We'll be back at 8.30 tomorrow UK time. Peck - you've got 2 days to say
yes/no !

Cheers
Phil

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--

Kevin E. Trenberth	e-mail: trenbert@ucar.edu
Climate Analysis Section, NCAR	[1]www.cgd.ucar.edu/cas/
P. O. Box 3000,	(303) 497 1318
Boulder, CO 80307	(303) 497 1333 (fax)
Street address: 3080 Center Green Drive, Boulder, CO 80301	

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</blockquote></x-html>

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

148. 2003-06-11

date: wed, 11 Jun 2003 12:32:18 +0000
from: "Mick Kelly" <m.kelly@uea.ac.uk>
subject: Keeping the roof on...
to: 'cru.all@uea.ac.uk'
-----boundary-LibPST-iamunique-741984459_--
Content-Type: text/plain; charset="utf-8"
I'm running the Norwich half marathon again this year, raising money to maintain the fabric of our local church in Cawston, St Agnes (http://www.norfolkcoast.co.uk/churches/ch_cawston.htm).
As I've run the Norwich half, as we veterans call it, five times in the past I'm reluctant to ask for sponsor money per se. But if anyone would like to place £5 or more on me finishing in 2hr 10 minutes or less then email me back. This means you pay if I finish in 2hr 10 minutes or less. If I don't you won't hear from me. See below for assessment of form...
Form
Previous personal best (2002) - 2 hrs 13 minutes
Injuries - sprained ankle August 2002, 6 months away from training, largely cleared up but occasional scar tissue problems
Weather forecast - temperature 20 C or more, dry, low winds - this seriously argues against a fast pace
Course - many hills considering it's Norfolk
Tactics - going for negative split (slow first half and faster second half) - good strategy - but pace runner injured so running solo (very bad news as Mick slows down when alone)
Attitude - very good
Odds - 20% chance Mick can break the 2hr 10min barrier, most likely to run slower than 2hr 15min this year

Mick Kelly Climatic Research Unit
School of Environmental Sciences
University of East Anglia Norwich NR4 7TJ
United Kingdom
Tel: 44-1603-592091 Fax: 44-1603-507784
Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

5284. 2003-06-11

cc: david.roberts@metoffice.com, andy.jones@metoffice.com,
jonathan.gregory@metoffice.com, jason.lowe@metoffice.com,
richard.betts@metoffice.com, tcrowley@duke.edu, k.briffa@uea.ac.uk,
t.osborn@uea.ac.uk, margaret.woodage@metoffice.com
date: wed, 11 Jun 2003 13:45:09 +0100 (BST)
from: Simon Tett <simon.tett@metoffice.com>
to: keith.williams@metoffice.com
Subject: Title and Abstract
BCC: simon.tett@metoffice.com
--text follows this line--
Keith (CC co-authors) -- here is my seminar title, co-authors and abstract.
Simon

Simulating the Recent Holocene
Simon F. B Tett, Richard Betts, Keith Briffa (CRU, UEA),
Tom J. Crowley (Duke), Jonathan Gregory (Reading), Andy Jones,
Jason Lowe, Tim Osborn (CRU, UEA), David L. Roberts and

cg2003

Margaret J. Woodage

A simulation of the last 500 years using natural forcings alone has been carried out. The forcings considered are volcanic aerosol, solar irradiance and orbital changes. Greenhouse gases and land-surface values are set to "pre-industrial" values. On multi-century timescales this simulation has a stable climate though multi-decadal variability, driven by external forcing, is present. If this is correct then the recent Holocene would have been stable in the absence of anthropogenic influences. Maximum changes in sea-level are about 2cm from 1820 to 1950. In the simulation glaciers would have reached their maximum advance in the early 18\th and mid-19\th centuries. No evidence of an orbital influence on simulated climate is found.

The simulation agrees well with proxy reconstructions of temperature though there is some evidence that the model may be over-sensitive. Natural forcing enhances variability. In particular tropical temperature decadal-variability is enhanced by a factor of two. Large-scale precipitation is also enhanced but only on 50-year time-scales is there a significant enhancement, relative to the control simulation, of northern hemisphere land precipitation.

A second experiment from 1750 to 1999 using both anthropogenic and natural forcings has just completed. The anthropogenic effects considered are changes in sulphate aerosol, greenhouse gases, ozone and land-surface changes. Preliminary results from this suggest an anthropogenic effect as early as the late 19th century.

--

Dr Simon Tett Managing Scientist, Data development and applications.
Met Office Hadley Centre Climate Prediction and Research
London Road Bracknell Berkshire RG12 2SY United Kingdom
Tel: +44 (0)1344 856886 Fax: +44 (0)1344 854898
E-mail: simon.tett@metoffice.com <http://www.metoffice.com>

100. 2003-06-12

date: Thu, 12 Jun 2003 10:16:48 +0000
from: "Mick Kelly" <m.kelly@uea.ac.uk>
subject: BP
to: Phil Jones (p.jones@uea.ac.uk)
-----boundary-LibPST-iamunique-1346431930_--
Content-Type: text/plain; charset="utf-8"

Phil

Can you introduce me as senior member of CRU who did a fair bit of work with BP in the 80s and Deputy Director of Graduate Studies for UEA. Latter might be helpful if we discuss studentship support.

If I get called out for this other ERSC meeting then apologise on my behalf. Don't think I will but I've just been put centre stage at 12 and might have to put some material for the visitors together later.

Thanks
Mick

Mick Kelly Climatic Research Unit
School of Environmental Sciences
University of East Anglia Norwich NR4 7TJ
United Kingdom
Tel: 44-1603-592091 Fax: 44-1603-507784
Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

2954. 2003-06-13

cg2003

cc: <p.liss@uea>, "Eastwood David Prof \ (VC0\) k340" <D.Eastwood@uea.ac.uk>, "Ros Pye" <ros.pye@uea.ac.uk>

date: Fri, 13 Jun 2003 11:54:13 +0100

from: "Trevor Davies" <T.D.Davies@uea.ac.uk>

subject: BP

to: <h.j.schellnhuber@uea.ac.uk>, <m.hulme@uea>, <t.oriordan@uea>, <e.l.jones@uea.ac.uk>

John/Mike,

Peter had some feedback from BP on the ride down to the station. The VC happened to be on

the same train as them, and had opportunity to talk over the day and gain further insight

into BP's ideas of where they see themselves in 20 years time.

They see some clear possibilities with some of the "corporate risk" work of Nick Pidgeon

and a connection with CRed. Ahead of the day they had anticipated cooperation of some sort

with TYN, although they had no clear view of what that might be. After the discussion in

the Callendar Room they felt that they still had not identified the 'big idea' around which

to start a relationship. Peter and the VC's view is that they had a presumption towards

being convinced if TYN can come up with right idea/form of relationship, and given TYN's

position/status, they would expect TYN to take the lead in this regard. They are obviously

prepared to think at the highest level - their reference to 3/4 of the top 20 in the

company (& reps of similar status from other major companies).

Can I suggest that we have de-briefing with PSL and the VC reporting back on impressions &

a start on what TYN may be able to suggest? Clearly there would then need to be consultation with TYN N&S etc.

PSL is away from the middle of next week. VC is happy to find an hour (courtesy of Ros). If

you are happy with this, could you ask Vanessa to try to fix up a meeting. My sec is away

today.

ENV has an internal exam board Mon 09.15m, but we should be OK from 12.30 onwards. I could

make before 09.00, 12.30-15.30, after 17.00.

Tuesday I could make before 08.30, 9-10, 15.30-16.30, 17.30-19.30

These times any use?

Trevor

Professor Trevor D. Davies

Dean

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ

Tel +44 (0)1603 592836

Fax +44 (0)1603 593792

78. 2003-06-16

date: Mon, 16 Jun 2003 07:58:55 +0100

from: Phil Jones <p.jones@uea.ac.uk>

subject: Fwd: 2003JD003695 Decision Letter

to: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

<x-flowed>

>Subject: 2003JD003695 Decision Letter

cg2003

>From: jgr-atmospheres@agu.org
>Reply-to: jgr@envsci.rutgers.edu
>Date: Sun, 15 Jun 2003 23:36 -0400
>To: p.jones@uea.ac.uk
>Cc:
>
>
>Dear Phil:
>
>Below please find 3 reviews of your paper "Changes in the Northern
>Hemisphere annual cycle - implications for paleoclimatology?." The
>reviewers have suggested revisions to your manuscript. Please take the
>reviewers' remarks into consideration and adequately address their
>questions and concerns with a revision of your manuscript.
>
>Please submit your revised manuscript and a detailed response to each
>question and comment of the reviews. The revised manuscript must be
>returned within one month of receipt of this letter. Failure to meet this
>deadline may result in the revised manuscript being handled as a new
>submission. If you feel that you cannot address all comments and revise
>the paper within one month, please contact me immediately.
>
>When you are ready to submit your revision, please use the link below.
>
><<http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A2Bc2aDy7A4lSh1I1A9eb9Druz30naEdqvBhOGLKQZ>>
>
>
>(NOTE: The link above automatically submits your login name and
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>please be aware that they will have access to your entire account for this
>journal.)
>
>Please note that all parts of the manuscript must be double-spaced and
>single-sided (including references, figure captions, and tables). Also,
>the references need to be on a page of their own, separated from the text
>of the manuscript. For further information on all editorial policies,
>please see our homepage at <http://www.envsci.rutgers.edu/jgr>
>
>Thank you for choosing the Journal of Geophysical Research - Atmospheres.
>
>Sincerely,
>
>Alan Robock
>Editor, JGR-Atmospheres
>
>-----
>
>At http://agu.org/pubs/au_contrib_rev.html for full instructions on how to
>prepare your final manuscript text file.
>
>For information regarding manuscript image requirements, please go to
><http://agu.org/pubs/guides3a.html>. It is critical that the correct image
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>resolution, line weights and color/grayscale requirements. Color figures
>that will appear as such in the print version of the journal should be
>submitted as CMYK. Images that will appear in color only in the HTML
>version on-line may be submitted in RGB.
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>http://agu.org/pubs/journal_forms/colorpricing.html
>

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>downloaded for completion and sent to AGU before your article may be
>published. The Publications Charges form also includes the NEW color
>pricing options which were revised in May 2002.
>(http://www.agu.org/pubs/journal_forms/PUBOPT_JGR02.pdf)
>
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>soon as you know that your manuscript is accepted. If you need assistance
>with file formats please e-mail jgr_atmospheres@agu.org (Natalie Reid) and
>quote your manuscript number. For more information on color charges,
>please contact Natalie Reid or author.help@agu.org.
>
>If you need Adobe Acrobat Reader to download the forms, it is available,
>free, on the internet at: <http://www.adobe.com/prodindex/acrobat/readstep.html>
>
>
>-----
>
>
>Reviewer Comments
>
>Reviewer #1 Evaluations:
>Assessment: Category 1
>Ranking: Excellent
>
>Reviewer #1(Comments):
>
>The authors of this manuscript are known as the best empirical
>climatologists of the modern world. In this paper they found that
>winters have warmed relative to summers during past two centuries
>compared to earlier part of the millennium. The paper discusses possible
>mistakes in interpretation of proxy, preinstrumental, climatic records
>related to seasonal cycle in climatic trends. And, we know that such
>mistakes are common in paleoclimatic reconstructions.
>The paper is short, well written and properly illustrated. I
>expect that it will be interesting for many readers of JGR-Atmosphere. I
>recommend it to be published as is.
>
>
>Reviewer #2 Evaluations:
>Assessment: Category 2
>Ranking: Very Good
>
>Reviewer #2(Comments):
>
>General Comments:
>
>This is an interesting manuscript, raising some important issues
>regarding seasonality of past temperature trends that are interesting in
>their own right, and may have potential implications for certain
>paleoclimate reconstructions. These issues are worthy of discussion in the
>literature, and JGR is an appropriate venue. The authors, as is typical,
>have done a careful job with their analysis, and it appears sound, as do
>the primary conclusions, although I have some specific reservations. The
>primary criticism is that the authors imply a greater generality to their
>conclusions than can actually be justified, given the limitations of the
>available data series. There are a number of important caveats that need
>to be invoked in the interpretation of the results, and the limitations in
>drawing large-scale conclusions from the limited data need to be
>acknowledged up front. There are a number of underlying issues regarding
>the nature of the seasonal and spatial details of past climate change (in
>particular, forced climate change) which likely impact the interpretation

>of the results, which are not given adequate discussion in the manuscript
 >at present. Given the space available in a JGR paper (vs. e.g. a GRL
 >article), there is no excuse for not providing more detailed discussion
 >where appropriate. I provide several specific comments below along these
 >lines which should be addressed in a revised version of the manuscript.

>
 >Specific Comments

>
 >1) Abstract--the generality of the conclusions are overstated in the
 >abstract. The evidence is only from Europe and China (i.e., only the
 >fringes of the Eurasian continent alone) but the wording argues that
 >implications apply to other regions. It isn't even clear that the
 >conclusions apply to the interior of the Eurasian continent, let alone any
 >of North America (see comments below). It is a leap of faith, then, to
 >assume that the results generalize to extratropical hemispheric (let
 >alone, full hemispheric) trends, and the authors need to be more cautious
 >in drawing general conclusions.

>
 >2) Introduction, first sentence: There is a potential "straw man" argument
 >being introduced here. Precisely which "annual temperature"
 >reconstructions are being referred to here? The statement made could
 >arguably apply to Crowley and Lowery (2000), which is based on scaling a
 >composite of largely extratropical (and mostly summer-sensitive) proxy
 >records against the annual mean Northern Hemisphere mean instrumental
 >series. It is far more difficult, however, to argue that the authors'
 >statements fairly characterize the Mann et al (1998;1999) annual mean
 >temperature reconstruction. In the latter case, half of the area of the
 >hemispheric mean surface temperature reconstruction comes from tropical
 >latitudes (i.e., latitudes below 30N), and the proxy indicators primarily
 >used to calibrate the tropical annual-mean patterns of variance are almost
 >certainly not boreal warm-season in nature (for the example, the
 >ENSO-scale patterns of tropical SST variance in the reconstruction are
 >calibrated, in large part, by a combination of cold-season drought
 >sensitive tree-ring data from Mexico, tropical tree-ring data, and
 >tropical corals and ice cores--none of which could be argued to exhibit a
 >boreal warm-season sensitivity bias!). The authors arguments cannot be
 >argued to apply to these reconstructions (as seems to be implied by later
 >comments--see below).

>
 >3) Discussion of Figures 1 and 2 on pages 5-6: the authors should compare
 >a single long-term composite series based on averaging the various
 >(potentially, standardized) station JJA-DJF series with that which is
 >available for the full NH back through the mid 19th century. The point
 >here is to see how well they compare in terms of the general trends
 >during the interval (back through the mid 19th century) of overlap--in
 >fact, based on inspection of e.g. Figure 1, I don't think that there will
 >be much similarity, and, if that is the case, then it demands extreme
 >caution in generalizing about the true large-scale or hemispheric nature
 >of inferred trends in summer-winter temperature differences based on the
 >sparse long series available to the authors.

>
 >4) Related to point #3 above, recent studies (see e.g. the discussion in
 >the Mann, 2002 piece which is in the reference list but not actually
 >cited in the text, and also the results of Shindell et al, 2003) have
 >shown that large seasonal differences in temperature trends are expected
 >in past centuries because of the seasonally-specific response, in
 >particular, to volcanic forcing (see Kirchner et al, 1999). The largest
 >seasonal differences are likely to occur in the continental centers, where
 >volcanic forcing tends to impart a large summer cooling but also typically
 >a sizeable dynamically-induced warming (related to the response of the
 >Northern Annual Mode, or 'AO' or 'NAO' to volcanic stratospheric aerosol
 >forcing) in the following winter. The large differences, however, are
 >observed over the continental centers, and in fringe regions such as

>Europe or China, the response may not even be of the same sign as the
 >continental mean response, which is dominated by the behavior of the
 >continental centers. Thus, any spatial network (proxy or instrumental)
 >which exhibits a bias with respect to the sampling of the continents is
 >likely to exhibit a bias in terms of the estimate of summer-winter
 >temperature differences (Mann, 2002). Since the authors instrumental
 >network only samples the fringes of the Eurasian continent, it is very
 >unlikely to capture the true winter-summer difference in Eurasian
 >continental mean temperature, let alone Northern Hemisphere extratropical
 >continental (Eurasia and North America) temperature, let alone Northern
 >Hemisphere extratropical mean (land and ocean) temperature, let alone true
 >Northern Hemisphere (tropical and extratropical, land and ocean)
 >temperature! Once again, this calls for caveats in the interpretation of
 >the present results with regard to hemisphere-scale implications.

>

>5) Related to the above, why don't the authors show, in Figure 1, the
 >results for some of the long available North American series (which
 >includes several long east coast series, but also a series in Minnesota
 >back to the early 19th century) to establish the similarity of the
 >longer-term summer-winter trends in the two continents (this too should
 >be included in the composite discussed in point #3 above).

>

>6) End of first paragraph on page 6, the authors might note that certain
 >modelling studies (Shindell et al, 2003) have indeed already looked at
 >potential seasonally-distinct temperature changes in past centuries, that
 >are associated with the seasonally-distinct signature of the response to
 >known natural climate forcings.

>

>7) Figure 3 indicates a relationship that holds during the latter 20th
 >century, presumably somewhat specific to the mix of internal and forced
 >variability that dominates over that period. This may not be
 >representative of the situation in earlier centuries, where the primary
 >pattern of forced variability is by volcanic and solar forcing which
 >impart distinct regional and seasonal signatures in the temperature field
 >(see Shindell et al, 2001;2003) that are likely to be quite different from
 >those associated with anthropogenic forcing (GHG and aerosol) which
 >dominate during the interval examined by the authors. Related to this,
 >have the series been detrended before calculating the correlations shown
 >in Figure 3? This has a bearing on the interpretation.

>

>8) 3rd paragraph on page 7, the discussion of previous work (e.g. Mann et
 >al, 1998;1999) here is misleading for the reasons spelled out in point #2
 >above. The arguments assuming a warm-season sensitivity bias do not apply
 >to the full hemispheric reconstruction but, at most, the extratropical
 >component of the reconstruction. The statement (2 sentences up from
 >bottom of paragraph) "Their implicit assumption that the relative
 >trends..." is not a fair statement in reference to the Mann et al
 >multiproxy reconstructions, and the discussion needs to be revised here.
 >An analysis (Rutherford et al, to be submitted) shows, using a common
 >statistical method, but distinct data sets, that the multiproxy network of
 >Mann et al calibrates and cross-validates cold-season variability more
 >skillfully than the tree-ring maximum latewood density ('MXD') density
 >network of Briffa and coworkers, while the Briffa et al MXD network, in
 >turn, calibrates warm-season variance more skillfully than the multiproxy
 >network. In short, the conclusions drawn here don't apply to
 >reconstructions of tropical surface temperature variability, nor to
 >multiproxy data used to reconstruct that variability, so the implications
 >of the authors results for multiproxy reconstructions of full Northern
 >Hemisphere annual mean temperature are not clear. The authors need to
 >downplay their conclusions in this regard.

>

>9) The authors and this reviewer are in common agreement that
 >seasonally-specific biases are likely to be present in most climate proxy

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>data, and that these biases need to be closely considered in the process of
>climate reconstruction. This is a fair point, and one worth emphasizing in
>the conclusions. But the specific conclusions of the authors in this study
>regarding summer-winter differences based on the series analyzed do not
>clearly generalize to other proxy-based surface temperature
>reconstructions (particularly multiproxy reconstructions with an equal
>tropical and extratropical emphasis) for the reasons spelled out above,
>and this point, in fairness, should be made.

>
>REFERENCES:

>
>Kirchner, I., G.L. Stenchikov, H.-F. Graf, A. Robock, and J.C. Antuna,
>Climate model simulation of winter warming and summer cooling following
>the 1991 Mount Pinatubo volcanic eruption, Journal of Geophysical
>Research, 104 (D16), 19039-19055, 1999.

>
>Shindell, D.T., Schmidt, G.A., Mann, M.E., Rind, D., Waple, A., Solar
>forcing of regional climate change during the Maunder Minimum, Science,
>294, 2149-2152, 2001.

>
>Shindell, D.T., Schmidt, G.A., Miller, R., Mann, M.E., Volcanic and Solar
>forcing of "Little Ice Age" Surface Temperature Changes, Journal of
>Climate, in press, 2003.

>
>
>Reviewer #3 Evaluations:
>Assessment: Category 1
>Ranking: Excellent

>
>Reviewer #3(Comments):

>
>Review of Jones et al. : "Changes in the Northern Hemisphere annual.."

>
>This paper addresses a very important problem in contemporary climate
>record analysis. It points out that several recent reconstructions of NH
>climate over the last thousand years might have some biases. The point
>being that the proxies used in those analyses were perhaps more sensitive
>to summer conditions than to mean annual conditions. This while recent
>instrumental records tell us that the winter temperatures are responsible
>for most of the warming in the annual average records.

>
>The present authors present data from several sites with 200-year records
>where instrumental (and other fairly reliable) data show that it is indeed
>the winter temperatures responsible for most of the recent climate change.
>I believe this is an extremely important contribution toward our gaining a
>better understanding of past climate records.

>
>The paper is well written and to the point. It can be published as is in
>my opinion.

>
>Caveat: I consider myself an expert on the overall problem of climate
>change, but I am not an expert in the details of the kind of data analysis
>involved in this project. It would be well to have another referee who is
>more versed in the arcane methods used in these analyses.

>
>
>
>
Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk

NR4 7TJ
UK

</x-flowed>

1646. 2003-06-16

date: Mon Jun 16 12:30:13 2003
 from: Keith Briffa <k.briffa@uea.ac.uk>
 subject: Re: Fitzroya RCS paper
 to: Edward Cook <drdendro@ldeo.columbia.edu>
 Ed (phoned without luck - but only 1 pm here)
 have received manuscript and am leaving for Brussels for a meeting until
 wednesday - will
 phone then pm. wish to discuss this
 and send comments- have NOT yet shown to Phil . wish to discuss work to produce
 one long
 tree-ring based reconstruction and visit to you (and NATO) . Very best wishes
 Keith
 At 04:58 PM 6/11/03 -0400, you wrote:
 Hi Keith,
 Here is paper I just thrashed out this week on coming up with a useful RCS
 chronology
 from the Fitzroya tree-ring data. I say "thrashed" because it was literally
 done from
 beginning to end in 3 days. However, I do think that there is some interesting
 stuff in
 it, especially with regards to the interpretation of the climate signal in
 Fitzroya. Any
 comments are as always appreciated. If you want to show it to Phil, that is
 fine.
 However, he should know that the data are not yet up for grabs for him and
 Mike to use.
 Admittedly, Phil might not like what I did and, therefore, not want to use it
 anyway.
 Cheers,
 Ed
 Fitzroya_RCS.pdf

--
 =====
 Dr. Edward R. Cook
 Doherty Senior Scholar and
 Director, Tree-Ring Laboratory
 Lamont-Doherty Earth Observatory
 Palisades, New York 10964 USA
 Email: drdendro@ldeo.columbia.edu
 Phone: 845-365-8618
 Fax: 845-365-8152
 =====

--
 Professor Keith Briffa,
 Climatic Research Unit
 University of East Anglia
 Norwich, NR4 7TJ, U.K.
 Phone: +44-1603-593909
 Fax: +44-1603-507784
 [1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

375. 2003-06-18

date: Wed Jun 18 12:33:52 2003
 from: Keith Briffa <k.briffa@uea.ac.uk>

cg2003

subject: Re: Sceptics' discourse

to: Timothy Carter <tim.carter@ymparisto.fi>

Hi Tim

]just back from couple of days in Brussels - will look at this when have time .
Cheers

Keith

At 09:57 AM 6/18/03 +0300, you wrote:

Dear Phil/Keith,

It was good to see you both last week.

I am copying you the English language part of an email I just received from a local

sceptic (a lawyer with no climate training at all!) which refers to some of your work. I

usually dispose of these mails as they come in, but this one seems to be calling into

question some published science. McIntyre is probably well known to you, but just in

case here it is. Sorry, but the figures referred to in this mail did not get through our

firewall.

Best regards,

Tim

From: [1]Steve McIntyre

To: [2]Climate Sceptics

Sent: Sunday, June 15, 2003 9:27 PM

Subject: [Climate Sceptics] More on Mann and Jones Datasets - Fennoscandia

Dear all,

More on the underlying datasets in the millenial datasets of Mann; Jones etc.

One of the prominent data compilation datasets is Fennoscandia, used in both

Bradley-Jones 1992 and Mann 1998, derived from Briffa et al. In fact, Briffa

makes

several essays at temperature reconstruction for northern Fennoscandia. There

seem to be

three main variants: (1) BETA1 is reported in Briffa et al 1990 (Nature) and

is based on

cubic splines; (2) a RCS version, both uncorrected and corrected , is reported

in

Climate Dynamics 7 (1992), where it is compared to the cubic spline version in

a useful

way (clearly showing the inappropriateness of cubic splines for long-term data analysis). Figure 8 in Clim Dyn 7 matches Fig 2(2) in Bradley-Jones 1992 and

my graph

produced from the Mann 1998 proxy 67 which all seem therefore to be the same dataset.

(3) a third reconstruction is reported on Fennoscandia by Briffa and Schweingruber in

Climate since 1500AD (1992) data on which is at [3]www.ngdc.noaa.gov. In c1500, Briffa

and Schweingruber do not reconcile to the prior discussions. I have graphed this dataset

together with the other one below. The correlation between the two datasets for the

overlapping period 1587-1975 is 0.032 an interestingly low correlation for what are

reconstructions produced from relate data. I have shown the two

The Climate Dynamics 7 reconstruction contains a fudge by Briffa et al, described as

follows:

The density chronology shows a low-frequency decline over the last century

which appears

anomalous in comparison with both the TRW data and the instrumental data over

the 19th

and 20th centuries. These facts suggest that the density-coefficients in the

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regression equation may be biased as would be the case if the density decline were not climate related (CO2 increases and/or the potential effects of increasing nitrogen input from remote sources may be implicated here.) &The residual MXD data (actual estimated) are plotted in Fig. 7. A systematic decline is apparent after 1750. By fitting a straight line through these residuals (1750-1980) and adding the straight-line values (with the sign reversed) to the RCS density curve, the anomalous post-1750 decline was removed. This corrected RCS curve was then used along with the RCS ring-width curve in a final reconstruction of the April-August temperatures. This hardly seems like justifiable statistical procedure. Without the fudge, the "reconstruction" shows declining temperatures in the 20th century. A very similar decline in residuals occurs from 1100 to 1250 and one wonders whether a similar adjustment would be allowable then. The Climate Dynamics article does not contain a description of the regression methodology and I have not yet consulted the predecessor article describing the regressions. Suffice it to say that the tree ring data is highly autocorrelated, as is (to a lesser extent) the temperature data. The meaning of such correlations is not clear. The reconstructions end up being a weighted sum of the tree ring widths over two summers and MXD s over two summers. The coefficients are very unstable under different reconstruction methodologies.

Regards, Steve McIntyre

--
Professor Keith Briffa,
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Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[4][http://www.cru.uea.ac.uk/cru/people/briffa\[5\]/](http://www.cru.uea.ac.uk/cru/people/briffa[5]/)

2530. 2003-06-18

cc: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk
date: Wed, 18 Jun 2003 16:46:22 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: 2003GL017814 Decision
to: Scott Rutherford <srutherford@gso.uri.edu>

Scott,
Forwarded this to Tim and Keith here. It does look good. I'm away tomorrow but I'll be back Friday.
If I spot anything I'll get back to you then.
Cheers
Phil
At 10:54 18/06/03 -0400, Scott Rutherford wrote:
Phil,
Attached is a revised figure that shows the Mann and Jones NH reconstruction instead of

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the Briffa and Osborn, 1999). I've also added the uncertainties. In Mike's e-mail he said the 2-sigma limits were ± 0.16 but I think those are actually 1-sigma. They are way too small and inconsistent with Mann and Jones 2003 to be 2-sigma. I used ± 0.32 for the uncertainties. I've also truncated the x-axis at AD 200 instead of 0. I've changed the figure legend to match but leave the actual text of the caption up to you since you have the final text version and know the background of the series. The figure is in Adobe Illustrator 10 with a pdf embedded.

Regards,

Scott

On Tuesday, June 17, 2003, at 11:42 AM, Phil Jones wrote:

Scott,

I'm off home now. Do you want to see if you can switch the two series around as

Mike

suggested. Replace the long Briffa one with the appended and alter caption accordingly.

I'll email Ellen and Judy to see if possible.

Cheers

Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu

X-Mailer: QUALCOMM Windows Eudora Version 5.1.1

Date: Tue, 17 Jun 2003 11:36:05 -0400

To: Phil Jones <p.jones@uea.ac.uk>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Fwd: 2003GL017814 Decision

Cc: Scott Rutherford <srutherford@gso.uri.edu>

Hi Phil,

Thanks--that all sounds very good. I'll go ahead and make these changes, and then send

you the PDF of the submitted file, hopefully later today.

I like your idea of checking w/ Ellen or Judy Jacobs if we can substitute in the NH

reconstruction (area and local-correlation weighted version) from the Mann and Jones

(2003) paper. When I originally asked, Judy said we probably couldn't do it, because it

was not accepted/in press. Now that it is, I'm sure we can substitute it for the long

Briffa series--I agree that would be better. I assume this is still possible, as long as

the piece hasn't gone into production--can you check w/ Judy and/or Ellen on this (and

cc to me)?

Also, would you mind working w/ Scott to get figure 1 modified appropriately--should

just be a simple switch of series. I've attached the ascii data for the Mann and Jones

NH reconstruction. We should probably also show the uncertainty limits as well for this

(slightly different shading color)? They are ± 0.16 for the 2 sigma limits. We'll also

need to modify the figure 1 caption, and to add the reference for Mann and Jones to the

Eos piece [Mann, M.E., Jones, P.D., Global Surface Temperatures over the Past two

Millennia, Geophys. Res. Lett., in press, 2003]. Can you and Scott cc me the

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modified

version of the Eos piece and figure when its done, if we go this route?
Hope to resubmit the GRL before I leave for Hawaii (if Lorraine lets me)...By

the way,

the borehole GRL paper should be out today or tomorrow!

talk to you later,

mike

At 11:53 AM 6/17/2003 +0100, Phil Jones wrote:

Mike,

Take a rest until the IUGG when you've got this off !! Subject to a few
alterations below,

I'm happy for you to send this back to GRL. Cover letter and responses to
reviewer are

fine.

Comments on new manuscript version.

1. You need to get rid of some tracked bits of text.

2. p3 line 8 of first para of methods, suggest adding possible to
uncertainties re the

dating -

they may be correct ! They appear to be over 1901-80 !

3. Middle para on this page, 1856-1980 should possibly be 1901-1980 as in the
Fig 1 caption.

I've used 1901-80 in all the local correlations - that you now plot in Fig

1. Maybe

you're

referring to correlations with the NH temperature average at this point.

4. Bottom of this para and the numbers you want. First can you add after

proxy

network on the

3rd last line (8 sites for the NH and 3 for the SH). This is just to make it
clear

which we've used

(see also comment later on Fig 2 caption).

I produced the NH and SH averages (weighted by area - so China and N. Russia
get a

bigger weight than the rest) and then calculated r-squared values (well r
values which

I

squared) over 1901-80 with the same hemisphere for both the land-only average
and the

land+marine average. Land/marine are better so I would go with these. On

the decadal

timescale (over these 80 years) the values you want are 0.73 for NH and 0.60
for SH.

This just goes to show that the instrumental record is too short to really
look at

this

properly. The values for land-only are 0.61 for NH and 0.20 for SH. By the
way the

same values for land-only with land+marine are 0.81 for NH and 0.64 for SH,
so the

73/60

numbers you'll use are amazingly high - especially as for the SH there are
only 3

series

and one of these only has data from 1957.

On the annual timescale the 73/60 numbers become 54/41 .

I wouldn't comment on the 73/60 numbers - we'll just wait to see if anyone
notices

them.

They should be an eye-opener to SB03 !! As I say though they are only based
on 80

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years
of data.
What we might think of with RoG is doing 2 NH reconstructions, putting
half our series into one and the other half in the other. Then we can look at low-freq over longer periods.
Need to choose which goes into which but we could do this maintaining spatial and proxy aspects.
Discuss more in Sapporo - I'll be at your hotel at 3pm on July 6.
5. Next para - it wasn't clear to me what the composites were so qualify by saying 'The hemispheric and global composites.'
6. 6th line of p4, change little to no or no hemispheric-scale - unless you're trying to refer to longer instrumental data. There is nothing before 1856 and some proxies don't go beyond 1980.
7. 2nd to last line of text on p5, suggest removing such.
8. Figure 2 caption. After sentence ending in AD 200, could add ' and all 8 back to AD 553 or 7 back to AD 256 and 8 back to AD 553. You've done this for the SH later.
Send me the submitted pdf. I've not heard any more about the EOS piece but Ellen has got it - I got an email from her to Judy.
I can send out this pdf if you want - to the group with the EOS piece and also to Ellen. I would suggest with EOS we add this series into Fig 1, back to AD200, possibly by replacing the long Briffa series.
Cheers
Phil
At 15:02 16/06/03 -0400, you wrote:
Thanks Phil,
We had a great wedding--Ray was there w/ Jane, now we're in San Fran, and I've promised Lorraine that I'll deal w/ email stuff this morning and maybe a bit tomorrow--otherwise its sightseeing. Glad to hear the seasonal paper is coming out soon--we can update the reference in ROG along w/ a few others soon, I hope. GRL is definitely faster--this one could appear in less than 2 months from the time of submission!
Hoping we can wrap up the revised version within the next couple days, before next leg of our trip (Hawaii)...
Attached is the revised version (w/ revised figures as provided by Scott included), cover letter, and response to reviewers, pending your final suggestions--yellow highlighted text indicates information that I am awaiting from you. Re, comment #5, I think we just need two numbers now to address the comment--the decadal correlations between the full NH and SH decadal instrumental series, and the series formed by

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arealy-averaging in each hemisphere only over the grid-boxes corresponding to the regions sampled by the proxy data. Do you have those two numbers, or can you calculate them easily enough? I suspect these are indeed quite high, and adding those (where I indicate in yellow highlighting) should be the last thing we need to do... Also, note that I've changed the way we smooth the series to preserve the late 20th century trend, like we did in the Eos piece. I've always estimated the uncertainties a bit more conservatively as described in text--so they're a bit expanded now. None of the conclusions change, although the globe is actually a bit more anomalous in the late 20th century when you spreserve the late 20th century trend in the smoothing, so I've tweaked the wording there just a bit... Once I hear back from you, I'll incorporate this final info, and any final comments you have, and resubmit via GEMS. Let me also suggest that we send out the revised draft to our Eos co-authors, and others like S. Solomon, and Ellen M-T and Mike Hulme, who are following these developments? Feel free to send it to others, now that it can be considered 'in press'. I don't think we should distribute it broadly, however, until we discuss e.g. a possible press release w/ Harvey Leiffert to coincide w/ the publication of the paper. Let me know what you think. looking forward to hearing back from you, thanks,
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cg2003

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3637. 2003-06-18

cc: Tom Wigley <wigley@ucar.edu>, "Michael E. Mann" <mann@virginia.edu>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>, Michael Oppenheimer <omichael@princeton.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, t.osborn@uea.ac.uk, jto@u.arizona.edu

date: Wed, 18 Jun 2003 08:17:34 +0100

from: Phil Jones <p.jones@uea.ac.uk>

subject: EOS and the GRL paper Mike has talked about

to: Kevin Trenberth <trenbert@cgd.ucar.edu>

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> Dear All,

Here's a brief update on the EOS article. It is currently with AGU and should go soon to

Ellen Mosley-Thompson for assessment/reviewing. Mike and I are trying to co-ordinate its

hopeful publication with the attached. This is the GRL paper that Mike has mentioned. Copy is

for your info, so don't pass around. Both reviews were positive and the attached is the

resubmitted version. If co-ordination isn't possible we will still replace the long Briffa et al

series (going back over the 2 millennia) in Figure 1 with the blue line from Figure 2a in the

GRL article. Text will alter, but only to refer to the new curve.

Mike is now finally on his honeymoon. He should be in Hawaii soon and we'll meet up during

the second week of IUGG.

I'm in discussion with AGU and Ellen about co-ordination as this should increase the impact

of both pieces. Mike or I will let you know when we hear more.

Cheers

Phil

PS This email only has Kevin once - apologies earlier !

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Attachment Converted: "c:\eudora\attach\mannjones-proxy-revised.pdf"

2064. 2003-06-20

cg2003

date: Fri, 20 Jun 2003 16:42:25 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Sceptics' discourse
to: Timothy Carter <tim.carter@ymparisto.fi>, k.briffa@uea.ac.uk

Tim,

Thanks for this. I've been in touch with this guy (Steve McIntyre) before. I think he works in the US. He asked me a few things about the instrumental data, then more, then more and asked for more data. I eventually gave up but he is quite able.

The Finn is Timo Hameranta (or something like that) and is right of right field !

Cheers

Phil

At 09:57 18/06/03 +0300, Timothy Carter wrote:

Dear Phil/Keith,

It was good to see you both last week.

I am copying you the English language part of an email I just received from a local

sceptic (a lawyer with no climate training at all!) which refers to some of your work. I

usually dispose of these mails as they come in, but this one seems to be calling into

question some published science. McIntyre is probably well known to you, but just in

case here it is. Sorry, but the figures referred to in this mail did not get through our

firewall.

Best regards,

Tim

From: [1]Steve McIntyre

To: [2]Climate Sceptics

Sent: Sunday, June 15, 2003 9:27 PM

Subject: [Climate Sceptics] More on Mann and Jones Datasets - Fennoscandia

Dear all,

More on the underlying datasets in the millennial datasets of Mann; Jones etc. One of the prominent data compilation datasets is Fennoscandia, used in both Bradley-Jones 1992 and Mann 1998, derived from Briffa et al. In fact, Briffa makes

several essays at temperature reconstruction for northern Fennoscandia. There seem to be

three main variants: (1) BETA1 is reported in Briffa et al 1990 (Nature) and is based on

cubic splines; (2) a RCS version, both uncorrected and corrected, is reported in

Climate Dynamics 7 (1992), where it is compared to the cubic spline version in a useful

way (clearly showing the inappropriateness of cubic splines for long-term data analysis). Figure 8 in Clim Dyn 7 matches Fig 2(2) in Bradley-Jones 1992 and

my graph produced from the Mann 1998 proxy 67 which all seem therefore to be the same dataset.

(3) a third reconstruction is reported on Fennoscandia by Briffa and Schweingruber in

Climate since 1500AD (1992) data on which is at [3]www.ngdc.noaa.gov. In c1500, Briffa

and Schweingruber do not reconcile to the prior discussions. I have graphed this dataset

together with the other one below. The correlation between the two datasets for the

overlapping period 1587-1975 is 0.032 an interestingly low correlation for what are

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reconstructions produced from relate data. I have shown the two
The Climate Dynamics 7 reconstruction contains a fudge by Briffa et al,
described as follows:
The density chronology shows a low-frequency decline over the last century
which appears anomalous in comparison with both the TRW data and the instrumental data over
the 19th and 20th centuries. These facts suggest that the density-coefficients in the
regression equation may be biased as would be the case if the density decline were not
climate related (CO2 increases and/or the potential effects of increasing nitrogen
input from remote sources may be implicated here.) &The residual MXD data (actual
estimated) are plotted in Fig. 7. A systematic decline is apparent after 1750. By fitting a
straight line through these residuals (1750-1980) and adding the straight-line values
(with the4 sign reversed) to the RCS density curve, the anomalous post-1750 decline was
removed.
This corrected RCS curve was then used along with the RCS ring-width curve in
a final reconstruction of the April-August temperatures.
This hardly seems like justifiable statistical procedure. Without the fudge,
the "reconstruction" shows declining temperatures in the 20th century. A very
similar decline in residuals occurs from 1100 to 1250 and one wonders whether a
similar adjustment would be allowable then.
The Climate Dynamics article does not contain a description of the regression
methodology and I have not yet consulted the predecessor article describing
the regressions. Suffice it to say that the tree ring data is highly
autocorrelated, as is (to a lesser extent) the temperature data. The meaning of such correlations is
not clear. The reconstructions end up being a weighted sum of the tree ring widths
over two summers and MXD s over two summers. The coefficients are very unstable under
different reconstruction methodologies.

Regards, Steve McIntyre

Prof. Phil Jones

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4903. 2003-06-20

cc: t.osborn@uea.ac.uk, mann@virginia.edu

date: Fri, 20 Jun 2003 10:16:02 -0400

from: Tom Crowley <tcrowley@duke.edu>

subject: Re: Fwd: Re: Climate Research

to: Phil Jones <p.jones@uea.ac.uk>

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Phil et al,

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here is the series discussed in Hegerl et al. (GRL, 2003) and further discussed in Crowley et al. (GRL, submitted): see the attached for a fuller description.

the series was regressed against the Jones et al. 30-90N (11 pt smoothed) instrumental record.

contact me if you need any more information, Tom

1000	-0.003
1001	-0.015
1002	-0.049
1003	-0.062
1004	-0.065
1005	-0.062
1006	-0.058
1007	-0.060
1008	-0.066
1009	-0.031
1010	-0.037
1011	-0.029
1012	-0.036
1013	-0.024
1014	-0.009
1015	0.021
1016	0.022
1017	0.020
1018	0.010
1019	0.003
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1021	-0.003
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1023	0.025
1024	-0.007
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1080	0.185
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1083	0.277
1084	0.318
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1092	0.282
1093	0.279
1094	0.271
1095	0.261
1096	0.251
1097	0.248
1098	0.264
1099	0.260
1100	0.258
1101	0.282
1102	0.294
1103	0.271
1104	0.230
1105	0.197
1106	0.171
1107	0.149
1108	0.135
1109	0.099
1110	0.083
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1591	-0.286
1592	-0.293
1593	-0.255
1594	-0.237
1595	-0.220
1596	-0.255
1597	-0.256
1598	-0.258
1599	-0.262
1600	-0.271
1601	-0.277
1602	-0.294
1603	-0.315
1604	-0.357
1605	-0.358
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1607	-0.348
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1609	-0.343
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1642	-0.295
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1646	-0.272
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1648	-0.207
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1667	-0.263
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1669	-0.262
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1752	-0.060
1753	-0.060
1754	-0.076
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1757	-0.035
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1761	-0.020
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1776	-0.110
1777	-0.105
1778	-0.120
1779	-0.135
1780	-0.161
1781	-0.185
1782	-0.207
1783	-0.219
1784	-0.195
1785	-0.214
1786	-0.205
1787	-0.184
1788	-0.170
1789	-0.107
1790	-0.086
1791	-0.054
1792	-0.058
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1794	-0.035
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1798	-0.091
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1838	-0.290
1839	-0.249
1840	-0.226
1841	-0.186
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1844	-0.132
1845	-0.111
1846	-0.095
1847	-0.063
1848	-0.053
1849	-0.029
1850	-0.041
1851	-0.061
1852	-0.087
1853	-0.082
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1855	-0.028
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1857	-0.034
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1864	-0.143
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1866	-0.178
1867	-0.197
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1906	-0.072
1907	-0.103
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1917	0.047
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1920	0.097
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1925	0.177
1926	0.171
1927	0.204
1928	0.220
1929	0.251
1930	0.249
1931	0.281
1932	0.296
1933	0.317
1934	0.340
1935	0.360
1936	0.369
1937	0.404
1938	0.409

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1939	0.405
1940	0.425
1941	0.449
1942	0.438
1943	0.435
1944	0.417
1945	0.423
1946	0.403
1947	0.394
1948	0.384
1949	0.360
1950	0.359
1951	0.328
1952	0.305
1953	0.292
1954	0.282
1955	0.305
1956	0.279
1957	0.278
1958	0.266
1959	0.198
1960	0.176

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4207. 2003-06-22

cc: Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>
date: Sun, 22 Jun 2003 21:01:40 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Yang et al
to: "Raymond S. Bradley" <rbradley@geo.umass.edu>, p.jones@uea.ac.uk,
mhughes@ltrr.arizona.edu, wigley@ncar.ucar.edu

Hi Ray,

I'm in Volcano National Park on my Honeymoon, so comment will be brief, by necessity...

In our GRL article, Phil and I weighted the records we used with respect to their decadal

correlations with the instrumental gridpoint surface temperature data for the same region

(numbers in parentheses in attached figure 1 from the paper), so if a series is

cg2003

truly crap

in an objectively determined sense, it got very low weight. The China series has a reasonable ($r=0.22$), but not great correlation--and it gets a moderate low weight.

In my opinion, this is a better approach than simply deeming a record crap a priori (and then getting criticized for not considering it). We considered all available records with appropriate resolution that are putative temperature estimates, and weighted them objectively.

We also did careful cross-validation on the resulting reconstruction using independent instrumental data, etc.---so I hardly think we are subject to criticism in how we used the available data, relative to other analyses that have been done...

As for the Eos piece, I think a similar point holds--not showing it at all would seem a conspicuous omission. We could add the local correlation values to each of the panels of

Figure 2, and comment briefly--this could be done at the proof stage.

I'll leave this to Phil (or Keith or Tim, who are helping out since Phil is also on vacation) to take care of, as I have promised not to get involved with this sort of stuff

until my honeymoon is over. Phil and I can discuss this, if need be, when we meet in Sapporo in a couple weeks,

mike

At 06:37 PM 6/22/2003 -0400, Raymond S. Bradley wrote:

Phil:

You commented that the Chinese series of Yang et al (GRL 2002) looked weird. Well,

that's because it's crap--no further comment on what stuff gets into GRL!

You appear to have used their so-called "complete" China record. You really should

consider what went into this --2 ice core delta 180 records of dubious relationship to temperature (one is cited as correlating with NW China temperatures at $r=0.2-0.4$), 3

tree ring series, one of which is a delta C-13 record of questionable climatic significance (to be generous). The other series include two records from a Taiwan

lake--a carbon/nitrogen isotope and a total organic carbon series (interpreted as

high="warm, wet") and an oxygen isotope series from cellulose in peat!!! (& don't ask

about the C-14 based chronology, interpolated to decadal averages!)

I loved this sentence:

"Although a quantitative relationship between the proxy records of the Jinchuan peat,

the Japan tree-ring series and the Taiwanese sediment records with modern climate data

are not given in the original works, the qualitative connectivity with temperature as

the dominant controlling factor has undoubtedly been verified"

Oh, undoubtedly!! And these are 4 of the 9 series going into the "complete China"

record..

Finally, they use another record based on "phenology" and (somehow) this provides a

winter temperature series....

cg2003

You just shouldn't grab anything that's in print and just use it 'cos it's there---that just perpetuates rubbish. This series needs to be removed from Figure 2 in the EOS forum piece--and if you included it in your GRL paper, I suggest that you reconsider it.

Ray

Raymond S. Bradley

Distinguished Professor

Director, Climate System Research Center*

Department of Geosciences

Morrill Science Center

611 North Pleasant Street

AMHERST, MA 01003-9297

Tel: 413-545-2120

Fax: 413-545-1200

*Climate System Research Center: 413-545-0659

<[1]<http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [2]<http://www.geo.umass.edu/climate/paleo/html>

Professor Michael E. Mann

Department of Environmental Sciences, Clark Hall

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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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62. 2003-06-24

date: Tue, 24 Jun 2003 14:17:15 +0000

from: "Mick Kelly" <m.kelly@uea.ac.uk>

subject: NOAA funding

to: Nguyen Huu Ninh (cered@hn.vnn.vn)

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Content-Type: text/plain; charset="utf-8"

Ninh

NOAA want to give us more money for the El Nino work with IGCN.

How much do we have left from the last budget? I reckon most has been spent but we need to show some left to cover the costs of the trip Roger didn't make and also the fees/equipment/computer money we haven't spent otherwise NOAA will be suspicious.

Politically this money may have to go through Simon's institute but there overhead rate is high so maybe not!

Best wishes

Mick

Mick Kelly Climatic Research Unit

School of Environmental Sciences

University of East Anglia Norwich NR4 7TJ

United Kingdom

Tel: 44-1603-592091 Fax: 44-1603-507784

Email: m.kelly@uea.ac.uk

Web: <http://www.cru.uea.ac.uk/tiempo/>

2023. 2003-06-24

date: Tue Jun 24 14:35:32 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: ice cores/China series

cg2003

to: wigley@ucar.edu

Tom
Tim has just told me of your message expressing concern about the China series ,
and your
statement of the necessity to "deal with Ray's comment" and add in the "small
adjustment to
the Figure Caption". .

We (I and Tim) decided to get this off as soon as possible to Ellen (AGU) , as
we had been
asked to do (and as requested by Ellen). Hence it went off earlier today (and
before your
message arrived). Mike was aware of Ray's comment and was happy to leave any
amendment to
the text "until the proof stage".

In my opinion it is not practical (or desirable) to try to "qualify " any one
record in
this limited format. It was a majority decision to leave the Mann and Jones
2000-year
series in the Figure 1 (as it was to remove the Briffa and Osborn tree-ring
based one) ,
and the details of the logic used to derive the Mann and Jones series is to be
found in the
(cited) text of their paper. Signing on to this letter , in my mind. implies
agreement with
the text and not individual endorsement of all curves by each author. I too have
expressed

my concern to Phil (and Ray) over the logic that you leave all series you want
in but just
weight them according to some (sometimes low) correlation (in this case based on
decadal
values).

I also believe some of the series that make up the Chinese record are
dubious or
obscure , but the same is true of other records Mann and Jones have used (e.g.
how do you
handle a series in New Zealand that has a -0.25 correlation?) . Further serious
problems
are still (see my and Tim's Science comment on the Mann 1999 paper) lurking with
the

correction applied to the Western US tree-ring PC amplitude series used (and
shown in
Figure 2). There are problems (and limitations) with ALL series used. At this
stage ,

singling out individual records for added (and unavoidably cursory added
description) is
not practical. We were told to cut the text and References significantly - and
further cuts
are implied by Ellen's messages to us.

If you wish to open this up to general discussion , it may be best to wait 'til
the proof
stage and then we can all consider the balance of emphasis - but we had also
better guard

against too "selective" a choice of data to present? If you want to get a
somewhat wider
discussion of this point going in the meantime , feel free to forward this to
whoever you

wish along with your disagreement , while we wait on the response from AGU.
Best wishes
Keith

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Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909

cg2003

Fax: +44-1603-507784

<http://www.cru.uea.ac.uk/cru/people/briffa/>

2670. 2003-06-24

cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

date: Tue, 24 Jun 2003 03:33:46 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: 2003ES000354 Decision Letter

to: Ellen Mosley-Thompson <thompson.4@osu.edu>, Phil Jones <p.jones@uea.ac.uk>

Hi Ellen,

I'm still travelling, and have only intermittent email access. I'm pretty sure Phil is

travelling now too, so I'm hoping Keith or Tim can help out here.

I think we actually discussed two small changes from the final version Phil sent you. This

involved adding Malcolm Hughes as a co-author (his name was accidentally left off the

list), and changing the wording of one sentence slightly. I believe that Tim and Keith have

these changes, and hopefully they can submit this via GEMS? If not, will have to wait until

Phil or I have a solid internet connection to do this (that will likely be at IUGG in

Sapporo in about 2 weeks).

Thanks for bringing this to our attention. Phil--if you're reading email, any way you can

help out here?

thanks all,

mike

At 04:36 PM 6/23/2003 -0400, Ellen Mosley-Thompson wrote:

Phil,

I just learned from AGU that you did not submit the revised version back to AGU via the

GEMS system. Can you or Mike do this as soon as possible? I would like to get this

paper moving through AGU. Fred Spilhaus still has to approve it - he approves all Forum

pieces - so this adds a layer that will cost us time.

Thanks

Ellen

P.S. I have copied everyone who might be able to handle this in your and Mike's

absence. Thanks

At 05:13 PM 06/20/2003 +0100, you wrote:

Dear Ellen,

I'm off on Sunday, but I've managed to get the revisions done. The revised pdf is

attached. This contains a reduced size manuscript by about 10 lines and we've reduced

the

references to the absolute minimum. This is still 30. If we go any lower we have to

change the

figures. As we are commenting on a paper we need to specifically reference all the

series we

use.

Thanks for going through so quickly.

If further changes are required I won't be here so can you email either Keith

Briffa

or Tim Osborn (k.briffa@uea.ac.uk, t.osborn@uea.ac.uk) .

cg2003

I will ask Keith and Tim to get the copyright forms rolling.

Cheers

Phil

At 13:50 18/06/03 -0400, eos@agu.org wrote:

Dear Dr. Mann: (copy to Phil Jones)

I am pleased to accept "On Past Temperatures and Anomalous late-20th Century Warmth" for publication in Eos with the provision that in your final submission you modify to the

first paragraph slightly so that it is fully consistent with the text of the AGU

statement on climate change and greenhouse gases:

[1]http://www.agu.org/sci_soc/policy/climate_change_position.html

Note that first sentence of your paper indicates that the AGU statement includes the

inference that there is a high probability I cannot find the words high probability in the AGU statement (unlike IPCC that does state "high probability."). It

is critical that the introductory paragraph is carefully constructed so as not to

diminish any of the points you make in the Forum piece. I suggest a modification of

your first paragraph - please feel free to further modify this.

Evidence from Gases," that there is a compelling basis for concern over future

climate changes, including increases in global mean surface temperatures, due to

increased concentrations of greenhouse gases, primarily from fossil fuel burning.

If this is too long, you might wish to break it into two sentences. This says the same

thing as your original intro sentence but is fully consistent with the text of the AGU

statement.

Also in the first paragraph would you agree to this change?

... such anomalous warm cannot be fully explained natural factors

(Added the word "fully" to indicate that some but not all of the anomalous warming can be explained by natural factors.)

Another suggestion is to remove the second reference to the AGU policy (second paragraph). What about ... these claims in light of the fact that they have

..... The content of the Forum piece is just fine, but I did find a few minor problems that

you need to fix in the final submission.

1) 3rd paragraph line 8 - reference to Jones et al. (1998) - this date occurs in several

places in the paper and should be Jones et al. 1999; e.g., point (2) line 3

2) page 2 - the second (2) point

last 3 lines: remove double period after U.S.; also that sentence reads awkwardly - try

a comma after the word 'cancelling'.

3) the second paragraph of point 2 (2); last three lines: this is awkward; the word

"apparent" is out of place; I think this should this read apparent coldness and

warmth differ

4) point 3) last line of first paragraph - change ... insight to (Remove in from

into)

5) references - the Jones et al. 1999 reference is formatted differently than the rest

cg2003

(put date at end).

Finally - everywhere throughout the text et al should be corrected to et al
(The period is consistently absent)

Before publication, your article will be edited to reflect the Eos newspaper style, including a possible change in the headline. We will send the edited version to you for review and final approval before the article is published.

Please note that before we can proceed with production work on your submission, a copyright transfer agreement and reprint order form must be completed and returned to

AGU. These forms may be printed* from the AGU web site:

[2]http://www.agu.org/pubs/journal_forms/EosCopyright.pdf

[3]http://www.agu.org/pubs/journal_forms/EosReprint_orders.pdf.

For information on the production process, please contact Shermona Grant, Eos Production Coordinator, at +202.777.7533 or sgrant@agu.org.

In the absence of information from you to the contrary, I am assuming that all authors listed on the manuscript concur with publication in its final accepted form and that

neither this manuscript nor any of its essential components have been published previously or submitted to another journal. The AGU Guidelines for Publication emphasize that: "It is unethical for an author to publish manuscripts describing essentially the same research in more than one journal of primary publication."

Thank you for your contribution to Eos.

Sincerely,
Ellen Mosley-Thompson
Editor, Eos

*If you need Adobe Acrobat Reader, it is freely available at:
[4]<http://www.adobe.com/prodindex/acrobat/readstep.html>

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3555. 2003-06-24

cc: mann@virginia.edu
date: Tue, 24 Jun 2003 14:06:25 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: ice cores/China series (FYI)

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cg2003

to: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Raymond S. Bradley" <rbradley@geo.umass.edu>

Thanks Keith,

I just read your email after reading the others. We actually eliminate records with

negative correlations (this is mentioned briefly in the GRL article,), and we investigated

a variety of weighting schemes to assure the basic robustness of the composite--but I

certainly endorse your broader point here. Many of these records have some significant

uncertainties or possible sources of bias, and this isn't the place to get into that. The

uncertainties get at this, at some level, and other places (e.g. the Reviews of Geophysics

paper Phil and I are drafting) will provide an opportunity to discuss these kinds of issues

in more detail--we will certainly be seeking advice (either officially or unofficially)

from each of you once we have finalized the draft of that...

Now back to my honeymoon...

mike

At 02:38 PM 6/24/2003 +0100, Keith Briffa wrote:

To keep you informed , here is a reply to Tom Wigley re his request to "deal with Ray's

Comments" re the China series in EOS piece

Tom

Tim has just told me of your message expressing concern about the China series , and

your statement of the necessity to "deal with Ray's comment" and add in the "small

adjustment to the Figure Caption". .

we (I and Tim) decided to get this off as soon as possible to Ellen (AGU) , as we had

been asked to do (and as requested by Ellen). Hence it went off earlier today (and

before your message arrived). Mike was aware of Ray's comment and was happy to leave any

amendment to the text "until the proof stage" .

In my opinion it is not practical (or desirable) to try to "qualify " any one record in

this limited format. It was a majority decision to leave the Mann and Jones 2000-year

series in the Figure 1 (as it was to remove the Briffa and Osborn tree-ring based one) ,

and the details of the logic used to derive the Mann and Jones series is to be found in

the (cited) text of their paper. Signing on to this letter , in my mind. implies

agreement with the text and not individual endorsement of all curves by each author. I

too have expressed my concern to Phil (and Ray) over the logic that you leave all series

you want in but just weight them according to some (sometimes low) correlation (in this

case based on decadal values). I also believe some of the series that make up the

Chinese record are dubious or obscure , but the same is true of other records Mann and

Jones have used (e.g. how do you handle a series in New Zealand that has a -0.25

correlation?) . Further serious problems are still (see my and Tim's Science comment on

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the Mann 1999 paper) lurking with the correction applied to the Western US tree-ring PC amplitude series used (and shown in Figure 2). There are problems (and limitations) with ALL series used. At this stage, singling out individual records for added (and unavoidably cursory added description) is not practical. We were told to cut the text and References significantly - and further cuts are implied by Ellen's messages to us. If you wish to open this up to general discussion, it may be best to wait 'til the proof stage and then we can all consider the balance of emphasis - but we had also better guard against too "selective" a choice of data to present? If you want to get a somewhat wider discussion of this point going in the meantime, feel free to forward this to whoever you wish along with your disagreement, while we wait on the response from AGU. Best wishes Keith Professor Keith Briffa, Climatic Research Unit University of East Anglia Norwich, NR4 7TJ, U.K. Phone: +44-1603-593909 Fax: +44-1603-507784 [1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4249. 2003-06-24

cc: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, "Raymond S. Bradley" <rbradley@geo.umass.edu>
date: Tue Jun 24 14:37:29 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: bradley comment
to: Tom Wigley <wigley@ucar.edu>

Hi Tom,
In Phil's absence I was just now looked at his PC because I needed some files/emails for a separate matter, and I noticed that you had emailed Phil/Ray/Mike concurring with Ray's concerns. Until I saw that, I hadn't realised that anyone else had commented on Yang et al.
Keith and I discussed exactly this issue this morning, and though Keith also had concerns about the record (I haven't read their paper, so can't comment) we decided to leave things as they were because: (i) Mike suggested adding correlations to the figure at the proof stage rather than now; (ii) I wasn't sure how to word a caveat about Yang et al. without

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making it seem odd that we were including a doubtful record and odd that we hadn't added

caveats about some of the other records.

The current status is that the version I circulated has been submitted back to EOS (because

of the reasons given above), and Ellen Mosley-Thompson has approved it. It needs to be

reviewed internally at AGU by either Fred Spilhaus or an Associate Editor. It will then be

edited to reflect the Eos newspaper style.

I've cc'd this to Mike and Phil to see what they want to do. I/we can put a hold on the

processing of the current submission and then submit a new version with revised figure and

caption. Alternatively we could wait and see what it's like after EOS have edited it, and

then make any final modifications at that stage.

Over to you/Mike/Phil.

Cheers

Tim

At 14:00 24/06/2003, you wrote:

Tim,

I think it is *extremely* important to cover Ray's point about Yang et al. and Mike

Mann's response about weighting. This requires a small addition to the Figure caption.

Tom.

5027. 2003-06-24

date: Tue Jun 24 10:09:31 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: Re: Helloooooo- back

to: "Raymond S. Bradley" <rbradley@geo.umass.edu>

Hi Ray

thanks for the communication -

True I had said to Malcolm that I thought you had expunged me from your "people to interact

with " list - most likely because of my extremely poor contribution to (and lack of frank

communication over) the Chapter in the PAGES book. Truth is , my back was bad and got

worse towards the end of last year and I had to drop a couple of things , which I really do

not like doing. I could have said a lot more about the Chapter but the main content was

good and as you put in so much effort on it I did not want to seem churlish . I still think

it is very useful summary of the state of things , which is what it was meant to be. I hope

that did not influence your judgement re that Palaeoclimate Group , for which you have

mistakenly chosen to include Phil instead of me !

Now to the comments re the EOS piece. I believe you criticised the inclusion of the 2000

(Eurasian) tree-ring series (since reiterated by Malcolm). Fair enough , though again

misguided in my opinion if on the basis of "contains few data " or " has weak climate

response" . I was perfectly happy to drop it (I never suggested its inclusion in the

first place), but I find it somewhat ironic that it should be replaced with the latest

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(Mann and Jones) series that contains the same three series plus a mixture of other far more dubious (not to say bad) series - I agree with the remarks you made re some of these (particularly the Chinese series) in your recent email to someone. I consider that this new series (plus the illustration of the Western US series in the EOS) piece will "stimulate further discussion " in the field , both between we palaeo-types and the Sceptics . I and Tim have been left to submit this and the balance of pressure seems to be to submit as is - if we remove the suspicious Chinese series we would have to delay things further (Ellen is hassling for us to submit) and , anyway, it is still contained in the Long series. I am of the opinion that the points made in the piece still stand - and by signing on , we are not individually sanctioning all the curves or data used in the illustrations (There are genuine problems with ALL of them). We will therefore , add Malcolm's name and submit the version we now have. Hope this OK with all. Finally, Mike and I have been asked (by Lennart Bengtsson) to present a paper at the CLIVAR/PAGES Conference next year in Baltimore [1]<http://www.clivar2004.org>. Our bit is about the climate (Global /Hemisphere) of the instrumental period , but I take this to be the last 1000 years . We will be asking our co conspirators (ie the EOS list) to be joint contributors (though Peck is presenting another similar subject (longer period) paper - the precise balance between these time scales needs to be struck yet). Also I am organising a session at a European Community Conference to be held next year in Holland - my session is "How warm was the Medieval period in the context of the late Holocene" and although I will probably not be asking you (or me!) to present one of the two invited papers (but I might end up asking you) I hope and expect that you, and the rest ,to agree to be authors of one of them. I hope you will be able to ? I believe you are writing a paper with Malcolm and Henry on the MWP? Can you give me an idea of its scope ? I am hoping to do something of a large review of the "contribution of tree-ring data to global climate histories" along with Ed and others. Sorry about you problems , but remember life is sweet and best wishes to Jane. Keith

At 02:31 PM 6/22/03 -0400, you wrote:

Hi Keef:

Why is it raining so much here? New York has just surpassed the June 1903 record of ~10inches, and it's only June 22nd....right now it's teeming down....could be a monsoon. No doubt global warming must have something to do with it...or that cut-off Low that's stuck here...

Malcolm said you are feeling a bit better after a pretty rough time of it. I've been

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meaning to write and give you my sympathy. I've occasionally had back problems that have been debilitating, but nothing like you've had, I'm sure. Anyway, I was happy to hear that things are picking up for you.
The last month here has been pretty grim--the Soon & Baliunas business has opened my eyes to the devious and cynical nature of the Bush Administration--it's far worse than I imagined. Pretty depressing. Then the University budget got slaughtered--we've had cuts amounting to 29% over the last couple of years....and I also had a couple of NSF proposals turned down....then Jane's knee problems forced us to cancel our walking holiday in France.

Time to move to Canada --or anywhere-- I think...

I reckon we've had an inch of rain in the past two hours.....high temperature for the year was back in April....
So I hope I cheered you up!

Ray
Raymond S. Bradley
Distinguished Professor
Director, Climate System Research Center*
Department of Geosciences
Morrill Science Center
611 North Pleasant Street
AMHERST, MA 01003-9297
Tel: 413-545-2120
Fax: 413-545-1200

*Climate System Research Center: 413-545-0659

<[3]<http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [4]<http://www.geo.umass.edu/climate/paleo/html>

--

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[5]<http://www.cru.uea.ac.uk/cru/people/briffa>[6]/

539. 2003-06-25

date: Wed Jun 25 13:40:32 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Fwd: Re: ice cores/China series
to: t.osborn@uea.ac.uk
Date: Wed, 25 Jun 2003 06:36:45 -0600
From: Tom Wigley <wigley@ucar.edu>
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.0.1)

Gecko/20020823

Netscape/7.0
X-Accept-Language: en-us, en
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: ice cores/China series
KEITH -- SEE BELOW
Keith Briffa wrote:

Tom

Tim has just told me of your message expressing concern about the China series, and your statement of the necessity to "deal with Ray's comment" and add in the

"small

adjustment to the Figure Caption". .

We (I and Tim) decided to get this off as soon as possible to Ellen (AGU) , as we had

been asked to do (and as requested by Ellen). Hence it went off earlier today (and

before your message arrived). Mike was aware of Ray's comment and was happy to leave any

amendment to the text "until the proof stage" .

YEAH, I REALIZE THIS -- AND I AGREE THAT IT WAS IMPORTANT TO GET THE DOCUMENT OFF

QUICKLY.

In my opinion it is not practical (or desirable) to try to "qualify " any one record in

this limited format. It was a majority decision to leave the Mann and Jones 2000-year

series in the Figure 1 (as it was to remove the Briffa and Osborn tree-ring based one) ,

and the details of the logic used to derive the Mann and Jones series is to be found in

the (cited) text of their paper.

YOU MISUNDERSTAND ME. OF COURSE IT WOULD BE SILLY TO SINGLE OUT A SPECIFIC ITEM. WHAT IS

NECESSARY IS A SENTENCE STATING THE *METHOD* -- I.E., THAT ITEMS ARE WEIGHTED BY THEIR

CALIBRATION PERFORMANCE.

Signing on to this letter , in my mind.

implies agreement with the text and not individual endorsement of all curves by each

author. I too have expressed my concern to Phil (and Ray) over the logic that you leave

all series you want in but just weight them according to some (sometimes low) correlation (in this case based on decadal values). I also believe some of the series

that make up the Chinese record are dubious or obscure , but the same is true of other

records Mann and Jones have used (e.g. how do you handle a series in New Zealand that

has a -0.25 correlation?) .

IT IS A DIFFICULT CALL -- WHETHER TO DUMP SERIES THAT HAVE NO SIGNIFICANT LINK TO

TEMPERATURE AND WHICH ARE, AS WELL, DUBIOUS ON A PRIORI GROUNDS; OR TO USE A WEIGHTING

SCHEME. IF ONE DID THIS BY SIMPLE MULTIPLE REGRESSION, THEN THINGS WOULD BE WEIGHTED

AUTOMATICALLY. HOWEVER, STATISTICALLY ONE SHOULD STILL DUMP THE LOW CORRELATION ONES.

I HAVE RESERVATIONS ABOUT WHAT MIKE AND PHIL HAVE DONE -- BUT THIS IS SOMETHING WE

SHOULD TALK ABOUT FACE TO FACE SOME DAY.

Further serious problems are still (see my and Tim's Science comment on the Mann 1999 paper) lurking with

the correction applied to the Western US tree-ring PC amplitude series used (and shown in

Figure 2). There are problems (and limitations) with ALL series used.

YEAH.

At this stage , singling out individual records

for added (and unavoidably cursory added description) is not practical.

I AM NOT SUGGESTING THIS -- AS THE ABOVE SHOULD MAKE CLEAR.

We were told to cut the text and References significantly - and further cuts are implied

by Ellen's messages to us.

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If you wish to open this up to general discussion , it may be best to wait 'til the proof stage and then we can all consider the balance of emphasis - but we had also better guard against too "selective" a choice of data to present? If you want to get a somewhat wider discussion of this point going in the meantime , feel free to forward this to whoever you wish along with your disagreement , while we wait on the response from AGU.

NO -- I'M HAPPY WITH KEEPING THINGS AT THIS LEVEL.

Best wishes

Keith

I WAS AT A MEETING IN BRECKENRIDGE YESTERDAY WHERE SUSAN SOLOMON GAVE AN HOUR LONG PRESENTATION ABOUT PLANS FOR THE 4AR WGI REPORT, DUE OUT IN 2007. IT WAS A COMPREHENSIVE TALK -- AND SHE HAS THINGS MUCH BETTER ORGANIZED THAT JOHN HOUGHTON EVER DID. SHE DID

SINGLE OUT TREE RINGS AS A VITAL COMPONENT OF THE PALEO RECORD.

Professor Keith Briffa,
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--

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[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

1678. 2003-06-25

date: Wed Jun 25 14:57:44 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: RE: Regarding paper submitted to The Holocene
to: "Isaksson, Elisabeth" <elli@npolar.no>

Dear Elisabeth

I am pasting below two reviews of your paper. I have had a third reviewer look briefly at the paper and at these reviews (particularly because of the delay on my part) and the third reviewer agreed that the other reviews were reasonable. You will see that all of them agree that the paper should not be published in its present form . The recent added delay has been while I then went through the paper and all reviews carefully myself to give what I believe is an objective opinion of my own . I too feel that I can not justify acceptance in the present form but I note that neither of the original reviewers recommend rejection .

The normal procedure at this stage would though be a polite rejection on the grounds of pressure of space and the apparent requirement for significant new work. I certainly will

not recommend this course of action and instead request that you and your

co-authors look

over these opinions and let me know whether you think it possible to deal adequately with them.

I would be happy to consider a revision . One referee has indicated their willingness to

review a re-submission though one has indicated that they are not prepared to look at the

paper again. Therefore I would have to go to a third reviewer but I would be happy to

approach one from among a number you might care to suggest. I would have to provide this

new reviewer with the original manuscript and referees' comments also.

I believe the reviewers are trying to be constructive but it seems they share doubts about

the way in which you are presenting the evidence as though the differing forcings acting on

the two ice core series are well known whereas the separation of their effects are not

easily achieved. In this respect I have to agree. I am also confused about the influence of

sea ice or temperature (or distance from the drilling site) . Also are you advocating the

use of one set of core data only in future wider studies? Can the data be interpreted as an

independent winter temperature record in this regard? This would be very valueable.

I would ask that you consider the reviews and let me know how you wish to progress matters.

I realise that this will be a disappointing reply , not helped by the unacceptable delay in

receiving it . This was my fault alone and I am very sorry for it.

Keith

P.S. As I am typing this I was just wondering also about the similarity of the two ice core

series over the 19 - 20th centuries . Given the differences in them prior to this period ,

does this fact represent a signal of anthropogenic

warming or some such of itself? Perhaps the two series could be differenced to identify

some local as opposed to far off climate signal . Probably nonsense but thought i would

mention it anyway.

Here are the reviews

REVIEWER 1

CLIMATE AND SEA VARIABILITY AROUND SVALBARD
ISAKSSON ET AL.

This paper addresses the interpretation of isotope data from ice cores from Svalbard. This

is an important study as more recent evidence suggests that the Arctic marginal seas in

this sector are an important indicator of climate change and an extended record of climate

variability for the region would be very useful. This paper considers some of the issues

that have led to the past neglect of such proxy records.

Unfortunately, I do not consider this paper warrants publication in its current state. Some

basic statistical lapses limit the confidence that can be placed on the correlation results

and the paper contains a considerable amount of speculation. Indeed, there is not much

fresh analysis in the paper and much recounting of the results of previously published work

coupled with unsupported speculation. The authors may well be correct in the views they

take but without harder evidence few others will be convinced.

Having said that, I do believe that a major revision backed by additional research could

result in a publishable paper and have, therefore, made a number of constructive comments

to this end. The scale of the proposed revisions mean that the paper would need to be

re-refereed. The suggestions below are indicative of the kind of revision that I regard as

necessary and should not be taken as a comprehensive list.

General comments

1. At present, bar graphics, this paper only contains one new analysis, the

correlation study between the various data series. The authors should carry through

this approach by remedying the statistical deficiencies noted below and bringing in

additional climate data to support the interpretation. For example, there is a

classic case of post hoc rationalisation on page 8 where a lack of correlation is

explained away. Further work is needed to test this explanation. Without this, the

authors are building castles on sand.

2. There is a lengthy paper on the links between the Iceland sea ice record and

climate parameters which addresses many of the points raised on page 7 (Kelly,

P.M., Goodess, C.M. and Cherry, B.S.G. (1987) Journal of Geophysical Research, 92

(C10), 10835-10843.) Many other references on this subject exist in the climatic or

glaciological literature and examination would most likely resolve some of the unresolved

issues here. The interactions between sea ice, air temperature and ocean temperature are well

known, despite the authors claim otherwise.

3. The structure of the paper needs attention as there is a tendency to return to

points already made or issues already discussed: cf. final paragraph on page 8 and

following would be better placed earlier when links between the core data and other

variables are considered. Again, this discussion is very speculative.

Statistical methods

1. The use of running means is not advisable due to phase distortions that can

occur at the interannual level (WMO Technical Note 79). All filtering should use

binomial filters or the equivalent.

2. Figure 3 alone does not support the interpretation that the data are surprisingly

similar both in trends and amplitude. And this claim is in fact contradicted on page 6, final paragraph. Perhaps there is similarity with the eye of a believer but the only real match is the warmth of the 20th century compared with the previous period. But even this differs in character between the two cores with a step change in one and a more gradual trend in the other. This kind of statement must be supported by statistics. Similarly, I don't see the cold period around 1780 in the upper record. There are two short warmish periods before and after the period stated but that is not the same thing! The discussion on pages 5-7 is full of similar claims with no statistical support. For example, the difference in the nature of the recent trends is noted on page 6 but then explained away by a speculative mechanism which is proposed but not tested. This is post hoc rationalisation and must be removed.

3. As far as I can tell the significance testing of the correlation coefficients has not made allowance for autocorrelation in the smoothed series. This is a serious error as autocorrelation can seriously inflate such statistics. Moreover, the possibility that the one significant result has occurred by chance must be discussed.

 REVIEWER 2

 This paper reveals much about the difficulty of interpreting oxygen isotope data in the simplistic way that many non ice specialists would wish. It demonstrates how a complicated mix of environmental variables affect the detailed numbers that are extracted from a single ice core and it clearly shows the pitfalls that the likes of Mann and his co-workers and imitators will experience in using these types of records to present global temperature series.

For this reason I would like to see this paper published, but I am unhappy with the way the authors present this evidence as though through rose-colored glasses. They conclude that their study shows that Svalbard ice cores provide important information on both local and regional climate variability in the Arctic, despite their relatively low altitude and periodic melt.

My interpretation of what they show would be that they show how it is not possible, on the basis of a single core, to know how to interpret changing isotopic values separately in terms of changing temperature and precipitation seasonality, surface ocean circulation (through its effect on sea ice), atmospheric circulation, and variable, very local conditions, such as wind.

The authors indulge in much hand-waving to explain away differences in the two ice core records, but make little attempt to test the theories using instrumental data. There are temperature records going back as far as the eighteenth century and mean sea level pressure maps going to the late nineteenth century. Surely these can be used to explore whether different parts of Svalbard (and the different isotope records) relate to temperatures and different circulation characteristics, say before and after the 1920s or

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during the twentieth century? Similarly, there is a lack of quantification when series are being compared. It does not inform us much to cite evidence of severe ice in the Baltic between 1880 and 1896 or cold in Uppsala in 1862-1871 and 1825-1884 that seems to coincide with cold at Lomonasovfonna between 1870 and 1890, if the longer term consistency or lack of consistency is not quantified. The reference to "changing" atmospheric conditions between 1880-1910, implied to be a cause of one of the major differences in the two ice core records, only confuses the reader, if this is not developed further by looking to see how atmospheric circulation across Svalbard changed. One page 7, it is stated that there is "correspondence" between the sea-ice extent record and the blocked Austonna record. No figures are given and it seems that only the post 1850 trends coincide. Correlations are given for smoothed isotope records and various annual temperature records, but the only one that appears significant is for Jan Mayen. The significance for this in Table 2 is almost certainly over-stated because no allowance has been made for coincident autocorrelation in the records, and no mention is made of the likely dependence of this result on trend. Not enough quantitative evidence is included in the comparisons or discussions and the justification for comparing individual summer, winter, or annual series should be explicit. This also goes for the comparisons with proxy data. I would have liked to see much more on the comparison with the NAO (see work of Lisa Barlow and Jim White) and the removal of the unconvincing discussion and vague reference to expected ENSO associations. There are long reconstructions of NAO by several authors, some based on accumulation or isotope data in Greenland. These could be compared over hundreds of years with these isotope data.

The paleodata comparisons are sketchy and do not seem well organised or systematic; again the discussion or conclusions are vague. What is needed here is some insight into how the Svalbard data would be expected to agree or disagree with the other records and a clearer discussion of the extent to

which

this is true or not. What is the logic for comparing summer-responsive tree rings and why pick out a northern Greenland ice core and not others? There likely are more records available from the Russian Arctic (ice core in Novia Zemlya and other tree records). The comparison with glacier data seems inappropriate and where is the comparison with earlier Svalbard ice core records (even at lower resolution)? This section would benefit from some plots to show the various series as well as more real correlations.

Other points I would mention briefly, and that the authors should discuss, are:

- different resolution of data through time - possible attenuation of seasonal or annual or decadal records;

- clear statement of dating uncertainties in their records;

- discussion of identifying specific seasonal records;

- consider model based evidence of sea ice controlling factors -

- there have been numerous studies (Walsh or Hibler?).

I would like to see this evidence published, but it needs to be presented in a clearer, more considered way and the similarities and differences between the records on Svalbard and further off must be quantified better and explained more logically.

I recommend this be reconsidered after major modification. I do not wish to review it if it is.

At 11:27 AM 6/24/03 +0200, you wrote:

Dear Keith,

It is now a whole year (!) since I submitted the paper "Climate and sea ice variability around Svalbard-inferences from two ice core $\delta^{18}O$ records" and I still have not got any

cg2003

response about it. Could you please tell me where things stand now?

Regards,

Elisabeth Isaksson

Dr. Elisabeth Isaksson

Norwegian Polar Institute

The Polar Environmental Centre

N-9296 Tromsø

Norway

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Fax. +47-77 75 05 01

e-mail:elli@npolar.no

[1]http://www.npolar.no/

[2]http://www.miljo.no

-----Original Message-----

From: Keith Briffa [[3]mailto:k.briffa@uea.ac.uk]

Sent: 2. mai 2003 10:00

To: Isaksson, Elisabeth

Subject: RE: Regarding paper submitted to The Holocene

Elisabeth

I am currently waiting on a new reviewer , having given up on one earlier one (after some nagging I caused some offence !). Another review I had was very cursory and ambivalent (and so not much use to me). I am promised another by the time I return from two short trips next week and so will forward a more detailed response then. Sorry about the delay, partly caused by some medical problems and I am now trying to work through a large backlog.

Keith

At 11:22 AM 4/23/03 +0200, you wrote:

>Dear Keith,

>Do you have any news on our submitted paper "Climate and sea ice

>variability around Svalbard-inferences from two ice core d18O records"? I

>would appreciate to hear were it is as at this point.

>

>Regards,

>Elisabeth Isaksson

>

>*****

>Dr. Elisabeth Isaksson

>Norwegian Polar Institute

>The Polar Environmental Centre

>N-9296 Tromsø

>Norway

>

>Ph. +47-77 75 05 15

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>[4]http://www.npolar.no/

>[5]http://www.miljo.no

>*****

>

>

>

>-----Original Message-----

>From: Keith Briffa [[6]mailto:k.briffa@uea.ac.uk]

>Sent: 10. desember 2002 12:33

>To: Isaksson, Elisabeth

>Subject: Re: Regarding paper submitted to The Holocene

>

>

>Elisabeth

>the problem has been referees. I sent it out to two , one of which sent

>back a very cursory "seems ok to me " response by email and the other who

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>consistently has not responded to requests for an update (though
>admittedly left too long before pushing). The paper was therefore only
>recently sent to another two referees who were asked to respond quickly . I
>will hassle these more strongly. It is very likely , unless some real
>problem is found, that this will be published - but I need at least one
>positive response from one of the new referees. I am sorry for this delay ,
>but I have to say that it is getting more difficult to get referees to
>respond (increasing workloads). I will let you know as soon as I hear
>more. Please contact me again in the new year if you don't hear. The
>journal is also receiving many papers now and we are having to become much
>harsher in selecting papers so as to try and keep a balance in
>area/techniques/proxies etc. We are keen to publish papers in your field so
>I ask you to patient for a little longer and we will try to make up some of
>the delay in the next stage. Thanks

>
>Keith

>At 10:34 AM 11/27/02 +0100, you wrote:

>>Dear Dr Briffa,
>>I submitted the paper "Climate and sea ice variability around Svalbard-
>>inferences from two ice core d18O records" to The Holocene in the end of
>>June and I wonder where in the process it is now. Thanks in advance!

>>
>>Regards,
>>Elisabeth Isaksson

>>
>>
>>
>>
>>*****

>>Dr. Elisabeth Isaksson
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>>e-mail:elli@npolar.no
>>[7]<http://www.npolar.no/>
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>>*****

>>
>--
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2192. 2003-06-25

date: Wed, 25 Jun 2003 11:03:45 -0400

from: "Tom Jacob" <Tom.Jacob@USA.dupont.com>

subject: REFLECTIONS ON BONN...

GLOBAL CLIMATE CHANGE CONTACTS: The UN Framework Convention on Climate Change (UNFCCC) convened its subsidiary bodies on Implementation (SBI) and on Scientific and Technical Advice (SBSTA) for their annual spring session in early June. Attendance was low, energy lower, and accomplishments minimal. The global effort to respond to climate change is facing an uncertain future.

The agenda for this year's June meeting was familiar. That in itself is significant. Process questions such as methodological issues relating to national reporting under the Kyoto Protocol, reporting frameworks and their review (Protocol articles #5, 7 and 8); adverse effects on developing countries resulting from climate mitigation efforts in the developed world (Convention Articles 4.8 and 4.9, and Protocol Article 2.3); and mainstays such as technology transfer and capacity building continue to occupy significant time, as does the very complex challenge of developing Protocol rules for accounting for the climate impacts of land use, land use change and forestry (LULUCF). All must be wrestled to the ground, ultimately, but all continue to be pushed forward without final resolution.

Significantly, the discussions on 4.8-9 and 2.3 continue to be particularly divisive, pitting the G77 & China, on the one hand, versus the developed nations on the other, over the question of what commitments were implied by these sections speaking directly to adverse effects on developing countries and "actions relating to funding, insurance and the transfer of technology." The issue, here is a very fundamental one of the obligation of wealthy developed nations to less developed nations.

Perhaps more indicative of the state of the global climate change effort was the prominence of two particular issues at this meeting: the first project-related reviews by the Executive Board of the Protocol's CLEAN DEVELOPMENT MECHANISM (CDM), which found all of the first 14 proposals for project accounting methodologies lacking; and the controversy over the BUDGET for this global effort, which found challenge to both its scale and structure. All of this, of course, took place in a negotiation dimmed by the shadow of increasing uncertainty over RUSSIAN RATIFICATION of the Kyoto Protocol. The following several pages review each of those three items, and offer some thoughts about what they may portend for the global response to climate change?

CDM ACTION: With respect to the CDM, the Protocol (Article 12) provides that projects implemented pursuant to this Kyoto mechanism can begin accruing tradeable emissions credits as of 2000. The Executive Board charged with overseeing this system, however, was not named until the Marrakech meeting in late 2001. It has had a huge task in creating the rules and procedures for this system, to account for and credit climate improvements for leading-edge projects in the developing world and enable those credits to be applied in the larger cap & trade regime in place in the Protocol-ratifying developed nations.

With its basic rules now in place and a queue building for the first project proposals seeking CDM approval under those rules, the Executive Board sat down on Saturday morning at the Bonn session. Sunday night -- 20 intense meeting hours later -- the Board closed out its session emotionally and intellectually spent, having sent all 14 of the project-related methodology proposals back to remedy shortcomings, and having done a significant amount of soul-searching in the process. Included among the projects were a number from the World Bank Prototype Carbon Fund and others

sponsored by The Netherlands ? the only country to have seriously taken on the challenge of funding such projects.

It was a sobering process for all. Environmental advocates proclaimed appropriately stringent decision making, while project proponents publicly and privately derided the process and what some perceived to be a re-writing of the guiding CDM directives agreed in Marrakech. The reality probably lies in between, but there can be no disagreement that this continues the uncertainties that have plagued CDM. Specifically at issue in Bonn were proposed methodologies for determining baseline emission scenarios (what emissions could be expected in the absence of such projects), what the emissions will be with such projects, how they will be measured and, critically, how projects of these particular types are determined to be "additional" under terms of the Protocol. That latter point is looming huge over the CDM process, as it is being interpreted as a determination of how the project would be proven not to have occurred "but for" the CDM.

The complex approach to these determinations has been developed and is now being implemented by an expert "Methodology Panel" charged with making recommendations to the Board. The Meth panel had recommended only one of the 14 proposed methodologies for approval (it was reversed by the Board on grounds that the associated monitoring methodology was not appropriately documented). Arising from the Meth panel's written analyses and recommendations were serious concerns regarding whether the rules developed for these baseline methodology reviews were in fact applying a criteria excluding projects that would be financially viable in their own right ? so called "investment additionality." Project proponents were all the more frustrated by the insular character of the advisory review, which did not afford proponents an opportunity to discuss their proposals directly with reviewers or Methodology Panel, or even to respond to questions and concerns.

A broad range of questions were raised by the CDM Board on both the methodology proposals and on the process applied by the Meth panel. They attempted to clarify a number of the latter points through clarifying interpretations of some key questions. The rejection by the COP of the EU idea of some "positive list" of categorically approved projects and methodologies has led the CDM Board to apply a "bottoms up" approach to their process. This means that a structure of approved methodologies that can help expedite projects through actual project approvals under CDM will be built only over time, through Methodology Panel recommendations and the methodology decisions of the Board. The June meeting marked the first real operation of that process. Its test, now, is twofold: 1) whether it can self-correct and deliver more consistent, workable guidance; and 2) whether that guidance will end up challenging a broad range of development projects to deliver leading-edge standards of greenhouse gas emissions or will be interpreted so narrowly that it leaves the field to only a small number of projects that cannot otherwise compete.

THE BUDGET: The proposed biennium budget of the UN Framework Convention was challenged on two levels in Bonn ? both significant and both led by the United States. Challenged were the proposed budget increase and the apportionment of the budget between work in pursuit of Framework Convention activities (to which the US is a Party, having formally ratified that instrument), and the activities advancing the Kyoto Protocol (from which the current US Administration has distanced itself). The proposed budget of \$37.1 MM included about \$5.9 MM work on the Protocol, according the analysis by the Secretariate ? about 16% of the total.

The budget proposal is approximately 2.3 times the level at the time the Kyoto Protocol was approved (1996-97), having been steadily ramped-up in the intervening years.

In the end, the SBI sent to the Conference of the Parties a recommendation that includes three options for the budget level: \$32.8 MM, 35.8 MM or "any other amount." It calls to the attention of the COP that the budget incorporates activities relating to the Protocol, pending the entry into force of that instrument and the convening of its first "Meeting of the

Parties" to assume responsibility at that point for its operation. The US did not get the separation of the budget items, but did clearly send its "shot across the bow" to remind other Parties to the Convention who will be Parties to the Protocol (assuming it enters into force), that the two instruments are not one and the same, and that the US will not be paying for the latter.

Given that the US is expected to be the largest contributor to the convention under the "indicative scale" system of voluntary contributions employed within the UN system, its status in such matters is crucial. The US has contributed toward the regular budget of the current biennium, but at a level reduced by its proportionate share of the activities geared to the "prompt start" of the CDM program discussed above. Even with that reduction, its \$1.5 MM credit to the current biennium is second only to Japan's \$2.6 MM.

Another indication of the challenge faced by the UNFCCC is the fact that midway through the second year of its current biennium budget, the contributions of countries are short by over \$8.5 MM ? approximately 30% of its biennial budget. Uncertainty regarding the timing and ultimately delivery on contributions is a perpetual fact of life on the global frontier. Countries have discretion over their budgets and exercise that discretion for any number of economic and political reasons ? the intergovernmental institutions are at their mercy.

In the case of the UNFCCC, the combination of the low ebb in energy, the ongoing difficulties in getting beyond posturing in key issue areas, the startup problems of the Kyoto mechanisms, the budget difficulties and the larger uncertainties of the Russian Federation's ratification are beginning to lead to an uncomfortable question: just how committed are the countries of the world to a globally coordinated response to climate change?

RUSSIAN FEDERATION STATUS: The growing uncertainty regarding Russia is perhaps indicative of some fundamental dynamics at work. With the withdrawal of the US from the Kyoto picture, the stakes for Russia changed dramatically. Suddenly, the bottom fell out of the emissions trading market ? a market in which the wrenching contraction of its manufacturing and economic base had secured for Russia a dominant position. It also tipped the scales such that its ratification of the Protocol would now be required in order for the instrument to come into force (as of early June, 110 countries have ratified the Protocol, but those countries cumulatively account for only 43.9% of the developed country 1990 emissions ? short of the 55% required).

Now Russia is studying its options. Significantly, it last year moved responsibility for its climate change policy out of the hands of its scientific ministries and vested leadership in its economic ministries. In Bonn, the Federation was careful to note that it is thoroughly reviewing the range of issues, and that it is conducting a full and precise assessment of economic and social consequences, "with economic development our first-order concern." Prominent considerations in that regard now reportedly includes not only the potential return on its inventory of tradeable emission credits (if it is able to cut the right deal with the EU), but also the question of how the economic development (re-development) of the Russian economy will fare over the longer term under a progressively more stringent Kyoto Protocol.

Certainly Russia is not alone in applying an economic decision calculus to the global climate process. Clearly the US has been doing so since before Kyoto, as manifest in its economically-driven decision to withhold US endorsement of any agreement that did not subject its economic competitors in the developing world to comparable constraints. Similarly, OPEC countries have been equally transparent in the economic imperative that drives their relentless pressure on Articles 4.8-9. And, of course, the imperative for economic growth is the driver for the developing countries in resisting any hint of the kinds of emission caps that are the key feature of the Kyoto Protocol's treatment of developed countries.

WHERE ARE WE HEADED? Missing from the shorter term economic decision calculus that seems to be driving many countries in this process, is the

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convergence in the longer term, of environmental and economic consequences if climate change is unabated, and the grim reality that the longer we wait to take prudent action, the more difficult if not impossible our task will be. This judgement, of course, presupposes the validity of the weight of current science in pointing the finger at anthropogenic emissions as the destabilizing element. It is important to understand that some at the core of US climate change policy still do not agree with this (and much of the US business community has sought shelter in their view). However, beyond the (transient?) questioning of the science is another question lurking in the background of the US stance that may be more important for the climate challenge in the long run. Its symptoms are all of the above and the current malaise of the UNFCCC process. It is simply the question of whether the established mode of massive "nothing is agreed till everything is agreed by everybody" negotiation can reasonably be expected to deliver on a task as monumental as restructuring the global economy. That question is unavoidable. More to the point, it will begin to come to the fore at COP-9 in Milan in December, regardless of the outcome in Russia. That is because the ultimate fate of the Kyoto Protocol, even if Russia ratifies and it comes into force, will increasingly be driven by expectations of what happens beyond the first commitment period. The evidence from Delhi of the first foray into discussions of post-first commitment period suggests that all of the dynamics implied above will have to be dealt with if the path to that longer term future is to be successfully charted? the deeply entrenched views of equity, complicated by motivating considerations of economic self-interest; the horrendously complex task of creating new global institutions from scratch that we see so evident in the painful emergence of the CDM; and the simple reality that we are trying to reorder the world on an uncertain budgetary shoestring. Interestingly, these same dynamics will be at work if Russia declines to ratify and forces the whole process back to the drawing board. The challenge in both circumstances will be to recognize the realities confronting us in addressing the long term challenge of climate change, and to fashion a process that can move us forward to a truly meaningful response. The next step in that journey may well be to take a hard, cold look at the way we've been doing our business?

- - - - -

Thomas R. Jacob

DuPont -- Senior Advisor, Global Affairs

Internet Address: tom.jacob@usa.dupont.com

Wilmington: 302-774-6873 fax: 773-2010

Washington: 202-728-3610 fax: 728-3649

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4712. 2003-06-25

cc: Phil Jones <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, "Raymond S. Bradley" <rbradley@geo.umass.edu>

date: Wed, 25 Jun 2003 07:31:08 -0600

from: Tom Wigley <wigley@ucar.edu>

subject: Caption

to: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

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<x-flowed>

Guys,

It seems that there was a misunderstanding about what I suggested re Yang. To be more specific, I suggest adding the following to the end of the Figure 2 caption:

"..... Note that individual series are weighted according to their quality in forming a composite hemispheric-scale time series." The word 'quality' here has been chosen carefully -- as something that is deliberately a bit ambiguous. The point here is to have something that we can fall back on if anyone criticizes *any* specific input series (*not* just Yang).

Please note that there are three incorrect affiliations in the ms :

I should be just Tom Wigley, NCAR, Boulder, CO.

I am no longer directly part of UCAR -- except insofar as UCAR being the umbrella organization for NCAR (which is irrelevant).

Caspar and Kevin (for consistency) should be

Caspar Ammann, NCAR, Boulder, CO.

Kevin Trenberth, NCAR, Boulder, CO.

For your information, we are all in CGD, which is a 'Division' within NCAR. Within this, Kevin and I are in the Climate Analysis Section (CAS) and Caspar is in a different Section. As far as affiliations go, however, these are irrelevant details.

Equally, some details in other persons affiliations could well be deleted.

Please fix these things at the proof stage.

Tom.

</x-flowed>

634. 2003-06-26

date: Thu Jun 26 13:51:23 2003

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Re: reprint order for EOS

to: vlb2d@virginia.edu, "Michael E. Mann" <mann@virginia.edu>

Dear Victoria,

I am attached the blank reprint order and billing form for the article that is being

published by AGU in their EOS publication. As you will see from Mike Mann's email (which

he cc'd to you and is copied below) he would like to arrange payment.

I asked AGU whether a delay would be a problem, as Mike says your finance system is down to

July 1st (presumably an end-of-year thing?). Their response was:

"We will need authorization of payment for the color figure ASAP. Without it, we can start

production, but can't publish."

So they can process the manuscript up to a certain point, but then have to wait for payment

authorisation. The billing form gives the option of entering a purchase order number that

will be sent after the form itself - perhaps that can be done now, even if the purchase

order itself can't yet be raised?

Anyway, here are the details for filling in (sorry, filling out!) the form:

Page 1:

EOS manuscript #: 2003ES000354

Article title: On Past Temperatures and Anomalous late-20th Century Warmth

Authors: Mann, Ammann, Bradley, Briffa, Crowley, Hughes, Jones, Oppenheimer, Osborn,

Overpeck, Rutherford, Trenberth, Wigley

Color,

1st color figure x \$1300 COST = \$1300

1 additional color figures x \$325 COST = \$325

Page 452

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Reprints,
First 100, *3* pages x \$40 COST = \$120
Additional reprints, *NONE*
Color printing surcharge \$750 COST = \$750
Covers,
NONE
EOS issues,
NONE
Estimated total cost \$2495
Please sign and date etc. the first page.
Page 2:
This is up to you to fill out.

Please feel free to contact me if you need further information, or are unable to complete the form. Page 2 of the form gives the the fax number to send it to.
Best regards
Tim

At 16:46 25/06/2003, Michael E. Mann wrote:

Hi Tim,
We can pay for these at U.Va.--lets order 100.. I'm cc'ing this to our finances person there, Victoria Beamer. Unfortunately, I think that the payment system is down until

July 1st, however, so we'd have to wait until then to issue an LPO. I think the form allows us to ask to be billed? That will buy us a bit of time,
mike

p.s. [this may be my last email access for at least a week...

At 10:35 AM 6/25/2003 +0100, Tim Osborn wrote:

Hi Phil and Mike,
not sure if both/either of you will be reading email today, but in case you are, here is a question for you:

From the responses so far, nobody has requested any reprints. The order form says:

Colour costs: \$1300 for first figure. \$325 for next figure.

So even with no reprints, the cost will be \$1625.

If you do want some reprints, the minimum order is 100 and the cost will be \$870 (expensive because of the colour). Extra reprints are just \$30 per 100.

0 reprints = \$1625

100 reprints = \$2495

200 reprints = \$2525

etc.

Please let me know which option you want to go for.

Please let me know who is paying, U.Virginia or UEA? I'll need to get purchase orders

sorted out, so if it is to be U.Virginia, Mike will need to tell me who to contact to organise it.

Cheers

Tim

Dr Timothy J Osborn

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2531. 2003-06-26

date: Thu, 26 Jun 2003 20:47:17 +0100
from: f028 <P.Jones@uea.ac.uk>
subject: FWD: RE: reality 101
to: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

FYI
Phil

>===== Original Message From Tim Barnett <tbarnett-u1@ucsd.edu> =====
>tom....i completely agree. just thought it was interesting to see their
>spin...tim

>At 12:30 PM 6/25/2003 -0400, Tom Crowley wrote:

>>Tim et al.,

>

>>I think the group might be construed as getting a little too political if
>>we tried to make noise about this. this is a judgement call but I would
>>rather not be involved on this - it does however seem entirely ok if
>>individuals want to make a personal statement on the matter.

>

>>my own personal view is that the Administration misrepresented available
>>information with respect to the cause for a war with Iraq, beneficiaries
>>of tax cuts, etc - misrepresenting climate information seems like small
>>change comparatively. since the Administration is basically going to get
>>away with the the other misrepresentations, why get embroiled as a group
>>on a matter that almost certainly not change their mind one iota?

>

>>tom

>

>

>

>>>dear detectors.....have a look at the following web site for the
>>>current US government rationale on global warming
>>><http://rpc.senate.gov/releases/2003/ev060203.pdf> This material will be
>>>used to rebut the McCain/Lieberman 'sense of the senate' resolution about
>>>global warming

>>

>>

>>>Do you find this scary? Do you have faith in the experts quoted? Do we
>>>attempt to rebut this? Have a nice day, if you can after reading
>>>this. best, tim

>>

>>---

>>>[This E-mail scanned for viruses by Declude Virus]

>

>

>---

>>Thomas J. Crowley
>>Nicholas Professor of Earth Systems Science
>>Dept. of Earth and Ocean Sciences
>>Nicholas School of the Environment and Earth Sciences
>>Box 90227
>>103 Old Chem Building Duke University
>>Durham, NC 27708

>

cg2003

>tcrowley@duke.edu
>919-681-8228
>919-684-5833 fax

[This E-mail scanned for viruses by Declude Virus]

4147. 2003-06-26

date: Thu, 26 Jun 2003 14:23:00 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: IMAGES meeting extended abstract
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Hi Keith,
With suitable revision, do you think that what I have written could go into The Holocene as a Forum article? I think that there is still enough controversy and uncertainty about all of this stuff that a Forum article would be justified. I still get a number of inquiries about the Esper paper and recently was invited by Crowley to talk at Duke (same meeting Phil was at). Let me know what you think.

Ed
>Ed tried to ring but this will do
>Have read the abstract and my comments are
>1/ thanks for keeping me in the loop and the citations
>2/ reads well, and quaint at times
>3/ page 2 half way down - the 2out of growing season2 signal could
>also arise through the winter climate preconditioning the growing
>season climate (ie cold winter = delayed soil thawing, or dry
>reducing groundwater recharge etc).
>4/ bottom of page with Figure 6 in - you say not clear why ECS has
>highest amplitude. Partly as you have scaled low-frequency against
>Mann (but you cover this later)
>5/ next page - a third way down - our purpose in re-calibrating ECS
>against high frequency data was not to say that was the "right" way
>- but only to demonstrate the sensitivity in the absolute amplitude
>of early fluctuations (trends) to the calibration procedure
>(important if the reconstruction is to be used to define the
>sensitivity of EBMs - such as in Crowley's work.
>Your point at the end re the 500-1000 year variability is well taken
>- and re-emphasised by spectra of the long Fennoscandian or Russian
>(or composite Eurasian) curves I et al have produced.
>
>ALL THE ABOVE ARE COMMENTS FOR DISCUSSION AND I WOULD NOT SUGGEST
>THE CHANGE OF EVEN A SINGLE CURVE.
>I would just mention that the lack of coherence in the below 20-year band
>should be explored more, through the local/regional calibration of
>the data and aggregation of reconstructions - as we will do - won't
>we?
>Keith
>
>
>At 09:51 AM 6/26/03 -0400, you wrote:
>>Hi Keith,
>>
>>Here is an extended abstract of a talk I am giving at the IMAGES
>>workshop to be held in Norway in August. Because it touches on a
>>number of issues relating to the Esper et al. (2002) work,
>>including some of yours, I am sending it to you for comments. I
>>need to send it to the meeting convenor by Friday, so any changes
>>you want me to make must be sent to me by Friday afternoon your
>>time. I don't think that there is anything in it that you would
>>STRONGLY disagree with.
>>

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>>I am NOT sending it to Phil or Mike for comments, but I am sure
>>they will see it at some point. Bradley will too since he will be
>>at the workshop as well.

>>

>>Cheers,

>>

>>Ed

>>--

>>=====

>>Dr. Edward R. Cook
>>Doherty Senior Scholar and
>>Director, Tree-Ring Laboratory
>>Lamont-Doherty Earth Observatory
>>Palisades, New York 10964 USA
>>Email: drdendro@ldeo.columbia.edu
>>Phone: 845-365-8618
>>Fax: 845-365-8152

>>=====

>>

>

>--

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=====

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Phone: 845-365-8618
Fax: 845-365-8152

=====

</x-flowed>

154. 2003-06-30

date: Mon, 30 Jun 2003 18:25:29 +0100
from: "Kuylenstierna, J.C." <jck1@york.ac.uk>
subject: New tiempo cpsts
to: Mick Kelly <m.kelly@uea.ac.uk>
-----boundary-LibPST-iamunique-2062861447_--
Content-Type: text/plain; charset="ISO-8859-1"

Hi Mick,

Sara has suggested that with the timetable given, that we ought to plan on the extension until end February 2004. I have then started to change the budget to add some more time. As we have already used the funds for one (June) issue of the three planned, I thought we would just add some days as follows:

Mick 5
Sarah 10
Mike Salmon 2.5
Gerry 4
Johan 4
Jenny 2

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This would increase the total funds to 1,315,813 from 1,178,000, an increase of 137813 SEK (about £10,000). The publication cost for March 2003 would be in the new proposal, but all the work will have been done in Jan/Feb.

Does that sound OK?

Johan

--

Johan Kuylenstierna
Director SEI-Y
University of York
Tel.: +44 1904 432892 (direct)
+44 1904 432897 (general)
Fax.: +44 1904 432898
Email.: jck1@york.ac.uk

190. 2003-06-30

cc: "Duckmanton, Jenny" <jmd4@york.ac.uk>, "Kuylenstierna, Johan" <jck1@york.ac.uk>

date: Mon, 30 Jun 2003 11:22:28 +0100

from: Jenny Duckmanton <jmd4@york.ac.uk>

subject: Re: Tiempo final invoice

to: Mick Kelly <m.kelly@uea.ac.uk>

-----boundary-LibPST-iamunique-117349456_--

Content-Type: text/plain; charset="us-ascii"

Ciao Mick

Just back from Tuscany and still ploughing through accumulated emails. Where the UEA invoice is concerned, I just opened an invoice from UEA for SEK 71,074.09 and would be most obliged if you could let me know if this is the correct amount, so I can get it paid?

Please give my regards to Sarah and let her know that Tuscany is still as beautiful as ever, but a bit more expensive than before but still cheaper than the UK. We also went to spend a few days in Umbria where some friends of ours had rented a lovely villa with magnificent views, gardens, pool, etc.

Best regards

Jenny

Mick Kelly wrote:

> Jenny

> UEA should send the final invoice on the old contract within a day or two. I

> am trying to see it before it goes to check it is for the right amount. In

> case I fail and it's not the right amount, please let me know asap!

> Thanks

> Mick

>

>

>

> Mick Kelly Climatic Research Unit

> School of Environmental Sciences

> University of East Anglia

> Norwich NR4 7TJ United Kingdom

> Tel: 44-1603-592091 Fax: 44-1603-507784

> Email: m.kelly@uea.ac.uk

> Web: <http://www.cru.uea.ac.uk/tiempo/>

>

--

Jenny Duckmanton
SEI-Y Coordinator
Stockholm Environment Institute-York
University of York
York YO10 5YW, UK
Tel: +44 (0)1904 432897
Fax: +44 (0)1904 432898
Email: jmd4@york.ac.uk

cg2003
website: <http://www.york.ac.uk/inst/sei/>

255. 2003-07-03

cc: mann@virginia.edu, Ben Santer <santer1@llnl.gov>
date: Thu, 03 Jul 2003 21:27:32 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Fwd: Climate Research
to: Mike Hulme <m.hulme@uea.ac.uk>, p.jones@uea.ac.uk, wigley@ncar.ucar.edu
Thanks Mike

It seems to me that this "Kinne" character's words are disingenuous, and he probably supports what De Freitas is trying to do. It seems clear we have to go above him.

I think that the community should, as Mike H has previously suggested in this eventuality, terminate its involvement with this journal at all levels--reviewing, editing, and submitting, and leave it to wither way into oblivion and disrepute,

Thanks,
mike

At 01:00 PM 7/3/2003 +0100, Mike Hulme wrote:

Phil, Tom, Mike,

So, this would seem to be the end of the matter as far as Climate Research is concerned.

Mike

To

CLIMATE RESEARCH

Editors and Review Editors

Dear colleagues,

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Chris de Freitas to present to me copies of the reviewers' evaluations for the 2 Soon et

al. papers.

I have received and studied the material requested.

Conclusions:

1) The reviewers consulted (4 for each ms) by the editor presented detailed, critical and helpful evaluations

2) The editor properly analyzed the evaluations and requested appropriate revisions.

3) The authors revised their manuscripts accordingly.

Summary:

Chris de Freitas has done a good and correct job as editor.

Best wishes,

Otto Kinne

Director, Inter-Research

--

Inter-Research, Science Publisher

Ecology Institute

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[3]www.eeiu.org

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2185. 2003-07-03

cc: p.jones@uea.ac.uk, "Michael E. Mann" <mann@virginia.edu>, Ben Santer
<santer1@llnl.gov>

date: Thu, 03 Jul 2003 09:20:28 -0600

from: Tom Wigley <wigley@ucar.edu>

subject: Re: Fwd: Climate Research

to: Mike Hulme <m.hulme@uea.ac.uk>

<x-flowed>

Mike,

Thanx -- but not quite the end.

A nebulous issue is the choice of referees, but we can probably never
get that information and Kinne can't evaluate this aspect.

Danny Harvey and I are still planning to follow up the concerns re the
paper we reviewed, rejected and never saw again until it was published.
What has happened since is that another crappy paper that Ben and I
rejected for J. Climate, a specific and unjust criticism of our work,
has now appeared in CR. Presumably the pipeline is deFreitas. So Danny
and I will raise this issue too.

Tom.

Mike Hulme wrote:

> Phil, Tom, Mike,

>

> So, this would seem to be the end of the matter as far as Climate
> Research is concerned.

>

> Mike

>

>> To

>> CLIMATE RESEARCH

>> Editors and Review Editors

>>

>> Dear colleagues,

>>

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>> Best wishes,
>> Otto Kinne
>> Director, Inter-Research
>> --
>> -----
>> Inter-Research, Science Publisher
>> Ecology Institute
>> Nordbunte 23,
>> D-21385 Oldendorf/Luhe,
>> Germany
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>> -----
>
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>
>
</x-flowed>

3155. 2003-07-03

date: Thu Jul 3 16:52:36 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Fwd: 03-19 Mann - climate change press release - first draft
to: k.briffa@uea

Keith - you might want to take a close look at this press release, in case (i)
it is
inappropriate, or (ii) you want to be contacted by the media because Phil & Mike
are in

Japan.
Cheers
Tim

Date: Thu, 03 Jul 2003 11:04:26 -0400
From: Harvey Leifert <HLeifert@agu.org>
Organization: AGU
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.1; en-US; rv:1.3.1)

Gecko/20030425

X-Accept-Language: en-us, en
To: "Michael E. Mann" <mann@virginia.edu>

cg2003

CC: f028 <P.Jones@uea.ac.uk>, tim Osborn <t.osborn@uea.ac.uk>
Subject: 03-19 Mann - climate change press release - first draft

Hi Mike,

Thanks for the background information, some of which I have incorporated into the draft below. Please send me your corrections and/or suggestions asap. In particular, is the title ok or too strong regarding human activity? I also need contact information (phone and email) for whichever authors you think should be able to handle media queries resulting from this release. In the case of you and Phil, I need Sapporo numbers, as well as your permanent ones. (Not all authors are AGU members, it seems, and therefore not in our database.) If the changes are not major, I'll just make them and issue the release; if you want to see a second draft, let me know. Thanks!

Regards,

Harvey

[Title] Leading Climate Scientists Reaffirm View That Late 20th Century Warming Was Unusual and Resulted From Human Activity
WASHINGTON - A group of leading climate scientists has reaffirmed the "robust consensus view" emerging from the peer reviewed literature that the warmth experienced on at least a hemispheric scale in the late 20th century was an anomaly in the previous millennium and that human activity likely played an important role in causing it. In doing, they refuted recent claims that the warmth of recent decades was not unprecedented in the context of the past thousand years.
Writing in the 8 July issue of the American Geophysical Union publication Eos, Michael Mann of the University of Virginia and 12 colleagues in the United States and United Kingdom endorse the position on climate change and greenhouse gases taken by AGU in 1998. Specifically, they say that "there is a compelling basis for concern over future climate changes, including increases in global-mean surface temperatures, due to increased concentrations of greenhouse gases, primarily from fossil-fuel burning."
The Eos article is a response to two recent and nearly identical papers by Drs. Willie Soon and Sallie Baliunas of the Harvard-Smithsonian Center for Astrophysics, published in Climate Research and Energy & Environment (the latter paper with additional co-authors). They challenge the generally accepted view that natural factors cannot fully explain recent warming and must have been supplemented by significant human activity, and their papers have received attention in the media and in the U.S. Senate.
Requests from reporters to top scientists in the field, seeking comment on the Soon and Baliunas position, lead to memoranda that were later expanded into the current Eos article, which was itself peer reviewed.

Mann and his colleagues rely on instrumental data for the past 150 years and "proxy" indicators, such as tree rings, ice cores, corals, and lake sediments to reconstruct the climate of earlier times. Most of the available data pertain to the northern hemisphere and show, according to the authors, that the warmth of the northern hemisphere over the past few decades is likely unprecedented in the last 1,000 years and quite possibly in the preceding 1,000 years as well.

Climate model simulations cannot explain the anomalous late 20th century warmth without taking into account the contributions of human activities, the authors say. They make three major points regarding Soon and Baliunas's recent assertions challenging these findings.

First, in using proxy records to draw inferences about past climate, it is essential to assess their actual sensitivity to temperature variability. In particular, the authors say, Soon and Baliunas misuse hydrological data in their effort to determine temperature.

Second, it is essential to distinguish between regional temperature anomalies and hemispheric mean temperature, which must represent an average of estimates over a sufficiently large number of distinct regions. For example, Mann and his co-authors say, the concepts of a "Little Ice Age" and "Medieval Warm Period" arose from the Eurocentric origins of historic climatology. The specific periods of coldness and warmth differed from region to region and as compared with data for the northern hemisphere as a whole.

Third, according to Mann and his colleagues, it is essential to define carefully the modern base period with which past climate is to be compared and to identify and quantify uncertainties. For example, they say, the most recent report of the Intergovernmental Panel on Climate Change (IPCC) carefully compares data for recent decades with reconstructions of past temperatures, taking into account the uncertainties in those reconstructions. IPCC concluded that late 20th century warmth in the northern hemisphere likely exceeded that of any time in the past millennium. The method used by Soon and Baliunas, they say, considers mean conditions for the entire 20th century as the base period and determines past temperatures from proxy evidence not capable of resolving trends on a decadal basis. It is therefore, they say, of limited value in determining whether recent warming is anomalous in a long term and large scale context.

The Eos article started as a memorandum that Michael Oppenheimer and Mann drafted to help inform colleagues who were being contacted by members of the media regarding the Soon and Baliunas papers and wanted an opinion from climate scientists and

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paleoclimatologists (scientists who study ancient climates) who were directly familiar with the underlying issues.

Mann and Oppenheimer learned that a number of other colleagues, including Tom Wigley of the University Corporation for Atmospheric Research (UCAR) in Boulder, Colorado; Philip Jones of the University of East Anglia's Climatic Research Unit in Norwich, United Kingdom; and Raymond Bradley of the University of Massachusetts in Amherst were

receiving similar media requests for their opinions on the matter. Their original memorandum evolved into a more general position paper jointly authored by a larger group of leading scientists in the field.

Mann says he sees the resulting Eos article as representing an even broader consensus of the viewpoint of the mainstream climate research community on the question of late 20th

century warming and its causes. The goal of the authors, he says, is to reaffirm support for the AGU position statement on climate change and greenhouse gases and clarify what

is currently known from the paleoclimate record of the past one-to-two thousand years and, in particular, what the bearing of this evidence is on the issue of the detection of human influence on recent climate change.

Notes for Journalists:

The article, "On Past Temperatures and Anomalous Late-20th Century Warmth. appears in

Eos, Volume 84, No. 27, 8 July 2003, page 256.

Authors (full list):

Michael Mann, University of Virginia, Charlottesville, Virginia;
Caspar Ammann and Kevin Trenberth, National Center for Atmospheric Research, Boulder, Colorado;
Raymond Bradley, University of Massachusetts, Amherst, Massachusetts;
Keith Briffa, Philip Jones, and Tim Osborn, Climatic Research Unit, University of East Anglia, Norwich, United Kingdom;

Tom Crowley, Nicholas School of the Environment and Earth Science, Duke University, Durham, North Carolina;

Malcolm Hughes, Laboratory of Tree-Ring Research, University of Arizona, Tucson, Arizona;

Michael Oppenheimer, Princeton University, Princeton, New Jersey;
Jonathan Overpeck, Department of Geosciences and Institute for the Study of Planet Earth, University of Arizona, Tucson, Arizona;

Scott Rutherford, University of Rhode Island, Narragansett, Rhode Island;
Tom Wigley, University Corporation for Atmospheric Research and National Center for

Atmospheric Research, Boulder, Colorado.

Journalists may obtain a pdf copy of this article by request to Harvey Leifert (hleifert@agu.org). Please provide your name, name of publication, phone, and email address.

AGU's position statement, Climate Change and Greenhouse Gases (1998), may be read at

cg2003

[1]http://www.agu.org/sci_soc/policy/climate_change_position.html. A peer reviewed

article, discussing the scientific background to the position statement appeared in Eos,

Volume 80, No 39, September 28, 1999, page 453, and may be read at

[2]http://www.agu.org/eos_elec/99148e.html.

Contact information for authors:

[TO COME]

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Harvey Leifert

Public Information Manager

American Geophysical Union

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Washington, DC 20009, USA

Phone: +1-202-777-7507

Fax: +1-202-328-0566

Email: hleifert@agu.org

Web: [3]http://www.agu.org/sci_soc/media.html

###

5273. 2003-07-03

date: Thu, 3 Jul 2003 13:06:25 +0200

from: Inter-Research Science Publisher <ir@int-res.com>

subject: Climate Research

to: n.w.arnell@soton.ac.uk, balling@asu.edu, Bryson.bates@per.ciw.csiro.au,
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cubasch@zedat.fu-berlin.de, rdedear@laurel.ocs.mq.edu.au, Chris.Doyle@lycos.co.uk,
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jan.goudriaan@staff.tpe.wau.nl, bph@virginia.edu, bhayden@alternet.edu,
horiet@adm.kais.kyoto-u.ac.jp, m.hulme@uea.ac.uk, gjones@sou.edu,
leathers@copland.udel.edu, legates@udel.edu, ray@enmech.csiro.au,
mike_levy@ncsu.edu, p.martens@math.unimaas.nl, G.R.MCGREGOR@bham.ac.uk,
vmeente@uga.cc.uga.edu, minami@niaes.affrc.go.jp, noda@mri-1.mri-jma-go.jp,
t_ogawa@eorc.nasda.go.jp, sala@criba.edu.ar, schulze@aqua.ccwr.ac.za, mds@uwm.edu,
seguin@avignon.inra.fr, myamada@cc.tuat.ac.jp, maurizio.Severini@uniroma1.it,
simon.shackley@umist.ac.uk, vsmetacek@awi-bremerhaven.de, Karen.Smoyer@ualberta.ca,
soulePT@Appstate.EDU, seinidst@alpha.nic.in, nico.stehr@t-online.de,
SUCKLING@COBRA.UNI.EDU, ctkmac@mail.ecc.u-tokyo.ac.jp, toth@iiasa.ac.at,
MGT@mhuh.zoology.wisc.edu, awashi@mail.ecc.u-tokyo.jp, white.45@osu.edu,
rob.wilby@kcl.ac.uk, yarbrough.james@epa.gov, zorita@gkss.de

<x-flowed>

To

CLIMATE RESEARCH

Editors and Review Editors

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</x-flowed>

1695. 2003-07-04

cc: Mike Hulme <m.hulme@uea.ac.uk>, p.jones@uea.ac.uk, Ben Santer

<santer1@llnl.gov>

date: Fri, 04 Jul 2003 07:51:43 -0600

from: Tom Wigley <wigley@ucar.edu>

subject: Re: Fwd: Climate Research

to: "Michael E. Mann" <mann@virginia.edu>

<x-flowed>

Mike (Mann),

I agree that Kinne seems like he could be a deFreitas clone. However,
what would be our legal position if we were to openly and extensively
tell people to avoid the journal?

Tom.

Michael E. Mann wrote:

> Thanks Mike

>

> It seems to me that this "Kinne" character's words are disingenuous, and
> he probably supports what De Freitas is trying to do. It seems clear we
> have to go above him.

>

> I think that the community should, as Mike H has previously suggested in
> this eventuality, terminate its involvement with this journal at all
> levels--reviewing, editing, and submitting, and leave it to wither way
> into oblivion and disrepute,

>

> Thanks,

>

> mike

>

> At 01:00 PM 7/3/2003 +0100, Mike Hulme wrote:

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>> Phil, Tom, Mike,

>>

>> So, this would seem to be the end of the matter as far as Climate
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>> Mike

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>>>

> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
 >
 </x-flowed>

3266. 2003-07-04

date: Fri, 4 Jul 2003 21:29:43 +0200
 from: Eystein Jansen <eystein.jansen@geo.uib.no>
 subject: Re: FP6-news?
 to: Keith Briffa <k.briffa@uea.ac.uk>
 <x-flowed>

Dear Keith, thanks for the update. I think I am reading much the same message as you do. I also agree that we need focus, and not too many groups involved. In terms of where the focus should be I agree that DOCC is too wide, and my feeling now is to dissolve it and reorganise under another heading with fewer groups, perhaps as an IP if Brussels allows. I do not have any preconceived notions as to where the co-ordinations should lie.

I agree with you that integration with biogeochemistry is not straight forward with Holocene climate variability except for the vegetation feedback which may be important.

I also know of one other palaeo-based initiative, ICON, dealing with the thermohaline circulation, coordinated by Rainer Zahn. We are involved. This will be submitted for the call just launched under the hot spots in the climate system heading, but may be brought over to the next call if unsuccessful (probably). We are involved there with a number of modelling centres and many of the palaeoceanography labs. I guess we should discuss a bit further after summer has passed what to do. I am very keen on the science of Holclim and hope to be able to develop this initiative with you and others.

Last thing - any idea of when the conference Brussels wants is going to happen?

I am away for two weeks on the Greek islands, but then I am back again.

Cheers,
 Eystein
 >Eystein

>I seem to keep getting distracted this week so I have not phoned
 >again. I can say the basics here though. I went to the meeting that
 >was also attended by Berger, Raynaud, Shackleton, Starke and
 >Zorita

>(in place of von Storch). The rationale for the meeting was nothing
 >more than The EC (Hans Brelén) felt that they ought to be organising
 >a palaeoclimate conference, but there was some hinting that this
 >might signal the new call (in Sept 04) but not imply any weighting
 >in the appraisal of proposals. It seems definite that there will be
 >money for a single (new instrument) project only, as we supposed.
 >Some at the meeting spoke about a range of time scales and possible
 >subject foci for the conference (and by implication also for the
 >call) but I still feel strongly, on the evidence of other projects
 >that I have heard are to be funded, that the need is for a sharper
 >focus than was involved in our DOCC concept, and that the HOLIVAR
 >approach is the optimum way forward. The problem will be scale of
 >initiative (15-20 million seems a maximum likely request, with
 >perhaps 12-15 a likely maximum award). The unified data / modelling
 >route, as outlined in the HOLCLIM NOI seems the most likely
 >candidate still. Obviously there remain difficulties even with this
 >, such as geographic focus, use of the integrated data for defining
 >future climate probabilities and links with socio-economic (impacts)
 >community. This is also likely to clash with the direct interests of
 >some major palaeoclimate scientists who focus on longer time scales
 >and stronger climate and response signals. It is easier to think of
 >climate forcings and the interaction of bio-geochemical cycles at
 >glacial /interglacial time scales, but I am not convinced that this

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>type of work would be a practical inclusion in this call. This is
>>still my opinion , but an admittedly (unashamedly) biased one.
>Keith

>

>

>At 07:34 PM 6/19/03 +0200, you wrote:

>>Dear Keith,

>>>I wonder if there are any news around the meeting with Brelen on
>>>FP6 that can be used. Lots of rumors around and not much specific
>>>knowledge, so if you have an update I'd appreciate it.

>>>Cheers,

>>>Eystein

>>

>>På mandag, 7. april 2003, kl. 10:46, skrev Keith Briffa:

>>

>>>>Eystein

>>>>your point is exactly correct , that only one project (and I
>>>>believe it should be an IP) will be allowed and with the shrinking
>>>>general scale of these things, it likely needs to be very clearly
>>>>focused (on integrating evidence and providing some
>>>>state-of-the-art product on climate history and its causes) . I am
>>>>not in Nice (have to go to 2 other meetings in May) . I am still
>>>>leaning towards your institute co-ordinating this . I have not
>>>>discussed anything with the rest of the HOLIVAR committee.
>>>>We do need some sort of meeting but only small - there is no
>>>>chance of a 25 million Euro project and many people are likely to
>>>>be disappointed . I have to be in Brussels for a meeting with
>>>>Brelen in June . What are you thinking about , re. a meeting?

>>>>Keith

>>>>At 10:01 PM 4/3/03 +0200, you wrote:

>>>>Dear Keith,

>>>>> I was just wondering whether you were coming the the EGS meeting
>>>>>in Nice next week, in order for us to exchange some ideas about
>>>>>how to proceed for FP6. Recent rumors says that the palaeoclimate
>>>>>variability item is in the books for the third call, and that the
>>>>>call will be issued by the turn of the year, thus we should start
>>>>>discussing how to proceed. So far my DOCC initiative is dormant,
>>>>>and I am more inclined to develop or take part in developing an
>>>>>IP if the call for proposals allow for one. But the size of these
>>>>>IPs seems to be diminishing, hence a careful focussing needs to
>>>>>be undertaken in order for there to be resources for the science
>>>>>teams. I would be happy to discuss idea with you on this in Nice
>>>>>or sometime else if you're not there.

>>>>>

>>>>>Cheers,

>>>>>Eystein

>>>>>

>>>>>

>>>>>

>>>>>Eystein Jansen

>>>>>prof/director

>>>>>Bjerknes Centre for Climate Research

>>>>>Allégaten 55, N5007 Bergen, Norway

>>>>>tel: +4755583491/secr:+4755589803/fax:+4755584330

>>>>>eystein.jansen@geo.uib.no, www.bjerknes.uib.no

>>>>>

>>>>>--

>>>>>Professor Keith Briffa,

>>>>>Climatic Research Unit

>>>>>University of East Anglia

>>>>>Norwich, NR4 7TJ, U.K.

>>>>>

>>>>>Phone: +44-1603-593909

cg2003

>>>Fax: +44-1603-507784
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>>><http://www.cru.uea.ac.uk/cru/people/briffa/>
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>--
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>Fax: +44-1603-507784
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><http://www.cru.uea.ac.uk/cru/people/briffa/>
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Fax: +47-55-584330

The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts

</x-flowed>

1084. 2003-07-06

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Raymond
Bradley <rbradley@geo.umass.edu>, Philip D Jones <P.Jones@uea.ac.uk>,
mann@virginia.edu

date: Sun, 06 Jul 2003 02:33:54 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: 03-19 Mann - climate change press release - first draft

to: Harvey Leifert <HLeifert@agu.org>

Hi Harvey,

Here is Phil's information:

Sapporo Prince Hotel (room #610), phone: (011) 511-3131

Phil and I have discussed various options for making ourselves available to the
press on

the 8th. The IUGG press office probably won't be of much use here, given time
zone

differences.

Given the 13 hour time difference, the most sensible approach is that Phil and I
will make

sure to be available to receive phone calls in our respective hotel rooms

Tuesday the 8th

evening Sapporo time (i.e., Tuesday the 8th morning U.S. east coast time). Phil
can be

available 9-11 PM Sapporo Time which corresponds to 8-10 AM U.S. morning of the

cg2003

8th east

coast time). while I can try to be available until a bit later (say, 9-12 PM Sapporo time,

which is approximately 8-11 AM U.S. east coast). Unfortunately, because of the time zone

change, it would be difficult for us to be available any later than that on morning/afternoon of the 8th east coast time, but if the press can be informed of the above

early morning windows during which we can be reached in Japan, hopefully they can reach us

for comment then. Otherwise, Ray, Tim, and Keith can all field inquiries and, of course,

both Phil and I can respond to email inquiries more flexibly.

Let us know if this sounds workable.

Re, the press release itself, Phil and I have the following five minor comments. Otherwise,

we're very happy with it:

1) first paragraph, 2nd sentence, typo "In do doing" should be "In so doing"...

2) 3rd paragraph, 2nd sentence, change "They challenge" to "These authors challenged" to

clarify that it is Soon, Baliunas that are being referred to here, not us!

3) 4th paragraph, 1st sentence, change "Mann and his colleagues" to "Paleoclimatologists

generally" to clarify that this is the mainstream approach, more general than just Mann and

colleagues...

4) 6th paragraph, 2nd sentence: see comment from my previous email about wording (use of term 'hydrological')

5) 9th paragraph, 2nd sentence: parenthetical statement "scientists who study ancient climates" should be moved to first usage of the term 'paleoclimatologists' above (see

comment #3).

At 11:04 AM 7/3/2003 -0400, you wrote:

Hi Mike,

Thanks for the background information, some of which I have incorporated into the draft

below. Please send me your corrections and/or suggestions asap. In particular, is the

title ok or too strong regarding human activity? I also need contact information (phone

and email) for whichever authors you think should be able to handle media queries

resulting from this release. In the case of you and Phil, I need Sapporo numbers, as

well as your permanent ones. (Not all authors are AGU members, it seems, and therefore

not in our database.) If the changes are not major, I'll just make them and issue the

release; if you want to see a second draft, let me know. Thanks!

Regards,

Harvey

[Title] Leading Climate Scientists Reaffirm View That Late 20th Century Warming Was

Unusual and Resulted From Human Activity

WASHINGTON - A group of leading climate scientists has reaffirmed the "robust consensus

view" emerging from the peer reviewed literature that the warmth experienced on at least

a hemispheric scale in the late 20th century was an anomaly in the previous millennium

and that human activity likely played an important role in causing it. In doing, they refuted recent claims that the warmth of recent decades was not unprecedented in the context of the past thousand years.

Writing in the 8 July issue of the American Geophysical Union publication *Eos*, Michael Mann of the University of Virginia and 12 colleagues in the United States and United Kingdom endorse the position on climate change and greenhouse gases taken by AGU in 1998. Specifically, they say that "there is a compelling basis for concern over future climate changes, including increases in global-mean surface temperatures, due to increased concentrations of greenhouse gases, primarily from fossil-fuel burning."

The *Eos* article is a response to two recent and nearly identical papers by Drs. Willie Soon and Sallie Baliunas of the Harvard-Smithsonian Center for Astrophysics, published in *Climate Research and Energy & Environment* (the latter paper with additional co-authors). They challenge the generally accepted view that natural factors cannot fully explain recent warming and must have been supplemented by significant human activity, and their papers have received attention in the media and in the U.S. Senate.

Requests from reporters to top scientists in the field, seeking comment on the Soon and Baliunas position, lead to memoranda that were later expanded into the current *Eos* article, which was itself peer reviewed.

Mann and his colleagues rely on instrumental data for the past 150 years and "proxy" indicators, such as tree rings, ice cores, corals, and lake sediments to reconstruct the climate of earlier times. Most of the available data pertain to the northern hemisphere and show, according to the authors, that the warmth of the northern hemisphere over the past few decades is likely unprecedented in the last 1,000 years and quite possibly in the preceding 1,000 years as well.

Climate model simulations cannot explain the anomalous late 20th century warmth without taking into account the contributions of human activities, the authors say. They make three major points regarding Soon and Baliunas's recent assertions challenging these findings.

First, in using proxy records to draw inferences about past climate, it is essential to assess their actual sensitivity to temperature variability. In particular, the authors say, Soon and Baliunas misuse hydrological data in their effort to determine temperature.

Second, it is essential to distinguish between regional temperature anomalies and hemispheric mean temperature, which must represent an average of estimates over a sufficiently large number of distinct regions. For example, Mann and his co-authors

say, the concepts of a "Little Ice Age" and "Medieval Warm Period" arose from the Eurocentric origins of historic climatology. The specific periods of coldness and warmth differed from region to region and as compared with data for the northern hemisphere as a whole.

Third, according to Mann and his colleagues, it is essential to define carefully the modern base period with which past climate is to be compared and to identify and quantify uncertainties. For example, they say, the most recent report of the Intergovernmental Panel on Climate Change (IPCC) carefully compares data for recent decades with reconstructions of past temperatures, taking into account the uncertainties in those reconstructions. IPCC concluded that late 20th century warmth in the northern hemisphere likely exceeded that of any time in the past millennium. The method used by Soon and Baliunas, they say, considers mean conditions for the entire 20th century as the base period and determines past temperatures from proxy evidence not capable of resolving trends on a decadal basis. It is therefore, they say, of limited value in determining whether recent warming is anomalous in a long term and large scale context.

The Eos article started as a memorandum that Michael Oppenheimer and Mann drafted to help inform colleagues who were being contacted by members of the media regarding the Soon and Baliunas papers and wanted an opinion from climate scientists and paleoclimatologists (scientists who study ancient climates) who were directly familiar with the underlying issues.

Mann and Oppenheimer learned that a number of other colleagues, including Tom Wigley of the University Corporation for Atmospheric Research (UCAR) in Boulder, Colorado; Philip Jones of the University of East Anglia's Climatic Research Unit in Norwich, United Kingdom; and Raymond Bradley of the University of Massachusetts in Amherst were receiving similar media requests for their opinions on the matter. Their original memorandum evolved into a more general position paper jointly authored by a larger group of leading scientists in the field.

Mann says he sees the resulting Eos article as representing an even broader consensus of the viewpoint of the mainstream climate research community on the question of late 20th century warming and its causes. The goal of the authors, he says, is to reaffirm support for the AGU position statement on climate change and greenhouse gases and clarify what is currently known from the paleoclimate record of the past one-to-two thousand years and, in particular, what the bearing of this evidence is on the issue of the detection of human influence on recent climate change.

Notes for Journalists:

The article, "On Past Temperatures and Anomalous Late-20th Century Warmth. appears in

Eos, Volume 84, No. 27, 8 July 2003, page 256.

Authors (full list):

Michael Mann, University of Virginia, Charlottesville, Virginia;

Caspar Ammann and Kevin Trenberth, National Center for Atmospheric Research, Boulder, Colorado;

Raymond Bradley, University of Massachusetts, Amherst, Massachusetts;

Keith Briffa, Philip Jones, and Tim Osborn, Climatic Research Unit, University of East

Anglia, Norwich, United Kingdom;

Tom Crowley, Nicholas School of the Environment and Earth Science, Duke University,

Durham, North Carolina;

Malcolm Hughes, Laboratory of Tree-Ring Research, University of Arizona, Tucson,

Arizona;

Michael Oppenheimer, Princeton University, Princeton, New Jersey;

Jonathan Overpeck, Department of Geosciences and Institute for the Study of Planet

Earth, University of Arizona, Tucson, Arizona;

Scott Rutherford, University of Rhode Island, Narragansett, Rhode Island;

Tom Wigley, University Corporation for Atmospheric Research and National Center for

Atmospheric Research, Boulder, Colorado.

Journalists may obtain a pdf copy of this article by request to Harvey Leifert (hleifert@agu.org). Please provide your name, name of publication, phone, and

email address.

AGU's position statement, Climate Change and Greenhouse Gases (1998), may be read at

[1]http://www.agu.org/sci_soc/policy/climate_change_position.html. A peer reviewed

article, discussing the scientific background to the position statement appeared in Eos,

Volume 80, No 39, September 28, 1999, page 453, and may be read at

[2]http://www.agu.org/eos_elec/99148e.html.

Contact information for authors:

[TO COME]

###

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Harvey Leifert

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Web: [3]http://www.agu.org/sci_soc/media.html

###

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Raymond Bradley <rbradley@geo.umass.edu>, Philip D Jones <P.Jones@uea.ac.uk>
date: Mon, 07 Jul 2003 16:56:08 -0400

from: Harvey Leifert <HLeifert@agu.org>
subject: Re: 03-19 Mann et al. - climate change press release issued
to: "Michael E. Mann" <mann@virginia.edu>
<x-flowed>

All,
We issued the press release at 3:40 p.m. EDT Monday, July 7. It was sent to 900 science writers worldwide on our distribution list and posted on EurekaAlert!, the AAAS web site for science press releases. Almost immediately, we received requests for the full article from The New York Times, USA Today, National Public Radio, Toronto Star, San Jose Mercury News, Cox Newspapers, Richmond Times-Dispatch, and four freelancers. It was too late for most Europeans to receive the release Monday, so we expect additional requests Tuesday morning.

Thanks to all for your help.

Harvey

--

Harvey Leifert
Public Information Manager
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Email: hleifert@agu.org
Web: http://www.agu.org/sci_soc/media.html

###

</x-flowed>

2979. 2003-07-08

date: Tue Jul 8 13:07:45 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Fwd: RE: Response to terrible climate op ed?
to: t.osborn@uea.ac.uk

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 5.1.1
Date: Mon, 07 Jul 2003 18:31:22 -0400
To: Raymond Bradley <rbradley@geo.umass.edu>,
Keith Briffa <k.briffa@uea.ac.uk>,
Philip D Jones <P.Jones@uea.ac.uk>,
Kevin Trenberth <trenbert@cgd.ucar.edu>,
Tom Wigley <wigley@ucar.edu>,
Michael Oppenheimer <omichael@princeton.edu>,
Tom Crowley <tcrowley@duke.edu>

From: "Michael E. Mann" <mann@virginia.edu>
Subject: Fwd: RE: Response to terrible climate op ed?
interesting timing, eh?

mike

Subject: RE: Response to terrible climate op ed?
Date: Mon, 7 Jul 2003 12:13:35 -0400

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: Response to terrible climate op ed?

Thread-Index: AcNEokfXBMkDiyQaTMwtTClt/YT0zAAAGP3g

From: "Profeta, Tim (Lieberman)" <Tim_Profeta@lieberman.senate.gov>

To: Aaron Rappaport <arappaport@ucsusa.org>,
"DesChamps, Floyd (Commerce)" <Floyd_DesChamps@commerce.senate.gov>,
Annie_Petsonk@environmentaldefense.org,
Elizabeth_Thompson@environmentaldefense.org,

cg2003

Melissa_Carey/EnvironmentalDefense@environmentaldefense.org,
"Wicke, Heather (McCain)" <Heather_Wicke@mccain.senate.gov>,
dlashof@nrdc.org, Symons@nwf.org, omichael@princeton.edu,
Alden Meyer <ameyer@ucsusa.org>, Peter Frumhoff <pfrumhoff@ucsusa.org>,
mann@virginia.edu

MMDf-WARNING: Parse error in original version of preceding line at
mail.virginia.edu

X-OriginalArrivalTime: 07 Jul 2003 16:13:36.0507 (UTC)

FILETIME=[B85E14B0:01C344A2]

X-MIME-Autoconverted: from quoted-printable to 8bit by
multiproxy.evsc.Virginia.EDU id

h67GDow06623

I think we need to get a scientists' oped out, very soon.

-----Original Message-----

From: Aaron Rappaport [[1]mailto:arappaport@ucsusa.org]

Sent: Monday, July 07, 2003 12:11 PM

To: DesChamps, Floyd (Commerce); Annie_Petsonk@environmentaldefense.org;

Elizabeth_Thompson@environmentaldefense.org;

Melissa_Carey/EnvironmentalDefense@environmentaldefense.org; Profeta,

Tim (Lieberman); Wicke, Heather (McCain); dlashof@nrdc.org;

Symons@nwf.org; omichael@princeton.edu; Alden Meyer; Peter Frumhoff;

mann@virginia.edu

Subject: Response to terrible climate op ed?

Are any scientists planning to rebut the terrible Schlesinger op ed that
appeared in

this morning's Washington Post? Coordinating on this would avoid duplication
of
effort. Our thinking is that a scientists' rebuttal would be more persuasive
than one
from enviros or politicians.

Schlesinger's op-ed appears to be a recycling for popular consumption of the
recent

Soon-Baliunas papers that questioned the existence of anthropogenic climate
change. To

rebut, one apparently has to call Fred Hyatt at the Washington Post to arrange
to

publish a "Taking Exception" column.

Thanks, Aaron

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[2]http://www.washingtonpost.com

The Washington Post

July 07, 2003, Monday, Final Edition

SECTION: EDITORIAL; Pg. A17

LENGTH: 1057 words

HEADLINE: Climate Change: The Science Isn't Settled

BYLINE: James Schlesinger

BODY:

Despite the certainty many seem to feel about the causes, effects and extent
of climate

change, we are in fact making only slow progress in our understanding of the
underlying

science. My old professor at Harvard, the great economist Joseph Schumpeter,
used to

insist that a principal tool of economic science was history -- which served
to temper

the enthusiasms of the here and now. This must be even more so in
climatological

science. In recent years the inclination has been to attribute the warming we
have

lately experienced to a single dominant cause -- the increase in greenhouse
gases. Yet

climate has always been changing -- and sometimes the swings have been rapid.
At the time the U.S. Department of Energy was created in 1977, there was

widespread

concern about the cooling trend that had been observed for the previous quarter-century.

After 1940 the temperature, at least in the Northern Hemisphere, had dropped about

one-half degree Fahrenheit -- and more in the higher latitudes. In 1974 the National

Science Board, the governing body of the National Science Foundation, stated:

"During the last 20 to 30 years, world temperature has fallen, irregularly at first but more

sharply over the last decade." Two years earlier, the board had observed:

"Judging from the record of the past interglacial ages, the present time of high temperatures should

be drawing to an end . . . leading into the next glacial age." And in 1975 the National

Academy of Sciences stated: "The climates of the earth have always been changing, and

they will doubtless continue to do so in the future. How large these future changes will

be, and where and how rapidly they will occur, we do not know."

These statements -- just a quarter-century old -- should provide us with a dose of

humility as we look into the more distant future. A touch of that humility might help

temper the current raging controversies over global warming. What has concerned me in

recent years is that belief in the greenhouse effect, persuasive as it is, has been

transmuted into the dominant forcing mechanism affecting climate change -- more or less

to the exclusion of other forcing mechanisms. The CO₂/climate-change relationship has

hardened into orthodoxy -- always a worrisome sign -- an orthodoxy that searches out

heretics and seeks to punish them.

We are in command of certain essential facts. First, since the start of the 20th

century, the mean temperature at the earth's surface has risen about 1 degree Fahrenheit. Second, the level of CO₂ in the atmosphere has been increasing for

more than 150 years. Third, CO₂ is a greenhouse gas -- and increases in it, other things

being equal, are likely to lead to further warming. Beyond these few facts, science remains

unable either to attribute past climate changes to changes in CO₂ or to forecast with

any degree of precision how climate will change in the future.

Of the rise in temperature during the 20th century, the bulk occurred from 1900 to 1940.

It was followed by the aforementioned cooling trend from 1940 to around 1975. Yet the

concentration of greenhouse gases was measurably higher in that later period than in the

former. That drop in temperature came after what was described in the National Geographic as "six decades of abnormal warmth."

In recent years much attention has been paid in the press to longer growing seasons and

shrinking glaciers. Yet in the earlier period up to 1975, the annual growing season in

England had shrunk by some nine or 10 days, summer frosts in the upper Midwest occasionally damaged crops, the glaciers in Switzerland had begun to advance

again, and sea ice had returned to Iceland's coasts after more than 40 years of its near absence. When we look back over the past millennium, the questions that arise are even more perplexing. The so-called Climatic Optimum of the early Middle Ages, when the earth's temperatures were 1 to 2 degrees warmer than today and the Vikings established their flourishing colonies in Greenland, was succeeded by the Little Ice Age, lasting down to the early 19th century. Neither can be explained by concentrations of greenhouse gases. Moreover, through much of the earth's history, increases in CO₂ have followed global warming, rather than the other way around. We cannot tell how much of the recent warming trend can be attributed to the greenhouse effect and how much to other factors. In climate change, we have only a limited grasp of the overall forces at work. Uncertainties have continued to abound -- and must be reduced. Any approach to policy formation under conditions of such uncertainty should be taken only on an exploratory and sequential basis. A premature commitment to a fixed policy can only proceed with fear and trembling.

In the Third Assessment by the International Panel on Climate Change, recent climate change is attributed primarily to human causes, with the usual caveats regarding uncertainties. The record of the past 150 years is scanned, and three forcing mechanisms are highlighted: anthropogenic (human-caused) greenhouse gases, volcanoes and the 11-year sunspot cycle. Other phenomena are represented poorly, if at all, and generally are ignored in these models. Because only the past 150 years are captured, the vast swings of the previous thousand years are not analyzed. The upshot is that any natural variations, other than volcanic eruptions, are overshadowed by anthropogenic greenhouse gases.

Most significant: The possibility of long-term cycles in solar activity is neglected because there is a scarcity of direct measurement. Nonetheless, solar irradiance and its variation seem highly likely to be a principal cause of long-term climatic change. Their role in longer-term weather cycles needs to be better understood.

There is an idea among the public that "the science is settled." Aside from the limited facts I cited earlier, that remains far from the truth. Today we have far better instruments, better measurements and better time series than we have ever had. Still, we are in danger of prematurely embracing certitudes and losing open-mindedness. We need to be more modest.

The writer, who has served as secretary of energy, made these comments at a symposium on the 25th anniversary of the Energy Department's CO₂/climate change program.

cg2003

LOAD-DATE: July 07, 2003

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202-223-6133, ext. 124
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--

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Fax: +44-1603-507784
[4][http://www.cru.uea.ac.uk/cru/people/briffa\[5\]/](http://www.cru.uea.ac.uk/cru/people/briffa[5]/)

1719. 2003-07-10

cc: p.jones@uea, pittock@uea.ac.uk, a.minns@uea.ac.uk, Wolfgang Cramer
<Wolfgang.Cramer@pik-potsdam.de>, <mann@virginia.edu>, simon.torok@csiro.au
date: Thu, 10 Jul 2003 07:41:07 +1200
from: "Jim Salinger" <j.salinger@niwa.co.nz>
subject: Re: cc. of letter to Climate Research
to: <simon.shackley@umist.ac.uk>, Robert Wilby <rob.wilby@kcl.ac.uk>,
tim.carter@vyh.fi, "N.W.Arnell" <N.W.Arnell@soton.ac.uk>,
p.martens@icis.unimaas.nl, Peter.Whetton@csiro.au, Mike Hulme <m.hulme@uea.ac.uk>
Dear Mike et al

I have just heard from a member of the department that the Editor who handled the
Soon and Baliunas paper that Otto Kinne asked for an explanation of the
criticisms.

The Editor has given these. Apparently Otto Kinne has accepted these and plans to
take no further action.

It is interesting to note that my informant also received the Soon and Baliunas
manuscript for review, and strongly recommended rejection.

I may be in position to learn more this evening.

Adios for now

Jim

On 12 Jun 2003 at 17:28, Mike Hulme wrote:

Dear Climate Research Review Editor,

Below is the letter that I have just sent to the publisher of Climate Research -
Otto

Kinne. I am copying this to you all (including ccs) since in my original email of
16 April

which originally raised my concerns I said I would keep you informed of my actions.

The letter is self-explanatory. Feel free to use this letter if you wish to follow

cg2003

up directly
with the journal. I have made my position clear.

Mike

Dr Jim Salinger, CRSNZ Tel: + 64 9 375 2053
NIWA Fax: + 64 9 375 2051
P O Box 109 695, (269 Khyber Pass Road) e-mail: j.salinger@niwa.co.nz
Newmarket, Auckland,
New Zealand

3705. 2003-07-10

date: Thu, 10 Jul 2003 12:38:55 +0100
from: "A. DAWSON" <GEX014@coventry.ac.uk>
subject: Sea ice and SST proxies
to: k.briffa@uea.ac.uk
Dear Keith,
Please find attached in confidence recent versions of sea ice and SST
proxies - these are being modified at present and changed around but
basically they both give seasonal signals for last 2k - and they raise
all sorts of Qs - I have highlighted a couple of rapid climate change
events - the older of which is presently being submitted for
publication.
Would be interested for your thoughts - note also the sea ice phase
within MWP.
best wishes
Alastair
Attachment Converted: "c:\eudora\attach\files.ppt"

3265. 2003-07-11

date: Fri Jul 11 13:33:49 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: Fwd: Re: Fwd: Re: Climate Research
to: Tom Crowley <tcrowley@duke.edu>
Hi Tom,
I'm not sure what format to try if ASCII doesn't work for you. I've attached
the same ones
again, in case it was just some random reason that corrupted the files. If this
doesn't
work, then please suggest a format I should try.
The name I have is Yamal not Yarnal. Yamal is coastwards (northward) of the
"Polar Urals"
and is at a lower elevation than the Polar Urals record. The latitude/longitude
I have for
it is:
67.5 N, 70 E
Hope that helps
Tim
At 21:40 07/07/2003, you wrote:
Hi Tim, thanks for sending the data - unfortunately I cannot open it, can you
send it in
some other format? tom
ps what is the location of the Yarnal site?
Hi Tom
Sorry for not replying sooner - its been a hectic week (or two)!

cg2003

The new Mann and Jones 2000-year series I don't actually have. It appears in Figure 1 of our EOS piece, of course, but Scott Rutherford generated that figure. I generated Figure 2 for EOS and that has the Yamal, Tornetrask, western US and western Greenland 018 stack in it. So I have these data and they are attached in the following files. western US and western Greenland are in file "mann12prox.dat". I didn't have time to extract just these two series from the full file, so the file contains 11 others series too. Please do **not** use the others because I'm not sure whether I am free to distribute them or not - I just haven't time to extract the 2 you want. I'm sure I can trust you not to use anything that I shouldn't have sent! The top of the file lists the 13 series and the start/end years. These are in the same order as the 13 columns of data that then follow (the first column is simply year AD). So you should be able to find "westgrpfisher.dat" and "wustrees.dat". The other files are "tornad.rcs" and "yamal.rcs" which are RCS-standardised tree-ring width series. I would really strongly suggest that you contact Keith Briffa about exactly what these series are and what the primary reference to them should be. The reason is that there are multiple version of Tornetrask and Yamal series and the differences are certainly not insignificant! I'm not sure what the "units" of any of these series are, so I would suggest you normalise them in some way or do your own calibration. Hope that helps
Cheers
Tim

At 16:28 30/06/2003, you wrote:

Tim, would it be possible to obtain the time series listed below, plus the west Greenland composite? (see below).

tom

X-Sieve: CMU Sieve 2.2

X-Sender: f028@pop.uea.ac.uk

Date: Fri, 20 Jun 2003 08:10:57 +0100

To: Tom Crowley <tcrowley@duke.edu>

From: Phil Jones <p.jones@uea.ac.uk>

Subject: Re: Fwd: Re: Climate Research

Cc: t.osborn@uea.ac.uk

X-Virus-Scanned: by amavisd-milter, Duke University ([1]<http://amavis.org/>)

Tom,

I'm off tomorrow to NCDC and then onto IUGG, so away 3 weeks in all. I've asked Tim, who's cc'd on this reply to send you what he can.

You also said sometime ago, you would send your new long series and your latest NH average. Can you do this sometime? Mike and I are making progress on RoG.

When we get back we will be working on the figures. I realise you may want to add something once

Tim sends you the series, so if I (and Mike) can get something by July 10 that would be

cg2003

great.

We will be sending whole or part drafts of the RoG piece around - we have most of the text, but we need the figures for people to look at as well. So you might get a draft in September.

Have a good few weeks.

cheers

Phil

At 12:33 19/06/03 -0400, you wrote:

Phil,

would it be possible to obtain the Yamal, Tornetrask, and w. U.S. series you illustrate in the eos article? I too am putting together a slightly different long composite and would like to include these records.

would it also be possible to obtain the 2000 year northern hemisphere series? is that 30-90N summer? whatever, we have extended our forcing time series back to before 1 AD and would like to compare with some longer data.

thanks and regards, Tom

Dear All,

Keith and I have discussed the email below. I don't want to start a discussion of it and I don't want you sending it around to anyone else, but it serves as a warning as to where the debate might go should the EOS piece come out.

I think it might help Tom (w) if you are still going to write a direct response to CR. Some of de Freitas' views are interesting/novel/off the wall to say the least. I am glad that he doesn't consider himself a paleoclimatologist - the statement about the LIA having the lowest temperatures since the LGM. The paleo people he's talked to didn't seem to mention the YD, 8.2K or the 4.2/3K events - only the Holocene Optimum. There are also some snipes at CRU and our funding, but we're ignoring these here. Also Mike comes in for some stick, so stay cool Mike - you're a married man now ! So let's keep this amongst ourselves . I have learned one thing. This is that the reviewer who said they were too busy was Ray. I have been saying this to loads of papers recently (something Tom(w) can vouch for). It is clear from the differences between CR and the ERE piece that the other 4 reviewers did not say much, so a negative review was likely to be partly ignored, and the article would still have come out. I say this as this might come out if things get nasty. De Freitas will not say to Hans von Storch or to Clare Goodess who the 4 reviewers were. I

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were. I

cg2003

believe his paleoclimatologist is likely to be Anthony Fowler, who does dendro at Auckland.
Cheers
Phil

X-Sender: f037@pop.uea.ac.uk
X-Mailer: QUALCOMM Windows Eudora Version 5.1
Date: Wed, 18 Jun 2003 09:29:22 +0100
To: c.goodess@uea,phil Jones <p.jones@uea.ac.uk>
From: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Fwd: Re: Climate Research
Clare, Phil,

Since Clare and CRU are named in it, you may be interested in Chris de Freitas' reply to the publisher re. my letter to Otto Kinne. I am not responding to this, but await a reply from Kinne himself.

Mike
From: "Chris de Freitas" <c.defreitas@auckland.ac.nz>
To: Inter-Research Science Publisher <ir@int-res.com>
Date: Wed, 18 Jun 2003 13:45:56 +1200
Subject: Re: Climate Research
Reply-to: c.defreitas@auckland.ac.nz
CC: m.hulme@uea.ac.uk
Priority: normal

X-mailer: Pegasus Mail for win32 (v3.12c)

Otto (and copied to Mike Hulme)

I have spent a considerable amount of my time on this matter and had my integrity attacked in the process. I want to emphasize that the people leading this attack are hardly impartial observers. Mike himself refers to "politics" and political incitement involved. Both Hulme and Goodess are from the Climate Research Unit of UEA that is not particularly well known for impartial views on the climate change debate. The CRU has a large stake in climate change research funding as I understand it pays the salaries of most of its staff. I understand too the journalist David Appell was leaked information to fuel a public attack. I do not know the source

Mike Hulme refers to the number of papers I have processed for CR that "have been authored by scientists who are well known for their opposition to the notion that humans are significantly altering global climate." How many can he say he has processed? I suspect the answer is nil. Does this mean he is biased towards scientists "who are well known for their support for the notion that humans are significantly altering global climate?"

Mike Hulme quite clearly has an axe or two to grind, and, it seems, a political agenda. But attacks on me of this sort challenge my professional integrity, not only as a CR editor, but also as an academic and scientist. Mike Hulme should know that I have never accepted any research money for climate change research, none from any "side" or lobby or interest group or government or industry. So I have no pipers to pay.

This matter has gone too far. The critics show a lack of moral imagination. And the Cramer affair is dragged up over and over again. People quickly forget that Cramer (like Hulme and Goodess now) was attacking Larry Kalkstein and me for approving manuscripts, in Hulme's words, "authored by scientists who are well known for their opposition to the notion that humans are significantly altering global climate."

I would like to remind those who continually drag up the Cramer affair that Cramer himself was not unequivocal in his condemnation of Balling et al's manuscript (the one Cramer refereed and now says I should have not had published - and what started all this off). In fact, he did not even recommend that it be rejected. He stated in his

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review: "My review of the manuscript is mainly with the conclusions of the work. For technical assessment, I do not myself have sufficient experience with time series analysis of the kind presented by the authors." He goes on to recommend: "revise and resubmit for additional review". This is exactly what I did; but I did not send it back to him after resubmission for the very reason that he himself confessed to ignorance about the analytical method used. Am I to trundle all this out over and over again because of criticism from a lobbyist scientists who are, paraphrasing Hulme, "well known for their support for the notion that humans are significantly altering global climate".

The criticisms of Soon and Baliunas (2003) CR article raised by Mike Hume in his 16 June 2003 email to you was not raised by any of the four referees I used (but is curiously similar to points raised by David Appell!). Keep in mind that referees used were selected in consultation with a paleoclimatologist. Five referees were selected based on the guidance I received. All are reputable paleoclimatologists, respected for their expertise in reconstruction of past climates. None (none at all) were from what Hans and Clare have referred to as "the other side" or what Hulme refers to as people well known for their opposition to the notion that humans are significantly altering global climate." One of the five referees turned down the request to review explaining he was busy and would not have the time. The remaining four referees sent their detailed comments to me. None suggested the manuscript should be rejected. S&B were asked to respond to referees comments and make extensive alterations accordingly. This was done.

I am no paleoclimatologist, far from it, but have collected opinions from other paleoclimatologists on the S&B paper. I summarise them here. What I take from the S&B paper is an attempt to assess climate data lost from sight in the Mann proxies. For example, the raising on lowering of glacier equilibrium lines was the origin of the Little Ice Age as a concept and still seems to be a highly important proxy, even if a little difficult to precisely quantify.

Using a much larger number of "proxy" indicators than Mann did, S&B inquired whether there was a globally detectable 50-year period of unusual cold in the LIA and a similarly warm era in the MWP. Further, they asked if these indicators, in general, would indicate that any similar period in the 20th century was warmer than any other era. S&B did not purport to do independent interpretation of climate time series, either through 50-year filters or otherwise. They merely adopt the conclusions of the cited authors and make a scorecard. It seems pretty evident to me that temperatures in the LIA were the lowest since the LGM. There are lots of peer-reviewed paleo-articles which assert the existence of LIA.

Frankly, I have difficulty understanding this particular quibble. Some sort of averaging is necessary to establish the 'slower' trends, and that sort of averaging is used by every single study - they average to bring out the item of their interest. A million year average would do little to enlighten, as would detailed daily readings. The period must be chosen to eliminate as much of the 'noise' as possible without degrading the longer-term signals significantly.

As I read the S&B paper, it was a relatively arbitrary choice - and why shouldn't it be? It was only chosen to suppress spurious signals and expose the slower drift that is inherent in nature. Anyone that has seen curves of the last 2 million years must recognize that an averaging of some sort has taken place. It is not often, however, that the quibble is about the choice of numbers of years, or the exact methodology - those are chosen simply to expose 'supposedly' useful data which is otherwise hidden from view.

Let me ask Mike this question. Can he give an example of any dataset where the S&B characterization of the source author is incorrect? (I

am not vouching for them , merely asking.)

S&B say that they rely on the original characterizations, not that they are making their own; I don't see a problem a priori on relying on characterizations of others or, in the present circumstances, of presenting a literature review. While S&B is a literature review, so is this section of IPCC TAR, except that the S&B review is more thorough.

The Mann et al multi-proxy reconstruction of past temperatures has many problems and these have been well documented by S&B and others. My reading of the IPCC TAR leads me to the conclusion that Mann et al has been used as the basis for a number of assertions: 1. Over the past millennium (at least for the NH) the temperature has not varied significantly (except for the European/North Atlantic sector) and hence the climate system has little internal variability. This statement is supported by an analysis of model behaviour, which also shows little internal variability in climate models. 2. Recent global warming, as inferred from instrument records, is large and unusual in the context of the Mann et al temperature reconstruction from multi-proxies. 3. Because of the previous limited variability and the recent warming that cannot be explained by known natural forcing (volcanic activity and solar insolation changes) human activity is the likely cause of the recent global change.

In this context, IPCC mounts a powerful case. But the case rests on two main foundations; the past climate has shown little variability and the climate models reflect the internal variability of the climate system. If either or both are shown to be weak or fallacious then the IPCC case is weakened or fails.

S&B have examined the premise that the globally integrated temperature has hardly varied over the past millennium prior to the instrumental record. I agree it is not rocket science that they have performed. They have looked at the evidence provided by researchers to see if the trend of the temperature record of the European/North Atlantic sector (which is not disputed by IPCC) is reflected in individual records from other parts of the globe (Their three questions). How objective is their assessment? From a purely statistical viewpoint the work can be criticised. But if you took a purely statistical approach you probably would not have sufficient data to reach an unambiguous conclusion, or you could try statistical fiddles to combine the data and end up with erroneous results under the guise of statistical significance. S&B have looked at the data and reached the conclusion that probably the temperature record from other parts of the globe follows the same pattern as that of the European/North Atlantic sector. Of the individual proxy records that I have seen I would agree that this is the case. I certainly have not found significant regions of the NH that were cold during the medieval period and warm during the Little Ice Age period that are necessary offsets of the European/North Atlantic sector necessary to reach a hemispherically flat pattern as derived by Mann et al. S&B have put forward sufficient evidence to challenge the Mann et al analysis outcome and seriously weaken the IPCC assertions based on Mann et al. Paleo reconstruction of temperatures and the global pattern over the past millennium and longer remains a fertile field for research. It suggests that the climate system is such that a major temporal variation as is universally recognised for the European/North Atlantic region would be reflected globally and S&B have given support to this view.

It is my belief that the S&B work is a sincere endeavour to find out whether MWP and LIA were worldwide phenomena. The historical evidence beyond tree ring widths is convincing in my opinion. The concept of "Little Ice Age" is certainly used practically by all Holocene paleo-climatologists, who work on oblivious to Mann's "disproof" of its existence.

Paleoclimatologists tell me that, for debating purposes, they are

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more inclined to draw attention to the Holocene Optimum (about 6000 BP) as an undisputed example of climate about 1-2 deg C warmer than at present, and to ponder the entry and exit from the Younger Dryas as an example of abrupt climate change, than to get too excited about the Medieval Warm Period, which seems a very attenuated version. However, the Little Ice Age seems valid enough as a paleoclimatic concept. North American geologists repeatedly assert that the 19th century was the coldest century in North America since the LGM. To that extent, showing temperature increase since then is not unlike a mutual fund salesman showing expected rate of return from a market bottom - not precisely false, but rather in the realm of sleight-of-hand.

Regards

Chris

Prof. Phil Jones

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Content-Type: application/octet-stream; name="mann12prox.dat"

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Content-Disposition: attachment; filename="tornad.rcs"

Attachment converted: Macintosh HD:tornad.rcs (????/----) (0001B5B7)

Dr Timothy J Osborn

Climatic Research Unit

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4664. 2003-07-11

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Raymond
Bradley <rbradley@geo.umass.edu>
date: Fri, 11 Jul 2003 18:27:37 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: 03-19 Mann et al. - climate change press release issued
to: Harvey Leifert <HLeifert@agu.org>, Phil Jones <p.jones@uea.ac.uk>

Thanks Harvey,
Yes, I've been in touch w/ a representative for the minority. I'm looking
forward to doing
this. Will keep you all posted of what happens,
mike
At 09:45 AM 7/11/2003 -0400, Harvey Leifert wrote:
Mike and Phil,
Perhaps more relevant than which media have already carried the story, copies
of your
Eos paper were distributed at a Senate briefing yesterday, and the minority
(i.e.,
Democratic Party) staff is inviting Mike to appear at a hearing later in the
month. (I
trust Mike got and responded to the message??)
Harvey
Phil Jones wrote:
Mike,
The New Scientist article is here in hard copy form. It isn't on the web
site - no
link to it.
It is fair and shows a diagram of the NH and SH (from the GRL paper !!!).
Titled
'Climatologists hit back at greenhouse sceptics'.
Fred Pearce didn't call Tim and Keith here earlier in the week. The only
call they
got was
from the Sunday Telegraph who may run with something, but they haven't called
back.
No-one called me in Japan, so I guess we will have to wait for the GRL
article. I'm
away
from August 2 for two weeks, so I hope you'll get more then. I will be here
the whole
of the
July 28 week.
The sceptics have seen it though from the two emails from Timo Hameranta.

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Cheers

Phil

At 21:17 10/07/03 -0400, Michael E. Mann wrote:

Dear Harvey,

Do you know if there has been anything in the press on this yet? I've seen the press

release posted on various outlets on the WEB, and there has been lots of interest from

politicos and other scientists on this. However, the only article that I'm aware of on

this is apparently slated for the July 12 issue of "New Scientist". I haven't seen the

article yet (not available online).

Perhaps Tim, Phil, or Ray have other information.

best regards,

mike

At 04:56 PM 7/7/2003 -0400, Harvey Leifert wrote:

All,

900 science We issued the press release at 3:40 p.m. EDT Monday, July 7. It was sent to

web site writers worldwide on our distribution list and posted on EurekaAlert!, the AAAS

for science press releases.

Almost immediately, we received requests for the full article from The New York Times,

Newspapers, USA Today, National Public Radio, Toronto Star, San Jose Mercury News, Cox

Europeans to Richmond Times-Dispatch, and four freelancers. It was too late for most

receive the release Monday, so we expect additional requests Tuesday morning.

Thanks to all for your help.

Harvey

--

Harvey Leifert

Public Information Manager

American Geophysical Union

2000 Florida Avenue, N.W.

Washington, DC 20009, USA

Phone: +1-202-777-7507

Fax: +1-202-328-0566

Email: [1]hleifert@agu.org

web: [2]http://www.agu.org/sci_soc/media.html

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Professor Michael E. Mann

Department of Environmental Sciences, Clark Hall

University of Virginia

Charlottesville, VA 22903

e-mail: [3]mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Public Information Manager

American Geophysical Union

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Charlottesville, VA 22903

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[8]http://www.evsc.virginia.edu/faculty/people/mann.shtml

5047. 2003-07-11

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Raymond
Bradley <rbradley@geo.umass.edu>

date: Fri, 11 Jul 2003 09:45:12 -0400

from: Harvey Leifert <HLeifert@agu.org>

subject: Re: 03-19 Mann et al. - climate change press release issued

to: Phil Jones <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>

Mike and Phil,

Perhaps more relevant than which media have already carried the story, copies of
your Eos

paper were distributed at a Senate briefing yesterday, and the minority (i.e.,
Democratic

Party) staff is inviting Mike to appear at a hearing later in the month. (I
trust Mike got

and responded to the message??)

Harvey

Phil Jones wrote:

Mike,

The New Scientist article is here in hard copy form. It isn't on the web
site - no

link to it.

It is fair and shows a diagram of the NH and SH (from the GRL paper !!!).

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Harvey Leifert

Public Information Manager

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Web: [7]http://www.agu.org/sci_soc/media.html

###

5321. 2003-07-11

cc: Tom Wigley <wigley@ucar.edu>, "Michael E. Mann" <mann@virginia.edu>, Mike Hulme <m.hulme@uea.ac.uk>

date: Fri, 11 Jul 2003 12:40:57 -0700

from: Ben Santer <santer1@llnl.gov>

subject: More on Climate Research.....

to: Phil Jones <p.jones@uea.ac.uk>, rls@email.unc.edu

Dear Phil,

In June 2003, Climate Research published a paper by David Douglass et al. The "et al." includes John Christy and Pat Michaels. Douglass et al. attempt to debunk the paper that Tom and I published in JGR in 2001 ("Accounting for the effects of volcanoes and ENSO in comparisons of modeled and observed temperature trends"; JGR 106, 28033-28059). The Douglass et al. paper claims (and purports to show) that collinearity between ENSO, volcanic, and solar predictor variables is not a serious problem in studies attempting to estimate the effects of these factors on MSU tropospheric temperatures. Their work has serious scientific flaws - it confuses forcing and response, and ignores strong temporal autocorrelation in the individual predictor variables, incorrectly assuming independence of individual monthly means in the MSU 2LT data. In the Douglass et al. view of the world, uncertainties in predictor variables, observations, etc. are non-existent. The error bars on their estimated ENSO, volcano, and solar regression coefficients are miniscule.

Over a year ago, Tom and I reviewed (for JGR) a paper by Douglass et al. that was virtually identical to the version that has now appeared in Climate Research. We rejected it. Prior to this, both Tom and I had engaged in a long and frustrating dialogue with Douglass, in which we attempted to explain to him that there are large uncertainties in the deconvolution of ENSO, volcano, and solar signals in short MSU records. Douglass chose to ignore all of the comments we made in this exchange, as he later ignored all of the comments we made in our reviews of his rejected JGR paper.

Although the Douglass et al. Climate Research paper is largely a criticism of our previously-published JGR paper, neither Tom nor I were asked to review the paper for Climate Research. Nor were any other coauthors of the Santer et al. JGR paper asked to review the Douglass et al. manuscript. I'm assuming that Douglass specifically requested that neither Tom nor I should be allowed to act as reviewers of his Climate Research paper. It would be interesting to see his cover letter to the journal.

In the editorial that you forwarded, Dr. Kinne writes the following:

"If someone wishes to criticise a published paper s/he must present facts and arguments and give criticised parties a chance to defend their position." The irony here is that in our own experience, the "criticised parties" (i.e., Tom and I) were NOT allowed to defend their positions.

Based on Kinne's editorial, I see little hope for more enlightened editorial decision making at Climate Research. Tom, Richard Smith and I will eventually publish a rebuttal to the Douglass et al. paper. We'll publish this rebuttal in JGR - not in Climate Research.

With best regards,

Ben

=====

Phil Jones wrote:

```
>
> Dear All,
> Finally back in the UK after Asheville and IUGG. Attached is an
> editorial from the
> latest issue of climate research. I can only seem to save it this way.
> Seems like we are
> now the bad guys.
>
> Cheers
> Phil
>
```

cg2003

> At 07:51 04/07/03 -0600, Tom Wigley wrote:
> >Mike (Mann),
> >I agree that Kinne seems like he could be a deFreitas clone. However, what
> >would be our legal position if we were to openly and extensively tell
> >people to avoid the journal?
> >Tom.
> >_____
> >
> >Michael E. Mann wrote:
> >>Thanks Mike
> >>It seems to me that this "Kinne" character's words are disingenuous, and
> >>he probably supports what De Freitas is trying to do. It seems clear we
> >>have to go above him.
> >>I think that the community should, as Mike H has previously suggested in
> >>this eventuality, terminate its involvement with this journal at all
> >>levels--reviewing, editing, and submitting, and leave it to wither way
> >>into oblivion and disrepute,
> >>Thanks,
> >>mike
> >>At 01:00 PM 7/3/2003 +0100, Mike Hulme wrote:
> >>
> >>>Phil, Tom, Mike,
> >>>
> >>>So, this would seem to be the end of the matter as far as Climate
> >>>Research is concerned.
> >>>
> >>>Mike
> >>>
> >>>>To
> >>>>CLIMATE RESEARCH
> >>>>Editors and Review Editors
> >>>>
> >>>>Dear colleagues,
> >>>>
> >>>>In my 20.06. email to you I stated, among other things, that I would
> >>>>ask CR editor Chris de Freitas to present to me copies of the
> >>>>reviewers' evaluations for the 2 Soon et al. papers.
> >>>>
> >>>>I have received and studied the material requested.
> >>>>
> >>>>Conclusions:
> >>>>
> >>>>1) The reviewers consulted (4 for each ms) by the editor presented
> >>>>detailed, critical and helpful evaluations
> >>>>
> >>>>2) The editor properly analyzed the evaluations and requested
> >>>>appropriate revisions.
> >>>>
> >>>>3) The authors revised their manuscripts accordingly.
> >>>>
> >>>>Summary:
> >>>>
> >>>>Chris de Freitas has done a good and correct job as editor.
> >>>>
> >>>>Best wishes,
> >>>>Otto Kinne
> >>>>Director, Inter-Research
> >>>>--
> >>>>-----
> >>>>Inter-Research, Science Publisher
> >>>>Ecology Institute
> >>>>Nordbunte 23,
> >>>>D-21385 Oldendorf/Luhe,

cg2003

> >>>>Germany
> >>>>Tel: (+49) (4132) 7127 Email: ir@int-res.com
> >>>>Fax: (+49) (4132) 8883 http://www.int-res.com <http://www.int-res.com/>
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> >>>>

> >>>>-----
> >>>>

> >>-----
> >> Professor Michael E. Mann
> >> Department of Environmental Sciences, Clark Hall
> >> University of Virginia
> >> Charlottesville, VA 22903

> >>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
> >> http://www.evsc.virginia.edu/faculty/people/mann.shtml
> >

> > Prof. Phil Jones
> Climatic Research Unit Telephone +44 (0) 1603 592090
> School of Environmental Sciences Fax +44 (0) 1603 507784
> University of East Anglia
> Norwich Email p.jones@uea.ac.uk
> NR4 7TJ
> UK

> >-----
> > Name: CR.txt
> CR.txt Type: Plain Text (text/plain)
> Encoding: quoted-printable
--

> >-----
PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!
Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-7638
FAX: (925) 422-7675
email: santer1@llnl.gov
> >-----

1736. 2003-07-12

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Raymond
Bradley <rbradley@geo.umass.edu>
date: Sat, 12 Jul 2003 07:35:29 -0400

cg2003

from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: 03-19 Mann et al. - climate change press release issued
to: Harvey Leifert <HLeifert@agu.org>, Phil Jones <p.jones@uea.ac.uk>

Dear all,

This translation from my colleague Gavin Schmidt,
mike

Global warming: Climatologists respond to the American right.
The scientific consensus against the ideologues: The heat is on!
Thirty eminent (anglo-saxon) scientists specialising in climate
change and the greenhouse effect have just re-affirmed in an
article in EOS that the anthropogenic contribution to global
warming is stronger than a simple hypothesis. The article is a
response to a series of publications that the Bush administration
are relying on to reject the terms of the Kyoto Protocol.
Among the authors in the EOS article, we find Michael Mann,
geophysicist from UVA. His research has evaluated the impact of
human activity on planetary climate. He has notably put forward
evidence of highly abnormal temperature rises in the 20th
Century. His studies, published in some of the most prestigious
journals, have contributed to what the National Academy of Science
and AGU recognised in 1998, as anthropogenic global warming.
Don't touch my car!

Despite the growing number of studies that indicate that the
current levels of GHG emissions are destabilising the global
climate, the American government still refuses to ratify the Kyoto
Protocol. Apart from ideological arguments concerning the 'sacred
character of the American way of life', the Bush administration
argues against the scientific case presented in the IPCC reports.
The administration particularly relies on the work of two
astrophysicists, Willie Soon and Sallie Baliunas. These two have
denied the reality of the warming during the 20th Century in two
different articles. These studies were little noticed in the
scientific world, but were nevertheless largely financed and
publicised by the "think tanks" on the American right. The intense
lobbying that led to the rejection of Kyoto was accompanied by the
creation of a series of scientific foundations linked to
ultraconservative groups which currently influence American
politics (such as the Heritage Foundation or the Moonies).
The objective of these research centres is to produce
counter-arguments and muddy the waters. If they admit to a global
warming, the "experts" of the George Marshall Coalition or
Institute attribute that to solar forcing. These same "experts" do
not hesitate at the same time to declare that there has not been
any warming, and that the data from IPCC has been falsified.

Climatologist versus Astrophysicists

For other "specialists", coming from the so-called Center for the Study
of Carbon Dioxide & Global Change, the greenhouse effect will even
be ecologically beneficial because it favours increased photosynthesis!
Denouncing the lack of seriousness and the scientific independence
of the work of Baliunas and Soon, Mann and his colleagues therefore
decided to refute their arguments one by one in EOS. In producing
a detailed review of the data and the models, they reject the
astrophysicists conclusions and reaffirm the existence of an
anthropogenic component to global warming.
They underline, amongst other points, that the publication of their
response in a peer-reviewed journal demonstrates that there does
exist a strong consensus of scientists with regard to [the causes
of] global warming.

At 09:42 AM 7/11/2003 -0400, Harvey Leifert wrote:

Phil,

You have also made it onto the junk science website. They reprinted the
release, with

cg2003

the introductory note (approximately), if you believe this they will tell you another!

On the other side, a French news site has proclaimed Mike et al. leaders of the anti-Bush-because-of -kyoto brigade. It's only in French, but if you can read it, it's

at [1]<http://www.transfert.net/a9101>

Regards,

Harvey

Phil Jones wrote:

Mike,

The New Scientist article is here in hard copy form. It isn't on the web site - no

link to it.

It is fair and shows a diagram of the NH and SH (from the GRL paper !!!). Titled

'Climatologists hit back at greenhouse sceptics'.

Fred Pearce didn't call Tim and Keith here earlier in the week. The only call they

got was

from the Sunday Telegraph who may run with something, but they haven't called back.

No-one called me in Japan, so I guess we will have to wait for the GRL article. I'm

away

from August 2 for two weeks, so I hope you'll get more then. I will be here the whole

of the

July 28 week.

The sceptics have seen it though from the two emails from Timo Hameranta.

Cheers

Phil

At 21:17 10/07/03 -0400, Michael E. Mann wrote:

Dear Harvey,

Do you know if there has been anything in the press on this yet? I've seen the press

release posted on various outlets on the WEB, and there has been lots of interest from

politicos and other scientists on this. However, the only article that I'm aware of on

this is apparently slated for the July 12 issue of "New Scientist". I haven't seen the

article yet (not available online).

Perhaps Tim, Phil, or Ray have other information.

best regards,

mike

At 04:56 PM 7/7/2003 -0400, Harvey Leifert wrote:

All,

We issued the press release at 3:40 p.m. EDT Monday, July 7. It was sent to 900 science

writers worldwide on our distribution list and posted on EurekaAlert!, the AAAS web site

for science press releases.

Almost immediately, we received requests for the full article from The New York Times,

USA Today, National Public Radio, Toronto Star, San Jose Mercury News, Cox Newspapers,

Richmond Times-Dispatch, and four freelancers. It was too late for most Europeans to

receive the release Monday, so we expect additional requests Tuesday morning.

Thanks to all for your help.

Harvey

--

cg2003

Harvey Leifert
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Fax: +1-202-328-0566
Email: [2]hleifert@agu.org
Web: [3]http://www.agu.org/sci_soc/media.html
###

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[5]http://www.evsc.virginia.edu/faculty/people/mann.shtml

Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
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[9]http://www.evsc.virginia.edu/faculty/people/mann.shtml

4749. 2003-07-12

cc: mann@virginia.edu
date: Sat, 12 Jul 2003 00:07:37 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: query regarding Soon et. al. rebuttal
to: "Regalado, Antonio" <Antonio.Regalado@wsj.com>

Dear Antonia,
Thanks for your message. Happy to hear your thinking of doing this article.
Just got back from Japan, so a bit jet lagged, but wanted to at least get an
initial
response to you. Please feel free to contact me over the weekend, by email or
otherwise, if
I can be of further help.
Some specific comments below.
best regards,
Mike M

cg2003

At 04:17 PM 7/11/2003 -0400, you wrote:

Prof. Mann,

Hi from the Wall Street Journal. I am thinking of citing your rebuttal to Soon et. al. in a news item I am writing for the newspaper.

You say that it is "only the past few decades during which n. hemisphere temperatures have exceeded the bounds of natural variability...". Do you mean the last few decades are the warmest of the last millennium only, not all of time, right?

yes, in fact, though, we can now say with a reasonable degree of confidence that Northern Hemisphere mean temperatures were higher during the past two decades than any other

interval during at least roughly the past *two* millennia (the extension to the past two millennia is afforded by a paper in press in the journal 'Geophysical Research Letters' by

Phil Jones and myself--a result of that paper was shown in our 'Eos' piece, but we'll issue a more specific press release on that result when the paper is slated to appear in a few

weeks). It's unclear how Northern Hemisphere average temperature (let alone global average temperature) varied during prior millennia (see below).

To what degree are n. hemisphere temperatures anomalous when compared to the entire paleoclimate record?

It's *possible* the conclusion holds for the last 6000 years, or even longer, but that's

speculative. When we go back beyond the past one or two millennia, the issue gets very

tricky--we no longer have annually-detailed proxy records which we can compare directly

against modern thermometer records. It is possible to do so with very long annually-resolved ice cores, tree-rings, corals, and historical records, which give us a

picture of changes over the past one-to-two millennia, but not with the sorts of evidence

(pollen, ocean sediments, coarsely-resolved ice cores, glacial advances and retreats) that

are available to provide longer-term insights. There is a good discussion of these issues

in the 2001 IPCC report, if you would like some additional detailed information:

[1]http://www.grida.no/climate/ipcc_tar/wg1/068.htm

That having been said, the 'mid-Holocene' interval (about 6000 years ago) when the

astronomical factors influencing the climate favoured greater insolation in the Northern Hemisphere summer, might have been warmer than the late 20th century. The

available evidence, though limited, suggests this--and a number of older model simulations suggested

that might be the case. Some recent work, using the best available current climate models,

suggests, however, that the temperatures were perhaps comparable to today back then. There

was another period prior to the last Ice Age (more than 120,000 years ago) called the

'Eemian' for which there is tentative evidence that global mean temperatures might have

been even higher than during the mid-Holocene. But 'tentative' is the key phrase--the

evidence is often restricted in where it's available, and whether it's telling us about

annual conditions (what we would like to know) or only, say, summer growing

cg2003

season

conditions.

It is almost certain that global mean temperatures were warmer during certain past

geological periods (e.g., the Cretaceous, when we suspect that CO₂ levels were higher than

today, and that the globe, w/ Dinosaurs wandering around near the poles, was almost

certainly warmer). These changes occurred over many millions of years, due to the influence

of plate motion on the production of CO₂ by geological sources (e.g. volcanic outgassing).

Of course, that warming occurred over many millions of years. The present warming is

occurring on a century time scale, so it is the *rate* of recent warming that may be

particularly anomalous in the long-term history of the climate.

Also, when was the last time CO₂ levels were as high as they are now, do you know?

There is still some debate about this. We now have excellent CO₂ records from ice cores

dating back to more than 400,000 years. The present CO₂ concentration appears higher than

at any time during that record. The longer-term evidence is more tenuous (based on trace

gases trapped in ambers, evidence from fossil leaf stomata, etc.), but it is quite likely

that CO₂ levels were higher as one gets back towards the Cretaceous period (e.g. more than

50 million years ago), precisely how much higher is still a subject of dispute.

The present

thinking is that current CO₂ levels are probably the highest in about 20 million years. See

again e.g. the IPCC 2001 report for details:

[2]http://www.grida.no/climate/ipcc_tar/wg1/107.htm#331

Thanks

happy to be of help

Antonio Regalado

Staff Reporter, The Wall Street Journal

212-416-3011 (Tel.)

917-686-3389 (Cell)

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2630. 2003-07-14

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Raymond Bradley <rbradley@geo.umass.edu>

date: Mon, 14 Jul 2003 08:24:02 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: 03-19 Mann et al. - climate change press release issued

to: Harvey Leifert <HLeifert@agu.org>, Phil Jones <p.jones@uea.ac.uk>

Dear All,

I guess this is actually "The Observer". A bit confusing,

mike

At 08:16 AM 7/14/2003 -0400, Michael E. Mann wrote:

Dear All,

cg2003

See the latest "Guardian Unlimited" in the UK:

[1]<http://www.guardian.co.uk/climatechange/story/0,12374,997248,00.html>

mike

At 09:45 AM 7/11/2003 -0400, Harvey Leifert wrote:

Mike and Phil,

Perhaps more relevant than which media have already carried the story, copies of your

Eos paper were distributed at a Senate briefing yesterday, and the minority (i.e.,

Democratic Party) staff is inviting Mike to appear at a hearing later in the month. (I

trust Mike got and responded to the message??)

Harvey

Phil Jones wrote:

Mike,

The New Scientist article is here in hard copy form. It isn't on the web site - no

link to it.

It is fair and shows a diagram of the NH and SH (from the GRL paper !!!). Titled

'Climatologists hit back at greenhouse sceptics'.

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best regards,

mike

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Richmond Times-Dispatch, and four freelancers. It was too late for most Europeans to

cg2003

receive the release Monday, so we expect additional requests Tuesday morning.
Thanks to all for your help.

Harvey

--

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[5]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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[10]http://www.evsc.virginia.edu/faculty/people/mann.shtml

772. 2003-07-15

date: Tue, 15 Jul 2003 12:05:15 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Fwd:

cg2003

to: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk

<x-flowed>

Keith, Tim

The series might be useful to you. Also see the para about Soon and AGU. Apparently

Soon is only in it for the science !!!

Cheers

Phil

>Date: Mon, 14 Jul 2003 16:15:14 -0400

>From: Ellen Mosley-Thompson <thompson.4@osu.edu>

>Subject:

>X-Sender: ethompso@pop.service.ohio-state.edu

>To: rbradley@geo.umass.edu, p.jones@uea.ac.uk,

>mann@multiproxy.evsc.virginia.edu

>X-Mailer: QUALCOMM Windows Eudora Version 5.2.1

>

>Ray, Phil and Mike,

>

>Lonnie has gone off to Peru for the next 7 weeks. Just before leaving he asked me to pull together the decadal averages (d018) for the six cores (3 from Tibet and 3 from So. America) that we used to create the composites shown in Figure 7 of our paper in the Highest volume (Climatic Change, 2003) and to send them to you.

>I have attached those data in an Excel file; please note that the recent decade is often incomplete and for Dundee and Guliya there are no data for the most recent decade. Also note that the time scale on Dundee is approximate and based on a model that assumes steady state conditions (i.e., constant accumulation). It is guided by a few marker horizons but should be viewed as an approximate time scale.

>

>If you have any questions I would be happy to try to answer them, but I think everything is pretty straightforward.

>

>I guess you guys are pretty tied up with all the fuss that the Forum piece kicked up.

>I thought I would gag when I heard that Soon contacted AGU to question the nature of the review (to which your piece was subjected) and claimed that this was a clear effort to politicize the issue (he would never do that). Of course he stressed that he and his colleagues were only interested in the science and not the political aspects of the issue). I heard that you had at least one inquiry from a congressional office (the minority party no doubt). I guess your piece will stir things up a little!

>

>Have a nice summer,

>Ellen

>

>

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

</x-flowed>

Attachment Converted:

"c:\eudora\attach\6-core-composite-for-Bradley-Jones-Mann.XLS"

774. 2003-07-15

date: Tue, 15 Jul 2003 13:28:03 -0400

cg2003

from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: Fwd: revised NH comparison manuscript
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Yeah, "moot" is the proper term. I will try to be mute on the issue for a while now.

I just got out of jury duty. I almost got on a jury for a murder trial with possible sequestering. My scheduled trip with Nic for his Uni orientation the end of this month saved the day.

Cheers,
Ed

>take your point re that's enough - but I have to point out your
>Freudian slip re "moot point" or as you would have it when associated
>with Mike Mann - hopefully "Mute point" !
>love to Michelle
>Keith
>
>

>At 09:32 AM 7/15/03 -0400, you wrote:

>>Hi Keith,

>>

>>Thanks for the paper and help in toning down Mike's efforts to put
>>a stake in the Esper heart. I quickly read the paragraph you
>>mention. Undoubtedly part of what is said is true, but it doesn't
>>explain it all of the differences between the original MBH
>>reconstruction and any of the other NH recons. Now that Mike has
>>moved on to a totally new NH recon, I suppose all of this is a mute
>>point. However, your Blowing Hot and Cold piece clearly showed that
>>the MBH estimates were undoubtedly deficient in low-frequency
>>variability compared to ANY other recon. Enough said. I need to
>>enjoy myself.

>>

>>Cheers,

>>

>>Ed

>>

>>>Ed

>>>Thought you should see this (in confidence) . Have succeeded in
>>>getting reasonable citation to your work and much toning down of
>>>criticism of Esper et al in first draft (see last paragraph
>>>before Section C) . Cheers
>>>Keith

>>>

>>>P.S. Do not ask me why Ray, Malcolm and Phil are on this cause I
>>>don't know - work cam out of stuff Tim did with Scott when
>>>visiting there last year.

>>>

>>>>Date: Tue, 3 Jun 2003 14:51:09 -0400

>>>>Subject: revised NH comparison manuscript

>>>>Cc: Mike Mann <mann@virginia.edu>

>>>>To: Malcolm Hughes <mhughes@ltrr.arizona.edu>,

>>>> Raymond Bradley <rbradley@geo.umass.edu>, Tim Osborn

>>>><t.osborn@uea.ac.uk>,

>>>> Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

>>>>From: Scott Rutherford <srutherford@gso.uri.edu>

>>>>X-Mailer: Apple Mail (2.552)

>>>>

>>>>

>>>>

>>>>Attached to this e-mail is a revision of the northern hemisphere
>>>>comparison manuscript. First some general comments. I tried as
>>>>best as possible to incorporate everyone's suggestions. Typically
>>>>this meant adding/deleting or clarifying text. There were cases

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>>>>where we disagreed with the suggested changes and tried to
>>>>clarify in the text why.
>>>>
>>>>In this next round of changes I encourage everyone to make
>>>>specific suggestions in terms of wording and references (e.g.
>>>>Rutherford et al. GRL 1967 instead of "see my GRL paper"). I
>>>>also encourage everyone to make suggestions directly in the file
>>>>in coloured text or by using Microsquish Word's "Track Changes"
>>>>function (this will save me deciphering cryptic penmanship;
>>>>although I confess, my writing is worse than anyone's). If you
>>>>would prefer to use the editing functions in Adobe Acrobat let me
>>>>know and I will send a PDF file. If you still feel strongly that
>>>>I have not adequately addressed an issue please say so.
>>>>I will incorporate the suggestions from this upcoming round into
>>>>a manuscript to be submitted. After review, everyone will get a
>>>>crack at it again.
>>>>
>>>>I will not detail every change made (if anyone wants the file
>>>>with the changes tracked I can send it). Here are the major
>>>>changes:
>>>>
>>>>1) removal of mixed-hybrid approach and revised discussions/figures
>>>>2) removal of CE scores from the verification tables
>>>>3) downscaling of the Esper comparison to a single figure panel
>>>>and one paragraph.
>>>>4) revised discussion of spatial maps and revised figure (figure 8).
>>>>5) seasonal comparisons have been revised
>>>>
>>>>Several suggestions have been made for where to submit. These are
>>>>listed on page 1 of the manuscript. Please indicate your
>>>>preference ASAP and I will tally the votes.
>>>>
>>>>I would like to submit by late July, so if you could please get
>>>>me comments by say July 15 that would be great. I will send out a
>>>>reminder in early July. If I don't hear from you by July 15 I
>>>>will assume that you are comfortable with the manuscript.
>>>>
>>>>Please let me know if you have difficulty with the file or would
>>>>prefer a different format.
>>>>
>>>>Regards,
>>>>
>>>>Scott
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>_____
>>>>Scott Rutherford
>>>>
>>>>Marine Research Scientist
>>>>Graduate School of Oceanography
>>>>University of Rhode Island
>>>>e-mail: srutherford@gso.uri.edu
>>>>phone: (401) 874-6599
>>>>fax: (401) 874-6811
>>>>snail mail:
>>>>South Ferry Road
>>>>Narragansett, RI 02882
>>>>
>>>>--
>>>>Professor Keith Briffa,

cg2003

>>>Climatic Research Unit
>>>University of East Anglia
>>>Norwich, NR4 7TJ, U.K.
>>>
>>>Phone: +44-1603-593909
>>>Fax: +44-1603-507784
>>>
>>><http://www.cru.uea.ac.uk/cru/people/briffa/>
>>>
>>>Attachment converted: Macintosh HD:nhcomparison_v7_1.doc
>>>(WDBN/MSWD) (0008AC53)
>>
>>
>>--

>>=====

>>Dr. Edward R. Cook
>>Doherty Senior Scholar and
>>Director, Tree-Ring Laboratory
>>Lamont-Doherty Earth Observatory
>>Palisades, New York 10964 USA
>>Email: drdendro@ldeo.columbia.edu
>>Phone: 845-365-8618
>>Fax: 845-365-8152
>>=====

>
>--
>Professor Keith Briffa,
>Climatic Research Unit
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>Norwich, NR4 7TJ, U.K.
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Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152

</x-flowed>

1155. 2003-07-15

cc: "Profeta, Tim (Lieberman)" <Tim_Profeta@lieberman.senate.gov>, "Loschnigg, Johannes (Lieberman)" <Johannes_Loschnigg@lieberman.senate.gov>

date: Tue, 15 Jul 2003 09:25:42 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: RE: Senate hearing

to: "Miller, Chris (EPW)" <Chris_Miller@epw.senate.gov>

This op-ed today in the "New Zealand Herald" from the 'editor' who published the
Soon at al

"Climate Research" paper:

[1]<http://www.nzherald.co.nz/storydisplay.cfm?storyID=3512583&thesection=news&thesubsection=dialogue>

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Such comments would seem to disqualify such an individual from being an editor of a scientific journal. Coupled with the attached 'Scientific American' piece, this seems to make a compelling case that something is rotten in the journal "Climate Research"....

perhaps worth keeping copies of these,

mike

At 04:04 PM 7/14/2003 -0400, Miller, Chris (EPW) wrote:

Thanks. This is very helpful. FYI - David Legates is likely to be a majority witness, as well as Soon.

-----Original Message-----

From: Michael E. Mann [[2]mailto:mann@virginia.edu]

Sent: Saturday, July 12, 2003 11:50 AM

To: Miller, Chris (EPW)

Cc: Profeta, Tim (Lieberman); Loschnigg, Johannes (Lieberman)

Subject: Re: Senate hearing

Dear Chris, Tim, Johannes:

I felt that it might be useful for you all to have the information I've tabulated below, in case a discussion of the relative scientific credentials of various

scientists emerges during the senate hearing later this month.

The 'Science Citation Index', along with the number of peer-reviewed publications

has long been used as measure of the reputation, impact, and credibility of a

scientists work (it is one of the key diagnostics used to determine tenure or

advancement at academic and scientific research institutions). It allows the

evaluation of not just the issue of how many publications an author has contributed

to the peer-reviewed scientific literature, but whether that scientists' work is

being read and acknowledged by his/her peers--i.e., is the work considered

important by the rest of the scientific community.

I've provided a relative comparison of myself, W. Soon, and S. Baliunas (these

change on a weekly basis, mine are the latest numbers through July 11, 2003 from the

ISI International database. One caveat to note: not all peer-reviewed publications

appear in the ISI--they need, for example, to have been cited at least once, and

some peer-reviewed journals are not entered into ISI, so the numbers give a good

overall picture, but the details would very depend on precisely how you did the

counting.

Provided are

(1) # of peer-reviewed publications (journal articles and other

reviewed manuscripts, book chapters, etc). Note that for Soon and Baliunas, almost

all of their reviewed papers have appeared in the 'astronomical literature', and

not the 'climate' literature. Note also that I haven't included manuscripts that

are 'in press'. This would add about 4 to my publication total, and I suspect no

more than 1

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to either Soon or Baliunas. My up-to-date CV can be accessed here:

[3]<http://www.people.virginia.edu/~mem6u/cv.htm>

or here:

[4]<http://www.people.virginia.edu/~mem6u/cv.pdf>

(2) the # total number of recorded citations (as provided by ISI International) of

their work by other scientists, and (3) the number of publications for which the

number of citations exceeded the various totals indicated.

I think the results would be eye-opening, if the issue of scientific credibility,

reputation, and respect by peers is raised in the course of the hearing (the

minority might indeed want to broach the topic itself),

mike

#>100 cited # of publications # of Citations

>80 >50 >20

Michael E. Mann 54 1217

3 5 12

Sally Baliunas 11 180

0 1 1

Willie Soon 3 7 142

0 0 0

Professor Michael E. Mann

Department of Environmental Sciences, Clark Hall

University of Virginia

Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Professor Michael E. Mann

Department of Environmental Sciences, Clark Hall

University of Virginia

Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\SciAmJune03-Appell-sidebar.pdf"

1352. 2003-07-15

date: Tue Jul 15 14:08:30 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: Fwd: revised NH comparison manuscript

to: Edward Cook <drdendro@ldeo.columbia.edu>

Ed

Thought you should see this (in confidence) . Have succeeded in getting reasonable citation

to your work and much toning down of criticism of Esper et al in first draft (see last

paragraph before Section C) . Cheers

Keith

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of stuff Tim did with Scott when visiting there last year.

Date: Tue, 3 Jun 2003 14:51:09 -0400

Subject: revised NH comparison manuscript

Cc: Mike Mann <mann@virginia.edu>

To: Malcolm Hughes <mhughes@ltrr.arizona.edu> ,

Raymond Bradley <rbradley@geo.umass.edu> , Tim Osborn <t.osborn@uea.ac.uk> ,

Keith Briffa <k.briffa@uea.ac.uk> , Phil Jones <p.jones@uea.ac.uk>

cg2003

From: Scott Rutherford <srutherford@gso.uri.edu>

X-Mailer: Apple Mail (2.552)

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you by July 15 I will assume that you are comfortable with the manuscript.

Please let me know if you have difficulty with the file or would prefer a different

format.

Regards,

Scott

Scott Rutherford

Marine Research Scientist

Graduate School of Oceanography

University of Rhode Island

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snail mail:

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Narragansett, RI 02882

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[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

3622. 2003-07-15

date: Tue, 15 Jul 2003 09:11:30 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: Fwd: Climatologists have used outdated time series analysis
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Hi Keith,
Sounds good. By the way, in what sense is the Esper domain too small.
I freely admit that it is best suited for the extra-tropics
(ca. >30N) and tried to make that painfully obvious in the Esper et
al. paper. So a critique of it based on domain size is self-serving,
as if it was not pointed out in the Esper et al. paper. There is no
way that you can say that the Mann et al. paleo-data domain is too proper
for a NH reconstruction either, particularly back before ca. 1600,
since it is mostly based on >30N data as well. I would expect that
issue to be mentioned as well in the Rutherford paper, but I am sure
it was not for reasons that we both understand. I am afraid that the
playing field is not fair in general.
C'est le vie, re Italy.

Cheers,

Ed

>Will get a copy of this and send. Of course I agree 100 per cent re
>"optimal methods" (yuk) versus traditional (dare I say , even local
>point) regressions - in fact I am actually an author on a Scott
>Rutherford (and others including Mann) paper that shows just this.
>Incidentally , this also concludes that differences between Mann and
>Esper NH curves are mostly a matter of spatial domain difference
>(with yours too small of course) .
>Be careful not to oil yourself too much because the smell of
>grilling fat will annoy the neighbours. Seems like the timing of
>your Italy jaunt does not suit us by the way so I think you are safe
>as regards a visit.

>Best wishes

>Keith

>At 06:20 AM 7/15/03 -0400, you wrote:

>>Hi Keith,

>>

>>Outdated as of June 28, 2003? Guilty as charged I guess. I'm not
>>familiar with this paper nor the authors. Of course I am skeptical.
>>In comparing my old fashioned least squares methods with advanced
>>'optimal' methods like RegEM (that Mike is enamored with) and
>>hierarchical Bayes, there is fuck-all difference in the results.
>>Connie Woodhouse's results with neural networks doesn't show much
>>either over linear regression. If you are able to get a pdf, please
>>email it to me. I am not in position to get it now. Am at the beach.

>>

>>Cheers,

>>

>>Ed

>>

>>>>Date: Mon, 14 Jul 2003 17:01:56 +0100 (BST)

>>>>From: Timo Hameranta <timohame@yahoo.co.uk>

>>>>Subject: Climatologists have used outdated time series analysis

>>>>methods (!?)

cg2003

>>>>To: climatesceptics@yahoogroups.com, jto@u.arizona.edu, k.briffa@uea.ac.uk,
>>>> legates@udel.edu, mann@virginia.edu, omichael@princeton.edu,
>>>> p.jones@uea.ac.uk, rbradley@geo.umass.edu, t.osborn@uea.ac.uk,
>>>> wigley@meeker.ucar.edu, wsoon@cfa.harvard.edu

>>>>

>>>>Dear all, see the study

>>>>

>>>>Godtliebse, F., L. R. Olsen, and J.-G. Winther, 2003.

>>>>Recent developments in statistical time series

>>>>analysis: Examples of use in climate research,

>>>>Geophys. Res. Lett., 30(12), 1654,

>>>>doi:10.1029/2003GL017229, June 28, 2003.

>>>>

>>>>Abstract

>>>>In this paper we present some recently developed time

>>>>series analysis methods. Further, we apply these

>>>>methods to a suite of climatological and synthetic

>>>>time series. We show what information (or statistical

>>>>significance) that can be drawn from such time series

>>>>and which otherwise, i.e. by simpler methods, would be

>>>>difficult to extract. We conclude by recommending the

>>>>use of advanced statistical time series analysis for a

>>>>wide range of applications connected to studies of

>>>>climate variability and climate change.

>>>>.....

>>>>

>>>>Well

>>>>

>>>>Timo Hämeranta

>>>>Moderator, Climatesceptics

>>>>

>>>>

>>>>

>>>>Yahoo! Plus - For a better Internet experience

>>>><http://uk.promotions.yahoo.com/yplus/yoffer.html>

>>>>

>>>>--

>>>>Professor Keith Briffa,

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>>>>University of East Anglia

>>>>Norwich, NR4 7TJ, U.K.

>>>>

>>>>Phone: +44-1603-593909

>>>>Fax: +44-1603-507784

>>>>

>>>><http://www.cru.uea.ac.uk/cru/people/briffa/>

>>>>

>>>>

>>>>--

>>=====

>>Dr. Edward R. Cook

>>Doherty Senior Scholar and

>>Director, Tree-Ring Laboratory

>>Lamont-Doherty Earth Observatory

>>Palisades, New York 10964 USA

>>Email: drdendro@ldeo.columbia.edu

>>Phone: 845-365-8618

>>Fax: 845-365-8152

>>=====

>>

>>--

>>Professor Keith Briffa,

>>Climatic Research Unit

cg2003

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>
>Phone: +44-1603-593909
>Fax: +44-1603-507784
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=====
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Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152
=====

</x-flowed>

1424. 2003-07-16

cc: p.williamson@uea.ac.uk
date: wed, 16 Jul 2003 15:06:49 +0100
from: John Shepherd <j.g.shepherd@soc.soton.ac.uk>
subject: Re: Fwd: Ocean carbon uptake
to: Mike Hulme <m.hulme@uea.ac.uk>, h.j.Schellnhuber@uea.ac.uk, r.warren@uea.ac.uk,
j.g.shepherd@soton.ac.uk

Dear all

I think it would be a Good Thing if Tyndall people (incl even me) were involved in

this venture, in some way. This "non-warming" effects of CO2 (e.g. acidification, and so suppression of calcification (e.g. of corals)) is shaping up to be a major concern. Could be a very hot political potato indeed....

John

At 14:17 16/07/2003 +0100, Mike Hulme wrote:

John, Rachel and John,

I guess this is most relevant for the three of you re. Tyndall CIAM. This seems an issue more for QUEST rather than core Tyndall territory, but I pass suggestion from Phil Williamson onto you anyway.

Mike

From: "Phil Williamson" <P.Williamson@uea.ac.uk>

To: "Mike Hulme" <m.hulme@uea.ac.uk>

Cc: <mva@soc.soton.ac.uk>, "Tim Jickells" <T.Jickells@uea.ac.uk>, "Philip Newton" <ppn@nerc.ac.uk>, <phb@socnet.soc.soton.ac.uk>

Subject: Ocean carbon uptake

Date: Thu, 10 Jul 2003 15:22:12 +0100

X-Mailer: Microsoft Outlook Express 5.50.4133.2400

Mike -

Martin Angel and Tim Jickells have both mentioned to me the discussion of ocean carbon

uptake at the Tyndall Conference earlier this week. One way of involving the Tyndall

Centre in future work in this area might be via attendance at a "town meeting" planned

for later this year on future marine research programmes - with emphasis on interactions

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between biogeochemistry, ocean ecosystems and wider Earth System processes.
This meeting is being organised by UK SCOR (Chair, Peter Burkill) and NERC
(Phil Newton and myself). Likely to be November, but I don't yet have the date. A label
for it may be "Defining the UK contribution to IMBER" where IMBER = Integrated Marine
Biogeochemistry & Ecosystem Research. That's an IGBP/SCOR programme,
temporarily known as OCEANS, that is being developed as a successor to JGOFS (Joint Global Ocean
Flux Study).
whilst nothing is certain, I would expect the processes affecting carbon
uptake/release in the ocean depth range 500-1000m to be of special interest to UK researchers
and internationally. Above those depths, SOLAS (Surface Ocean-Lower Atmosphere)
now has lead responsibility. Either Tim or Peter Liss can tell you more about that,
eg the NERC-funded UK SOLAS programme, soon to start.
I hope this helpful
Best regards
Phil

School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
dir tel 01603 593111
fax 01603 507714
[1]p.williamson@uea.ac.uk

670. 2003-07-17

date: Thu, 17 Jul 2003 12:35 -0400

from: jgr-atmospheres@agu.org

subject: 2003JD003856 Review Instructions for Journal of Geophysical

to: K.Briffa@uea.ac.uk

Dear Dr. Briffa:

Thank you for agreeing to review manuscript number 2003JD003856 entitled "On
reconciliation of borehole and proxy based temperature reconstructions over the
last five centuries" by Shaopeng Huang for possible publication in the Journal of
Geophysical Research - Atmospheres. Your efforts are greatly appreciated.
Our goal is to complete the initial review process in about four weeks, and the
assigned due date for this project is August 16, 2003. We would appreciate your
completing and returning the review on or before this date.

To view the manuscript, review form, and instructions please click on the link
below.

<<http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A1Bc4crx2A6mTr5F4A9s1rmu19UljvvsYYCYDENIwZ>>

(NOTE: The link above automatically submits your login name and password. If you
wish to share this link with colleagues, please be aware that they will have access
to your entire account for this journal.)

It would be most helpful if: (1) the review is prepared in anonymous format
suitable for transmission to the author; (2) the review comments on the paper's
originality, significance, and/or usefulness to the JGR readership; and (3) the
review includes a specific recommendation (e.g., publish as is, publish after
revision, or reject).

If you prepare your detailed comments outside the GEMS system and copy-and-paste
them into the review form, please scroll through these comments before submitting
the review to ensure that all characters are rendered correctly and that no
incorrect font substitution has occurred.

Reviewers are kindly requested to consider the originality of the scientific work
and to evaluate the scope of the manuscript with respect to the broad readership of

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the Journal. In particular they should warn the Editor if they feel that the work may be too specialized, too regional in scope, or that its wording makes it unnecessarily difficult or unappealing for readers from outside the field. Suggestions that make the manuscript shorter without altering its content are particularly welcome.

Thank you again for your help and support of our journal.

Sincerely,

Alan Robock

Editor, JGR-Atmospheres

If you have any questions or need more information, please contact Joanne Gregory or Katie Simonson at:

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Fax: +1 732 932-1038
E-mail: jgr@envsci.rutgers.edu

2202. 2003-07-17

date: Thu, 17 Jul 2003 14:37:42 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: Fwd: climate story
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Flesh out a 2-3 page proposal and I will pass it by Broecker. He is the first-order filter before anything would go to Gary Comer. I might have to put myself in as lead P.I. however to get the "first-order filter" to look at it.

>Why are you and that scientist with a reputation for big .. planning
>some extended tree-ring work (WITH SELECTED EUROPEANS OF COURSE)!

>>X-Sender: f028@pop.uea.ac.uk
>>X-Mailer: QUALCOMM Windows Eudora Version 5.1.1

>>Date: Thu, 17 Jul 2003 16:51:27 +0100
>>To: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk
>>From: Phil Jones <p.jones@uea.ac.uk>
>>Subject: Fwd: climate story

>>
>>
>> FYI - worth a read !
>>

>>>From: "Regalado, Antonio" <Antonio.Regalado@wsj.com>
>>>To: "Regalado, Antonio" <Antonio.Regalado@wsj.com>
>>>Subject: climate story
>>>Date: Thu, 17 Jul 2003 10:45:27 -0400
>>>X-Mailer: Internet Mail Service (5.5.2654.89)
>>>X-MIME-Autoconverted: from 8bit to quoted-printable by
>>>secinsgproxy.dowjones.com id h6HEjZh12696
>>>

>>>
>>>Thank you for your help with this article. Please keep me in mind if you
>>>have climate science related news in the future. -- Antonio
>>>

>>>Antonio Regalado
>>>Staff Reporter, The Wall Street Journal
>>>212-416-3011 (Tel.)
>>>917-686-3389 (Cell)
>>>
>>>

>>>
>>>
>>>Weather Vane: Billionaire Opens His Deep Pockets For Climate Theory ---
>>>Lands' End Founder Throws Millions Into Hunt for Data Showing Cataclysmic
>>>Shifts --- Why the Akkadians Dried Up
>>>By Antonio Regalado
>>>2,353 words
>>>17 July 2003
>>>The Wall Street Journal
>>>A1
>>>English
>>>(Copyright (c) 2003, Dow Jones & Company, Inc.)
>>>In May, billionaire Gary Comer and four climate experts boarded his Cessna
>>>Caravan and took off in search of a catastrophe.
>>>Flying low over southwestern Ontario, the group scanned the ground for
>>>boulders left behind by an ancient flood. The deluge, involving 2,000 cubic
>>>miles of fresh water from a prehistoric lake nearby, sent temperatures over
>>>the North Atlantic plummeting about 12,700 years ago, according to a theory
>>>advanced by scientists on the flight.
>>>The cataclysm -- triggered by the melting of glaciers at the close of the
>>>last ice age -- poses an urgent question for the present: Could global
>>>warming also set off unexpected and extreme climate shifts, such as
>>>substantial regional drops in temperature or long droughts?
>>>Some scientists think it's a possibility, and now their research is getting
>>>a major boost from Mr. Comer, 75 years old. The founder and former chairman
>>>of Lands' End Inc. sold the company to Sears, Roebuck & Co. last year,
>>>pocketing just over half the proceeds from the \$1.9 billion cash deal. Since
>>>witnessing unusual ice conditions on an Arctic cruise, Mr. Comer has started
>>>handing out millions of dollars to researchers trying to document so-called
>>>abrupt climate change.
>>>The idea is that the Earth's climate can sometimes behave more like a switch
>>>than a dial, jumping in a matter of years between dramatically different
>>>conditions. At the time of the big flood in Ontario, temperatures in
>>>Greenland dropped by 18 degrees Fahrenheit. The flood also probably upset
>>>ocean currents and changed rainfall patterns as far away as the Asian
>>>monsoon.
>>>Abrupt climate change is a wild card in the divisive debate over the causes
>>>of global warming. For many, the chief culprits are so-called greenhouse
>>>gases formed by the burning of fossil fuels, such as oil and coal. These
>>>gases are thought to be insulating the planet like a blanket, causing
>>>temperatures to rise. A United Nations report predicts that average
>>>temperatures will increase 2.5 degrees to 10.4 degrees by 2100, throwing
>>>Arctic ecosystems into turmoil and threatening coastal communities with
>>>rising sea levels as glaciers melt and warming oceans expand.
>>>While there is broad consensus among scientists that global temperatures are
>>>rising because of fossil-fuel use, the extent and consequences of the
>>>warming remain uncertain. Such doubts now form the basis of the Bush
>>>administration's climate policy, which opposes costly reductions in
>>>emissions of carbon dioxide and other greenhouse gases.
>>>For some scientists concerned about the warming, abrupt climate change has
>>>become a rallying point. Not only does the theory offer worst-case
>>>scenarios, it co-opts one of the arguments favored by skeptics of global
>>>warming -- namely that scientists aren't certain about how the climate
>>>works.
>>>"What concerns me and a lot of people is that we are provoking a system
>>>about which we lack a total understanding," says Wallace S. Broecker, a
>>>geochemist at Columbia University who was among the first to outline the
>>>abrupt-change theory, in the mid-1980s. A feisty 71-year-old with a
>>>reputation for big ideas and for challenging fellow scientists, Dr. Broecker
>>>has become Mr. Comer's closest adviser.
>>>The evidence for sudden climate swings is beginning to find a wider
>>>audience. Last January, Robert Gagosian, director of the Woods Hole
>>>Oceanographic Institution, on Cape Cod, told the World Economic Forum at its

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>>>meeting in Davos, Switzerland, that abrupt change could have the perverse
>>>effect of lowering temperatures in industrialized parts of the globe. A
>>>Senate bill would allocate \$60 million to research on ancient ice and mud,
>>>and the Bush administration plans to highlight abrupt change in a major new
>>>strategic plan for climate-change research, due out this month.
>>>Archaeologists have linked the collapse of several civilizations to large
>>>climate changes. A long dry spell may have caused the decline of the
>>>Akkadian empire in Mesopotamia around 4,200 years ago. Researchers have
>>>unearthed a 180-kilometer-long wall built by a later kingdom to keep out
>>>refugees from newly arid regions.
>>>Hollywood is also taking note. News Corp.'s 20th Century Fox is in
>>>post-production for "The Day After Tomorrow," a big-budget movie in which
>>>global warming sets off a new ice age and Dennis Quaid plays a
>>>paleoclimatologist who battles encroaching glaciers. A studio description
>>>says the film "revolves around an abrupt climate change that has cataclysmic
>>>consequences for the planet."
>>>Critics of such notions -- and there are plenty -- say the yo-yoing of the
>>>climate over the millennia simply shows that man's influence may be grossly
>>>overestimated. They add that Mr. Comer isn't the first big donor to hand
>>>over money to scientists peddling an alarmist message.
>>>"Anyone who studies weather knows that it is variable, but suddenly it is
>>>being treated as a boogeyman," says Richard Lindzen, an atmosphere expert at
>>>the Massachusetts Institute of Technology. He notes that the biggest shifts,
>>>such as the one that occurred 12,700 years ago, happened under ice-age
>>>conditions, when mile-thick ice sheets dominated climate processes.
>>>Mr. Comer grew up on the South Side of Chicago, where his father was a
>>>railroad conductor, and worked for a time as a copy writer at Young &
>>>Rubicam. After quitting to travel to Europe, he decided to turn his hobby of
>>>competitive sailing into a business and founded Lands' End. The small
>>>mail-order operation grew to employ more than 6,000 people, but battles with
>>>his board made the job increasingly unpleasant, Mr. Comer says. A
>>>down-to-earth man who drives a six-year-old Lincoln Towncar and plays down
>>>his wealth, Mr. Comer concedes that with the gas-guzzling auto, in addition
>>>to his fleet of airplanes and boats, his lifestyle is responsible for
>>>prodigious amounts of carbon-dioxide emissions. But he doesn't see personal
>>>change as the solution.
>>>The former executive brings a degree of political independence to the
>>>climate debate. He says he made campaign donations to Bill Bradley and John
>>>McCain in the 2000 election, but couldn't bring himself to vote for either
>>>of the big-party candidates. He says that prior to his Arctic cruise, he had
>>>never given much thought to global warming.
>>>When Mr. Comer steered his 150-foot yacht Turmoil toward the Northwest
>>>Passage two summers ago, the crew expected to be blocked by sea ice.
>>>Instead, the ship slipped easily through open waters. An experienced Arctic
>>>traveler on board said the ice conditions were the mildest he had ever seen.
>>>The Turmoil was just the 94th ship to make the transit from the Atlantic to
>>>the Pacific through the Arctic islands of Canada since Roald Amundsen first
>>>did so in 1905.
>>>"It's obvious something is happening. But no one is really interested in
>>>doing anything about it," Mr. Comer said recently over a diner breakfast of
>>>bacon and eggs.
>>>After he returned from the Northwest Passage to his home outside Chicago, he
>>>typed "global warming" into the Google search engine. A fan of Tom Clancy
>>>and Joseph Conrad novels, he had read of 19th-century explorers who died in
>>>the passage, and he thought his own trip had been too easy. On the Internet,
>>>he found a debate between environmentalists and energy interests -- "one
>>>predicting the end of the world and the other saying nothing is happening,"
>>>he says.
>>>Mr. Comer initially considered launching a web site of his own to counter
>>>the energy industry's arguments, but he decided it would get lost in the
>>>noise. Instead, he called the Woods Hole Oceanographic Institution.
>>>"I don't want to go out and tilt at windmills and waste my time, so I have
>>>focused on the scientists to help them do their job," he says.

>>>Mr. Comer wanted a splashy news conference, but Woods Hole, the world's
>>>largest independent ocean-research center, was more interested in collecting
>>>data than in setting off political fireworks. A Woods Hole oceanographer
>>>named William Curry came to Chicago and explained to Mr. Comer that
>>>researchers weren't sure whether there was actually less ice or if it was
>>>being moved elsewhere by wind. Soon the conversation turned to speculation.
>>>If the polar ice melted, Dr. Curry said, it could cause abrupt climate
>>>change.

>>>The scenario he laid out goes like this: Increasing rainfall and melting ice
>>>caused by global warming could lead to a buildup of fresh water in the North
>>>Atlantic. That influx could shut down circulating ocean currents that
>>>normally draw warm salty water from the tropics along with vast amounts of
>>>heat.

>>>Stopping those currents might disrupt the redistribution of heat around the
>>>globe. In fact, there is evidence that Atlantic currents may already be
>>>under pressure. A few months after the Chicago meeting, British scientists
>>>writing in the journal Nature showed that salinity has dropped measurably in
>>>the North Atlantic during the past 40 years. The Woods Hole graphics
>>>department turned the data into an interactive program that Dr. Curry
>>>e-mailed to Mr. Comer.

>>>Shortly afterward, Mr. Comer agreed to give Woods Hole \$1 million to seed a
>>>program that would place buoys in the Atlantic to monitor changes in
>>>salinity, temperatures and ocean currents. According to an internal Woods
>>>Hole funding document, Mr. Comer's money came with the proviso that he
>>>wanted the research "kicked into high gear."

>>>Paleoclimatic research has exploded in the past several years, thanks to
>>>data found in ice cores, tree rings, coral and ocean sediment. The abrupt
>>>changes are the most striking feature of that data, but the ocean-currents
>>>theory is just one explanation. The atmosphere plays a much bigger role in
>>>climate, and many scientists expect tropical air to contain the mechanisms
>>>of abrupt change.

>>>Mr. Comer had been reaching out to other top scientists. He had written to
>>>Dr. Broecker at Columbia University, saying he was looking for ways to "make
>>>a difference" where he felt the government wasn't. A friend also put Mr.
>>>Comer in touch with F. Sherwood Rowland, a professor at the University of
>>>California at Irvine, who had shared a Nobel Prize for showing that
>>>chlorofluorocarbon gases used in spray bottles and refrigerators could
>>>deplete the ozone layer, an important shield against solar radiation. The
>>>chemicals were later banned when a huge hole in the ozone layer was detected
>>>over the Antarctic.

>>>In May 2002, Dr. Rowland and his wife, Joan, flew to Victoria, British
>>>Columbia, for a cruise on the Turmoil. Mr. Comer joined them after closing
>>>the sale of his company to Sears. Privately, scientists hope he will provide
>>>much more funding than he has. But Mr. Comer, who has also given \$40 million
>>>for a new children's hospital in Chicago that will bear his name, sees his
>>>role as seeding research, not carrying it across the finish line. "The
>>>government has really got to step in," he says.

>>>Dr. Rowland and Mr. Comer were chatting on the bridge when the billionaire
>>>asked, "If I wanted to put \$1 million into climate-change research, what
>>>should I do?" Dr. Rowland says he had a quick answer: provide 10 two-year
>>>fellowships to newly minted Ph.D.s recruited into climate-change science.
>>>"One to work with me, and another nine to other scientists I could pick
>>>out."

>>>The program soon rose to \$6.9 million for 23 research groups, as Mr. Comer
>>>huddled several weeks later with Drs. Rowland and Broecker in New York. They
>>>gave \$300,000 to an expert developing new ice-dating techniques, and an
>>>equal sum to Lonnie Thompson, an Ohio State University researcher known as
>>>the "Indiana Jones of paleoclimatology," who scales mountains in Latin
>>>America in search of rare tropical glaciers.

>>>Last month, Maine Sen. Susan Collins introduced the Abrupt Climate Change
>>>Research Act of 2003, a bill that would give the National Oceanic and
>>>Atmospheric Administration \$60 million in additional funds to implement a
>>>major study of ancient climate records. Sen. Collins, a Republican, has

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>>>parted ways with the Bush administration by calling for a reduction in
>>>greenhouse-gas emissions from power plants to 1990 levels.
>>>The administration has opposed mandating limits, arguing that the economic
>>>costs aren't justified by available science. The wait-and-see policy assumes
>>>that if warming occurs, it will do so gradually over the next century,
>>>leaving time to invent new energy sources or to simply adapt.
>>>That assumption could be wrong. In a 2002 report titled "Abrupt Climate
>>>Change: Inevitable Surprises," the National Academy of Sciences in
>>>Washington concluded that sudden regional climate shifts could be triggered
>>>by human activities.
>>>That possibility is starting to influence policy discussions, which have
>>>until now focused largely on the threat of steady warming. This month, the
>>>Bush administration is expected to release a major report outlining a new
>>>national research strategy for climate change. According to Mr. Bush's
>>>science adviser, John Marburger, abrupt climate change is identified as a
>>>"priority area" in the report, which he has seen. "It is clearly one of the
>>>things that needs to be looked at in the short term," says Dr. Marburger.
>>>Before Mr. Comer set out on the expedition to Ontario in May, he had his
>>>Dassault Falcon jet collect Dr. Broecker and other members of the team at
>>>Chicago's Midway Airport. They gathered for a day of meetings at his
>>>Wisconsin home, and later watched the sunset from a five-story,
>>>glass-enclosed tower that soars above the estate.
>>>During the three-day field trip, the group couldn't locate the path of the
>>>ancient flood. A chagrined University of Manitoba geologist named James
>>>Teller explained that he had predicted the flow using topographical maps, as
>>>he had never had enough funds or reason to rent a plane. Now Mr. Comer has
>>>sent out invitations for a new expedition in September. He thinks the water
>>>went north, into Hudson Bay.
>>>Document j0000000020030717dz7h00030
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>>>rights reserved.

>>
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>>Climatic Research Unit Telephone +44 (0) 1603 592090
>>School of Environmental Sciences Fax +44 (0) 1603 507784
>>University of East Anglia
>>Norwich Email p.jones@uea.ac.uk
>>NR4 7TJ
>>UK

>>-----

>
>--
>Professor Keith Briffa,
>Climatic Research Unit
>University of East Anglia
>Norwich, NR4 7TJ, U.K.
>
>Phone: +44-1603-593909
>Fax: +44-1603-507784
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><http://www.cru.uea.ac.uk/cru/people/briffa/>
--

=====
Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152
=====

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3403. 2003-07-17

cc: abp@dar.csiro.au, a.minns@uea.ac.uk, c.goodess@uea.ac.uk,
j.salinger@niwa.cri.nz, N.W.Arnell@soton.ac.uk, Peter.Whetton@csiro.au,
p.martens@icis.unimaas.nl, rob.wilby@kcl.ac.uk,
simon.shackley@umist.ac.uk, simon.torok@csiro.au, tim.carter@vyh.fi
date: Thu, 17 Jul 2003 09:53:11 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Antwort: Re: CR and the editorial/paper in the latest issue
to: Mike Hulme <m.hulme@uea.ac.uk>, "Wolfgang Cramer (PIK)"
<Wolfgang.Cramer@pik-potsdam.de>, Hans.von.Storch@gkss.de
<x-flowed>

Dear Mike, Hans et al,
I told Hans I wouldn't email him whilst he was on holiday, but
Mike's and the exchange
has prompted me to share a few sentences I received in an email on
Tuesday. Here it is, it
relates to the paper in EOS and comes from someone within AGU.

I thought I would gag when I heard that Soon contacted AGU to question the
nature of the review (to which your piece was subjected) and claimed that
this was a clear effort to politicize the issue (he would never do that).
Of course he stressed that he and his colleagues were only interested in
the science and not the political aspects of the issue.

In CRU we have only issued a few press releases related to articles that
were about
to come out - related to extremes of daily rainfall over the
UK/Iberia. We've normally waited
for the press to contact us. They only do wrt Nature and Science articles
- never for J. Climate,
JGR, IJC and CR. The EOS article got little publicity yet Soon still
implies that we are trying to
politicize the issue. I've never experienced so many calls/emails about
the Soon/Baliunas article
back in March/April.

So maybe here's a test - do you issue a press release when a paper
comes out or are
you just happy the article has finally made it into print? I think all
of us fall in the later
category. CRU's press releases have generally fallen flat or been
subsumed by world events.

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

</x-flowed>

630. 2003-07-18

cc: "Trevor Davies" <T.D.Davies@uea.ac.uk>, <m.hulme@uea>
date: Fri, 18 Jul 2003 15:27:00 +0100
from: "Tim Jickells" <T.Jickells@uea.ac.uk>
subject: Meeting Sept 8
to: <solomon@a1.noaa.gov>

cg2003

Dear Dr Solomon,
I gather from Trevor Davies that you have agreed to participate in the celebration of the opening of the Zuckerman Institute here at UEA and to speak at our atmospheric chemistry meeting being held as part of the opening events. I am responsible for organising that event under the auspices of our new Laboratory for Global Marine and Atmospheric Chemistry. I am very pleased and honoured that you can join us and I look forward to an exciting day. I have been at sea for 6 weeks so am still catching up on events here but the abstract and talk that you have offered Trevor seem to me to fit in perfectly with our plans. The other speakers that day will be:
Dr Eric Wolff of the British Antarctic Survey who will talk about long term changes in atmospheric chemistry.

Prof. Phil Jones of CRU here at UEA who will talk on long term climate change

Prof. Doug Wallace from Kiel in Germany who will talk on the role of air-sea interactions in climate.

Prof. Peter Brimblecombe from UEA who will talk on recent and future changes in atmospheric chemistry at the indoor and local scale.

Prof. Helen ApSimon from Imperial College who will talk on recent and future changes in atmospheric chemistry at the regional and continental scale.

We anticipate an audience predominantly of natural scientists but also with policy makers and others here for other parts of the World Sustainability Days and the ZICER Opening events and I hope all the talks will be relevant to policy, though I note your point in the letter to Trevor that we are discussing science relevant to policy not prescribing policy. I will draft a detailed agenda for the day when I return from vacation in a few weeks but I was anticipating having your talk first followed by the others and was going to assign 40 minutes to your presentation with 10 minutes for questions, please let me know if this is suitable. Please also let me know your travel plans when you know them and we can organise accommodation. You are of course invited to any parts of the Sustainability Events which run from Sept 4-8.

I attach a first advertisement for the meeting, there will be additional publicity generated for the whole series of events for the Sustainability Days and the Zuckerman Institute Opening.

Thanks you again for agreeing to join us.

With best wishes

Tim

Tim Jickells

Director Laboratory for Global marine and Atmospheric Chemistry

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ UK

tel (0)1603 593117

fax (0)1603 507719

Attachment Converted: "c:\eudora\attach\AdvertZicerOpening1.ppt"

2280. 2003-07-22

date: Tue, 22 Jul 2003 14:32:45 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: letter to Senate

to: Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, mann@virginia.edu, jto@u.arizona.edu, omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>

Dear fellow Eos co-authors,

Given the continued assault on the science of climate change by some on Capitol Hill,

Michael and I thought it would be worthwhile to send this letter to various members of the

U.S. Senate, accompanied by a copy of our Eos article.

cg2003

Can we ask you to consider signing on with Michael and me (providing your preferred title and affiliation). We would like to get this out ASAP.
Thanks in advance,
Michael M and Michael O

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\EOS.senate letter-final.doc"

2658. 2003-07-22

date: Tue, 22 Jul 2003 12:16:13 -0700
from: "Malcolm Hughes" <mhughes@lrr.arizona.edu>
subject: Re: letter to Senate
to: Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, mann@virginia.edu, jto@u.arizona.edu, omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>

Dear Michaels,
Count me in. I know that it would be lunacy to start a group edit of your short letter, but you might consider replacing two terms that will be seen as jargon, and may be unduly opaque. You could replace "anthropogenic" with something like "resulting from human actions" and "paleoclimatic" with "studies of ancient (or past) climates".

Title, etc.: Malcolm K. Hughes, Professor, Laboratory of Tree-ring Research, University of Arizona
Cheers, Malcolm

>
> Dear fellow Eos co-authors,
>
> Given the continued assault on the science of climate change by some
> on Capitol Hill, Michael and I thought it would be worthwhile to send
> this letter to various members of the U.S. Senate, accompanied by a
> copy of our Eos article.
>
> Can we ask you to consider signing on with Michael and me (providing
> your preferred title and affiliation). We would like to get this out
> ASAP.
>
> Thanks in advance,
>
> Michael M and Michael O
>
>
>

> Professor Michael E. Mann
> Department of Environmental Sciences, Clark Hall
> University of Virginia
> Charlottesville, VA 22903
>

> _ e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Malcolm Hughes
Professor of Dendrochronology
Laboratory of Tree-Ring Research

cg2003

University of Arizona
Tucson, AZ 85721
520-621-6470
fax 520-621-8229

4674. 2003-07-22

date: Tue Jul 22 09:34:02 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Did my files get though to you?
to: "A. DAWSON" <GEX014@coventry.ac.uk>

Alastair

sorry - they did - I have no immediate comments but will get back to you ,cheers

Keith

At 10:09 AM 7/18/03 +0100, you wrote:

Keith,

have not heard anything from you - did my diagrams reach you ok?

alastair

----- Original Message -----

Subject: Sea ice and SST proxies

Date: Thu, 10 Jul 2003 12:38:55 +0100

From: "A. DAWSON" <GEX014@coventry.ac.uk>

Organization: Coventry University

To: k.briffa@uea.ac.uk

Dear Keith,

Please find attached in confidence recent versions of sea ice and SST proxies - these are being modified at present and changed around but basically they both give seasonal signals for last 2k - and they raise all sorts of Qs - I have highlighted a couple of rapid climate change events - the older of which is presently being submitted for publication.

Would be interested for your thoughts - note also the sea ice phase within MWP.

best wishes

Alastair

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784

[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

1700. 2003-07-23

cc: Jonathan Overpeck <jto@u.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>

date: Wed, 23 Jul 2003 20:13:12 -0600

from: Tom Wigley <wigley@ucar.edu>

subject: Re: letter to Senate

to: Michael Oppenheimer <omichael@Princeton.EDU>

<x-flowed>

Folks,

Here are some thoughts about the Soon issue, partly arising from talking to Ben.

What is worrying is the way this BS paper has been hyped by various groups. The publicity has meant that the work has entered the consciousness of people in Congress, and is given prominence in some

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publications emanating from that sector. The work appears to have the imprimatur of Harvard, which gives it added credibility. So, what can we as a community do about this? My concerns are two-fold, and I think these echo all of our concerns. The first is the fact that the papers are simply bad science and the conclusions are incorrect. The second is that the work is being used quite openly for political purposes. As scientists, even though we are aware of the second issue, we need to concentrate on exposing the scientific flaws. We also need to do this in as authoritative a way as possible. I do not think it is enough to speak as individuals or even as a group of recognized experts. Even as a group, we will not be seen as having the 'power' of the Harvard stamp of approval.

What I think is necessary is to have the expressed support of both AGU and AMS. It would also be useful to have Harvard disassociate themselves from the work. Most importantly, however, we need the NAS to come into the picture. With these 4 institutions, together with us (and others) as experts, pointing out clearly that the work is scientific rubbish, we can certainly win this battle.

I suggest that we try to get NAS to set up a committee to (best option) assess the science in the two BS papers, or (less good, but still potentially very useful) assess the general issue of the paleo record for global- or hemispheric-scale temperature changes over the past 1000 years. The second option seems more likely to be acceptable to NAS. This is arguably an issue of similar importance to the issue of climate sensitivity uncertainties which NAS reviewed earlier this year (report still in preparation).

I am not sure how to fold AGU and AMS into this -- ideas are welcome. Similarly, perhaps some of you know some influential Harvard types better than I do and can make some suggestions here.

The only way to counter this crap is to use the biggest guns we can muster. The Administration and Congress still seem to respect the NAS (even above IPCC) as a final authority, so I think we should actively pursue this path.

Best wishes,

Tom.

Michael Oppenheimer wrote:

> Dear All:

>

> Since several of you are uncomfortable, it makes good sense to step back and
> think about a more considered approach. My view is that scientists are fully
> justified in taking the initiative to explain their own work and its relevance in
> the policy arena. If they don't, others with less scruples will be heard
> instead. But each of us needs to decide his or her own comfort zone.

>

> In this case, the AGU press release provides suitable context, so it may be that
> neither a separate letter nor another AGU statement would add much at this time.
> But this episode is unlikely to be the last case where clarity from individuals
> or groups of scientists will be important.

>

> Michael

>

>

>

> Tom Wigley wrote:

>

>

>>Folks,

>>

>>I am inclined to agree with Peck. Perhaps a little more thought and time
>>could lead to something with much more impact?

>>

>>Tom.

>>

>>
>>Jonathan Overpeck wrote:
>>
>>>Hi all - I'm not too comfortable with this, and would rather not sign -
>>>at least not without some real time to think it through and debate the
>>>issue. It is unprecedented and political, and that worries me.
>>>
>>>My vote would be that we don't do this without a careful discussion first.
>>>
>>>I think it would be more appropriate for the AGU or some other
>>>scientific org to do this - e.g., in reaffirmation of the AGU statement
>>>(or whatever it's called) on global climate change.
>>>
>>>Think about the next step - someone sends another letter to the
>>>Senators, then we respond, then...
>>>
>>>I'm not sure we want to go down this path. It would be much better for
>>>the AGU etc to do it.
>>>
>>>What are the precedents and outcomes of similar actions? I can imagine a
>>>special-interest org or group doing this like all sorts of other
>>>political actions, but is it something for scientists to do as individuals?
>>>
>>>Just seems strange, and for that reason I'd advise against doing
>>>anything with out real thought, and certainly a strong majority of
>>>co-authors in support.
>>>
>>>Cheers, Peck
>>>
>>>
>>>
>>>>>Dear fellow Eos co-authors,
>>>>>
>>>>>Given the continued assault on the science of climate change by some
>>>>>on Capitol Hill, Michael and I thought it would be worthwhile to send
>>>>>this letter to various members of the U.S. Senate, accompanied by a
>>>>>copy of our Eos article.
>>>>>
>>>>>Can we ask you to consider signing on with Michael and me (providing
>>>>>your preferred title and affiliation). We would like to get this out ASAP.
>>>>>
>>>>>Thanks in advance,
>>>>>
>>>>>Michael M and Michael O
>>>>>
>>>>>_____
>>>>> Professor Michael E. Mann
>>>>> Department of Environmental Sciences, Clark Hall
>>>>> University of Virginia
>>>>> Charlottesville, VA 22903
>>>>>_____
>>>>>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
>>>>> http://www.evsc.virginia.edu/faculty/people/mann.shtml
>>>>>
>>>>>Attachment converted: Macintosh HD:EOS.senate letter-final.doc
>>>>>(WDBN/MSWD) (00055FCF)
>>>>>
>>>>>
>>>>>
>>>>>--
>>>>>
>>>>>Jonathan T. Overpeck

cg2003
>>>Director, Institute for the Study of Planet Earth
>>>Professor, Department of Geosciences
>>>
>>>Mail and Fedex Address:
>>>
>>>Institute for the Study of Planet Earth
>>>715 N. Park Ave. 2nd Floor
>>>University of Arizona
>>>Tucson, AZ 85721
>>>direct tel: +1 520 622-9065
>>>fax: +1 520 792-8795
>>>http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
>>><http://www.ispe.arizona.edu/>
>>
</x-flowed>

2096. 2003-07-23

cc: Jonathan Overpeck <jto@u.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>
date: wed, 23 Jul 2003 21:22:26 -0400
from: Michael Oppenheimer <omichael@Princeton.EDU>
subject: Re: letter to Senate
to: Tom Wigley <wigley@ucar.edu>

Dear All:

Since several of you are uncomfortable, it makes good sense to step back and think about a more considered approach. My view is that scientists are fully justified in taking the initiative to explain their own work and its relevance in the policy arena. If they don't, others with less scruples will be heard instead. But each of us needs to decide his or her own comfort zone. In this case, the AGU press release provides suitable context, so it may be that neither a separate letter nor another AGU statement would add much at this time. But this episode is unlikely to be the last case where clarity from individuals or groups of scientists will be important.

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> Folks,

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> could lead to something with much more impact?

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4032. 2003-07-23

cc: Jonathan Overpeck <jto@u.arizona.edu>, Caspar M Ammann <ammann@ucar.edu>,
Page 523

cg2003

Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenberth@cgd.ucar.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, mann@virginia.edu
date: Wed, 23 Jul 2003 23:18:30 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: letter to Senate
to: Tom Wigley <wigley@ucar.edu>, Michael Oppenheimer <omichael@princeton.edu>

Tom,
Thanks for your email, and your (and Ben's) thoughtful comments on all of this...
I think the Eos piece has gone a long way to discrediting the 'science' behind the "BS" papers (well, technically, "SB", but I prefer the reverse order too). The paper Phil and I have in press in GRL (hopefully to appear within a few weeks now) will reinforce this. But the BS papers certainly got a lot more mileage than they should have. The fact that the forces of disinformation were able to get that much mileage out of these two awful papers written by those clowns should remain a real cause for concern. Their ability to repeatedly co-opt the Harvard news office remains a real problem. Nobody I've talked to at Harvard is happy about this, and there's been talk of action on the part of various of the faculty, but nobody seems willing or able to mount enough of an effort to get anything done about this. Apparently there was a threat of a lawsuit against Harvard last time folks there tried to do something about Baliunas, and so they may have lost their nerve. But I know our Harvard colleagues are not happy about continually having their institutional name dragged through the mud. If someone has close ties w/ any individuals there who might be in a position to actually get some action taken on this, I'd highly encourage pursuing this.
Re, an NAS committee--this is an interesting idea. But I wonder if a committee on BS would be overkill, perhaps giving these fools just the stage that they're looking for.
An alternative would be, as you say, to take this on in the context of another more general NAS panel. Coincidentally, there is already a panel on "Radiative Forcing Effects on Climate" which convenes this fall. I believe the panel makeup is now in the public domain (or will be within days, on the NAS website) so there's no secret here. I'm on the panel. Daniel Jacob will be chairing it, and others on it are Jeff Kiehl, Francis Zwiers, Roni Avissar, Judith Lean, Stuart Gaffin, Lynn Russell. Also on the panel will be Ramanathan, Pielke Sr, Gerard Bond, Ulrike Lohmann, and Hadi Dowlatabadi (whom I don't know). Its a somewhat odd makeup, and I suspect that consensus will not be easy (there are at least a couple obvious trouble spots), but there is certainly a core group of reasonable folks on the panel, and this could be an opportunity to clarify the state of the science

cg2003

on long-term forced variability (including e.g. comparisons of model simulations and reconstructions of the past 1000 years). This, at least indirectly, would deal w/ the BS issue.

I'm interested in the thoughts of others on any of the above.

cheers,

mike

At 08:13 PM 7/23/2003 -0600, Tom Wigley wrote:

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work appears to have the imprimatur of Harvard, which gives it added credibility. So, what can we as a community do about this? My concerns are two-fold, and I

think these echo all of our concerns. The first is the fact that the papers are simply bad

science and the conclusions are incorrect. The second is that the work is being used quite openly for political purposes.

As scientists, even though we are aware of the second issue, we need to concentrate on exposing the scientific flaws. We also need to do this in as authoritative a

way as possible. I do not think it is enough to speak as individuals or even as a group of recognized experts. Even as a group, we will not be seen as having the 'power'

of the Harvard stamp of approval.

AMS. What I think is necessary is to have the expressed support of both AGU and It would

also be useful to have Harvard disassociate themselves from the work. Most importantly,

however, we need the NAS to come into the picture. With these 4 institutions, together

with us (and others) as experts, pointing out clearly that the work is scientific

rubbish, we can certainly win this battle.

I suggest that we try to get NAS to set up a committee to (best option) assess the

science in the two BS papers, or (less good, but still potentially very useful) assess

the general issue of the paleo record for global- or hemispheric-scale temperature

changes over the past 1000 years. The second option seems more likely to be acceptable

to NAS. This is arguably an issue of similar importance to the issue of climate

sensitivity uncertainties which NAS reviewed earlier this year (report still in

preparation).

I am not sure how to fold AGU and AMS into this -- ideas are welcome.

Similarly, perhaps

some of you know some influential Harvard types better than I do and can make some suggestions here.

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The only way to counter this crap is to use the biggest guns we can muster.
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4584. 2003-07-23

cc: J.A.Matthews@swansea.ac.uk, G.M.Young@Swansea.ac.uk, dhunzicker2002@yahoo.com,
pcamill@carleton.edu
date: Wed, 23 Jul 2003 22:33:04 -0500
from: Phil Camill <pcamill@carleton.edu>
subject: Holocene manuscript
to: K.Briffa@uea.ac.uk

Dear Keith,
I have not yet received an editorial response or reviews for the manuscript
entitled "Using a new 672-year tree-ring drought reconstruction from
west-central Montana to evaluate severe drought teleconnections in the
western US and possible climatic forcing by the Pacific Decadal Oscillation"
by Hunzicker and Camill. This manuscript has been in review for 14 months.
Can you indicate when I can expect these materials?
Many thanks,
Phil

Dr. Phil Camill
Assistant Professor of Biology and Environmental Studies
Carleton College, Department of Biology
One North College St.
Northfield, MN 55057
phone: (507) 646-5643
fax: (507) 646-5757

276. 2003-07-24

cc: Tom Wigley <wigley@ucar.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Caspar M
Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes
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<t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Ben Santer
<santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>
date: Thu, 24 Jul 2003 09:38:13 -0700

cg2003

from: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>
subject: Re: letter to Senate
to: "Michael E. Mann" <mann@virginia.edu>, Michael Oppenheimer
<omichael@Princeton.EDU>

Colleagues,

I'm very torn between being drawn into endless exchanges outside normal scientific discourse (e.g. tit-for-tat with the Idsos group) and leaving the field open to them. They clearly have the resources to do fairly careful literature searches, even if there are some serious conceptual problems in their writings, and there is a real audience for their kind of materials, both in print publication and on the web. I fear that you would find more colleagues and grad students than you would like to think read their materials and are influenced by them. Apart from anything else they respond better to the heavily referenced articles by Idso or Soon than to "ex cathedra" statements like the recent editorial by Barnett and Somerville. I know this to be the case in the paleo community, although there the picture is complicated by the differences in scientific approach of those working on interannual to century time scales (i.e. folks like us) and those working on millennial and longer time scales (notably Wally Broecker, Wjbjorn Karlen, but many others too). One consequence of this intersection of differing sources of scepticism (*sensu stricto*) is that an appeal to the NAS could be counterproductive - remember the poor treatment of high-res paleo in the NAS report requested by the White House the other year.

Let's learn from these guys. We don't have to strain to publish in the peer-reviewed literature - it's our normal way of working. We do have to find a more effective way of publicizing and interpreting these publications, when appropriate, to a wider audience, including policy makers. How best to do this?

Cheers, Malcolm

.

>

> Tom, Mike et al:

> 1. Making the S B papers the sole or main subject of an NRC committee
> would be a mistake. 2. But dispensing of them as a minor part of an
> NRC examination of paleoclimate makes sense. Some of you may recall
> the Idso, Newell contratemps of 20 years ago, and as I recall, this is
> how it was handled. 3. For the near term, the rebuttal paper in Eos is
> a terrific example of what can and should be done in such
> circumstances, and the AGU press release is more than I would have
> expected. We've provided all the necessary ammunition. The best you
> can do now is be responsive if reporters or Congressional staff call.
> 4. For the long haul, in addition to the NRC committee route, some
> thought needs to be given to more formal ways to respond to such
> situations, which I expect to continue to arise indefinitely. This is
> one role for IPCC and NRC, but both are painfully slow. Perhaps AGU
> and AMS and AAAS need to see their roles as partly to provide a venue
> for such clarifications. The key this time was rapid turnover. Maybe
> Don Kennedy and Science could be engaged in this somehow. Michael

>

> "Michael E. Mann" wrote:

>

> Tom,
> Thanks for your email, and your (and Ben's) thoughtful comments on
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Malcolm Hughes
Professor of Dendrochronology
Laboratory of Tree-Ring Research
University of Arizona
Tucson, AZ 85721
520-621-6470
fax 520-621-8229

286. 2003-07-24

cc: Phil Camill <pcamill@carleton.edu>, dhunzicker2002@yahoo.com
date: Thu, 24 Jul 2003 15:21:55 -0500
from: Phil Camill <pcamill@carleton.edu>
subject: Re: Holocene manuscript- sorry
to: Keith Briffa <k.briffa@uea.ac.uk>

Dear Keith,
I have been contacted by the lead author (D. Hunzicker), and he is

cg2003

enthusiastic about resubmitting after substantial revision. We feel that we can address several of the reviewers comments about sample size and core inclusion.

First, we both agree that our original statement in the methods unfortunately misled the reviewers into believing that we didn't use cores with missing rings. This is not the case. Second, I wrote the section on sample size, and, unfortunately, I misinterpreted the lead author's description of how he established the chronology vis-a-vis sample size. The following explanation, which will be clarified in a potential revision, shows that we did use most (84%) of climatically sensitive cores collected: Paraphrased notes from D. Hunzicker:

1) 152 is the total number of trees I cored. Of those, I only measured the rings of 118 cores, the remaining 34 being young (mostly less than 180 years) and composed entirely or almost entirely of rapidly growing, complacent, juvenile growth rings. I, therefore, sampled 118 trees with sufficiently long (and potentially sensitive) records for dendrochronological analysis.

2) Of the 118 cores, 61 were used to establish the chronology (many with missing rings), and 57 cores were rejected due to "complacency, unresolvable sections of missing rings, or low interseries correlation values." Probably half of these 57 cores (~30) were from trees growing near the lake level or in small, potentially wetter, ravines on the slope where you would expect a weak climate signal. I cored them anyway because they were some of the largest trees in the region and to increase sample size, but I anticipated that they may be problematic, which they were. These 30 cores were therefore rejected.

3) Approximately 13 of the 57 were rejected for other reasons such as multiple breakage points.

4) The remaining 12 rejected samples were extremely difficult to include in the chronology and may have been included had replicate cores been sampled from each tree to aid in identifying multiple sets of missing rings. Given logistical and time constraints during sampling, however, I opted for increasing the sample size of the number of replicate trees rather than pseudoreplicated cores from a single tree. It's possible we can re-investigate these 12 samples.

Thus, of the 118 cores with long records, only 73 were truly suitable for analysis. Of these cores, 84% (61 cores) were used to build the chronology, only 12 cores were omitted because of being extremely difficult to include in the chronology.

We would be grateful for any initial thoughts you might have.

All the best,

Phil

--On Thursday, July 24, 2003, 1:49 PM -0500 "Phil Camill"

<pcamill@carleton.edu> wrote:

>

> Thanks, Keith. I will contact the author, D. Hunzicker, and see how he
> wants to proceed. The reviews below were helpful, and we would like to
> revise the manuscript to improve its quality. A first place to start will
> be to simplify the approach, focusing mainly on the new chronology and its
> comparison with Cook et al's data set (i.e., reviewer 2's suggestion to
omit

> the fft of sunspots and ENSO, which are well reviewed in the literature).

>

> My first read of the reviews were mixed. Reviewer 1's comments that we
only

> included trees with no missing rings is false. Our chronology includes
> several cores with missing rings but where identifying and incorporating
> missing rings was fairly straightforward. And there were plenty of hard
> hours at the scope looking for and incorporating them. Perhaps we should
> back off our statement about omitting cores with "complacency,
unresolvable

> sections of missing rings, or low interseries correlation values" which
was

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> obviously misleading. Nonetheless, there will always be cores that are
> simply too difficult to verify with COFECHA, and I was surprised to see
> his/her reaction that 61 cores couldn't establish an adequate chronology.
I
> doubt there there is literature suggesting a minimum fraction of cores
that
> should be incorporated into a chronology.
>
> We will also re-analyze the response function analysis and climate data,
> although our approach in examining both raw and detrended data were pretty
> exhaustive. It may turn out that reviewer 1 is correct that climate data
> "are not guaranteed to be homogeneous, especially in the mountain west
> during the early 20th century." Given that we analyzed both pre-whitened
> and standardized data sets, do you or the reviewer have additional
> suggestions on how to proceed?
>
> If we were to undertake substantial revision in an effort to resubmit, is
> there an appropriate timetable that would work for you, say our getting a
> manuscript to you sometime in the next 3-5 months?
>
> Thanks again, Keith.
> All the best,
> Phil
>
>
>
> --On Thursday, July 24, 2003, 1:57 PM +0100 "Keith Briffa"
> <k.briffa@uea.ac.uk> wrote:
>
>>
>> Phil
>> I am really sorry for the delay , that was a result of initially tardy
>> reviewers, my subsequent illness, and then a delay while John is away
> while
>> I wished to consult with him . The reason for the last wish will be clear
>> when you read the reviews below.
>> The referees are not enthusiastic and in their private comments to me one
>> is strongly negative and the other ambivalent. The pressure on space
means
>
>> that this would normally be a rejection (and we have, since your
> submission
>> ,developed new , stricter rules regarding possible re-submission.)
>> However, in the circumstances (the delay that is down to me) , I am
>> overruling these and (despite not discussing it with John) asking you to
>> read these reviews and come back with a frank opinion of whether you
>> consider them fair and the paper publishable with some work . I am doing
>> this because I believe it is. If you can answer these remarks and feel
you
>
>> can submit a valid manuscript that accounts for them - I will review your
>> argument (without recourse to the reviewers) and if I agree , I guarantee
>> speedy process through the last publication phase.
>> Again , you and I are well aware that this manuscript could have been
> dealt
>> with much better and I am really sorry for it.
>> Keith
>>
>> REVIEWERS REMARKS FOLLOW
>>
>> Referee 1
>>
>> Review of David Hunzicker and Phil Camill: "Using a new 672-year

tree-ring

>
 >> drought reconstruction from westcentral MontanaŠ" submitted to the
 > Holocene.
 >> This is a well written, well executed paper that I would unfortunately
 not
 >
 >> recommend for publication in the Holocene. It's a shame to read a paper
 >> like this. It is very well informed, well referenced, places the work in
 a
 >
 >> good scientific context, and includes strong statistical analyses.
 > However,
 >> the attention paid to the analyses and interpretation of the
 > reconstruction
 >> was evidently not paid so carefully to the fundamental tree-ring
 > chronology
 >> development. They call it "crossdating," but the best I can tell from the
 >> limited discussion it was simply computerized correlation matching of
 >> measured time series, with a massive culling of the data to pare down to
 >> those time series that produced straightforward correlations in a COFECHA
 >> analysis. I was astounded to read that their final chronology used only
 61
 >
 >> out of the 152 trees sampled for the study. The 60% of the trees not
 >> included apparently suffered from "complacency, unresolvable sections of
 >> missing rings, or low interseries correlation values." This appears to be
 >> the first penalty for not applying rigorous dendrochronological methods
 to
 >
 >> the chronology development. I find it incredible that over half of the
 >> Ponderosa pine samples would not be useful. I can't help but suspect that
 >> by relying on COFECHA output, without any hard-nosed microscope work and
 >> rigorous crossdating with the wood samples themselves, you at best
 default
 >
 >> to the simple, straightforward trees without missing rings. That is, you
 >> default to a less climatically sensitive subset of trees. This appears to
 >> be the second penalty for the seemingly inexperienced, quick and dirty
 >> chronology development.
 >> These authors have obviously worked hard on this study and bring
 excellent
 >
 >> analytical skills and knowledge of the literature. The paper itself is
 >> exceptionally well written (with a minor complaint concerning the over
 > use,
 >> and at times incorrect use of the term "teleconnection"). But the
 >> calibration and validation reported in the paper are clearly awful, and
 >> that surely ought not be the case for Ponderosa pine on moisture-stressed
 >> sites in Montana. One hates to be non-supportive of their work, so much
 of
 >
 >> which is high quality, but it seems to come down to fundamentals, and
 here
 >
 >> the fundamental dendrochronology and chronology development are in
 >> question. And I also do not think it advisable to publish a
 reconstruction
 >
 >> that explains maybe 21% of the variance in the instrumental climate data,
 >> when using an arid site conifer as the predictor (the persistence in the
 >> standard chronology may be inflating even that figure). I just can't
 >> believe the calibration could be so weak. It seems they need to revisit

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>> their chronology development work, and dig deeper into the climate
> response
>> of their chronology. Then look very carefully at climate data itself.
> These
>> climate data are not guaranteed to be homogeneous, especially in the
>> mountain West during the early 20th century. If all this could be done,
> and
>> if the variance explained in both the calibration and verification
periods
>
>> could be improved, then publication in the Holocene would be well
> justified.
>>
>> Referee 2
>>
>>
>> Review of "Using a New 672-Year Tree-Ring Drought Reconstruction from
>> West-Central Montana to Evaluate Severe Drought Teleconnections in the
>> Western U.S. and Possible Climatic Forcing by the Pacific Decadal
>> Oscillation" by D.A. Hunzicker and P. Camill
>>
>> This paper is reasonably well written, but has some problems in it that
>> bother me. The first issue relates to the tree-ring chronology that was
>> developed at Lindberg Lake. Anytime less than half of the core samples
(61
>
>> or 152) are used in developing a chronology, this is cause for concern.
> The
>> fact that there are "unresolvable sections of missing rings" (p. 10) can
>> mean a lot of things. However, ponderosa pine is known to cross-date
well,
>
>> which includes "locating" locally-absent rings during the cross-dating
>> phase, so it is surprising that the authors have chosen not to work
> through
>> these problems. Presumably, the trees with missing rings are also those
>> most sensitive to drought, so isn't there a chance that the chronology
>> being analyzed in this paper is less sensitive to drought than it ought
to
>
>> be? I also wonder how much their chronology is truly contributing to the
>> overall stated goal of this paper, i.e. evaluating "Severe Drought
>> Teleconnections in the Western U.S. and Possible Climatic Forcing by the
>> Pacific Decadal Oscillation". The authors extensively use the PDSI
>> reconstructions of Cook et al. (1999) in their analyses. Aside from the
>> increased length of their new tree-ring chronology, what does it
> contribute
>> that was not possible simply by using the Cook et al. reconstructions to
>> test for teleconnections and forcing. None of the indices of forcing
> (ENSO,
>> PDO, sunspots) extend back before the beginning of the Cook et al.
>> reconstructions, so there is little to be gained in using one longer
> series
>> from west-central Montana in this analysis. One could point to Fig. 3,
>> which compares the MT reconstruction vs the SWDI series. But even this
>> comparison is limited in its overall contribution to the paper. I also
>> don't like the use of the FFT for estimating power spectra, even if the
>> confidence limits are determined by bootstrapping. The power spectra
>> calculated by the FFT are still inconsistent estimates. A more
> contemporary
>> and consistent method of spectral estimation, like the Multi-Taper
Method,
>

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>> should be used.
>> For the reasons stated above, I do not consider this paper to be ready
for
>
>> publication as is. I will leave it to the Editor to decide how to proceed
>> with it past this point.
>>
>>
>>
>>
>>
>>
>> At 10:33 PM 7/23/03 -0500, you wrote:
>>
>>>Dear Keith,
>>>
>>>I have not yet received an editorial response or reviews for the
> manuscript
>>>entitled "Using a new 672-year tree-ring drought reconstruction from
>>>west-central Montana to evaluate severe drought teleconnections in the
>>>western US and possible climatic forcing by the Pacific Decadal
> Oscillation"
>>>by Hunzicker and Camill. This manuscript has been in review for 14
> months.
>>>
>>>Can you indicate when I can expect these materials?
>>>
>>>Many thanks,
>>>Phil
>>>
>>>
>>>
>>>*****
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>>>Carleton College, Department of Biology
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ENTS: <http://www.acad.carleton.edu/curricular/ENTS/index.html>

689. 2003-07-24

cc: Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, ethomps@magnus.acs.ohio-state.edu
date: Thu, 24 Jul 2003 13:02:56 -0400
from: "Michael E. Mann" <menn@virginia.edu>
subject: Re: letter to Senate
to: Jonathan Overpeck <jto@u.arizona.edu>, Michael Oppenheimer <omichael@princeton.edu>

Thanks Peck,

These are interesting thoughts to mull over. Perhaps Ellen can comment at some point about whether it would be possible to get AGU to take a more active role. I realize this could be a tricky issue...

Re, Michael contacting Donald Kennedy about some possible activity on AAAS' part, that seems like a great idea too.

Now, back to putting out some fires (not the AZ kind, but the DC kind),
mike

At 09:24 AM 7/24/2003 -0700, Jonathan Overpeck wrote:

Hi all - the debate on whether or not our author team should send letters/reprints

directly to members of Congress is a tough one for sure. I think the ideal thing would

be if AGU would be willing to send a copy of our paper, along with their stand on

climate change, and an affirmation that this stand is even stronger now than it was x

years ago when it was first taken. The good press release was positive, but it's only

part of what I think their responsibility should be. I, for one, would be willing to

co-sign a letter to the AGU pres and whomever, to do just this. Then, it's not just a

couple scientists who wrote a paper, but the largest professional society in

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the field

sending a message to Congress. I feel that is their job. I've cc'd this to Ellen

Mosley-Thompson as someone who might have thoughts and influence.

I agree that we, as individuals have a responsibility too. But, would it make a

precedent. AGU

could have a much larger impact.

Have you seen...

[1]<http://www.washingtonpost.com/wp-dyn/articles/A40912-2003Jul10.html>

that some This is a well written piece that should have impact. Although I do understand

in Congress really don't understand where the science stands, I think more of them are

smart enough to know (or have staff who know). The problem is less science, and more

politics - policy. The Anderson op ed piece hits the mark. Focusing on the science is

only a diversion in some respects (although, I think we need more science on the issue too).

I am not closed to the idea of action - indeed, I admire all of you for being leaders in

this area. However, I really want to make sure we've thought it all out before doing

something that is pretty rare. Let the political interests lobby Congress. In the

meantime, the science gets more and more rock hard on this issue thanks to the hard work

of people like you.

I could be wrong... (not about the hard work ;))

One idea - it would help to have feedback from a conservative Congressman (they are all

men, right?) on this, or his staffer. I have one such friend, and he wouldn't like the

idea. I've tried to give him a balanced view of the issue, but he views it as political

pressure. This guy is very smart, so you see, the issue isn't all about the science. Any

positive feedback from the intended audience - not from Democrats (they already know

that Soon et al was politically motivated poor science).

cheers, Peck

Dear All:

Since several of you are uncomfortable, it makes good sense to step back and think about a more considered approach. My view is that scientists are fully justified in taking the initiative to explain their own work and its relevance in

the policy arena. If they don't, others with less scruples will be heard instead. But each of us needs to decide his or her own comfort zone.

In this case, the AGU press release provides suitable context, so it may be that

neither a separate letter nor another AGU statement would add much at this time.

But this episode is unlikely to be the last case where clarity from individuals

or groups of scientists will be important.

Michael

Tom Wigley wrote:

Folks,

I am inclined to agree with Peck. Perhaps a little more thought and time could lead to something with much more impact?

Tom.

Jonathan Overpeck wrote:

> Hi all - I'm not too comfortable with this, and would rather not sign -
 > at least not without some real time to think it through and debate the
 > issue. It is unprecedented and political, and that worries me.
 > >
 > My vote would be that we don't do this without a careful discussion first.
 >
 > I think it would be more appropriate for the AGU or some other
 > scientific org to do this - e.g., in reaffirmation of the AGU statement
 > (or whatever it's called) on global climate change.
 >
 > Think about the next step - someone sends another letter to the
 > Senators, then we respond, then...
 >
 > I'm not sure we want to go down this path. It would be much better for
 > the AGU etc to do it.
 >
 > What are the precedents and outcomes of similar actions? I can imagine a
 > special-interest org or group doing this like all sorts of other
 > political actions, but is it something for scientists to do as individuals?
 >
 > Just seems strange, and for that reason I'd advise against doing
 > anything with out real thought, and certainly a strong majority of
 > co-authors in support.
 >
 > Cheers, Peck
 >
 >
 >
 >> Dear fellow Eos co-authors,
 >>
 >> Given the continued assault on the science of climate change by some
 >> on Capitol Hill, Michael and I thought it would be worthwhile to send
 >> this letter to various members of the U.S. Senate, accompanied by a
 >> copy of our Eos article.
 >>
 >> Can we ask you to consider signing on with Michael and me (providing
 >> your preferred title and affiliation). We would like to get this out ASAP.
 >>
 >> Thanks in advance,
 >>
 >> Michael M and Michael O
 >
 >>
 >>

 >> Professor Michael E. Mann
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 >> University of Virginia
 >> Charlottesville, VA 22903
 >>
 >> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
 >> [2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
 >
 >>
 >> Attachment converted: Macintosh HD:EOS.senate letter-final.doc
 >> (WDBN/MSWD) (00055FCF)
 >
 >
 >
 > --
 >
 > Jonathan T. Overpeck

cg2003
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> [3]http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
> [4]<http://www.ispe.arizona.edu/>
Content-Type: text/x-vcard; charset=us-ascii;
name="omichael.vcf"
Content-Transfer-Encoding: 7bit
Content-Description: Card for Michael Oppenheimer
Content-Disposition: attachment;
filename="omichael.vcf"
Attachment converted: Macintosh HD:omichael 1.vcf (TEXT/tttx) (0005693F)
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2719. 2003-07-24

cc: "Michael E. Mann" <mann@virginia.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>
date: Thu, 24 Jul 2003 10:00:44 -0600
from: Tom Wigley <wigley@ucar.edu>
subject: Re: letter to Senate
to: Michael Oppenheimer <omichael@Princeton.EDU>
<x-flowed>

Michael,
So we need to push for my second option -- a more general paleo review by NAS/NRC. There are already and soon to come such reviews, but this is not the same thing. NAS would rely on and also critique this literature to some degree (this literature includes BS).
I agree that the Eos piece serves an extremely useful purpose, but it is too technical for politicians. It is, however, perfect for us when we need published support for our communications with the press and others.

Re involving AMS, AGU and AAAS, I don't know how to do this. Any ideas?
Tom.

+++++

Michael Oppenheimer wrote:

> Tom, Mike et al:

>

> 1. Making the S B papers the sole or main subject of an NRC committee
> would be a mistake.

>

> 2. But dispensing of them as a minor part of an NRC examination of
> paleoclimate makes sense. Some of you may recall the Idso, Newell
> contratemps of 20 years ago, and as I recall, this is how it was handled.

>

> 3. For the near term, the rebuttal paper in Eos is a terrific example of
> what can and should be done in such circumstances, and the AGU press
> release is more than I would have expected. We've provided all the
> necessary ammunition. The best you can do now is be responsive if
> reporters or Congressional staff call.

>

> 4. For the long haul, in addition to the NRC committee route, some
> thought needs to be given to more formal ways to respond to such
> situations, which I expect to continue to arise indefinitely. This is
> one role for IPCC and NRC, but both are painfully slow. Perhaps AGU and
> AMS and AAAS need to see their roles as partly to provide a venue for
> such clarifications. The key this time was rapid turnover. Maybe Don
> Kennedy and Science could be engaged in this somehow.

>

> Michael

>

>

> "Michael E. Mann" wrote:

>

>> Tom,

>>

>> Thanks for your email, and your (and Ben's) thoughtful comments on all
>> of this...

>>

>> I think the Eos piece has gone a long way to discrediting the
>> 'science' behind the "BS" papers (well, technically, "SB", but I
>> prefer the reverse order too). The paper Phil and I have in press in
>> GRL (hopefully to appear within a few weeks now) will reinforce this.
>> But the BS papers certainly got a lot more mileage than they should
>> have. The fact that the forces of disinformation were able to get that
>> much mileage out of these two awful papers written by those clowns
>> should remain a real cause for concern.

>>

>> Their ability to repeatedly co-opt the Harvard news office remains a
>> real problem. Nobody I've talked to at Harvard is happy about this,
>> and there's been talk of action on the part of various of the faculty,
>> but nobody seems willing or able to mount enough of an effort to get
>> anything done about this. Apparently there was a threat of a lawsuit
>> against Harvard last time folks there tried to do something about
>> Baliunas, and so they may have lost their nerve. But I know our
>> Harvard colleagues are not happy about continually having their
>> institutional name dragged through the mud. If someone has close ties
>> w/ any individuals there who might be in a position to actually get
>> some action taken on this, I'd highly encourage pursuing this.

>>

>> Re, an NAS committee--this is an interesting idea. But I wonder if a
>> committee on BS would be overkill, perhaps giving these fools just the
>> stage that they're looking for. An alternative would be, as you say,
>> to take this on in the context of another more general NAS panel.
>> Coincidentally, there is already a panel on "Radiative Forcing

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>> Effects on Climate" which convenes this fall. I believe the panel
>> makeup is now in the public domain (or will be within days, on the NAS
>> website) so there's no secret here. I'm on the panel. Daniel Jacob
>> will be chairing it, and others on it are Jeff Kiehl, Francis Zwiers,
>> Roni Avissar, Judith Lean, Stuart Gaffin, Lynn Russell. Also on the
>> panel will be Ramanathan, Pielke Sr, Gerard Bond, Ulrike Lohmann, and
>> Hadi Dowlatabadi (whom I don't know). Its a somewhat odd makeup, and I
>> suspect that consensus will not be easy (there are at least a couple
>> obvious trouble spots), but there is certainly a core group of
>> reasonable folks on the panel, and this could be an opportunity to
>> clarify the state of the science on long-term forced variability
>> (including e.g. comparisons of model simulations and reconstructions
>> of the past 1000 years). This, at least indirectly, would deal w/ the
>> BS issue.

>>
>> I'm interested in the thoughts of others on any of the above.

>>
>> cheers,

>>
>> mike

>>
>> At 08:13 PM 7/23/2003 -0600, Tom Wigley wrote:

>>
>>> Folks,
>>>
>>> Here are some thoughts about the Soon issue, partly arising from
>>> talking to Ben.

>>>
>>> What is worrying is the way this BS paper has been hyped by various
>>> groups. The publicity has meant that the work has entered the
>>> consciousness of people in Congress, and is given prominence in some
>>> publications emanating from that sector. The work appears to have the
>>> imprimatur of Harvard, which gives it added credibility.

>>>
>>> So, what can we as a community do about this? My concerns are
>>> two-fold, and I think these echo all of our concerns. The first is
>>> the fact that the papers are simply bad science and the conclusions
>>> are incorrect. The second is that the work is being used quite openly
>>> for political purposes.

>>>
>>> As scientists, even though we are aware of the second issue, we need
>>> to concentrate on exposing the scientific flaws. We also need to do
>>> this in as authoritative a way as possible. I do not think it is
>>> enough to speak as individuals or even as a group of recognized
>>> experts. Even as a group, we will not be seen as having the 'power'
>>> of the Harvard stamp of approval.

>>>
>>> What I think is necessary is to have the expressed support of both
>>> AGU and AMS. It would also be useful to have Harvard disassociate
>>> themselves from the work. Most importantly, however, we need the NAS
>>> to come into the picture. With these 4 institutions, together with us
>>> (and others) as experts, pointing out clearly that the work is
>>> scientific rubbish, we can certainly win this battle.

>>>
>>> I suggest that we try to get NAS to set up a committee to (best
>>> option) assess the science in the two BS papers, or (less good, but
>>> still potentially very useful) assess the general issue of the paleo
>>> record for global- or hemispheric-scale temperature changes over the
>>> past 1000 years. The second option seems more likely to be acceptable
>>> to NAS. This is arguably an issue of similar importance to the issue
>>> of climate sensitivity uncertainties which NAS reviewed earlier this
>>> year (report still in preparation).

>>>

cg2003

>>> I am not sure how to fold AGU and AMS into this -- ideas are welcome.
>>> Similarly, perhaps some of you know some influential Harvard types
>>> better than I do and can make some suggestions here.

>>> The only way to counter this crap is to use the biggest guns we can
>>> muster. The Administration and Congress still seem to respect the NAS
>>> (even above IPCC) as a final authority, so I think we should actively
>>> pursue this path.

>>> Best wishes,
>>> Tom.

>>>

>>>

>>>

>>>

>>> Michael Oppenheimer wrote:

>>>

>>>> Dear All:

>>>> Since several of you are uncomfortable, it makes good sense to step
>>>> back and
>>>> think about a more considered approach. My view is that scientists
>>>> are fully
>>>> justified in taking the initiative to explain their own work and its
>>>> relevance in
>>>> the policy arena. If they don't, others with less scruples will be
>>>> heard

>>>> instead. But each of us needs to decide his or her own comfort zone.
>>>> In this case, the AGU press release provides suitable context, so it
>>>> may be that

>>>> neither a separate letter nor another AGU statement would add much
>>>> at this time.

>>>> But this episode is unlikely to be the last case where clarity from
>>>> individuals

>>>> or groups of scientists will be important.

>>>> Michael

>>>>

>>>> Tom Wigley wrote:

>>>>

>>>>

>>>>> Folks,

>>>>>

>>>>> I am inclined to agree with Peck. Perhaps a little more thought and
>>>>> time

>>>>> could lead to something with much more impact?

>>>>>

>>>>> Tom.

>>>>>

>>>>>

>>>>> Jonathan Overpeck wrote:

>>>>>

>>>>>

>>>>>> Hi all - I'm not too comfortable with this, and would rather not
>>>>>> sign -

>>>>>> at least not without some real time to think it through and debate
>>>>>> the

>>>>>> issue. It is unprecedented and political, and that worries me.

>>>>>>

>>>>>> My vote would be that we don't do this without a careful
>>>>>> discussion first.

>>>>>>

>>>>>> I think it would be more appropriate for the AGU or some other

cg2003

>>>>> scientific org to do this - e.g., in reaffirmation of the AGU
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>>>>> (or whatever it's called) on global climate change.
>>>>>
>>>>> Think about the next step - someone sends another letter to the
>>>>> Senators, then we respond, then...
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>>>>> what are the precedents and outcomes of similar actions? I can
>>>>> imagine a
>>>>> special-interest org or group doing this like all sorts of other
>>>>> political actions, but is it something for scientists to do as
>>>>> individuals?
>>>>>
>>>>> Just seems strange, and for that reason I'd advise against doing
>>>>> anything with out real thought, and certainly a strong majority of
>>>>> co-authors in support.
>>>>>
>>>>> Cheers, Peck
>>>>>
>>>>>
>>>>>
>>>>>
>>>>> Dear fellow Eos co-authors,
>>>>>
>>>>> Given the continued assault on the science of climate change by some
>>>>> on Capitol Hill, Michael and I thought it would be worthwhile to
>>>>> send
>>>>> this letter to various members of the U.S. Senate, accompanied by a
>>>>> copy of our Eos article.
>>>>>
>>>>> Can we ask you to consider signing on with Michael and me (providing
>>>>> your preferred title and affiliation). We would like to get this
>>>>> out ASAP.
>>>>>
>>>>> Thanks in advance,
>>>>>
>>>>> Michael M and Michael O
>>>>>
>>>>> _____
>>>>> Professor Michael E. Mann
>>>>> Department of Environmental Sciences, Clark Hall
>>>>> University of Virginia
>>>>> Charlottesville, VA 22903
>>>>> _____
>>>>>
>>>>> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434)
>>>>> 982-2137
>>>>> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
>>>>>
>>>>> Attachment converted: Macintosh HD:EOS.senate letter-final.doc
>>>>> (WDBN/MSWD) (00055FCF)
>>>>>
>>>>>
>>>>>
>>>>> --
>>>>>
>>>>> Jonathan T. Overpeck

cg2003

>>>>> Director, Institute for the Study of Planet Earth
>>>>> Professor, Department of Geosciences
>>>>>
>>>>> Mail and Fedex Address:
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>>>>> direct tel: +1 520 622-9065
>>>>> fax: +1 520 792-8795
>>>>> http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
>>>>> <http://www.ispe.arizona.edu/>
>>>>>
>>>
>>>
>>
>> _____
>> Professor Michael E. Mann
>> Department of Environmental Sciences, Clark Hall
>> University of Virginia
>> Charlottesville, VA 22903
>> _____
>> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
>> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
>>
</x-flowed>

2872. 2003-07-24

date: Thu, 24 Jul 2003 13:56:39 +0100
from: "Bruce Tofield" <b.tofield@uea.ac.uk>
subject: Biofuels meeting
to: "Mike Hulme" <m.hulme@uea.ac.uk>

Mike

If you could extend me an invitation to the biofuels meeting I would be most grateful.

Are there any UK depts addressing lignocellulosic conversion to ethanol in a major way?

Many thanks

Bruce

Dr Bruce Tofield

Innovation and change, CRed

Tel: 01603-592583

mob: 07787-512556

e: [[1](mailto:b.tofield@uea.ac.uk)]b.tofield@uea.ac.uk

3321. 2003-07-24

date: Thu Jul 24 13:57:44 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Holocene manuscript- sorry
to: Phil Camill <pcamill@carleton.edu>

Phil

I am really sorry for the delay , that was a result of initially tardy reviewers, my

subsequent illness, and then a delay while John is away while I wished to consult with him

. The reason for the last wish will be clear when you read the reviews below.

The referees are not enthusiastic and in their private comments to me one is strongly

negative and the other ambivalent. The pressure on space means that this would normally be

a rejection (and we have, since your submission ,developed new , stricter rules regarding possible re-submission.)

However, in the circumstances (the delay that is down to me) , I am overruling these and

(despite not discussing it with John) asking you to read these reviews and come back with a

frank opinion of whether you consider them fair and the paper publishable with some work .

I am doing this because I believe it is. If you can answer these remarks and feel you can

submit a valid manuscript that accounts for them - I will review your argument (without

recourse to the reviewers) and if I agree , I guarantee speedy process through the last

publication phase.

Again , you and I are well aware that this manuscript could have been dealt with much

better and I am really sorry for it.

Keith

REVIEWERS REMARKS FOLLOW

Referee 1

Review of David Hunzicker and Phil Camill: "Using a new 672-year tree-ring drought

reconstruction from westcentral Montana" submitted to the Holocene.

This is a well written, well executed paper that I would unfortunately not recommend for

publication in the Holocene. It's a shame to read a paper like this. It is very well

informed, well referenced, places the work in a good scientific context, and includes

strong statistical analyses. However, the attention paid to the analyses and interpretation

of the reconstruction was evidently not paid so carefully to the fundamental tree-ring

chronology development. They call it "crossdating," but the best I can tell from the

limited discussion it was simply computerized correlation matching of measured time series,

with a massive culling of the data to pare down to those time series that produced

straightforward correlations in a COFECHA analysis. I was astounded to read that their

final chronology used only 61 out of the 152 trees sampled for the study. The 60% of the

trees not included apparently suffered from "complacency, unresolvable sections of missing

rings, or low interseries correlation values." This appears to be the first penalty for not

applying rigorous dendrochronological methods to the chronology development. I find it

incredible that over half of the Ponderosa pine samples would not be useful. I can't help

but suspect that by relying on COFECHA output, without any hard-nosed microscope work and

rigorous crossdating with the wood samples themselves, you at best default to the simple,

straightforward trees without missing rings. That is, you default to a less climatically

sensitive subset of trees. This appears to be the second penalty for the seemingly

inexpert, quick and dirty chronology development.

These authors have obviously worked hard on this study and bring excellent

analytical

skills and knowledge of the literature. The paper itself is exceptionally well written

(with a minor complaint concerning the over use, and at times incorrect use of the term

"teleconnection"). But the calibration and validation reported in the paper are clearly

awful, and that surely ought not be the case for Ponderosa pine on moisture-stressed sites

in Montana. One hates to be non-supportive of their work, so much of which is high quality,

but it seems to come down to fundamentals, and here the fundamental dendrochronology and

chronology development are in question. And I also do not think it advisable to publish a

reconstruction that explains maybe 21% of the variance in the instrumental climate data,

when using an arid site conifer as the predictor (the persistence in the standard

chronology may be inflating even that figure). I just can't believe the calibration could

be so weak. It seems they need to revisit their chronology development work, and dig deeper

into the climate response of their chronology. Then look very carefully at climate data

itself. These climate data are not guaranteed to be homogeneous, especially in the mountain

west during the early 20th century. If all this could be done, and if the variance

explained in both the calibration and verification periods could be improved, then

publication in the Holocene would be well justified.

Referee 2

Review of "Using a New 672-Year Tree-Ring Drought Reconstruction from West-Central Montana

to Evaluate Severe Drought Teleconnections in the Western U.S. and Possible Climatic

Forcing by the Pacific Decadal Oscillation" by D.A. Hunzicker and P. Camill

This paper is reasonably well written, but has some problems in it that bother me. The

first issue relates to the tree-ring chronology that was developed at Lindberg Lake.

Anytime less than half of the core samples (61 or 152) are used in developing a chronology,

this is cause for concern. The fact that there are "unresolvable sections of missing rings"

(p. 10) can mean a lot of things. However, ponderosa pine is known to cross-date well,

which includes "locating" locally-absent rings during the cross-dating phase, so it is

surprising that the authors have chosen not to work through these problems. Presumably, the

trees with missing rings are also those most sensitive to drought, so isn't there a chance

that the chronology being analyzed in this paper is less sensitive to drought than it ought

to be? I also wonder how much their chronology is truly contributing to the overall stated

goal of this paper, i.e. evaluating "Severe Drought Teleconnections in the Western U.S. and

Possible Climatic Forcing by the Pacific Decadal Oscillation". The authors extensively use

the PDSI reconstructions of Cook et al. (1999) in their analyses. Aside from the

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increased

length of their new tree-ring chronology, what does it contribute that was not possible simply by using the Cook et al. reconstructions to test for teleconnections and forcing.

None of the indices of forcing (ENSO, PDO, sunspots) extend back before the beginning of

the Cook et al. reconstructions, so there is little to be gained in using one longer series

from west-central Montana in this analysis. One could point to Fig. 3, which compares the

MT reconstruction vs the SWDI series. But even this comparison is limited in its overall

contribution to the paper. I also don't like the use of the FFT for estimating power

spectra, even if the confidence limits are determined by bootstrapping. The power spectra

calculated by the FFT are still inconsistent estimates. A more contemporary and consistent

method of spectral estimation, like the Multi-Taper Method, should be used.

For the reasons stated above, I do not consider this paper to be ready for publication as

is. I will leave it to the Editor to decide how to proceed with it past this point.

At 10:33 PM 7/23/03 -0500, you wrote:

Dear Keith,

I have not yet received an editorial response or reviews for the manuscript entitled "Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western US and possible climatic forcing by the Pacific Decadal Oscillation" by Hunzicker and Camill. This manuscript has been in review for 14 months. Can you indicate when I can expect these materials?

Many thanks,

Phil

Dr. Phil Camill

Assistant Professor of Biology and Environmental Studies

Carleton College, Department of Biology

One North College St.

Northfield, MN 55057

phone: (507) 646-5643

fax: (507) 646-5757

--

Professor Keith Briffa,

Climatic Research Unit

University of East Anglia

Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

3680. 2003-07-24

cc: Jonathan Overpeck <jto@u.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>,

ethompso@magnus.acs.ohio-state.edu

date: Thu, 24 Jul 2003 09:38:40 -0700

from: Jonathan Overpeck <jto@u.arizona.edu>

subject: Re: letter to Senate

cg2003

to: Michael Oppenheimer <omichael@Princeton.EDU>
<x-flowed>

Also makes sense... seems like things are getting done on multiple fronts. Sounds like your NAS comm., Mike, is the "weight in on solar forcing" committee. That explains the make up in my mind. Have fun... the NAS word on this issue will be important, and relevant to the issues (and papers) we've been debating.

cheers, peck

>Tom:

>

>I wasn't suggesting that AGU get involved...I agree that it's

>neither necessary nor

>a good use of AGU at this point. Rather, both our paper and the AGU

>press release

>are already public documents, so those in Washington with an

>interest in this, like

>my former colleagues, will likely just put the two together and

>circulate them on

>the Hill.

>

>Michael

>

>Tom Crowley wrote:

>

>> this has gotten too complicated.....

>>

>> I doubt AGU is going to contact the senators on this officially - I

>> am not sure they should. why doesn't Mike M., as the senior author

>> on the paper, just send an information copy to John McCain -

>> practically the only Republican who has power and still seems

>> somewhat sane. Mike does not need the permission of either his

>> co-authors or AGU to do this - it is simply an information item as a

>> point of note.

>>

>> Tom

>>

>> >Dear All:

>> >

>> >Since several of you are uncomfortable, it makes good sense to

>>step back and

>> >think about a more considered approach. My view is that

>>scientists are fully

>> >justified in taking the initiative to explain their own work and its

>> >relevance in

>> >the policy arena. If they don't, others with less scruples will be heard

>> >instead. But each of us needs to decide his or her own comfort zone.

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>> >In this case, the AGU press release provides suitable context, so it

>> >may be that

>> >neither a separate letter nor another AGU statement would add much

>> >at this time.

>> >But this episode is unlikely to be the last case where clarity from

>> >individuals

>> >or groups of scientists will be important.

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>> >Michael

>> >

>> >

>> >

>> >Tom Wigley wrote:

>> >

>> >> Folks,

>> >>

cg2003

>> >> I am inclined to agree with Peck. Perhaps a little more thought and time
>> >> could lead to something with much more impact?

>> >>
>> >> Tom.

>> >>
>> >> Jonathan Overpeck wrote:

>> >> > Hi all - I'm not too comfortable with this, and would rather
>>not sign -

>> >> > at least not without some real time to think it through and debate the
>> >> > issue. It is unprecedented and political, and that worries me.

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>>discussion first.

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>> >> > scientific org to do this - e.g., in reaffirmation of the

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>> >> > political actions, but is it something for scientists to do as
>> >>individuals?

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>> >> > Just seems strange, and for that reason I'd advise against doing
>> >> > anything with out real thought, and certainly a strong majority of
>> >> > co-authors in support.

>> >> >
>> >> > Cheers, Peck

>> >> >
>> >> >
>> >> >
>> >> >> Dear fellow Eos co-authors,

>> >> >> Given the continued assault on the science of climate change by some
> > >> >> on Capitol Hill, Michael and I thought it would be
>worthwhile to send

>> >> >> this letter to various members of the U.S. Senate, accompanied by a
>> > >> copy of our Eos article.

>> > >>
>> > >> Can we ask you to consider signing on with Michael and me (providing
>> > > >> your preferred title and affiliation). We would like to get
>> >this out ASAP.

>> >> >>
>> >> >> Thanks in advance,

>> >> >> Michael M and Michael O

>> >> >
>> >> >> _____
>> >> >> Professor Michael E. Mann
>> >> >> Department of Environmental Sciences, Clark Hall
>> >> >> University of Virginia
>> >> >> Charlottesville, VA 22903

>> >> >>
>>

cg2003
 >> >> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX:
 >>(434) 982-2137
 >> >> http://www.evsc.virginia.edu/faculty/people/mann.shtml
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 >> >> >
 >> >> > Jonathan T. Overpeck
 >> >> > Director, Institute for the Study of Planet Earth
 >> >> > Professor, Department of Geosciences
 >> >> > Mail and Fedex Address:
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 >> >> > Institute for the Study of Planet Earth
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 >> >> > direct tel: +1 520 622-9065
 >> >> > fax: +1 520 792-8795
 >> >> > http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
 >> >> > http://www.ispe.arizona.edu/
 >> >
 >> >Content-Type: text/x-vcard; charset=us-ascii;
 >> > name="omichael.vcf"
 >> >Content-Transfer-Encoding: 7bit
 >> >Content-Description: Card for Michael Oppenheimer
 >> >Content-Disposition: attachment;
 >> > filename="omichael.vcf"
 >> >
 >> >Attachment converted: Macintosh HD:omichael 2.vcf (TEXT/ttxt) (0001BAD6)
 >>
 >> --
 >> Thomas J. Crowley
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 >> Dept. of Earth and Ocean Sciences
 >> Nicholas School of the Environment and Earth Sciences
 >> Box 90227
 >> 103 Old Chem Building Duke University
 >> Durham, NC 27708
 >>
 >> tcrowley@duke.edu
 >> 919-681-8228
 >> 919-684-5833 fax
 >>
 >> >Content-Type: text/x-vcard; charset=us-ascii;
 >> > name="omichael.vcf"
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 >> --
 >> Jonathan T. Overpeck
 >> Director, Institute for the Study of Planet Earth
 >> Professor, Department of Geosciences
 >> Mail and Fedex Address:
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cg2003

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http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
<http://www.ispe.arizona.edu/>
</x-flowed>

1431. 2003-07-25

date: Fri Jul 25 18:06:55 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: FW: EU funding
to: "Xianfu Lu" <xianfu@waikato.ac.nz>

Xianfu and Dick,
Indeed we are aware of this! It is the 2nd call of a three call programme under FP6. This has been exercising us considerably over the last 18 months, and the political fall out across Europe of these FP6 actions is considerable - and too complex to go into. At this stage, Tyndall is unlikely to be leading any bids (by the way, these are much, much bigger projects than conventional under FP5, FP4, etc.), but may have a role in a bid on innovative tools for sustainability (impact) assessments. This seems quite a specialist area the Commission are looking for. Alex Haxeltine is co-ordinating Tyndall's input into this, so if you think you have a role to offer then contact him in the first place. There may be a 3rd call topic more closely aligned to integrated climate change assessment, due out early 2004.

Cheers,
Mike

At 18:05 25/07/2003 +1200, you wrote:

Mike,
Below is a message from the University research coordinator. Are you aware of this? Dick was wondering whether there is any scope for IGCI to participate in any proposal(s) by Tyndall-led consortium. One possible area of research could be on the development of integrated modelling tools for climate change research. Any thoughts on this?

Thanks,
Xianfu

-----Original Message-----

From: Richard Bedford [[1]mailto:rdb@waikato.ac.nz]
Sent: 25 July 2003 06:40
To: r.warrick@waikato.ac.nz; n.ericksen@waikato.ac.nz
Subject: EU funding
Dick and Neil

Not sure whether you have seen this information on EU funding:

13. CALL FOR PROPOSALS UNDER EU FRAMEWORK PROGRAMME 6

A call for proposals on Global Change and Ecosystems research has been made under the EU Framework Programme 6. Research proposals could relate to: impact of greenhouse gases and atmospheric pollutants on climate, ozone depletion and carbon sinks; water cycle, biodiversity and ecosystems, desertification and natural disasters, sustainable land management, forecasting and modelling, including global climate change; complementary and cross-cutting research. There are 180 million euros available in the fund which closes on 9 October 2003. Proposals must be submitted by a European consortium of

cg2003

researchers, but may include non-European third parties which offers opportunities for New Zealand to become involved in this research initiative.

Information on the specific call for proposals and potential partners is available at [2]http://fp6.cordis.lu/fp6/call_details.cfm?CALL_ID=78

Cheers

Dick

--

Professor R. D. Bedford FRSNZ
Deputy Vice-Chancellor (Research)
University of Waikato
P.B. 3105
Hamilton
NEW ZEALAND
Phone: (64-7) 838-4770
Fax: (64-7) 838-4538
e-mail: rdb@waikato.ac.nz

4218. 2003-07-25

cc: CR-editors@int-res.com, CR-revieweditors@int-res.com, kinne@int-res.com

date: Fri, 25 Jul 2003 11:33:34 -0400

from: Bob Davis <red3u@virginia.edu>

subject: Re: Draft CR editorial

to: Hans.von.Storch@gkss.de

Dear Hans:

It has yet to be demonstrated to me that there is any problem. A paper has been published that some people disagree with...the authors have responded. Isn't this the nature of the same scientific process that has worked just fine for centuries? Many papers have been published with which I have disagreed, but I never viewed the "process" to be flawed. Honest scientists have differences of opinion. That is clearly the case here. You should know that I know the parties on BOTH sides of this particular issue and am not taking sides.

I cannot agree with your editorial since, in my view, there is no problem with the peer-review process. Otto Kinne has already written what I feel is the appropriate, and the only necessary, response. You can send that to the Congressional staffer. There is no need for any additional response on our part, and to do so seriously undermines the integrity of this journal in the science community.

Regards,

Bob Davis

>Folks,

>

>if there shall be an editorial in the next issue of Climate Research, this
>editorial must be completed until Monday noon time. It would be about 1
>page, not more than two. Not much time, but I think we should try it. This
>editorial would also be sent to this person from the US senate who was
>inquiring about the review process at CR. I have prepared a draft now, and
>I ask you to read it and come up with constructive comments.

>

>For me it is important that we admit that the result of the review process
>of Soon & Baliunas was insufficient, without "damaging" the responsible
>editor. We should have been more vigilant after we had seen that actually
>two critical comments were written on the first Soon paper.

>

>On the other hand I want to avoid the perception that we would police
>controversial articles. Quite the contrary, we should be proud of having
>such articles, but it should be made explicit that the material IS
>controversial and that other quarters look at the evidence differently.
>One way of doing so would be to invite comments to be published together
>with the original article.

>

cg2003

>Obviously, English is not my native language. I am sure that some helpful
>people at Inter-Research will help me to straighten out my clumsy
>formulations - but I would appreciate your help also in this respect.

>
>Regards

>
>Hans
>Editor-in-Chief, Climate Research

>
>-----
>Hans von Storch; Institute for Coastal Research, GKSS Research Centre,
>Geesthacht, Germany
><http://w3g.gkss.de/staff/storch> storch@gkss.de
>ph: + 49 4152 87 1831, mobile +49 171 212 2046 fx +49 4152 87 2832
>presently: Kaspervej 2, 4673 Roedvig, Denmark, ph 0045 5650 6760

>
>
>-----
>
>Draft editorial

>
>Until now, Climate Research had a rather liberal procedure of processing
>submitted manuscripts. A group of several editors operated independently.
>Manuscripts dealing with 'Äbasic and applied research devoted to all
>aspects of climate - present, past and future; effects of human societies
>and organisms on climate; effects of climate on the ecosphere.'Ä were and
>are welcome. Before publication they were subjected to a formal
>peer-review: 'ÄManuscripts are critically evaluated by at least 3
>reviewers. The editor decides on acceptance or rejection. Acceptable
>manuscripts are usually returned to the author for consideration of
>comments and criticism.'Ä (
><http://www.int-res.com/journals/misc/instruct.html>) This approach worked
>out mostly fine, with a broad range of interesting and good articles. In
>fact, CR has managed to become a leading journal in interdisciplinary
>climate research.

>
>However, in recent months the procedure did function less well. In
>particular one article, by Soon and Baliunas (CR 23: 89-110), has caused
>considerable discussion. In fact, it was not the first article by these
>authors, which was perceived by different readers as methodically
>questionable (CR 18:259-275; CR 22:185-186/177-188; CR24:91-92/ 93-94).
>Also the recent article drew severe critique, which was made public by a
>thorough analysis of the results in the Transaction of the AGU, EOS (vol
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>presented in that paper. The statement itself may be true, but the
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>required quality control of the review process. In particular, when
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>publisher have decided to change the routine. In particular the office of
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>I have been asked to take on the responsibility as Editor-in-Chief of
>Climate Research and I have accepted per 1. August 2003. An immediate
>consequence is that authors are requested to send manuscripts to the
>Editor-in-Chief; requests of authors to have their manuscript processed by

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>a specific editor are welcome, but are not necessarily fulfilled.
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>truth. In fact climate science is to some extent a social process, with
>many extra-scientific influences. Climate science is definitely in a
>postnormal stage, and we have to make sure that publications are not just
>reconfirming preconceived concepts, or concepts we have gotten to be used
>of. Ludwig's Fleck remarkable analysis 'Genesis and Development of a
>Scientific Fact' describes this syndrome, which eventually leads to a
>dogmatization and stand-still of science. Thus, we need a certain level of
>liberalism. Articles must be allowed to present additional to its hard,
>and reproducible facts a certain amount of creative speculation. However,
>papers must be explicit where facts end and where fantasy begins.
>Hans von Storch, 24 July 2003

Robert E. Davis
Associate Professor and
Chair, Faculty Senate
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Charlottesville, Virginia 22904-4123
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5025. 2003-07-25

cc: Hans.von.Storch@gkss.de, CR-editors@int-res.com, CR-revieweditors@int-res.com,
kinne@int-res.com

date: Fri, 25 Jul 2003 14:01:23 -0400

from: Vernon Meentemeyer <vmeente@uga.edu>

subject: Re: Draft CR editorial

to: Bob Davis <red3u@virginia.edu>

Hello all: I am Vern Meentemeyer, a former regional editor for CR. In a few words, I want to say that I agree with Bob. The process has worked and just because some people disagree with the conclusions of the Soons and Baliunas paper is not sufficient reason for a long explanation, or more rules and restrictions placed on the editors. Let's use caution and avoid overreaction. Best.....

Bob Davis wrote:

> Dear Hans:

>

> It has yet to be demonstrated to me that there is any problem. A paper has
> been published that some people disagree with...the authors have responded.
> Isn't this the nature of the same scientific process that has worked just
> fine for centuries? Many papers have been published with which I have
> disagreed, but I never viewed the "process" to be flawed. Honest
> scientists have differences of opinion. That is clearly the case here.
> You should know that I know the parties on BOTH sides of this particular
> issue and am not taking sides.

>

> I cannot agree with your editorial since, in my view, there is no problem
> with the peer-review process. Otto Kinne has already written what I feel
> is the appropriate, and the only necessary, response. You can send that to
> the Congressional staffer. There is no need for any additional response on
> our part, and to do so seriously undermines the integrity of this journal
> in the science community.

>

> Regards,

>

> Bob Davis

>

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> >Folks,
> >
> >if there shall be an editorial in the next issue of Climate Research, this
> >editorial must be completed until Monday noon time. It would be about 1
> >page, not more than two. Not much time, but I think we should try it. This
> >editorial would also be sent to this person from the US senate who was
> >inquiring about the reivew process at CR. I have prepared a draft now, and
> >I ask you to read it and come up with constructive comments.
> >
> >For me it is important that we admit that the result of the review process
> >of Soon & Baliunas was insufficient, without "damaging" the reponsible
> >editor. We should have been more vigilant after we had seen that actually
> >two critical comments were written on the first Soon paper.
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> >On the other hand I want to avoid the perception that we would police
> >controversial articles. Quite the contrary, we should be proud of having
> >such articles, but it should be made explicit that the material IS
> >controversial and that other quarters look at the evidence differently.
> >One way of doing so would be to invite comments to be published together
> >with the original article.
> >
> >Obviously, English is not my native language. I am sure that some helpful
> >people at Inter-Research will help me to straighthen nout to clumsy
> >formulations - but I would appreciate aour help also in this respect.
> >
> >Regards
> >
> >Hans
> >Editor-in-Chief, Climate Research
> >
> >-----
> >Hans von Storch; Institute for Coastal Research, GKSS Research Centre,
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> >ph: + 49 4152 87 1831, mobile +49 171 212 2046 fx +49 4152 87 2832
> >presently: Kaspervej 2, 4673 Roedvig, Denmark, ph 0045 5650 6760
> >
> >
> >-----
> >
> >Draft editorial
> >
> >Until now, Climate Research had a rather liberal procedure of processing
> >submitted manuscripts. A group of several editors operated independently.
> >Manuscripts dealing with 'Äbasic and applied research devoted to all
> >aspects of climate - present, past and future; effects of human societies
> >and organisms on climate; effects of climate on the ecosphere.'Ä were and
> >are welcome. Before publication they were subjected to a formal
> >peer-review: 'ÄManuscripts are critically evaluated by at least 3
> >reviewers. The editor decides on acceptance or rejection. Acceptable
> >manuscripts are usually returned to the author for consideration of
> >comments and criticism.'Ä (
> ><http://www.int-res.com/journals/misc/instruct.html>) This approach worked
> >out mostly fine, with a broad range of interesting and good articles. In
> >fact, CR has managed to become a leading journal in interdisciplinary
> >climate research.
> >
> >However, in recent months the procedure did function less well. In
> >particular one article, by Soon and Baliunas (CR 23: 89-110), has caused
> >considerable discussion. In fact, it was not the first article by these
> >authors, which was perceived by different readers as methodically
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> >Soon and Baliunas paper 'ÁAcross the world, many records reveal that the
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>
> -----
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2092. 2003-07-26

cc: Michael Oppenheimer <omichael@Princeton.EDU>, "Michael E. Mann"
<mann@virginia.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Caspar M Ammann

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<ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn
<t.osborn@uea.ac.uk>, Ben Santer <santer1@llnl.gov>, Steve Schneider
<shs@stanford.edu>

date: Sat, 26 Jul 2003 11:55:21 -0600 (MDT)
from: Kevin Trenberth <trenbert@cgd.ucar.edu>
subject: Re: letter to Senate
to: Tom Wigley <wigley@ucar.edu>

Hi all:

I have been off for a few days actually on vacation and find an avalanche of emails on this.

Just wrt the NRC/NAS: they will not carry out a study usually unless it is funded somehow and that usually means it is requested and paid for by the feds or others. Good ideas are not enough.

Kevin

On Thu, 24 Jul 2003, Tom Wigley wrote:

> Michael,

>

> So we need to push for my second option -- a more general paleo review
> by NAS/NRC. There are already and soon to come such reviews, but this is
> not the same thing. NAS would rely on and also critique this literature
> to some degree (this literature includes BS).

>

> I agree that the Eos piece serves an extremely useful purpose, but it is
> too technical for politicians. It is, however, perfect for us when we
> need published support for our communications with the press and others.

>

> Re involving AMS, AGU and AAAS, I don't know how to do this. Any ideas?

>

> Tom.

> ++++++

>

> Michael Oppenheimer wrote:

> > Tom, Mike et al:

> >

> > 1. Making the S B papers the sole or main subject of an NRC committee
> > would be a mistake.

> >

> > 2. But dispensing of them as a minor part of an NRC examination of
> > paleoclimate makes sense. Some of you may recall the Idso, Newell
> > contratemps of 20 years ago, and as I recall, this is how it was handled.

> >

> > 3. For the near term, the rebuttal paper in Eos is a terrific example of
> > what can and should be done in such circumstances, and the AGU press
> > release is more than I would have expected. We've provided all the
> > necessary ammunition. The best you can do now is be responsive if
> > reporters or Congressional staff call.

> >

> > 4. For the long haul, in addition to the NRC committee route, some
> > thought needs to be given to more formal ways to respond to such
> > situations, which I expect to continue to arise indefinitely. This is
> > one role for IPCC and NRC, but both are painfully slow. Perhaps AGU and
> > AMS and AAAS need to see their roles as partly to provide a venue for
> > such clarifications. The key this time was rapid turnover. Maybe Don
> > Kennedy and Science could be engaged in this somehow.

> >

> > Michael

> >

> >

> > "Michael E. Mann" wrote:

> >

> >> Tom,

> >>
> >> Thanks for your email, and your (and Ben's) thoughtful comments on all
> >> of this...
> >>
> >> I think the Eos piece has gone a long way to discrediting the
> >> 'science' behind the "BS" papers (well, technically, "SB", but I
> >> prefer the reverse order too). The paper Phil and I have in press in
> >> GRL (hopefully to appear within a few weeks now) will reinforce this.
> >> But the BS papers certainly got a lot more mileage than they should
> >> have. The fact that the forces of disinformation were able to get that
> >> much mileage out of these two awful papers written by those clowns
> >> should remain a real cause for concern.
> >>
> >> Their ability to repeatedly co-opt the Harvard news office remains a
> >> real problem. Nobody I've talked to at Harvard is happy about this,
> >> and there's been talk of action on the part of various of the faculty,
> >> but nobody seems willing or able to mount enough of an effort to get
> >> anything done about this. Apparently there was a threat of a lawsuit
> >> against Harvard last time folks there tried to do something about
> >> Baliunas, and so they may have lost their nerve. But I know our
> >> Harvard colleagues are not happy about continually having their
> >> institutional name dragged through the mud. If someone has close ties
> >> w/ any individuals there who might be in a position to actually get
> >> some action taken on this, I'd highly encourage pursuing this.
> >>
> >> Re, an NAS committee--this is an interesting idea. But I wonder if a
> >> committee on BS would be overkill, perhaps giving these fools just the
> >> stage that they're looking for. An alternative would be, as you say,
> >> to take this on in the context of another more general NAS panel.
> >> Coincidentally, there is already a panel on "Radiative Forcing
> >> Effects on Climate" which convenes this fall. I believe the panel
> >> makeup is now in the public domain (or will be within days, on the NAS
> >> website) so there's no secret here. I'm on the panel. Daniel Jacob
> >> will be chairing it, and others on it are Jeff Kiehl, Francis Zwiers,
> >> Roni Avissar, Judith Lean, Stuart Gaffin, Lynn Russell. Also on the
> >> panel will be Ramanathan, Pielke Sr, Gerard Bond, Ulrike Lohmann, and
> >> Hadi Dowlatabadi (whom I don't know). Its a somewhat odd makeup, and I
> >> suspect that consensus will not be easy (there are at least a couple
> >> obvious trouble spots), but there is certainly a core group of
> >> reasonable folks on the panel, and this could be an opportunity to
> >> clarify the state of the science on long-term forced variability
> >> (including e.g. comparisons of model simulations and reconstructions
> >> of the past 1000 years). This, at least indirectly, would deal w/ the
> >> BS issue.
> >>
> >> I'm interested in the thoughts of others on any of the above.
> >>
> >> cheers,
> >>
> >> mike
> >>
> >> At 08:13 PM 7/23/2003 -0600, Tom Wigley wrote:
> >>
> >>> Folks,
> >>>
> >>> Here are some thoughts about the Soon issue, partly arising from
> >>> talking to Ben.
> >>>
> >>> What is worrying is the way this BS paper has been hyped by various
> >>> groups. The publicity has meant that the work has entered the
> >>> consciousness of people in Congress, and is given prominence in some
> >>> publications emanating from that sector. The work appears to have the
> >>> imprimatur of Harvard, which gives it added credibility.

> >>>
> >>> So, what can we as a community do about this? My concerns are
> >>> two-fold, and I think these echo all of our concerns. The first is
> >>> the fact that the papers are simply bad science and the conclusions
> >>> are incorrect. The second is that the work is being used quite openly
> >>> for political purposes.
> >>>
> >>> As scientists, even though we are aware of the second issue, we need
> >>> to concentrate on exposing the scientific flaws. We also need to do
> >>> this in as authoritative a way as possible. I do not think it is
> >>> enough to speak as individuals or even as a group of recognized
> >>> experts. Even as a group, we will not be seen as having the 'power'
> >>> of the Harvard stamp of approval.
> >>>
> >>> What I think is necessary is to have the expressed support of both
> >>> AGU and AMS. It would also be useful to have Harvard disassociate
> >>> themselves from the work. Most importantly, however, we need the NAS
> >>> to come into the picture. With these 4 institutions, together with us
> >>> (and others) as experts, pointing out clearly that the work is
> >>> scientific rubbish, we can certainly win this battle.
> >>>
> >>> I suggest that we try to get NAS to set up a committee to (best
> >>> option) assess the science in the two BS papers, or (less good, but
> >>> still potentially very useful) assess the general issue of the paleo
> >>> record for global- or hemispheric-scale temperature changes over the
> >>> past 1000 years. The second option seems more likely to be acceptable
> >>> to NAS. This is arguably an issue of similar importance to the issue
> >>> of climate sensitivity uncertainties which NAS reviewed earlier this
> >>> year (report still in preparation).
> >>>
> >>> I am not sure how to fold AGU and AMS into this -- ideas are welcome.
> >>> Similarly, perhaps some of you know some influential Harvard types
> >>> better than I do and can make some suggestions here.
> >>>
> >>> The only way to counter this crap is to use the biggest guns we can
> >>> muster. The Administration and Congress still seem to respect the NAS
> >>> (even above IPCC) as a final authority, so I think we should actively
> >>> pursue this path.
> >>>
> >>> Best wishes,
> >>> Tom.
> >>>
> >>>
> >>>
> >>>
> >>>
> >>>
> >>> Michael Oppenheimer wrote:
> >>>
> >>>> Dear All:
> >>>> Since several of you are uncomfortable, it makes good sense to step
> >>>> back and
> >>>> think about a more considered approach. My view is that scientists
> >>>> are fully
> >>>> justified in taking the initiative to explain their own work and its
> >>>> relevance in
> >>>> the policy arena. If they don't, others with less scruples will be
> >>>> heard
> >>>> instead. But each of us needs to decide his or her own comfort zone.
> >>>> In this case, the AGU press release provides suitable context, so it
> >>>> may be that
> >>>> neither a separate letter nor another AGU statement would add much
> >>>> at this time.

> >>>> But this episode is unlikely to be the last case where clarity from
> >>>> individuals
> >>>> or groups of scientists will be important.
> >>>> Michael

> >>>> Tom Wigley wrote:

> >>>> Folks,
> >>>> I am inclined to agree with Peck. Perhaps a little more thought and
> >>>> time
> >>>> could lead to something with much more impact?

> >>>> Tom.

> >>>> Jonathan Overpeck wrote:

> >>>> Hi all - I'm not too comfortable with this, and would rather not
> >>>> sign -
> >>>> at least not without some real time to think it through and debate
> >>>> the
> >>>> issue. It is unprecedented and political, and that worries me.

> >>>> My vote would be that we don't do this without a careful
> >>>> discussion first.

> >>>> I think it would be more appropriate for the AGU or some other
> >>>> scientific org to do this - e.g., in reaffirmation of the AGU
> >>>> statement
> >>>> (or whatever it's called) on global climate change.

> >>>> Think about the next step - someone sends another letter to the
> >>>> Senators, then we respond, then...

> >>>> I'm not sure we want to go down this path. It would be much better
> >>>> for
> >>>> the AGU etc to do it.

> >>>> What are the precedents and outcomes of similar actions? I can
> >>>> imagine a
> >>>> special-interest org or group doing this like all sorts of other
> >>>> political actions, but is it something for scientists to do as
> >>>> individuals?

> >>>> Just seems strange, and for that reason I'd advise against doing
> >>>> anything with out real thought, and certainly a strong majority of
> >>>> co-authors in support.

> >>>> Cheers, Peck

> >>>>>> Dear fellow Eos co-authors,

> >>>>>> Given the continued assault on the science of climate change by some
> >>>>>> on Capitol Hill, Michael and I thought it would be worthwhile to
> >>>>>> send
> >>>>>> this letter to various members of the U.S. Senate, accompanied by a

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> >>>>>> copy of our Eos article.
> >>>>>>
> >>>>>> Can we ask you to consider signing on with Michael and me (providing
> >>>>>> your preferred title and affiliation). We would like to get this
> >>>>>> out ASAP.

> >>>>>>
> >>>>>> Thanks in advance,
> >>>>>>
> >>>>>> Michael M and Michael O

> >>>>>>
> >>>>>> _____
> >>>>>> Professor Michael E. Mann
> >>>>>> Department of Environmental Sciences, Clark Hall
> >>>>>> University of Virginia
> >>>>>> Charlottesville, VA 22903

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> >>>>>> Attachment converted: Macintosh HD:EOS.senate letter-final.doc
> >>>>>> (WDBN/MSWD) (00055FCF)

> >>>>>>
> >>>>>>
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> >>>>>>
> >>>>>> Jonathan T. Overpeck
> >>>>>> Director, Institute for the Study of Planet Earth
> >>>>>> Professor, Department of Geosciences

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> >>>>>> http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
> >>>>>> <http://www.ispe.arizona.edu/>

> >>>>>>
> >>>>>>
> >>>>>>
> >>>>>>

> >>>>>> _____
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> >>>>>>
>
>
>

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2106. 2003-07-26

cc: Otto Kinne <kinne@int-res.com>, CR-editors@int-res.com,
CR-reviewededitors@int-res.com
date: Sat, 26 Jul 2003 13:53:27 +1200
from: "Chris de Freitas" <c.defreitas@auckland.ac.nz>
subject: Re: Draft CR editorial
to: Hans.von.Storch@gkss.de

Dear Hans

I do not believe your editorial is necessary. In fact, I feel it will be counterproductive.

Controversy is nothing new to the global warming theme. Strong disagreement is what drives the debate. The journal literature is full of it. Indeed, papers occur in all science journals on a whole range of topics that, from time to time, one or another scientist disputes. Science is the battleground of ideas. The editorial is an overreaction. Moreover, by suggesting that there were procedural oversights, when there were not, and by naming people for using "questionable" methods, when this is a matter of opinion, takes the whole thing too far. It will damage the integrity of the journal. I believe Otto Kinne's recent editorial in CR is sufficient.

Regards

Chris de Freitas

On 24 Jul 2003, at 20:22, Hans.von.Storch@gkss.de wrote:

Date sent: Thu, 24 Jul 2003 20:22:57 +0200
From: Hans.von.Storch@gkss.de
Subject: Draft CR editorial
To: CR-editors@int-res.com, CR-reviewededitors@int-res.com
Copies to: Otto Kinne <kinne@int-res.com>

> Folks,

>

> if there shall be an editorial in the next issue of Climate Research,
> this editorial must be completed until Monday noon time. It would be
> about 1 page, not more than two. Not much time, but I think we should
> try it. This editorial would also be sent to this person from the US
> senate who was inquiring about the review process at CR. I have
> prepared a draft now, and I ask you to read it and come up with
> constructive comments.

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> process of Soon & Baliunas was insufficient, without "damaging" the
> responsible editor. We should have been more vigilant after we had seen
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> controversial articles. Quite the contrary, we should be proud of
> having such articles, but it should be made explicit that the material
> IS controversial and that other quarters look at the evidence
> differently. One way of doing so would be to invite comments to be
> published together with the original article.

>

> Obviously, English is not my native language. I am sure that some
> helpful people at Inter-Research will help me to straighten out to
> clumsy formulations - but I would appreciate your help also in this
> respect.

>

> Regards

>
> Hans
> Editor-in-Chief, Climate Research
>
> -----
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> ph: + 49 4152 87 1831, mobile +49 171 212 2046 fx +49 4152 87 2832
> presently: Kaspervej 2, 4673 Roedvig, Denmark, ph 0045 5650 6760
>
> -----
>
> Draft editorial
>
> Until now, Climate Research had a rather liberal procedure of
> processing submitted manuscripts. A group of several editors operated
> independently. Manuscripts dealing with "basic and applied research
> devoted to all aspects of climate - present, past and future; effects
> of human societies and organisms on climate; effects of climate on the
> ecosphere." were and are welcome. Before publication they were
> subjected to a formal peer-review: "Manuscripts are critically
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> CR24:91-92/ 93-94). Also the recent article draw severe critique,
> which was made public by a thorough analysis of the results in the
> Transaction of the AGU, EOS (vol 84, No. 27, 256). I find this
> critique well-taken. The major conclusion of Soon and Baliunas paper
> "Across the world, many records reveal that the 20th century is
> probably not the warmest nor a uniquely extreme climatic period of the
> last millennium." can not be concluded from the evidence presented
> in that paper. The statement itself may be true, but the methodology
> used to arrive at this conclusion was flawed. On the other hand, the
> review process at CR was formally in order. Four different reviewers
> were involved. Thus, the editorial board of CR had to admit that the
> formal review rules are not sufficient to guarantee the required
> quality control of the review process. In particular, when
> controversial manuscripts have to be processed, the responsibility
> should not be placed on a single editor. Therefore the editorial board
> and the publisher have decided to change the routine. In particular
> the office of an Editor-in-Chief has been created, who shall supervise
> the quality of the review process and help individual editors with
> controversial manuscripts. I have been asked to take on the
> responsibility as Editor-in-Chief of Climate Research and I have
> accepted per 1. August 2003. An immediate consequence is that authors
> are requested to send manuscripts to the Editor-in-Chief; requests of
> authors to have their manuscript processed by a specific editor are
> welcome, but are not necessarily fulfilled. Only naive people think
> that climate science has only to do with facts and truth. In fact
> climate science is to some extent a social process, with many
> extra-scientific influences. Climate science is definitely in a
> postnormal stage, and we have to make sure that publications are not

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> just reconfirming preconceived concepts, or concepts we have gotten to
> be used of. Ludwig's Fleck remarkable analysis of "Genesis and
> Development of a Scientific Fact" describes this syndrome, which
> eventually leads to a dogmatization and stand-still of science. Thus,
> we need a certain level of liberalism. Articles must be allowed to
> present additional to its hard, and reproducible facts a certain
> amount of creative speculation. However, papers must be explicit where
> facts end and where fantasy begins. Hans von Storch, 24 July 2003
>

1515. 2003-07-29

cc: f.matthies
date: Tue Jul 29 12:38:54 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: Research Council funding bid: Environment and Health
to: "Nicky Warren" <nwarr@nerc.ac.uk>

Dear Nicky,

Thank you for this.

We are indeed interested in the climate change related aspects of the agenda you are

identifying. We are keen to give greater prominence in the Tyndall Centre to the health

dimension of climate change and to this end we are engaging with the MRC in the lead up to

our business plan for Tyndall Phase 2 (we wish to explore whether the MRC would join the

funding consortium post-2005). We are also meeting in September with Ros Rouse in the ESRC

about aspects of our agenda.

I have asked one of my team - Franziska Matthies - to respond to your invitation and she

should do so before the 11 August (I will then be on leave). Franziska is helping us

develop our ideas in this area.

So thank you for your approach and I hope we can remain in contact as these ideas develop.

Regards,

Mike

At 14:18 25/07/2003 +0100, you wrote:

Dear Mike,

Louisa Watts suggested that you might be able to have some input into a theme on "Environment and Health" which NERC is considering including in its portfolio of bids to the next government spending review.

Background

Environment and Health is likely to become a priority area for NERC over the next few years which is why we are considering including this theme as part of our portfolio of bids to the 2004 spending review. Our bids need to be received by the Office of Science and Technology in late autumn this year, but in the first instance it will be considered in a joint Research Councils meeting in September. I am co-ordinating the development of the Environment and Health theme for this meeting.

How you could help

Any comments you have on this theme would be very helpful, particularly in terms of:

- * science areas that should be covered in this theme and why they are important

- * which parts of the theme we should be placing emphasis on (as particularly important research issues, scientifically and/or strategically)

- * why this research is timely and why the UK should be doing it

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- * how the UK might exploit the results
- * if there are any fields in particular in which we need more trained people

- * what realistic outputs/outcomes we should be aiming for

- * how much investment might be needed (including any major capital items/infrastructure that you think would be needed)

I've attached a copy of the current outline which was developed initially by NERC and MRC as a first attempt. I appreciate this is holiday season but if you are able to comment, it would be very helpful to have your ideas by 11th August. Developing this theme will be an iterative process until the final bid is submitted in the autumn, however, so any comments you sent me past this date would still be useful.

Many thanks,
Best wishes
Nicky Warren

Dr Nicky Warren
Terrestrial and Freshwater Sciences Manager
Natural Environment Research Council
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United Kingdom
+44 (0)1793 411588 (Direct Line)
+44 (0)1793 411545 (Fax)
email: nwarr@nerc.ac.uk

1595. 2003-07-29

cc: <David.Warrilow@defra.gsi.gov.uk>, <geoff.jenkins@metoffice.com>,
<pastott@metoffice.com>, <cathy.johnson@defra.gsi.gov.uk>,
<maria.noguer@defra.gsi.gov.uk>

date: Tue, 29 Jul 2003 18:00:28 +0100

from: Hans.Verolme@fco.gov.uk

subject: Soon again: Modeling used by U.N., EPA questioned

to: <simon.brown@metoffice.com>, <m.hulme@uea.ac.uk>

Today the Senate will hold a hearing on the never-ending story of the hockey stick. Michael

Mann and Willie Soon are slated to testify. Below please find Greenwire's preview, which

cites a report by another sceptic think-tank, the Independent Institute (hyperlink below).

In case you were not aware, the Senate Environment Cie. chair, Inhofe, is aligned with the

sceptics. But don't despair, your recent debunking of the Soon and Baliunas paper for the

Marshall Institute has found its way to sympathetic Senate staff, stripped of its origins.

Senators Jeffords and Clinton will hold their feet to the fire.

Peter's paper in GRL has also been provided to the NY Times science editor. I suggested he

review it in the context of last week's science strategy release and this week's Earth

Observation Summit. Let's see.

We will formally report on the EOS and fold in related issues.

HANS

Modeling used by U.N., EPA questioned

Lauren Miura, Greenwire reporter

A panel of researchers attacked the science used to help guide global and U.S. climate

change policy yesterday, as representatives from both sides of the debate geared up to

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testify on the issue during a Senate committee hearing.

At issue are the models used to predict how much temperatures will rise, particularly the model that produced the so-called "hockey stick" graph showing a sharp rise in Northern Hemisphere temperatures during the past two decades.

That model, and the study that produced it, is widely cited as evidence that 1990-2000 was the warmest decade in the last millennium. It has been featured in reports from United Nations Intergovernmental Panel on Climate Change, the Clinton Administration's 2000 report

Climate Change Impacts on the United States and subsequently, U.S. EPA's 2001 Climate Action Report.

But with the release of a new report yesterday, the free-market-oriented Independent Institute charged the IPCC-favored "hockey stick" graph is faulty, in part because the model is based on a "severely limited" sample and assumes a wide margin of error.

The IPCC panel, made up of thousands of scientists from around the globe, estimated in 2001 that atmospheric concentrations of carbon dioxide (CO₂) have increased by almost one-third since 1750 to their highest level in at least 420,000 years and possibly as long as 20 million years.

A member of the IPCC was scheduled to testify today to support Sen. Jim Jeffords' (I-Vt.) view that manmade CO₂ emissions from industrial plants, electric utilities and motor vehicles are the leading contributor to climate change. Meanwhile Senate Environment and Public Works Committee chairman Sen. James Inhofe (R-Okla.) contends that the Earth's warming climate is not caused by manmade emissions but is instead determined by a more far-reaching set of historical trends (Environment & Energy Daily, July 28).

"The U.N. Intergovernmental Panel on Climate Change used a temperature record for the last 1,000 years that can only be called a scientific outlier," said Patrick J. Michaels, a professor of environmental science at the University of Virginia and a senior fellow at the Cato Institute.

Michaels said the "hockey stick" model is outside the scientific norm because it does not acknowledge what scientists refer to as "the Little Ice Age" ending in the late 19th century and a "Medieval Warm Period" before that, as "hundreds and hundreds" of other studies do.

Further, the Independent Institute says satellite data show an upward global temperature trend of 0.06 degrees celsius per decade, "several times less than what was forecast by computer models that served as the basis for the original 1992 Framework Convention on Climate Change."

"Climate models cannot take into account the very complicated feedbacks in the atmosphere, specifically clouds and water vapor," said Independent Institute research fellow and former

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EPA official S. Fred Singer.

Even if the computer models were right, Michaels said, the average temperatures would increase only 1.6 degrees celsius over the next 100 years. "If something appears to be moderate and you couldn't stop it anyway, shouldn't that be the end of the issue?" Michaels asked.

Environmentalists downplayed the report's reliability. Jeff Fiedler of the Natural Resources Defense Council described the Independent Institute's scientific panel as "pretty much a who's who of the remaining climate skeptics out there," adding that most of the panelists are outside the mainstream of climate research.

Click here to download a copy of the report

[1]<http://www.independent.org/tii/news/030728story.html>

For more information on the Foreign & Commonwealth Office visit:

<http://www.fco.gov.uk> For information about the UK visit: <http://www.i-uk.com>

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Office and its missions overseas may be monitored centrally. This is done to ensure the integrity of the system.

2007. 2003-07-30

cc: plemke@awi-bremerhaven.de, ewwo@bas.ac.uk, r.r.dickson@cefas.co.uk, maria.noguer@defra.gsi.gov.uk, mccave@esc.cam.ac.uk, studhope@glg.ed.ac.uk, B.Turrell@marlab.ac.uk, rwood@metoffice.com, sfbtett@metoffice.com, ppn.NERC.NERC@nerc.ac.uk, j.m.slingo@reading.ac.uk, p.j.valdes@reading.ac.uk, j.lowe@rhbnc.ac.uk, JYM.SOC.NERC.SOC.NERC@soc.soton.ac.uk, Peter Challenor <PC@soc.soton.ac.uk>, a.j.watson@uea.ac.uk, k.briffa@uea.ac.uk, m.hulme@uea.ac.uk, haugan@gfi.uib.no, C Gommenginger <CGI@soc.soton.ac.uk>, Meric Srokosz <MAS@soc.soton.ac.uk>, lkeigwin@whoi.edu

date: Wed, 30 Jul 2003 20:58:43 +0100

from: Simon Tett <simon.tett@metoffice.com>

subject: Re: RAPID: Dutch & Norwegian Collaboration

to: Philip Newton <ppn@nerc.ac.uk>

Phil,

I think there is some merit in what you suggest though to be honest it depends on what

you mean by "RAPID" science. My concerns are:

- 1) It will change the focus of the 2nd AO. This could damage our integrative work and reduce our ability to pull together a RAPID community
 - 2) It could delay the AO -- the impact of this is that less science will be done as more will need to be spent on the admin of the program as the program lifetime will increase
 - 3) The 2nd AO is the last change that the SC have to steer the program -- by trying to integrate with Norway & Holland we make it more difficult.
 - 4) Isn't FP6 the right place to get trans-euro funding?
- Perhaps we could have a 2.5 AO which has £3/4 Million funding (3 projects) that could be joint with Norway/Holland or other European funding agencies. .

Philip Newton wrote:

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Thanks Eric,

We could adopt the model you suggest, and I would welcome the views of others. The driver for going for more explicitly collaborative proposals is the chance of getting access to matching money from CREST, which could be up to several million pounds. The feeling is that we are more likely to succeed with that if the national commitment to a collaborative programme is more explicitly strong (ie genuine joint proposals). Moreover, it seems to me that collaborative work is likely to be stronger if there is an initial constraint to write a single proposal (despite the 'success' of the NSF venture, even with a strongly focused AO, the US principal investigators had to have arms strongly twisted to write their 'synergistic' proposals, as they were deeply sceptical that anything could get funded through a joint process).

Why these countries and not, for example, France, Germany.....? Well, we have tried to get other countries with strong profiles in the RAPID science area involved, but without success to date. But if we can pull the CREST matching money out of the hat, it may be that we could use some of the matching funds to lever in one or two more countries in some way. And then there is always FP6: even though WATCHER will not fly, there are two likely RAPID-relevant bids in the offing that we know of.

I acknowledge that the science that RAPID would end up funding in the second round would be different (at about 25% level if we commit about 25% of second round funds to the joint call). Presumably, this means that we will not fund the lowest quartile of proposals that we would otherwise have funded. But instead, we will fund other projects, and with an additional 1.8M pounds (even w/o CREST monies), all addressing RAPID second AO objectives. The trick is to get the focus of the joint call right, and then to follow that up to ensure that researchers from the three countries have an opportunity to write some high-quality focused proposals that enable us to fund some excellent and RAPID-relevant research. It is on this issue that it would be helpful in particular to have the SC feedback I am requesting, on the strengths of communities in these countries.

Best wishes,

Phil

"Eric W Wolff" [1]<ewwo@bas.ac.uk> 07/21/03 06:16pm >>>

Dear Phil,

This sounds like a good attempt to get some coordination but perhaps adds a level of complication that might not be in the best interests of RAPID. My concern is that the effect in the UK will be that we restrict a fair proportion of the second call budget to proposals that have a Norwegian or Dutch collaborator. I am wondering why we would end up making one of our main drivers the need to collaborate with these particular nationalities (there are several other countries that make at least as obvious a connection scientifically).

I'd like to hear the views of others, as this idea is new on me. And I don't want to dampen the excellent opportunities for synergy that having a matched call could bring. But I do wonder if the end result might be merely to exclude some excellent proposals, or alternatively to spawn some artificial and unnecessary quasi-collaborations. Can we not find a model more like the one with NSF, where synergistic proposals scored well, and could be coordinated, but were not a prerequisite for the funding from each nation.

Best wishes

Eric

Eric Wolff
British Antarctic Survey
High Cross
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Cambridge CB3 0ET

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Fax: +44 1223 221279
Alternate fax: +44 1223 362616
"Philip Newton" [3]<ppn@nerc.ac.uk> 21/07/03 15:06:13 >>>
Dear Steering Committee,

The following information is not in the public domain, and out of respect for the concerned research councils should remain confidential until further notice.

For those of you present at the 2 June 2003 Steering Committee meeting, you will recall that I was due to meet with representatives from the Dutch Research Council (NWO) on 20 June, to build on the interest in RAPID they have shown throughout our programme development (e.g. NWO attended RAPID launch Town Meeting; Hendrik van Aken attended PIs kick-off meeting). The meeting was a positive one, culminating in a proposal for the NWO to put up about 1.5M Euros for investment in RAPID-oriented science. The proposal is to identify a subset of NERC's second AO that is of strategic interest to NWO (probably thermohaline-related), and hold a joint call and evaluation for proposals jointly proposed between UK and Dutch researchers. The call would be part of RAPID's autumn 2003 call (the Dutch delaying their plans by 4 months to fit in). The principle agreed would be that we would be aiming to do jointly what neither single nation would or could otherwise do, and that the national programmes of both countries must benefit from the collaborative work. This proposal then gave me leverage to go to the Norwegian Research Council - whose funding rounds for NoClim have unfortunately never coincided with ours, despite Peter's great efforts - to offer them a time-limited opportunity, with a carrot of a chance (20%?) of matching money from the EU (through a marine-CREST initiative...). Happily, the Norwegian RC have made a strategic decision to find 1MEuros to allow them to participate in an autumn call with us and the Dutch, on the same conditions I outlined above for the projected bi-lateral with the Dutch. Clearly there is a lot of detail to sort out; it will be more complex than the NSF joint venture (though the fact that we have done the NSF exercise undoubtedly enticed). The joint call will need to be part of RAPID's second AO. I envisage that we'd put about 1-1.5m pounds of our second call money up against theirs, and the idea is that proposals to it would have to have investigators (PIs/co-Is) from at least two of the three countries. Each RC could fund only its own researchers (which will complicate...). It seems that both countries would effectively be happy to use the NERC mechanisms, adding 1 or 2 people to our SC for decision meetings (not necessarily as full members). You may wonder why you have not yet been consulted on the joint venture, beyond being aware that we have continually been searching

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for joint opportunities at the RC level with Norway and Holland, to enhance the delivery of RAPID's science objectives. This is because events have been rapid (this has all come together in the last 5 weeks), and I felt we needed to get to a certain point of 'solidity' about what could happen first.

However, Meric and I would now welcome your views on how best to take this forward, especially in terms of using your knowledge of activities in Holland and Norway to help identify the most appropriate subsets of RAPID's science objectives for the joint part of the call.

For example, in what areas do these countries have especial strengths (e.g. intellectual, infrastructure, ongoing programmes/activities) that would help us deliver certain RAPID science objectives? In cases

you identify, are you aware whether those areas are also (related to) strategic objectives in that nation's programmes?

I realise that entering into this joint venture with Norway and Holland will cause some complications, but if it is set up in the right way, then I am sure that the benefits to the programme, to the science-area, and to those involved, will dwarf such inconveniences. If the attempt to secure 'matching' money through the CREST initiative

succeeds, then our flexibility and scope will be further enhanced. Please be assured that we do not underestimate the complexities here,

and acknowledge that we need to plan the joint component of the AO very carefully, provide support to allow joint proposals to be developed, avoid the pitfalls of EUROCORES.... But with your help and advice, I'm sure we can considerably enhance the RAPID programme and this science area through this venture.

Best wishes,

Phil

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3941. 2003-07-30

cc: Phil Jones <p.jones@uea.ac.uk>, Tom Wigley <wigley@ucar.edu>, Michael Oppenheimer <omichael@Princeton.EDU>, "Michael E. Mann" <mann@virginia.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Ben Santer <santer1@lrl.gov>, Steve Schneider <shs@stanford.edu>

date: Wed, 30 Jul 2003 10:12:30 -0600
from: Kevin Trenberth <trenbert@cgd.ucar.edu>
subject: Re: letter to Senate
to: "Karl, Tom" <Thomas.R.Karl@noaa.gov>
<x-flowed>

Hi all

Please see the attached letter from Rick Anthes which is in response to a letter from John McCain about the current Senate Bill on climate change. I had a strong hand in the content.

Kevin

All,

Attached is the final version of the letter that we faxed to McCain today. I know some of you wanted a chance to make comments but there just was not enough time left. I received McCain's request when I came to work early yesterday (the 29th) morning. His letter asked for a response by today, in time for him and Lieberman to introduce an amendment to the Energy bill. With a huge amount of help from Kevin, I put together this response yesterday morning--it took about 6 hours of going back and forth. Then Cindy assembled the raw material into a good first draft letter yesterday afternoon.

Rick

--

Dr. Richard A. Anthes
Phone: 303-497-1652
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--

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Street address: 3080 Center Green Drive, Boulder, CO 80301

</x-flowed>

Attachment Converted: "c:\eudora\attach\McCainRick7-30-03.doc"

866. 2003-07-31

date: Thu Jul 31 09:43:33 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: We would like you to work with us to lead the DEFRA
to: "Andrew Bradbury" <Andrew.Bradbury@mouchel.com>

Dear Andrew,

Thanks for your invitation re. SDRN.

Much as I support this initiative, an extra set of responsibilities at this time does not

fit well with my professional life and so I will have to decline your invitation.

cg2003

With best wishes,
Mike Hulme

At 14:47 30/07/2003 +0100, you wrote:

Dear Dr Hulme,

I tried to call you yesterday to discuss but you were in a meeting. I explain my inquiry below.

Mouchel is a professional services company based in Surrey. We specialise in the provision of support services to Government. We are currently providing professional services to the Highways Agency, Networkrail, the Scottish Executive, Environment Agency and DEFRA. Due to our ongoing involvement in DEFRA Research and Development Activities DEFRA has invited us to bid to operate the Sustainable Development Research Network (SDRN) for a three year period (with a possible further two year extension). We are aware you have provided constructive support to this and other sustainable development initiatives in the past. We intend to bid for this contract and we would like you to consider joining our team in a leadership capacity. We feel that the SDRN needs to be led by people who not only understand the sustainable development research agenda inside and out but who also have the profile to help us engage a wide range of research organisations at the highest level in the activities of the network. To this end we intend to set up a Stakeholder Board to oversee the work of the SDRN network. Specifically the board will:

- contribute to development of a three year strategy for the SDRN
 - review progress against this strategy
 - spearhead a new drive for evidenced based SD policy making
 - facilitate delivery of SDRN programme of work and open bottlenecks through high level dialogue with participating research organisations
- We are currently contacting a small number of people we consider have the appropriate professional experience to ask them whether they would (if we are successful in our bid) like to be a member of the SDRN Stakeholder Board. The commitment we envisage is between 3-5 days a year. We will cover the expenses associated with your attendance but we cannot pay a fee for your time. We intend to make the SDRN a high profile organisation and I hope that the profile and interest associated with this SDRN leadership position will be a significant reward for the time you commit.

In addition, we will establish a new system of Policy Theme Focus Groups to build stronger links between Government and evidence providers with a focus on specific priority areas. If successful in our bid we would like to engage your services as a specialist advisor on a fee paid basis to facilitate one of these Groups. The time commitment would be approximately 10 days over a 10 month period at some stage of the commission, concluding with a presentation to the SDRN annual conference to present outcomes.

As a company we are committed to making the SDRN a great success and I hope you will consider contributing to the initiative. If you wish to be involved all I need at this stage is:

- a letter on headed notepaper stating your willingness to be part of our stakeholder board and Policy Theme Focus Groups (I attach an example)
- a CV
- a day fee rate.

As ever with these things time is short. We have to submit our proposals on 6th August. I would therefore be grateful if you could respond as soon as possible.

If you would like to speak to me about this please call me on 01932 337 161 or 0790 151 6035 (after 6 pm)

Kind regards

Andrew Bradbury, Mouchel Consulting Limited

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2868. 2003-07-31

date: Thu Jul 31 13:42:49 2003

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Mann series

to: simon.tett@metoffice.com

Ooops! Forgot the attachments last time!

Hi Simon,

the attached files contain the Mann et al. 1999 series and uncertainties for comparison with HadCM3. These are calibrated against the full hemisphere (land+ocean, tropics+extratropics) annual mean temperature.

mann_nh1000.dat

contains the unfiltered series from 1000 to 1980. It is calibrated, so represent K anomalies, but relative to a 1902-1980 baseline. To convert it to anomalies relative to 1961-1990 baseline, subtract 0.12 from every value.

mann_nherr1000.dat

contains the 1 and 2 standard errors for the same series. Two columns give the errors for the unfiltered series, then two columns give them for "lowf" which are the errors appropriate for a 40-year smoothed record. I don't have errors for any other time scale.

It will be interesting to see the comparison.

Cheers

Tim

4954. 2003-07-31

date: Thu Jul 31 16:12:47 2003

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Climate Research resignations

to: "Rob Wilby" <rob.wilby@environment-agency.gov.uk>

Hi Rob,

did you know that Hans von Storch and Clare Goodess (and one other) resignations from

Climate Research have made the Wall Street Journal and the US Senate?!

are you considering your position?

Cheers

Tim

July 31, 2003

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11071376.jpg

Page 574

DEBATING GLOBAL WARMING

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"

Global Warming Skeptics

Are Facing Storm Clouds

By ANTONIO REGALADO

Staff Reporter of THE WALL STREET JOURNAL

A big flap at a little scientific journal is raising questions about a study that has been embraced by conservative politicians for its rejection of widely held global-warming theories.

The study, by two astronomers at the Harvard-Smithsonian Center for Astrophysics, says

the 20th century wasn't unusually warm compared with earlier periods and contradicts

evidence indicating man-made "greenhouse" gases are causing temperatures to rise.

Since being published last January in Climate Research, the paper has been widely

promoted by Washington think tanks and cited by the White House in revisions made to a

recent Environmental Protection Agency report. At the same time, it has drawn stinging

rebukes from other climate scientists.

This week, three editors of Climate Research resigned in protest over the journal's

handling of the review process that approved the study; among them is Hans von Storch,

the journal's recently appointed editor in chief. "It was flawed and it shouldn't have

been published," he said.

Dr. von Storch's resignation was publicly disclosed Tuesday by Sen. James Jeffords (I.,

Vt.), a critic of the administration's environmental policies, during a hearing of the

Senate Environment and Public Works Committee called by its chairman, Sen. James Inhofe

(R., Okla.).

The debate over global warming centers on the extent to which gases released from the

burning of fossil fuels -- mainly carbon dioxide -- are trapping the sun's heat in the

Earth's atmosphere, creating a greenhouse effect. The political fight has intensified as

the Senate votes on a major energy bill. Sens. John McCain (R., Ariz.) and Joseph

Lieberman (D., Conn.) planned to introduce an amendment this week that would cap

carbon-dioxide emissions at 2000 levels starting in 2010 for select industries. The Bush

administration is opposed to imposing caps, and the measure isn't expected to become

law.

The Harvard study has become part of skeptics' arguments. Mr. Inhofe, who is leading the

opposition to the emissions measures, cited the research in a speech on the Senate floor

Monday in which he said, "the claim that global warming is caused by man-made emissions

is simply untrue and not based on sound science."

The paper was authored by astronomers Willie Soon and Sallie Baliunas, and looked at

studies of tree rings and other indicators of past climate. Their basic conclusion: The 20th century wasn't the warmest century of the past 1,000 years. They concluded temperatures may have been higher during the "Medieval Warm Period," the time during which the Norse settled Greenland.

Dr. Soon couldn't be reached and Dr. Baliunas declined comment. In his testimony before Mr. Inhofe's committee, Dr. Soon reiterated the findings of his study, which was partly funded by the American Petroleum Institute.

Dr. Soon's findings contradict widely cited research by another scientist, Michael E. Mann of the University of Virginia. Dr. Mann's reconstruction of global temperatures shows a distinct pattern shaped like a hockey stick: Temperatures stayed level for centuries, with a sudden upturn during recent decades.

A reference to Dr. Soon's paper previously found its way into revisions suggested by the White House to an EPA report on environmental quality. According to an internal EPA memorandum disclosed in June, agency scientists were concerned the version containing the White House edits "no longer accurately represents scientific consensus on climate change." Dr. Mann's data showing the hockey-stick temperature curve was deleted. In its place, administration officials added a reference to Dr. Soon's paper, which the EPA memo called "a limited analysis that supports the administration's favored message."

The EPA says the memo appears to be an internal e-mail between staffers but isn't an "official" document. A spokesman at the White House's Council on Environmental Quality says the addition of the citation to Dr. Soon's paper to the draft report was suggested during an interagency review process overseen by the White House.

Dr. Mann and 13 colleagues published a critique of Dr. Soon's paper in *Eos*, a publication of the American Geophysical Union, this month. They said the Harvard team's methods were flawed and their results "inconsistent with the preponderance of scientific evidence."

Then, last week Dr. von Storch was contacted by Sen. Jeffords's staff, which was looking into the paper in preparation for Tuesday's hearing, where Dr. Soon and Dr. Mann were scheduled to appear. After hearing from Sen. Jeffords, Dr. von Storch says he decided to speed an editorial into print criticizing publication of the paper.

But publisher Otto Kinne blocked the move, saying that while he favored publication of the editorial, Dr. von Storch's proposals were still opposed by some of the other editors. "I asked Hans not to rush the editorial," Mr. Kinne said in an e-mail.

That is when Dr. von Storch resigned, followed by two other editors.

--John J. Fialka contributed to this article.

Professor Michael E. Mann

cg2003
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

719. 2003-08-01

date: Fri Aug 1 11:24:24 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: IPCC 4th Assessment Report - inputs to WGI
to: "Andrew Watson" <a.watson@uea.ac.uk>

Thanks Andy - I've forwarded on these comments, along with some of my own.
Mike

At 12:48 22/07/2003 +0100, you wrote:

Hi Mike

I feel that the AR4 planning process should be aware of the problems that have arisen from the TAR WGI treatment of the ocean carbon cycle. Perhaps Susan Solomon's invitation is a good opportunity to raise this?

I'm not sure of the reason why, but the degree of insight on the marine side of the carbon cycle was considerably less than that on the terrestrial or atmospheric side in the TAR. Almost un-noticed, for the TAR, the IPCC moved from the use of ocean carbon cycle models as the primary method of gauging the ocean sink, to the use of atmospheric O₂/N₂ measurements. They did not notice (or at any rate did not highlight) the significance of the large discrepancy between these two techniques when applied to the period of the 1990s, which was a clear indication of something amiss in the assumptions underlying the O₂/N₂ method. One result is that their preferred estimates of the size of the land and ocean sinks were out of date before they were published, and quite substantially wrong. This has not enhanced the IPCC's reputation in this area of science.

Getting the ocean CO₂ fluxes right is important because we are much closer to being able to specify the ocean sink over wide areas from primary measurement and understanding, than is the case for terrestrial sinks and sources. Thus the main constraints on the natural CO₂ sinks come from a combination of atmospheric measurements, and ocean studies. Being fully up to speed with what is happening in marine CO₂ studies is therefore critical to the IPCC WGI.

Cheers

Andy Watson

----- Original Message -----

From: "Mike Hulme" <m.hulme@uea.ac.uk>

To: <p.jones@uea.ac.uk>; <k.briffa@uea.ac.uk>; <j.palutikof@uea.ac.uk>;

<t.osborn@uea.ac.uk>; <p.liss@uea.ac.uk>; <t.jickells@uea.ac.uk>;

<a.watson@uea.ac.uk>; <m.penkett@uea.ac.uk>; <s.raper@uea.ac.uk>;

<h.j.schellnhuber@uea.ac.uk>

Cc: <t.d.davies@uea.ac.uk>

Sent: Thursday, July 17, 2003 2:19 PM

Subject: IPCC 4th Assessment Report - inputs to WGI

> I thought I would circulate this invitation from Susan Solomon (IPCC WGI
> Chair) inviting ideas from Convening Lead Authors in the 3rd Assessment
for

> consideration in the Working Group 1 report of the IPCC 4th

> Assessment. You may have received a similar invitation anyway, or have

> other routes into the IPCC AR4 scoping process (my apologies if you have),

> but before I reply with any thoughts of my own I would be happy to include
> suggestions from other senior ENV "climate" scientists about what WGI

> should address/emphasise etc. in the next IPCC report (2007) that you feel
> has not been well covered in previous reports.

>

> what Susan is looking for is clear from the attached. If people want to
> respond separately, fine ... equally I am happy to collate others ideas

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> with my own and submit it as a more collective set of views from UEA.
> 28th
> July is the deadline.
>
> Thanks,
>
> Mike

1363. 2003-08-01

cc: <laura.middleton@uea.ac.uk>, "Flack Chris Mrs \ (ENV\)" <C.Flack@uea.ac.uk>,
"John Schellnhuber" <H.J.Schellnhuber@uea.ac.uk>
date: Fri, 1 Aug 2003 13:19:00 +0100
from: "John Schellnhuber" <H.J.Schellnhuber@uea.ac.uk>
subject: Re: proposed dinners on Thursday 4th September
to: "T Davies" <T.D.Davies@uea.ac.uk>, "'Mike Hulme'" <m.hulme@uea.ac.uk>,
<t.oriordan@uea.ac.uk>, <r.k.turner@uea.ac.uk>

Dear Trevor,

I had another phone conversation with Sir Crispin the other day, and the present state-of-affairs is like this:

He thinks that a Zuckerman Board Preparatory Dinner, chaired by him in a nice place on the evening of 4 September 2003, is a very good idea. He would like to receive from you, however, an official invitation letter sketching the aims and scope of the meeting. I will actually visit Sir Crispin in his Cotswolds home on 16 August, so I could discuss specifics with him already. Of course, we should invite all the people, whom we want to see on the ZB and who are attending the ZICER opening anyway, to the dinner as well, but Sir Crispin may have his own ideas about the composition.

I suggest that you list in your letter to him, whom we have in mind so far. I basically agree with the shortlist you presented in your email, but here are a few comments.

We clearly need some strong stakeholder involvement at the strategic level, so let us consider only boardroom creatures. Bernie Bulkin from BP would be fine with me - I do not know whether Chris Mottershead from the same company is an alternative. We have to have our VC on board. Bob Watson would be great, but we should also think of either Bill Clark or Steve Schneider - both of them are coming to SD3! Bob Constanza is another option, but how about Sir Eric Ash to represent the RS?

So much for now. Others will have other notions - this could be sorted out before and during the dinner.

Cheers,
John

----- Original Message -----

From: "T Davies" <T.D.Davies@uea.ac.uk>
To: "'Mike Hulme'" <m.hulme@uea.ac.uk>; <t.oriordan@uea.ac.uk>;
<h.j.schellnhuber@uea.ac.uk>; <r.k.turner@uea.ac.uk>
Cc: <laura.middleton@uea.ac.uk>; "Flack Chris Mrs (ENV)" <C.Flack@uea.ac.uk>
Sent: Tuesday, July 29, 2003 11:23 AM
Subject: RE: proposed dinners on Thursday 4th September
> I'm happy to arrange a small dinner on 4th - Chris please remind me when
> I return.

>
> In the meantime, we need to ensure that there are no crossed wires over
> membership of the ZICER Advisory Board. I sent an email to Kerry & John
> yesterday summarising where we are, since life had become a little
> complex following Sir Crispin's interventions on our behalf.

>
> As it stands at the moment:

>
> Chair Crispin Tickell
> David King (invited by CT)
> Lord Peyton (invited by CT)
> Pentreath (invited by RKT - chair of the CSERGR board)

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> Paul Ekins (asked informally by TD on RKT's recommendation, now followed
> up by a formal invitation)
>
> Possibles
>
> Bob Watson (has offered support for ENV) - TD about to invite him to
> offer support via membership of ZICER Board
> Chair of the TYN advisory board
> BP - Bulkin or his colleague who visited)
> Someone from AVIVA (I have contact)
> Someone from Powergen
>
> Has someone asked Bill Clark?????
>
> At this stage, I think it necessary to hold so that we can determine
> terms of refs & give Crispin Tickell "ownership" of his board.
>
>
> Trevor
>
>
> -----Original Message-----
> From: Mike Hulme [mailto:m.hulme@uea.ac.uk]
> Sent: 28 July 2003 17:44
> To: t.oriordan@uea.ac.uk; h.j.Schellnhuber@uea.ac.uk;
> t.d.davies@uea.ac.uk; r.k.turner@uea.ac.uk
> Cc: laura.middleton@uea.ac.uk
> Subject: proposed dinners on Thursday 4th September
> Importance: High
>
> Dear Trevor, Kerry, John and Tim
>
> Following the SD3 planning last week, I am circulating this email in
> relation to the idea for a small dinner on the evening of the ZICER
> opening
> - i.e., Thursday 4th September.
>
> This idea comes from two directions - Tim O'Riordan wishing to entertain
> a
> small number of his guests to the following day's Governance Seminar
> supported by the ESRC Science and Society programme (e.g. Steve Rayner,
> Bill Clark, Frank Biermann, etc.), and John Schellnhuber suggesting we
> make
> good use of a few VIPs for ZICER who will be here on the Thursday (e.g.
> Sir
> Crispin Tickell, Bill Clark, Paul Zuckerman, etc.) re. an embryonic
> ZICER
> "Council". I believe John has talked with Trevor about this latter
> idea.
>
> Since the purpose of these two small groups is rather different, it
> seems
> best to keep them separate. It will also be important that Bill Clark
> be
> invited to the ZICER "council" dinner.
>
> So this email is simply to make sure that all parties understand the
> conversations that have happened and to ensure that suitable liaison
> occurs
> to avoid confusion. So I think the ball is in Tim's court re. his
> "governance" dinner and Trevor, Kerry, and John's court re. the ZICER
> dinner.
>

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> Thanks,
>
> Mike
>
>
>

3547. 2003-08-01

cc: <plemke@awi-bremerhaven.de>, <r.r.dickson@cefas.co.uk>,
<maria.noguer@defra.gsi.gov.uk>, <marotzke@dkrz.de>, <mccave@esc.cam.ac.uk>,
<haugan@gfi.uib.no>, <studhope@glg.ed.ac.uk>, <B.Turrell@marlab.ac.uk>,
<cgl@mercury.soc.soton.ac.uk>, <rwood@meto.gov.uk>, <sfbtett@meto.gov.uk>,
<ppn@nerc.ac.uk>, <p.j.valdes@bristol.ac.uk>, <a.j.thorpe@reading.ac.uk>,
<j.lowe@rhbnc.ac.uk>, <jym@soc.soton.ac.uk>, <mas@soc.soton.ac.uk>,
<P.Challenor@soc.soton.ac.uk>, <pc@soc.soton.ac.uk>, <a.j.watson@uea.ac.uk>,
<k.briffa@uea.ac.uk>, <m.hulme@uea.ac.uk>, <lkeigwin@who.edu>

date: Fri, 01 Aug 2003 16:56:59 +0100

from: "Eric W Wolff" <ewwo@bas.ac.uk>

subject: Re: Modelling intercomparison job description

to: <j.m.slingo@reading.ac.uk>

Dear Julia,

I just came back, very tired, from a conference in the US. Perhaps I was not careful in my wording, and I am sorry if that was so. As I re-read what I wrote, I cannot for the life of me see why any group would think I was aiming at them. It just seemed to me inevitable that someone sat in any modelling group would see the world in a certain way, and see priorities in a certain way. For that reason, I prefer that the person has a measure of independence. It's not a question of lack of trust or anything else. If I was doing an analytical data intercomparison I would use blind tests for the same kind of reason.

I cannot withdraw an accusation I did not make. You must be well aware that, as a non-modeller, I don't know CGAM or any of the other groups well enough to have any opinion about past behaviour. I had no intention of starting a spat, and hope we can now forget it.

On the substantive issue, the question seems to remain (1) whether we put out an advert for an individual, or an AO for a group, (2) whether the person reports formally to the modelling sub-group or to the institute they sit in.

Best wishes

Eric

Eric Wolff
British Antarctic Survey
High Cross
Madingley Road
Cambridge CB3 0ET
United Kingdom
E-mail: ewwo@bas.ac.uk
Phone: +44 1223 221491
Fax: +44 1223 221279
Alternate fax: +44 1223 362616

>>> Julia Slingo <j.m.slingo@reading.ac.uk> 01/08/03 16:03:40 >>>

Eric,

As one of the modelling groups in question, I would like to refute your suggestions that we might 'absorb the person to do more of their work, or that there might be axes to grind about which models are "superior".'

I can assure you that CGAM always works with the best interests of the community in mind, and that we have never been accused of using staff employed on specific projects to do our own work. Nor do we have 'axes to grind'. I have spent many years working in the area of model intercomparison and in the assessment of model performance (I have just produced a very detailed report for the Hadley Centre on CGAM's assessment of HadAM3/HadCM3 which has been very well received). Any comments on why one model might be 'superior' have always been based on sound scientific reasoning, properly supported by objective evidence from model

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results in comparison against observations.

I trust you will withdraw your remarks unless of course they can be justified.

Julia.

Eric W Wolff wrote:

> Dear all,

>

> Peter makes a good point. But I'd be loathe to see one of the modelling groups actually take over and run the intercomparison, for fear that either they absorbed the person to do more of their work, or that there might be axes to grind about which models are "superior". On the other hand I can see also that we can't expect a fairly junior person to go at this by themselves. We need to find some compromise where they can get all assistance from a modelling group, but with independent management. I think that was Meric's intention in having the person reporting to him and the sub-group, but located in a modelling group.

> Provided we retain this kind of arrangement, I am equally content that this be run as a grant competition for institutions rather than a job advert for individuals. But my concern would be that that would probably delay the start of the intercomparison for several months at least.

>

> Eric

>

> -----

> Eric Wolff

> British Antarctic Survey

> High Cross

> Madingley Road

> Cambridge CB3 0ET

> United Kingdom

>

> E-mail: ewwo@bas.ac.uk

> Phone: +44 1223 221491

> Fax: +44 1223 221279

> Alternate fax: +44 1223 362616

>

> ??? Peter Challenor ?P.Challenor@soc.soton.ac.uk? 31/07/03 16:31:31 ???

> At the moment the plan appears to be that we find an individual who

> then attaches themselves to a willing institution, rather like a NERC

> fellow. Are we sure we want to proceed this way? The alternative is to

> issue an AO and have institutions bid to carry out the intercomparison.

> I think this has some clear advantages over the currently proposed

> method.

>

> 1. If an institution is contracted to carry out the intercomparison if

> the individual leaves it is their responsibility to find someone else

> to do the rest of the work. COAPEC is having difficulty finding a

> replacement core team member at the moment.

>

> 2. The current advert says that part of the researcher's role is 'to

> investigate and apply statistical and climate dynamical methods to

> compare results between models and with observations'. This sounds like

> we are asking for some innovative research and as such I think we

> should be asking for some details of what is proposed, either from the

> candidates for the job or (I think better) from a PI in an institution.

>

> What does everyone else think?

>

> Peter

--

Prof. Julia Slingo

Director, NCAS Centre for Global Atmospheric Modelling

Department of Meteorology

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University of Reading
Earley Gate
Reading RG6 6BB
Tel: +44 (0)118 378 8424
Fax: +44 (0)118 378 8316
Email: j.m.slingo@reading.ac.uk
Web: <http://www.cgam.nerc.ac.uk/>

4559. 2003-08-01

date: Fri, 01 Aug 2003 13:50:08 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Aug 1 Science issue
to: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, Caspar Ammann <ammann@ucar.edu>, Gabi Hegerl <hegerl@duke.edu>, t.osborn@uea.ac.uk
<x-flowed>
> Dear All,

The letter exchange on pp595-6 is worth a read. The Science Editor-in-Chief's response is a fantastic put down ! Brilliant - should be rammed down Singer's throat when he does similar things in the future. I hope Kennedy enjoyed writing it as much as I enjoyed reading it.

I can't see Singer writing to Science again !

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

</x-flowed>

4570. 2003-08-01

date: Fri Aug 1 09:56:46 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Mann series
to: simon.tett@metoffice.com

Hi Simon,
an extra bit of information to go with the Mann record is that the "lowf" standard error column actually seems more applicable to the record when it is smoothed with a 50-year filter rather than a 40-year filter. But I've got hold of the calibration residuals from Mike now, and can check the answer by using an alternative method - and if it works, I can quantify errors for any smoothing filter we want.

Cheers
Tim

4848. 2003-08-01

cc: Meric Srokosz <mas@soc.soton.ac.uk>, lkeigwin@whoi.edu,
plemke@awi-bremerhaven.de, ewwo@bas.ac.uk, r.r.dickson@cefas.co.uk,
maria.noguer@defra.gsi.gov.uk, mccave@esc.cam.ac.uk, haugan@gfi.uib.no,

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studhope@glg.ed.ac.uk, B.Turrell@marlab.ac.uk, rwood@metoffice.com,
sfbtett@metoffice.com, j.m.slingo@reading.ac.uk, p.j.valdes@bristol.ac.uk,
j.lowe@rhnbc.ac.uk, jym@soc.soton.ac.uk, pc@soc.soton.ac.uk, a.j.watson@uea.ac.uk,
k.briffa@uea.ac.uk, m.hulme@uea.ac.uk, ppn@nerc.ac.uk, cg1@mercury.soc.soton.ac.uk,
marotzke@dkrz.de

date: Fri, 01 Aug 2003 19:22:41 +0100

from: Richard Wood <richard.wood@metoffice.com>

subject: Re: Modelling intercomparison job description

to: Peter Challenor <P.Challenor@soc.soton.ac.uk>

Dear All,

A few further thoughts on the modelling post, partly inspired by recent email discussions.

1. I think it's important that the modelling subcommittee (MSC) has a strong role in the direction of this work. What we are trying to do (I think) is develop a coordinated, UK-wide modelling effort, which if it is successful will go a fair way towards the ultimate deliverable of RAPID (i.e. 'improving our ability to quantify the probability and magnitude of future rapid change in climate'). The MSC itself is an important entity in building this 'community' approach, and the existence of the post would provide both a stick to beat the MSC (myself included!) into doing this, and a resource to help it to happen. If it turns out that there are modelling groups participating in the intercomparison, or engaged in closely related research, who are not currently represented on the MSC, I suggest we consider co-opting a representative onto the MSC. The (enlarged) MSC would at all times remain responsible and accountable to the full RAPID SSC.

2. The individual appointed will need a single person for day-to-day management, and will need to be located somewhere where they can talk to other modellers. How these items are decided is tricky. I had imagined that we would try to recruit someone fairly experienced and self-propelled (Meric has suggested RA2 level), who would be able to play an important role in moving things on scientifically. That would make these issues less critical. However we must recognise that such people are in short supply, so we may need to be flexible in our management arrangements depending on who we can get. I certainly think we must make the post appear as attractive as possible when we advertise it (Meric: I will have another look at the advert and may have a couple of suggestions to help this).

3. I understand there could be sensitivities about fair allocation of resources etc. This would be a new way of working for all of us - a much stronger integration of effort across the UK modelling community - and if we go for it we may have to find a compromise between ultimate perceived fairness and getting started in time to deliver something by the end of RAPID (the timeline in the strawman I sent out in May assumed a start date of Oct 03). Ultimately (as I understand it) the SSC as a whole is responsible for the delivery of the stated aims of the programme, through allocation of RAPID resources within the frameworks provided by NERC. We may need to be bold to achieve this. I'm sure Phil and colleagues will be willing to advise on what is possible.

Richard

--

Richard Wood Manager, Ocean Model Validation and Techniques
Met Office Hadley Centre for Climate Prediction and Research
London Road Bracknell Berkshire RG12 2SY United Kingdom
Tel: +44 (0)1344 856641 Fax: +44 (0)1344 854898
E-mail: richard.wood@metoffice.com <http://www.metoffice.com>

102. 2003-08-04

cc: 'a.ogden@uea.ac.uk'

date: Mon, 04 Aug 2003 07:50:31 +0000

from: "Mick Kelly" <m.kelly@uea.ac.uk>

subject: What a scorcher...

cg2003

to: 'Phil Jones'; David Viner (d.viner@uea.ac.uk)

-----boundary-LibPST-iamunique-1881879640_-_-

Content-Type: text/plain; charset="utf-8"

Phil and David

If we break the high temperature record this week, we really should be prepared to capitalize on this press-wise.

How about we draft a short press release? This has been a classic global warming summer circulation-wise so we can dress the story up a bit.

I'm around Thursday-Friday this week but only briefly Wednesday. Either of you in Wednesday in case it breaks then?

Mick

Mick Kelly Climatic Research Unit
School of Environmental Sciences
University of East Anglia Norwich NR4 7TJ
United Kingdom
Tel: 44-1603-592091 Fax: 44-1603-507784
Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

1981. 2003-08-04

cc: Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth
<trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Tom Wigley <wigley@ucar.edu>,
Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith
Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer
<omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>, Gabi Hegerl
<hegerl@duke.edu>, Mike MacCracken <mmaccrac@comcast.net>, Ellen Mosley-Thompson
<thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu,
wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Urs Neu
<urs.neu@sanw.unibe.ch>, Jürg Beer <beer@hermes.emp-eaw.ch>
date: Mon, 04 Aug 2003 16:02:36 +0200
from: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
subject: Shaviv & Veizer in GSA Today
to: "Michael E. Mann" <mann@virginia.edu>
<x-flowed>

Dear colleagues,

the Soon&Baliunas paper has given political lobbyists a field day in their attempts to confuse the public and decision-makers about the state of global warming science. It is quite interesting how a lobby organisation like the Marshall Institute manages to get a paper like that into the peer-reviewed literature with the help of a sympathetic editor, against reviewer concerns, and then capitalise on that right away in Senate hearings and the media. There clearly is a wider and well-funded strategy behind such activities, which has something to do with why the US has backed out of the Kyoto protocol. These same US organisations are also active here in Europe trying to influence policy, albeit so far with less success.

In the face of such sophisticated lobbying we scientists should not be too naive. Although simply doing good science remains our main job, I think at some points we need to intervene in the public debate and try to clarify what is science and what is just political lobbying. In particular, I feel that it is important to not let bad, politically motivated science stand unchallenged in the peer-reviewed literature - it is too easy to just shrug and ignore an obviously bad paper. Hence I greatly appreciate that Mike and his co-authors responded in Eos to the errors in the Soon&Baliunas paper.

I feel another recent paper may require a similar scientific response, the one by Shaviv&Veizer (attached). It derives a supposed upper limit for the CO2-effect on climate (i.e., 0.5 C warming for CO2 doubling),

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based on paleoclimatic data on the multi-million-year time scale. This paper got big media coverage here in Germany and I guess it is set to become a climate skeptics classic: the spin is that GCMs show a large CO2 sensitivity, but climate history proves it is really very small. Talking to various colleagues, everyone seems to agree that most of this paper is wrong, starting from the data themselves down to the methodology of extracting the CO2 effect.

I think it would be a good idea to get a group of people together to respond to this paper (in GSA today). My expertise is good for part of this and I'd be willing to contribute. My questions to you are:

1. Does anyone know of any other plans to respond to this paper?
2. Would anyone like to be part of writing a response?
3. Do you know people who may have the right expertise? Then please forward them this mail.

Best regards, Stefan

--

Prof. Stefan Rahmstorf

Potsdam Institute for Climate Impact Research (PIK)

For contact details, reprints, movies & general infos see:

<http://www.pik-potsdam.de/~stefan>

</x-flowed>

Attachment Converted: "c:\eudora\attach\shaviv-veizer-03.pdf"

3013. 2003-08-04

cc: n.nicholls@bom.gov.au, Peter.Whetton@csiro.au, Roger.Francey@csiro.au, David.Etheridge@csiro.au, Ian.Smith@csiro.au, Simon.Torok@csiro.au, Willem.Bouma@csiro.au, pachauri@teri.res.in, Greg.Ayers@csiro.au, Rick.Bailey@csiro.au, Graeme.Pearman@csiro.au, mmaccrac@comcast.net, tcrowley@duke.edu, rbradley@geo.umass.edu,
date: Mon, 04 Aug 2003 09:05:47 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: RE: Recent climate sceptic research and the journal Climate

to: "Jim Salinger" <j.salinger@niwa.co.nz>, Phil Jones <p.jones@uea.ac.uk>, Barrie.Pittock@csiro.au, m.hulme@uea.ac.uk, "Neville Nicholls"

<n.nicholls@bom.gov.au>

Dear Jim,

Thanks for your continued interest and help w/ all this. It's nice to know that our friends down under are doing their best to fight the misinformation. It is true that the skeptics twist the truth clockwise rather than counterclockwise in the Southern Hemisphere?

There was indeed a lot of activity last week. Hans von Storch's resignation as chief editor of CR, which I think took a lot of guts, couldn't have come at a better time. It was on the night before before the notorious "James Inhofe", Chair of the Senate "Environment and Public Works Committee" attempted to provide a public stage for Willie Soon and David Legates to peddle their garbage (the Soon & Baliunas junk of course, but also the usual myths about the satellite record, 1940s-1970s cooling, "CO2 is good for us" and "but water vapor is the primary greenhouse gas!").

Fortunately, these two are clowns, neither remotely as sharp as Lindzen or as slick as Michaels, and it wasn't too difficult to deal with them. Suffice it to say, the event did

not go the way Inhofe and the republicans had hoped. The democrats, conveniently, had

received word of Hans' resignation, but the republicans and Soon/Legates had

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not. So when,
quite fittingly, Jim Jeffords (you may remember--he's the U.S. senator who was
in the news
a couple years ago for tilting the balance of power back to the democrats when
he left the
republican party in protest) hit them with this news at the hearing, they were
caught
completely off guard. The "Wall Street Journal" article you cited was icing on
the cake.
Inhofe, who rails against the liberal media, will have a difficult time doing so
against
the WSJ!
Also of interest to you (attached) might be the op-ed that Ray Bradley, Phil,
and I have
written and submitted to the "Seattle News Tribune" in response to an op-ed by
Baliunas
(also attached) that some industry group has been sending around to various
papers over the
last week. Only two (Providence Journal and Seattle NT) have thusfar bitten...
There is a rumour that Harvard may have had enough w/ their name being dragged
through the
mud by the activities of Baliunas and Soon, and that "something is up". Baliunas
and Soon,
as alluded to in the WSJ article, are now no longer talking to the media. Will
keep you
posted on that...
mike

At 03:58 PM 8/4/2003 +1200, Jim Salinger wrote:

Dear Mike et al

I also share Neville's thanks to you all for the reasoned and evaluated
responses over
the last few months. They have been good, and separated out 'academic
standards'

from 'academic freedom', which we have to be careful not to abuse.

I also note the following, come through over the weekend from the Wall Street
Journal

(below) and would also compliment those of you who, with Hans Von Storch
resigned

your editorships when information that should be published was clearly
supressed.

If you have further information that you feel free to share on last week's
events then

we
in New Zealand would appreciate hearing it, as we have been extremely
concerned

about academic standards in the reviewing of articles from New Zealand
sources.

Again thanks to all on your stands.

Best regards

Jim

>>>> July 31, 2003

>>>> DEBATING GLOBAL WARMING

>>>>

>>>> Global Warming Skeptics

>>>> Are Facing Storm Clouds

>>>>

>>>> By ANTONIO REGALADO

>>>> Staff Reporter of THE WALL STREET JOURNAL

>>>>

>>>> A big flap at a little scientific journal is raising questions about

>>>> a study that has been embraced by conservative politicians for its

>>>> rejection of widely held global-warming theories.

>>>>

>>>> The study, by two astronomers at the Harvard-Smithsonian Center for Astrophysics, says the 20th century wasn't unusually warm compared with earlier periods and contradicts evidence indicating man-made "greenhouse" gases are causing temperatures to rise.

>>>> Since being published last January in Climate Research, the paper has been widely promoted by Washington think tanks and cited by the White House in revisions made to a recent Environmental Protection Agency report. At the same time, it has drawn stinging rebukes from other climate scientists.

>>>> This week, three editors of Climate Research resigned in protest over the journal's handling of the review process that approved the study; among them is Hans von Storch, the journal's recently appointed editor in chief. "It was flawed and it shouldn't have been published," he said.

>>>> Dr. von Storch's resignation was publicly disclosed Tuesday by Sen. James Jeffords (I., Vt.), a critic of the administration's environmental policies, during a hearing of the Senate Environment and Public Works Committee called by its chairman, Sen. James Inhofe (R., Okla.).

>>>> The debate over global warming centers on the extent to which gases released from the burning of fossil fuels -- mainly carbon dioxide -- are trapping the sun's heat in the Earth's atmosphere, creating a greenhouse effect. The political fight has intensified as the Senate votes on a major energy bill. Sens. John McCain (R., Ariz.) and Joseph Lieberman (D., Conn.) planned to introduce an amendment this week that would cap carbon-dioxide emissions at 2000 levels starting in 2010 for select industries. The Bush administration is opposed to imposing caps, and the measure isn't expected to become law.

>>>> The Harvard study has become part of skeptics' arguments. Mr. Inhofe, who is leading the opposition to the emissions measures, cited the research in a speech on the Senate floor Monday in which he said, "the claim that global warming is caused by man-made emissions is simply untrue and not based on sound science."

>>>> The paper was authored by astronomers Willie Soon and Sallie Baliunas, and looked at studies of tree rings and other indicators of past climate. Their basic conclusion: The 20th century wasn't the warmest century of the past 1,000 years. They concluded temperatures may have been higher during the "Medieval Warm Period," the time during which the Norse settled Greenland.

>>>> Dr. Soon couldn't be reached and Dr. Baliunas declined comment. In his testimony before Mr. Inhofe's committee, Dr. Soon reiterated the findings of his study, which was partly funded by the American Petroleum Institute.

>>>> Dr. Soon's findings contradict widely cited research by another scientist, Michael E. Mann of the University of Virginia. Dr. Mann's reconstruction of global temperatures shows a distinct pattern shaped >> like a hockey stick: Temperatures stayed level for centuries, with a sudden upturn during recent decades.

>>>> A reference to Dr. Soon's paper previously found its way into revisions suggested by the White House to an EPA report on environmental quality. According to an internal EPA memorandum disclosed in June, agency scientists were concerned the version containing the White House edits "no longer accurately represents scientific consensus on climate change." Dr. Mann's data showing the

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>>>> hockey-stick temperature curve was deleted. In its place,
>>>> administration officials added a reference to Dr. Soon's paper, which
>>>> the EPA memo called "a limited analysis that supports the
>>>> administration's favored message."

>>>>
>>>> The EPA says the memo appears to be an internal e-mail between
>>>> staffers but isn't an "official" document. A spokesman at the White
>>>> House's Council on Environmental Quality says the addition of the
>>>> citation to Dr. Soon's paper to the draft report was suggested during
>>>> an interagency review process overseen by the White House.

>>>>
>>>> Dr. Mann and 13 colleagues published a critique of Dr. Soon's paper
>>>> in Eos, a publication of the American Geophysical Union, this month.
>>>> They said the Harvard team's methods were flawed and their results
>>>> "inconsistent with the preponderance of scientific evidence."

>>>>
>>>> Then, last week Dr. von Storch was contacted by Sen. Jeffords's
>>>> staff, which was looking into the paper in preparation for Tuesday's
>>>> hearing, where Dr. Soon and Dr. Mann were scheduled to appear. After
>>>> hearing from Sen. Jeffords, Dr. von Storch says he decided to speed
>>>> an editorial into print criticizing publication of the paper.

>>>>
>>>> But publisher Otto Kinne blocked the move, saying that while he
>>>> favored publication of the editorial, Dr. von Storch's proposals were
>>>> still opposed by some of the other editors. "I asked Hans not to rush
>>>> the editorial," Mr. Kinne said in an e-mail.

>>>>
>>>> That is when Dr. von Storch resigned, followed by two other editors.

>>>>
>>>> --John J. Fialka contributed to this article.

On 30 Jul 2003 at 8:26, Neville Nicholls wrote:

> Dear Mike et al:

>

> Despite my reluctance to get involved in preparing a public response
> to the SB03 papers, and my feeling that we would be better off
> ignoring it, I have to record my appreciation of the job you have done
> in preparing the EOS 8 July commentary. I thought it was an excellent,
> scientific, calm evaluation of SB03. Fortuitously, it arrived the same
> day I had to prepare a brief about SB03 for my political masters. It
> was very helpful to have your commentary to include in this brief.

>

> Many thanks.

>

> Neville Nicholls

> Bureau of Meteorology Research Centre

> PO Box 1289K, Melbourne, AUSTRALIA, 3001

> Street address: 13th floor, 150 Lonsdale Street, Melbourne, AUSTRALIA,

> 3000 Phone: +61 3 9669 4407; Fax: +61 3 9669 4660

>

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P O Box 109 695, (269 Khyber Pass Road) e-mail: j.salinger@niwa.co.nz

Newmarket, Auckland,

New Zealand

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia

cg2003
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\SeattleNewsTribune-oped-final.doc"
Attachment
Converted: "c:\eudora\attach\BaliunasProvidenceJournal25Jul03.pdf"

4724. 2003-08-04

cc: "Michael E. Mann" <mann@virginia.edu>, Raymond Bradley
<rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil Jones
<p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley
<tcrowley@duke.edu>, Tom Wigley <wigley@ucar.edu>, Scott Rutherford
<srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith Briffa
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<jto@u.arizona.edu>, <stocker@climate.unibe.ch>, Urs Neu <urs.neu@sanw.unibe.ch>,
Jürg Beer <beer@hermes.emp-eaw.ch>
date: Mon, 4 Aug 2003 18:57:27 -0700 (PDT)
from: Eric Steig <steig@ess.washington.edu>
subject: Re: Shaviv & Veizer in GSA Today
to: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
Stephan,

It is perhaps worth noting that there is a strong paleo-argument for CO2 sensitivity being much LARGER than implied by the glacial-interglacial cycles. No one has to my knowledge been able to get high Eocene temperatures in the Arctic, even when including 8*modern CO2 levels. David Battisti has argued that this ought to at least suggest that CO2 sensitivity may be much higher, not lower, than IPCC projections. Of course, bringing up this "absence of knowledge" could backfire unless one were very careful about the writing. Still, you might consider asking David about this.

On another note, I enjoyed reading your "GISP2 clock" paper in GRL. I saw an very nice poster at INQUA by Linda Hinnov. Seems that the 1500-year clock is in GRIP as well. (The original timescale shows a bifurcated spectral peak which she shows is due to artifacts in the dating). I encourage you to contact her. Seems that the 1500-year event spacing isn't going away, much as many of us would like it to! I had hoped we could relegate it to chance but your work and Hinnov's has convinced me otherwise.

Eric Steig

On Mon, 4 Aug 2003, Stefan Rahmstorf wrote:

> Date: Mon, 04 Aug 2003 16:02:36 +0200
> From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
> To: Michael E. Mann <mann@virginia.edu>
> Cc: Raymond Bradley <rbradley@geo.umass.edu>,
> Malcolm Hughes <mhughes@lrr.arizona.edu>,
> Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>,
> Tom Crowley <tcrowley@duke.edu>, Tom Wigley <wigley@ucar.edu>,
> Scott Rutherford <srutherford@gso.uri.edu>,
> Caspar Ammann <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>,
> Tim Osborn <t.osborn@uea.ac.uk>,
> Michael Oppenheimer <omichael@princeton.edu>,
> Steve Schneider <shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>,
> Mike MacCracken <mmaccrac@comcast.net>,
> Ellen Mosley-Thompson <thompson.4@osu.edu>,
> Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu,
> wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch,
> Urs Neu <urs.neu@sanw.unibe.ch>,
> "[ISO-8859-1] Jürg Beer" <beer@hermes.emp-eaw.ch>

cg2003

> Subject: Shaviv & Veizer in GSA Today
>
> Dear colleagues,
>
> the Soon&Baliunas paper has given political lobbyists a field day in
> their attempts to confuse the public and decision-makers about the state
> of global warming science. It is quite interesting how a lobby
> organisation like the Marshall Institute manages to get a paper like
> that into the peer-reviewed literature with the help of a sympathetic
> editor, against reviewer concerns, and then capitalise on that right
> away in Senate hearings and the media. There clearly is a wider and
> well-funded strategy behind such activities, which has something to do
> with why the US has backed out of the Kyoto protocol. These same US
> organisations are also active here in Europe trying to influence policy,
> albeit so far with less success.
>
> In the face of such sophisticated lobbying we scientists should not be
> too naive. Although simply doing good science remains our main job, I
> think at some points we need to intervene in the public debate and try
> to clarify what is science and what is just political lobbying. In
> particular, I feel that it is important to not let bad, politically
> motivated science stand unchallenged in the peer-reviewed literature -
> it is too easy to just shrug and ignore an obviously bad paper. Hence I
> greatly appreciate that Mike and his co-authors responded in Eos to the
> errors in the Soon&Baliunas paper.
>
> I feel another recent paper may require a similar scientific response,
> the one by Shaviv&Veizer (attached). It derives a supposed upper limit
> for the CO2-effect on climate (i.e., 0.5 C warming for CO2 doubling),
> based on paleoclimatic data on the multi-million-year time scale. This
> paper got big media coverage here in Germany and I guess it is set to
> become a climate skeptics classic: the spin is that GCMs show a large
> CO2 sensitivity, but climate history proves it is really very small.
> Talking to various colleagues, everyone seems to agree that most of this
> paper is wrong, starting from the data themselves down to the
> methodology of extracting the CO2 effect.
>
> I think it would be a good idea to get a group of people together to
> respond to this paper (in GSA today). My expertise is good for part of
> this and I'd be willing to contribute. My questions to you are:
> 1. Does anyone know of any other plans to respond to this paper?
> 2. Would anyone like to be part of writing a response?
> 3. Do you know people who may have the right expertise? Then please
> forward them this mail.
>
> Best regards, Stefan
>
> --
> Prof. Stefan Rahmstorf
> Potsdam Institute for Climate Impact Research (PIK)
> For contact details, reprints, movies & general infos see:
> <http://www.pik-potsdam.de/~stefan>
>
>

3655. 2003-08-06

cc: <Patrick.Hofstetter@wwf.ch>, <morgan@wwf.de>, "Sible Schone" <SSchone@wwf.nl>,
"Catarina Cardoso" <CCardoso@wwf.org.uk>, <jleemorgan@wwfepo.org>, "Oliver Rapf"
<ORapf@wwfepo.org>, <ljam@wwfthai.org>, "Katherine Silverthorne"
<Katherine.Silverthorne@WWFUS.ORG>, "Lara Hansen" <Lara.Hansen@WWFUS.ORG>
date: Wed, 06 Aug 2003 15:06:03 +0200
from: "Stephan Singer" <SSinger@wwfepo.org>

cg2003

subject: economic costs of european heat wave

to: <grassl@dkrz.de>, <klaus.hasselmann@dkrz.de>, <per.carstedt@ecosystem.se>, <muel@ermine.ox.ac.uk>, <michael.grubb@ic.ac.uk>, <joyeeta.gupta@ivm.vu.nl>, <Carlo.Jaeger@pik-potsdam.de>, <Martin.Welp@pik-potsdam.de>, <Bert.Metz@rivm.nl>, <m.hulme@uea.ac.uk>, <a-michaelowa@wwfepo.org>, <Berk@wwfepo.org>, <hedger@wwfepo.org>

dear all,

i think we all have seen [if not commented on] the devastating heat wave presently in europe - gives us a feeling on truly global warming. WWF has assured some money - a few thousand EUROS what is not much to be honest but at least a start - to ask an economist with climate policy understanding to assess in a short but fleshy paper [max 10 pages] the economic costs of these weather extremes in europe. This can be put in context with the mitigation costs of ambitious climate policies which are often quoted as a barrier to clean technologies unfortunately. I think, we as an NGO working on climate policy need such a document pretty soon for the public and for informed decision makers in order to get a) a debate started and b) in order to get into the media the context between climate extremes/desasters/costs and finally the link between weather extremes and energy - just the solutions parts what still is not communicated at all.

In short, can you advise us on a competent author who is readily available [can be one of you, of course], to bring together the conventionally accessible costs of reduced transport loads on rivers, in railway networks, forest fires, disruption of water supply and irrigation, closure of hydro power and even nuclear in some locations, health costs, agricultural failures [if accessible] etc etcetc...resulting from the heat wave?

Of course, i could not sent this e-mail to all competent sceintists, so fell free to share please and come back to me - at best ASAP

many regards

stephan singer

Stephan Singer

Head of European Climate and Energy Policy Unit

WWF, the conservation organization

E-mail: ssinger@wwfepo.org

www.panda.org/epo - Stay up-to-date with WWF's policy work in the capital of Europe

www.passport.panda.org - take action on global conservation issues - have you got your Passport yet?

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1215. 2003-08-07

date: Thu, 7 Aug 2003 16:34:06 -0400

from: aiacc <aiacc@agu.org>

subject: Update on AIACC Synthesis Activities

to: bscholes@csir.co.za, hewitson@egs.uct.ac.za, hcnr@sudanmail.net, goutbi@yahoo.com, esiegfried@tellus.org, atgay@ucad.sn, jadejuwo@oauife.edu.ng, desanker@virginia.edu, DUBEOP@mopipi.ub.bw, ogunlade@energetic.uct.ac.za, p_batima@yahoo.com, anond@start.or.th, jratna@itmin.com, rlasco@laguna.net, yongyuan.yin@sdri.ubc.ca, wfer@ariel.efis.ucr.ac.cr, barros@at.fcen.uba.ar, agimenez@inia.org.uy, cgay@servidor.unam.mx, conde@servidor.unam.mx, gunab@glaucus.fcien.edu.uy, rawlinsa@carec.paho.org, achen@uwimona.edu.jm, koshy_k@usp.ac.fj, abouhadid@ruafah@rusys.eg.net, adepetua@unijos.edu.ng, nyongao@hisen.org, rolph@seychelles.sc, knas@connect.co.ke, ian.burton@ec.gc.ca, crrhcr@racsa.co.cr, tom.downing@sei.se, saleemul.huq@iied.org,

cg2003

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lindam@atd.ucar.edu, Peter.Whetton@csiro.au, m.hulme@uea.ac.uk,
ian.burton@ec.gc.ca, crrhcr@sol.racsa.co.cr, tom.downing@sei.se,
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desanker@virginia.edu, Roger.Jones@csiro.au, marengo@cpotec.inpe.br,
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Virji <hvirji@agu.org>, Roland Fuchs <rfuchs@agu.org>,
<adesherbinin@ciesin.columbia.edu>, <crosenzweig@giss.nasa.gov>, sberesford@agu.org
<x-flowed>

To: AIACC Project PIs, Technical Committee, Mentors
Sara and I want to update you on plans for synthesis of the AIACC
project. We are preparing proposals to send to the Rockefeller
Foundation to convene a series of 3 conferences at their Bellagio
Conference Center. The 3 conferences are intended to assist with
synthesis of the AIACC project. The proposed themes of the 3
conferences are:

- 1) Vulnerability to climate change in the developing world
- 2) Managing climatic risks (aka adaptation)
- 3) Food and water security in a changing climate

The conferences would take place in 2005 -- after all or nearly all
of the aiacc projects will have completed their investigations. Each
conference would be organized around a set of questions related to
the main theme. AIACC participants would be invited to submit
abstracts of case study papers that address the questions, drawing
upon their aiacc work. From the submitted abstracts, 16 would be
selected to participate (space limitations at Bellagio prevent us
from inviting papers from all 24 projects to each conference). Those
selected would write and distribute drafts of their case study papers
several weeks in advance of the conference and would be asked to
review 3 of the papers written by other participants before the
conference. The case study papers would be presented at the
conference. But the main work of the conference would be for all
participants to jointly co-author a synthesis paper. The synthesis
paper is intended to draw upon the individual case studies to seek
answers to the conference questions that are robust and can be
generalized across many of the systems, livelihoods and regions that
are the subject of your individual projects. There would also be time
for participants to refine their own papers.

We would publish the synthesis and case study papers of each
conference in a book and submit the synthesis papers to peer review
journals for publication. We might also distribute the synthesis
papers to policy makers in various venues (UNFCCC/COP etc).
Attached is a draft proposal for the first of the 3 conferences that
Sara and I have prepared. We would be glad to receive any feedback on
the proposal and ideas for the 2nd and 3rd conferences. We will be
finalizing the proposal in the next couple weeks. Please also share
any ideas that you have for other synthesis activities that you think
AIACC should undertake.

And if you have ideas of other venues where we might convene these
conferences, please let us know. We selected Bellagio because they
would cover lodging and meal costs during the conferences and offer
the possibility of covering the transportation costs for developing
country participants. Without this support it would be difficult for
AIACC to convene the synthesis conferences.

Kind regards,

Neil

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791. 2003-08-08

cg2003

date: Fri, 8 Aug 2003 12:25:43 +0100 (BST)
from: "Ian Harris (Harry)" <I.Harris@uea.ac.uk>
subject: RE: FW: HOLSMEEER: Data Please!
to: Keith Briffa <k.briffa@uea.ac.uk>

Keith,
Draft response to Roland. D'you think it's a bit harsh?

Harry
Hi Roland,

Yes, we have copies of the paper. In our view it does not impinge on the Holsmeer project work. We've tried to make it clear in meetings that correlation with the NAO is widespread, and that the only deduction to be made from a strong correlation with the NAO is that the measured process is affected by the weather.

The main thrust of this paper is that by calculating non-hydrostatic sea levels, the effects of wind can be decoupled from the effects of pressure. They can then determine the sensitivity of sea level to either, on a spatial basis.

I hope that sets your mind at rest :-)

Cheers

Harry

On Tue, 22 Jul 2003, Roland Gehrels wrote:

> Hi Harry

>

> Have you seen this paper?

>

> Cheers, Roland

>

>

>

> GEOPHYSICAL RESEARCH LETTERS, VOL. 30, NO. 7, 1403, doi:10.1029/2003GL017041, 2003

>

> Sea-level dependence on the NAO over the NW European Continental Shelf

>

> S. L. Wakelin, P. L. Woodworth, R. A. Flather, and J. A. Williams

>

> Proudman Oceanographic Laboratory, Bidston Observatory, Birkenhead,

> Merseyside, UK

>

> Abstract

>

> [1] The connection between changes in the North Atlantic Oscillation (NAO) and the sea level over the northwest European continental shelf is investigated for the period 1955-2000 using a two-dimensional model of tides and storm surges. There is a clear spatial pattern in the correlation between sea level and the NAO on a winter-mean timescale. Correlations are positive (>0.8) in the northeast and negative (<-0.7) in the south. The sensitivity of the sea level to the NAO is strongest in the southern North Sea (up to 96 mm per unit NAO index), where most of the sensitivity is present also in the non-hydrostatic component of sea level. The relationships are validated using observed data recorded at coastal tide gauges.

>

> Received 3 February 2003; accepted 11 March 2003; published 10 April 2003.

>

> Index Terms: 4552 Oceanography: Physical: Sea level variations; 3339 Meteorology and Atmospheric Dynamics: Ocean/atmosphere interactions (0312, 4504); 4215 Oceanography: General: Climate and interannual variability (3309).

>

> -----Original Message-----

> From: Ian Harris (Harry) [mailto:I.Harris@uea.ac.uk]

> Sent: Mon 30/06/2003 11:43

> To: Roland Gehrels

> Cc:

> Subject: Re: FW: HOLSMEEER: Data Please!

cg2003

Hi Roland,

On Fri, 27 Jun 2003, Roland Gehrels wrote:

> Dear Harry

> Just realised we haven't discussed this last email I sent, perhaps it
> got lost. Any comments/ideas?

Politely put ;-)

> Cheers, Roland

> -----Original Message-----

> From: Roland Gehrels

> Sent: 16 April 2003 11:52

> To: 'Ian Harris'

> Subject: RE: HOLSMEER: Data Please!

> Dear Harry

> I am still waiting for dates from Oban and Aarhus, so can't really say
> when I can provide plots. Once I have the dates sorted I will send you
> the four sea-level records from the saltmarsh sites (mean tide level
> height plotted against calendar years for the last 2000 years).

Great.

> In Nice there was a group from Southampton (Mike Tsimplis and others)
> who were doing extensive work on the influence of the NAO on North
> Atlantic sea level. There were 92 tide-gauge records in their analyses
> I think. Are you familiar with that work? I didn't have a chance to
> speak to them as the authors never seem to be present at their
> posters. But I am a bit concerned that we are not re-inventing the
> wheel....

I'm not aware that the major thrust of Holsmeer is to establish links with
the NAO, so I'm not overly worried. I'll pass your concerns on to Phil &
Keith as they may have a more political perspective ;-)

> As for the analyses you did in November, I am sorry I haven't given
> you much feedback yet. The correlation with winter sea levels seems
> very strong. This work is useful when it comes to writing up the
> coastal hazard prediction section for the project. It will tell us
> something about possible increase of flooding frequencies if NAO
> variability is going to be more extreme. My concern is that the salt
> marsh does not record monthly sea levels - it would be more
> decadal-type fluctuations. I suppose if all the sedimentation takes
> place in the winter time the correlation would be useful but it is
> more likely that sedimentation is very slow and more or less
> continuous from year to year. The correlation between the NAO and
> annual sea levels for Cascais and Reykjavik seems more relevant for
> the salt-marsh reconstructions. Are those correlations (0.33 and
> -0.34) significant? Have you come up with anything when you look for
> decadal-scale patterns and lead/lags?

I've not addressed any of these issues yet; but I've put them in the list.

cg2003

> Let me know what you think and whether I can be of any help

> I'll be sure to!

> Cheers

> Harry

> -----Original Message-----

> From: Ian Harris [mailto:i.harris@uea.ac.uk]

> Sent: 15 April 2003 17:32

> To: James Scourse; Graham Forsythe; Phil Jones; Roland Gehrels; Bill

> Austin; Tracy Shimmield; Jon Eiriksson; Dierk Hebbeln; Jan Backman; Hans

> Petter Sejrup; Fatima Abrantes; Guillermo FrancZs-Pedraz; Karen Luise

> Knudsen

> Subject: HOLSMEER: Data Please!

> Dear Partners,

> Dr Phil Jones informs me that at the recent EGS Conference in Nice, it
> was decided that CRU would be inundated with data, both instrumental
> and proxy, from all Holsmeer partners, the better to fulfil our
> obligations under Workpackage 4. We will compare neighbouring proxy
> series with each other and with instrumental data, and provide suitable
> plots for the Delmenhorst meeting.

> Please could you all reply to this letter, giving me an idea of what
> you will be able to provide and when you will be able to provide it.

> As usual I will attempt to decode any data format but my preference is
> for space-, tab- or comma-separated values in a text file.

> I do look forward to hearing from you and trust you are all in fine
> health.

> Cheers

> Harry

> Ian "Harry" Harris

> Climatic Research Unit

> University of East Anglia

> Norwich NR2 4HG

> United Kingdom

> Ian Harris ("Harry")
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> Norwich NR4 7TJ
> Tel 01603 593818
> Fax 01603 507784
> -----

> All opinions stated are those of the author and do not necessarily reflect
> those of the Climatic Research Unit or of the University of East Anglia.
> -----

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All opinions stated are those of the author and do not necessarily reflect those of the Climatic Research Unit or of the University of East Anglia.

4017. 2003-08-12

cc: Keith Briffa <k.briffa@uea.ac.uk>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, Caspar Ammann <ammann@ucar.edu>, heger1@duke.edu

date: Tue, 12 Aug 2003 10:55:50 -0400

from: Tom Crowley <tcrowley@duke.edu>

subject: POLL ON SOON-BALIUNAS

to: "Michael E. Mann" <mann@virginia.edu>

<x-flowed>

Hi there,

we need some data on Soon and Baliunas. one of my concerns is that they only publish in low impact journals and completely bypass the normal give and take of presentations at open scientific meetings (for example, I think I have probably heard 100 presentations overall from the people on this mailing list).

it is therefore very important to inquire for the sake of our exchanges with reporters/legislators etc as to how often any of you may have heard Soon or Baliunas give a talk in an open meeting, where they could defend their analyses.

please respond to me as to whether you have heard either of them present something on their paleo-analyses (I think I heard Baliunas speak once on her solar-type star work, but that doesn't count).

I will let you know the results of the poll so that we may all be on the same grounds with respect to the data and reporting such information to press inquiries/legislators etc.

further fyi I list below the journal impact for six geophysical/climate/paleoclimate journals:

Paleoceanography 3.821

J. Climate 3.250

J. Geophysical Res. (Climate) 2.245

Geophysical Research Letters 2.150

The Holocene 1.852

Climate Research 1.016

Science and Nature are much higher (26-30) but their citation numbers are I believe inflated with respect to our field because their citation ranking also includes many very widely cited biology publications.

hope to hear from you soon, Tom

--

Thomas J. Crowley
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Nicholas School of the Environment and Earth Sciences
Box 90227
103 Old Chem Building Duke University
Durham, NC 27708
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cg2003

919-681-8228
919-684-5833 fax
</x-flowed>

1711. 2003-08-14

cc: rpomerance@aecs-inc.org, asocci@cox.net, Andy Revkin <anrevk@nytimes.com>, Jeff Nesmith <jeffn@coxnews.com>, David Appell <appell@nasw.org>, Chris_Miller@epw.senate.gov, Johannes_Loschnigg@lieberman.senate.gov, arappaport@ucsusa.org, ben Santer <santer1@llnl.gov>, mcarey@environmentaldefense.org
date: Thu, 14 Aug 2003 09:53:06 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Mann and Jones (GRL)
to: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Malcolm Hughes <mhughes@lrr.arizona.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Tom Wigley <wigley@ucar.edu>, Gabi Hegerl <hegerl@duke.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Mike MacCracken <mmaccrac@comcast.net>, Mark Eakin <Mark.Eakin@noaa.gov>, Tas van Ommen <tas.van.ommen@utas.edu.au>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, thompson.3@osu.edu, drdendro@ldeo.columbia.edu, tcronin@usgs.gov, j.salinger@niwa.co.nz, jto@u.arizona.edu, dverardo@nsf.gov, Christopher.d.miller@noaa.gov, ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, Michael Schlesinger <schlesin@atmos.uiuc.edu>, Natasha@atmos.uiuc.edu

Dear Colleagues,
FYI, the following article has just appeared in GRL, and is available online:
Mann, M.E., Jones, P.D., Global surface temperature over the past two millennia, Geophysical Research Letters, 30 (15), 1820, doi: 10.1029/2003GL017814, 2003.
It can be downloaded (pdf format) here:
[1]<ftp://holocene.evsc.virginia.edu/pub/mann/mannjones03.pdf>
best regards,
mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3681. 2003-08-19

cc: Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, "Michael E. Mann" <mann@virginia.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu, wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Urs Neu <urs.neu@sanw.unibe.ch>, Jürg Beer <beer@hermes.emp-eaw.ch>
date: Tue, 19 Aug 2003 09:00:33 -0600
from: Tom Wigley <wigley@ucar.edu>
subject: Re: FW: Shaviv & Veizer in GSA Today
to: André Berger <berger@astr.ucl.ac.be>

<x-flowed>

Andre,

I have been closely involved in the CR fiasco. I have had papers that I refereed (and soundly rejected), under De Freitas's editorship, appear later in the journal -- without me seeing any response from the authors. As I have said before to others, his strategy is first to use mainly referees that are in the anti-greenhouse community, and second, if a paper is rejected, to ignore that review and seek another more 'sympathic' reviewer. In the second case he can then (with enough reviews) claim that the honest review was an outlier.

I agree that an ethics committee is needed and I would be happy to serve on such a committee. It would have to have endorsement by international societies, like Roy. Soc., US Nat. Acad., Acad. Europ., plus RMS, AMS, AGU, etc.

Jim Titus mentioned to me that in the legal profession here people are disbarred for behavior like that of De Freitas (and even John Christy -- although this is a more subtle case). We cannot do that of course, but we can alert the community of honest scientists to such behavior and formally discredit these people.

The Danish Acad. did something like this recently, but were not entirely successful.

In the meantime, I urge people to dissociate themselves from Climate Research. The residual 'editorial' (a word I use almost tongue in cheek) board is looking like a rogues' gallery of skeptics. Those remaining who are credible scientists should resign.

Tom.

+++++

André Berger wrote:

> Dear Stefan,

> Dear Mike,

> Dear Colleagues,

>

> I admire the courage of Stefan and of all other colleagues who are
> willing to answer these highly controverted papers (garbage as Marty
> said). I am personally tired of analysing these papers, having quit
> doing this for the Ministry and European Commission some 5 years ago.

>

> Nevertheless, I am also sad when I see these papers, mostly because they
> succeeded to be published. So not only we have to teach their authors
> the Science of climate but also the reviewers and/or the
> editors/publishers who have accepted them. This is a huge effort. I,
> personally, would like to see an International Committee of Ethics (or
> something like this) in Geo-Sciences be created as it is the case for
> Medical Sciences and Biotechnology.

>

> I have been told that AMS has such a Committee who is a kind of super
> peer-review telling what is wrong in some declarations, papers, books
> Is anybody willing to participate in an attempt to create such a
> Committee within AGU-EGU-IUGG ... ?

>

> In the meantime, I am please to send you here attached an email by R.L.
> Park on Soon, Baliunas, Seitz and others.

>

> Best Wishes and Regards,

>

> André BERGER

>

> -----

>

> WHAT'S NEW Robert L. Park Friday, 8 Aug 03 Washington, DC
> 2. POLITICAL CLIMATE: WHAT'S RIGHT FOR THE AMERICAN PEOPLE?
> One of the purported abuses cited in the minority staff report
> involved the insertion into an EPA report of a reference to a

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> paper by Soon and Baliunas that denies global warming (WN 1 Aug
> 03). To appreciate its significance, we need to go back to March
> of 1998. We all got a petition card in the mail urging the
> government to reject the Kyoto accord (WN 13 Mar 98). The cover
> letter was signed by "Frederick Seitz, Past President, National
> Academy of Sciences." Enclosed was what seemed to be a reprint
> of a journal article, in the style and font of Proceedings of the
> NAS. But it had not been published in PNAS, or anywhere else. The
> reprint was a fake. Two of the four authors of this non-article
> were Soon and Baliunas. The other authors, both named Robinson,
> were from the tiny Oregon Institute of Science and Medicine in
> Cave Junction, OR. The article claimed that the environmental
> effects of increased CO2 are all beneficial. There was also a
> copy of Wall Street Journal op-ed by the Robinsons (father and
> son) that described increased levels of CO2 in the atmosphere as
> "a wonderful and unexpected gift of the industrial revolution."
> There was no indication of who had paid for the mailing. It was
> a dark episode in the annals of scientific discourse.

>
>
>
>
>
>
>

> At 10:59 4/08/2003 -0400, Mike MacCracken wrote:

>
>> You all might want to get in on response to this paper.

>>
>> Mike

>>
>> -----

>> From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
>> Date: Mon, 04 Aug 2003 16:02:36 +0200
>> To: "Michael E. Mann" <mann@virginia.edu>
>> Cc: Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes
>> <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin
>> Trenberth
>> <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Tom Wigley
>> <wigley@ucar.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar
>> Ammann
>> <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn
>> <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve
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>> MacCracken
>> <mmacrac@comcast.net>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric
>> Steig <steig@ess.washington.edu>, jmahilman@ucar.edu,
>> wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Urs
>> Neu <urs.neu@sanw.unibe.ch>, Jürg Beer <beer@hermes.emp-eaw.ch>
>> Subject: Shaviv & Veizer in GSA Today

>>
>> Dear colleagues,

>>
>> the Soon&Baliunas paper has given political lobbyists a field day in
>> their attempts to confuse the public and decision-makers about the state
>> of global warming science. It is quite interesting how a lobby
>> organisation like the Marshall Institute manages to get a paper like
>> that into the peer-reviewed literature with the help of a sympathetic
>> editor, against reviewer concerns, and then capitalise on that right
>> away in Senate hearings and the media. There clearly is a wider and
>> well-funded strategy behind such activities, which has something to do
>> with why the US has backed out of the Kyoto protocol. These same US
>> organisations are also active here in Europe trying to influence policy,

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>> albeit so far with less success.

>>
>> In the face of such sophisticated lobbying we scientists should not be
>> too naive. Although simply doing good science remains our main job, I
>> think at some points we need to intervene in the public debate and try
>> to clarify what is science and what is just political lobbying. In
>> particular, I feel that it is important to not let bad, politically
>> motivated science stand unchallenged in the peer-reviewed literature -
>> it is too easy to just shrug and ignore an obviously bad paper. Hence I
>> greatly appreciate that Mike and his co-authors responded in Eos to the
>> errors in the Soon&Baliunas paper.

>>
>> I feel another recent paper may require a similar scientific response,
>> the one by Shaviv&Veizer (attached). It derives a supposed upper limit
>> for the CO2-effect on climate (i.e., 0.5 C warming for CO2 doubling),
>> based on paleoclimatic data on the multi-million-year time scale. This
>> paper got big media coverage here in Germany and I guess it is set to
>> become a climate skeptics classic: the spin is that GCMs show a large
>> CO2 sensitivity, but climate history proves it is really very small.
>> Talking to various colleagues, everyone seems to agree that most of this
>> paper is wrong, starting from the data themselves down to the
>> methodology of extracting the CO2 effect.

>>
>> I think it would be a good idea to get a group of people together to
>> respond to this paper (in GSA today). My expertise is good for part of
>> this and I'd be willing to contribute. My questions to you are:
>> 1. Does anyone know of any other plans to respond to this paper?
>> 2. Would anyone like to be part of writing a response?
>> 3. Do you know people who may have the right expertise? Then please
>> forward them this mail.

>>
>> Best regards, Stefan

>>
>> --
>> Prof. Stefan Rahmstorf
>> Potsdam Institute for Climate Impact Research (PIK)
>> For contact details, reprints, movies & general infos see:
>> <http://www.pik-potsdam.de/~stefan>

>>
> *****
> Prof. A. BERGER
> Université catholique de Louvain
> Institut d'Astronomie et de Géophysique G. Lemaître
> 2 Chemin du Cyclotron
> B-1348 LOUVAIN-LA-NEUVE
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> Tel. +32-10-47 33 03
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> E_mail: berger@astr.ucl.ac.be
> <http://www.astr.ucl.ac.be> <<http://www.astr.ucl.ac.be/>>
> *****
>
</x-flowed>

1139. 2003-08-20

cc: Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert
<marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera
<kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, Stefan Rahmstorf
<rahmstorf@pik-potsdam.de>, "Michael E. Mann" <mann@virginia.edu>, Raymond
Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil
Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley

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<t.osborn@uea.ac.uk>, Michael Oppenheimer <michael@princeton.edu>, Steve
Schneider <shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson
<thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu,
wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Urs Neu
<urs.neu@sanw.unibe.ch>, Jürg Beer <beer@hermes.emp-eaw.ch>
date: Wed, 20 Aug 2003 09:46:56 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Peer review and Royal Society
to: Tom Wigley <wigley@ucar.edu>, André Berger <berger@astr.ucl.ac.be>

Dear All,

There was a large article in the English newspaper The Guardian yesterday
which reminded me that the Royal Society here in the UK is conducting a review of how
science is reported to the public and the peer-review process. Most of the
impetus here seems to be about the GM crop debate. The article reminded me about an
email that came in July about the initiative.

I'll email Brian Hoskins a brief summary based on Andre's email and some of
the other respondents.

Cheers

Phil

The Royal Society is to launch a wide-ranging consultation among scientists,
the media,

and the public next month, into the best way to communicate the results of
original

research. In the spotlight will be an issue central to the practice of
science-the peer
review process.

Under the chairmanship of Patrick Bateson, the society's biological secretary,
a working

group will produce guidance on best practice, to be published sometime in the
fall. It

will be sent to anyone receiving funding from the Royal Society and to the
fellows, and

it will be disseminated to the wider scientific community both within and
outside

industry. A separate brief is to be produced for the public.

The reports will identify ways in which peer review can be improved to
increase public

confidence in research. They will also consider alternatives to peer review
for

assessing the quality of research results released to the public.

Read the full article at [1]<http://www.biomedcentral.com/news/20030721/02>

[AOL: <a href="[2]<http://www.biomedcentral.com/news/20030721/02>">Read it
here]

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

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3044. 2003-08-21

cc: <d.eastwood@uea.ac.uk>, "'Tim O'Riordan'" <T.Oriordan@uea.ac.uk>

date: Thu, 21 Aug 2003 08:54:53 +0100

from: "Rosie Cullington" <R.Cullington@uea.ac.uk>

cg2003

subject: Governance for Sustainability

to: <J.Greenaway@uea.ac.uk>, <A.Kemp-welch@uea.ac.uk>, <brian.salter@uea.ac.uk>, "Kate Brown" <k.brown@uea.ac.uk>, "Neil Adger" <n.adger@uea.ac.uk>, <M.Stallworthy@uea.ac.uk>, "Mike Hulme" <m.hulme@uea.ac.uk>, "Tim O'Riordan" <t.oriordan@uea.ac.uk>, "Andy Jordan" <a.jordan@uea.ac.uk>, "Andrew Lovett" <a.lovett@uea.ac.uk>, "Andy Jones" <a.p.jones@uea.ac.uk>, "Adrian Martin" <adrian.martin@uea.ac.uk>, "Alan Bond" <alan.bond@uea.ac.uk>, "Chris Foxall" <c.foxall@uea.ac.uk>, "Dick Cobb" <d.cobb@uea.ac.uk>, "Elaine Colk" <elaine.colk@uea.ac.uk>, "Ian Bateman" <i.bateman@uea.ac.uk>, "Iain Lake" <i.lake@uea.ac.uk>, "Matt Cashmore" <m.cashmore@uea.ac.uk>, "Nick Pidgeon" <n.pidgeon@uea.ac.uk>, "Peter Simmons" <p.simmons@uea.ac.uk>, "Robin Haynes" <r.haynes@uea.ac.uk>, "Kerry Turner" <r.k.turner@uea.ac.uk>, "Simon Gerrard" <s.gerrard@uea.ac.uk>, "Trevor Davies" <t.d.davies@uea.ac.uk>

Dear Friends

You may recall I put in a bid under the University's New Professional Initiative, for a possible Chair and Programme in Governance for Sustainability.

The aim was not just to establish UEA as a lead institution in this area. It was also designed to create a core of joint research support amongst social scientists in the wider arena of governance.

I have just hear from the Vice-Chancellor that there is no money in the NPI for this position, and that it is unlikely that there will be any new move in the NPI direction for at least 18 months. This is a setback. But I hope we can still be positive about establishing a network of interested researchers in this theme.

So I hope that it may be possible to maintain a dialogue on this general topic. As many of you know, I am organising a workshop on this theme as part of the Zuckerman week on Friday 5 September. Many of you have been invited to that. If anyone else would like to come, please let me know. The workshop will be held in the ZICER Seminar Room at 10.00 on Friday 5 September. A copy of the programme is attached.

In addition, Brian Salter and I would like to establish a governance network across the university. To this end he has suggested a regular seminar on the topic. The first one is scheduled for Monday 24 November at 17.00 (in a room to be agreed). Ted Tapper of the University of Sussex will talk of the politics of governance and the RAE. I will also summarise the main findings of the 5 September workshop.

We would be grateful if you would keep this date in your diaries. We will give you more details in due course. If you know of any colleague who also might be interested, please also let me know.

Best wishes

Tim O'Riordan

(t.oriordan@uea.ac.uk)

Rosie Cullington

Faculty Secretary

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ UK

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Email. r.cullington@uea.ac.uk

Office Hours - 0830-1630 GMT/BST

Attachment Converted: "c:\eudora\attach\Programme.doc"

5168. 2003-08-22

cc: André Berger <berger@astr.ucl.ac.be>, Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley

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jto@u.arizona.edu, stocker@climate.unibe.ch, Jürg Beer <beer@hermes.emp-eaw.ch>,
Ben Santer <santer1@llnl.gov>, robert Berner <robert.berner@yale.edu>,
mann@virginia.edu

date: Fri, 22 Aug 2003 12:13:22 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Shaviv & Veizer in GSA Today

to: urs.neu@sanw.unibe.ch, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>

Dear All,

This is biased coverage provided by the "World Business Council", attempting to
provide a
platform for the two contrarians here (Zachichi and Shaviv).

Ben Santer, David Parker, and I have also given presentations and press
briefings here, and

the Italy media has been pretty good so far about presenting our side (i.e.,
the consensus
view) on climate change. Look out for better coverage.

Re, the Shaviv and Veizer paper--after seeing Shaviv present this, I'm now more
convinced

than ever that there is not one single scientifically defensible element at all
to what he

has done--the statistics, supposed climate reconstruction, and supposed "Cosmic
Ray Flux"

estimates are all almost certainly w/out any legitimate underpinning. Those w/
the

appropriate expertise on the specifics really need to get a response out ASAP.
My

understanding is that something is indeed already in the works from Stefan et
al...

mike

At 05:48 PM 8/22/2003 +0200, Urs Neu wrote:

Dear Stefan, dear colleagues

The following link shows, that the Shaviv and Veizer paper is widely
"used" and gets more and more impact (or does more and more harm). So it
seems to be important to get a comment as quickly as possible.

Yours, Urs

[1]<http://www.wbcsd.org/plugins/DocSearch/details.asp?type=DocDet&DocId=2058>

Stefan Rahmstorf wrote:

> Dear André ,

>

> thanks for the encouragement. I was aware of the fake PNAS paper sham,
> but did not know that Soon and Baliunas were involved in that one - so
> that is useful information.

> The Shaviv & Veizer paper had its appearance recently on a prime
> political discussion programme ("Presseclub") on the first German TV
> channel. One journalist on the programme mentioned the senate hearing
> surrounding the Soon&Baliunas paper as an example of dubious lobbyist
> activities in the US. Another journalist responded by saying that not
> only lobbyists but also serious scientists were questioning global
> warming, and talked about Shaviv and Veizer as example.

>

> Cheers, Stefan

>

> --

> Stefan Rahmstorf

> Potsdam Institute for Climate Impact Research (PIK)

> For contact details, reprints, movies & general infos see:

> [2]<http://www.pik-potsdam.de/~stefan>

--

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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2814. 2003-08-23

cc: Phil Jones <p.jones@uea.ac.uk>, Gavin Schmidt <gavin@isis.giss.nasa.gov>,
Michael Oppenheimer <omichael@princeton.edu>, Mike MacCracken
<mmaccrac@comcast.net>, Tom Crowley <tcrowley@duke.edu>, cfk@lanl.gov,
jhansen@giss.nasa.gov, Ellen Mosley-Thompson <thompson.4@osu.edu>,
rbradley@geo.umass.edu, mhughes@lrr.arizona.edu, Keith Briffa
<k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tim Osborn
<t.osborn@uea.ac.uk>, Gabi Hegerl <hegerl@duke.edu>, Stefan Rahmstorf
<rahmstorf@pik-potsdam.de>, jto@u.arizona.edu, Eric Steig
<steig@ess.washington.edu>, mann@virginia.edu
date: Sat, 23 Aug 2003 04:04:54 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: [Fwd: VS: [Climate Sceptics] Mann & Jones on 1800 yrs proxies]
to: Tom Wigley <wigley@ucar.edu>

Thanks Tom,

I agree--the issue is not completely settled, and thanks for the reference (any possibility

you can send me a reprint?). The point here of course is that we are talking a potential

effect, w/ as you say, at best a weak signal--hardly the dominating overprint that is

argued by the Idso brothers! (by the way, weren't they a circus act at one point??),

mike

At 12:48 PM 8/22/2003 -0600, Tom Wigley wrote:

Mike,

Thanks for your clarifications.

With regard to the CO2 fertilization effect on tree ring width, I wrote a paper a number

of years ago pointing out that there were signal-to-noise problems in identifying and

quantifying such factors.

Wigley, T.M.L., Jones, P.D. and Briffa, K.R., 1987: Detecting the effects of acidic

deposition and CO2-fertilization on tree growth. (In) Methods of Dendrochronology.

Vol. 1, Proceedings of the Task Force Meeting on Methodology of Dendrochronology:

Kraków, Poland, 26 June 1986, (eds. L. Kairiukstis, Z. Bednarsz and E. Feliksik),

International Institute for Applied Systems Analysis, Agricultural Academy of Kraków,

Polish Academy of Science, WOSI Wspólna Sprawa 38/37 no. 20, 239253.

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1988.

While I am confident that you are correct, and that this is not a crucial factor, I think one should be careful about denying its existence. There are, furthermore, additional obfuscating factors that make the effects of CO2 fertilization on ring widths hard to identify.

Perhaps more important is the fact that many tree ring based reconstructions use density data, and the jury is still out on whether more CO2 increases or decreases density.

Tom.

+++++

Michael E. Mann wrote:

Dear Colleagues,

Several of you have inquired about the below claims by the notorious "Idso brothers" which relates to the paper by Mann and Jones that appeared in GRL a couple weeks ago.

Of course, it's the usual disinformation we've come to expect from these folks, but a few details on why:

1) The supposed "CO2 fertilization" argument is a ruse. The only evidence that such an effect might actually play some role in tree-growth trends has been found in high

elevation sites in western North America (consult Malcolm Hughes for more details). As

in Mann et al '99 (GRL), any such effect, to the extent it might exist, has been removed

from the relevant series used in the latest (Mann and Jones) paper through the removal

of anomalous differences between low-elevation and high-elevation western North American

temperature trends during the post 1800 period, prior to use of the data in climate

reconstruction.

2) We haven't in the past extended the proxy reconstruction beyond 1980 because many of

the proxy data drop out. However, the repeated claim by the contrarians that post-1980

proxy data don't show the warming evident in the instrumental record has finally

prompted me to go ahead and perform an additional analysis in which the proxy-reconstruction is extended forward as recently as at all possible (to

1995, for which 3 out of 8 of the NH records are available, and 1 of the 5 SH records

are available). The SH and GLB reconstructions are thus obviously tenuous at best, but they

do address, to the extent at all possible, the issue as to whether or not the proxy

reconstructions show the post-1980 warming--and they do.

See the attached plot which compares the NH (blue), SH (green), and GLB (red) series

through 1995. The late 20th century is the nominal maximum for all 3 series

without any consideration of the information in the instrumental mean series. This thus

refutes the 2nd criticism cited by the Idso brothers.

One note about the 40 year smoothing. As in the trends in the instrumental series shown

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by Mann and Jones, a boundary constraint on the 40-year smooth has been used that minimizes the 2nd derivative at the boundary--this trends to preserve the trend near the end of the series and has been argued as the optimal constraint in the present of nonstationary behavior near the end of a time series (Park, 1992; Ghil et al, 2002). I favor the use of this constraint in the smoothing of records that exhibit a significant trend as one approaches the end of the available data. This might be worth talking about in the next IPCC when the subject of adopting uniform standards for smoothing data, etc. are discussed... In retrospect, Phil and I should have included this analysis in the GRL article, but its always hard to know what specifics the contrarians are going to target in their attacks. This analysis however, will be included in a review paper by Jones and Mann on "climate in past millennia" that is presently being finalized for "Reviews of Geophysics". I hope that helps clarify any questions any of you might have had. Please feel free to pass this information along to anyone who might benefit from it. Now, back to fighting the "Shaviv and Veizer" propaganda along w/ Ben Santer and David Parker out in Italy...

mike

----- Original Message -----

Subject: VS: [Climate Sceptics] Mann & Jones on 1800 yrs proxies

Date: Wed, 20 Aug 2003 13:52:40 +0300

From: Timo Hämeranta <timo.hameranta@pp.inet.fi>

To: <climatesceptics@yahooogroups.com>

CC: "Charles F. \"chick\" Keller" <cfk@lanl.gov>, "Kirill Ya.

Kondratyev" <kondratyev@KK10221.spb.edu>, "Michael C. MacCracken"

<mmaccrac@comcast.net>, "S. Fred Singer" <singer@sepp.org>, "Sallie

Baliunas" <baliunas@cfa.harvard.edu>, "Carl Wunsch" <cwunsch@mit.edu>,

"David R. Legates" <legates@udel.edu>, "George Kukla"

<kukla@ldeo.columbia.edu>, "James E. Hansen" <jhansen@giss.nasa.gov>,

"Tom Wigley" <wigley@meeker.ucar.edu>, "Willie Soon" <wssoon@cfa.harvard.edu>

Dear all,

GRL finally published the study

Mann, Michael E. and Phil D. Jones, 2003. Global surface temperatures over the past two millennia, Geophysical Research Letters Vol. 30, No. 15, 1820, 10.1029/2003GL017814, August 14, 2003

Abstract

[1] We present reconstructions of Northern and Southern Hemisphere mean surface temperature over the past two millennia based on high-resolution ?proxy? temperature data which retain millennial-scale variability. These reconstructions indicate that late 20th century warmth is unprecedented for at least roughly the past two millennia for the Northern Hemisphere. Conclusions for the Southern Hemisphere and global mean temperature are limited by the sparseness of available proxy data in the Southern Hemisphere at present.

We already noticed the study in

Mann, Michael, Caspar Ammann, Kevin Trenberth, Raymond Bradley, Keith Briffa, Philip Jones, Tim Osborn, Tom Crowley, Malcolm Hughes, Michael Oppenheimer, Jonathan Overpeck, Scott Rutherford, and Tom Wigley, 2003. On Past Temperatures and Anomalous Late-20th Century Warmth. Eos, Vol. 84, No. 27, page 256, July 8, 2003

There we found that " an extension back through the past 2000

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years based on eight long reconstructions [Mann and Jones,2003]."

CO2 Science Magazine today presents the study as follows:

Was Late 20th Century Warming Really Unprecedented Over the Past Two Millennia?

Mann, M.E. and Jones, P.D. 2003. Global surface temperatures over the past two millennia. Geophysical Research Letters 30: 10.1029/2003GL017814.

What was done

Using 23 individual proxy records from 8 distinct regions in the Northern Hemisphere and 5 proxy records from the Southern Hemisphere, the authors constructed Northern and Southern Hemispheric and global mean temperature histories over the period AD 200 to as close as they could get to the present employing a 40-year lowpass filter of the data.

What was learned

Mann and Jones say their temperature reconstructions indicate that "late 20th century warmth is unprecedented for at least roughly the past two millennia for the Northern Hemisphere." They also say their data and analysis "suggest a similar, but less definitive conclusion, for the global mean."

Although we and many others have many bones to pick with many aspects of Mann and Jones' analysis, we will here focus on just a couple of points and temporarily grant them the benefit of the doubt in those other areas. First of all, granting them almost everything they have done, it can readily be seen from their own graph of their own results that the end point of their reconstructed global mean temperature history is not the warmest period of the prior 1800 years. In fact, their treatment of the data depicts three earlier warmer periods: one just prior to AD 700, one just after AD 700 and one just prior to AD 1000 (see figure below).

Reconstructed global temperature anomaly (based on 1961-1990 instrumental reference period) adapted from Mann and Jones (2003).

The globe only becomes warmer in the 20th century when its measured temperatures are substituted for its reconstructed temperatures. This approach is clearly unacceptable; it is like comparing apples and oranges. If one has only reconstructed temperatures from the distant past, one can only validly compare them with reconstructed temperatures from the recent past.

Another important point that is ignored by Mann and Jones is that the last century witnessed a dramatic increase in atmospheric CO2 concentration, which everyone knows is an effective aerial fertilizer. It also witnessed a dramatic increase in atmospheric nitrogen deposition, which further enhances plant growth. Consequently, as tree-ring data comprise the bulk of the proxy temperature information employed by Mann and Jones, their reconstructed global mean temperature history must possess a non-temperature-induced pseudo-warming signal driven by CO2- and nitrogen-induced increases in growth that make 20th century warming appear significantly greater than it really is. Hence, there could well be still other periods of the past 1800 years (in addition to the three we have already noted) when the global mean temperature was also warmer than it was at the end of their reconstructed record in the 20th century.

What it means

Mann and Jones have clearly failed to demonstrate the key point they desired to make in their paper. Their data, however, speak for themselves in clearly demonstrating that late 20th century warmth was not unprecedented over the past two millennia.

????

We have already discussed about this study in July under title ?Empire Strikes back on Soon et al.?

All the best

Timo Hämeranta

Moderator, Climatesceptics

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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2424. 2003-08-26

date: Tue, 26 Aug 2003 16:18:11 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Fwd: European TT draft
to: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
Keith, Tim

These may be of interest.

Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 5.1.1
Date: Tue, 26 Aug 2003 08:21:39 -0400
To: Phil Jones <p.jones@uea.ac.uk>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Fwd: European TT draft

Phil,

Have you seen this draft from Juerg? I gave him a few minor comments.

Mentioned your

recent paper w/ Tim and Keith on seasonal cycle changes. Looks like its
formatted for

Nature (?).

We should probably try to reference in the ROG paper. Thoughts?

The conclusion of anomalous late 20th century European winter temperatures
seems

important, and I'm not sure this has been established confidently in past
work, so seems

like novel conclusion. Of course, the paper is not yet submitted, so we would
have to

reference it as "submitted"...

thoughts?

mike

Delivered-To: mem6u@virginia.edu
Date: Sun, 24 Aug 2003 22:31:11 +0200
From: Juerg Luterbacher <juerg@giub.unibe.ch>
To: mann@virginia.edu

Subject: European TT draft

User-Agent: Internet Messaging Program (IMP) 3.1

X-Originating-IP: 130.92.5.100

X-Virus-checked: by University of Berne

Hello Mike

I hope you are very well!

Here, summer seems to come to an end soon, maybe the hottest ever,
dry and so sunny, great!

Finally, I have just finished a first draft on the 500 year European
temperature variability. We already talked about it a bit.

I would like to ask you whether you could read it through once and give me
your

opinion, your comments, corrections, etc? It would be great if you could tell
me what you think about it and what to change, add, etc.

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I have attached the text as a winword document and the three figures as .pdf.

We will now leave for our honey moon to Norway (connected with the workshop in Bergen) and will come back the 9th of September. Would it be possible for you to read it over until then? This would really be great! Please, could you keep it confident, thanks.

Now, I wish you a good time, thanks very much in advance and many greetings from Bern
Juerg

This mail sent through IMP at [1]<http://mail.unibe.ch>

Professor Michael E. Mann
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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Attachment Converted: "c:\eudora\attach\manuscript_TT.doc" Attachment Converted:
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"c:\eudora\attach\Figure2.pdf"
Attachment Converted: "c:\eudora\attach\Figure3.pdf"

445. 2003-08-27

cc: <Roger.Jones@csiro.au>
date: Wed, 27 Aug 2003 09:35:13 +1000
from: <Simon.Torok@csiro.au>
subject: RE: Barrier Reef icon project
to: <nick.brooks@uea.ac.uk>, <m.hulme@uea.ac.uk>, <n.adger@uea.ac.uk>,
<e.tompkins@uea.ac.uk>

Hi Nick,

I like the social angle. The scientific angle has been done to some extent but I'm not aware of the angle you've suggested being done.

Roger Jones has just been involved in a Reef study that received a fair degree of media coverage (see <http://www.dar.csiro.au/news/2003/mr08.html> for a summary, with a link to the report at the bottom).

Most work on the Reef would be done by AIMS (Australian Institute of Marine Science - <http://www.aims.gov.au/>), James Cook University (<http://www.jcu.edu.au/>) and the Reef Cooperative Research Centre (<http://www.reef.crc.org.au/>) so it would be worth a quick look at their publications.

Cheers, Simon.

-----Original Message-----

From: Nick Brooks [<mailto:nick.brooks@uea.ac.uk>]

Sent: Tuesday, 26 August 2003 11:14 PM

To: dust-health; Neil Adger; Torok, Simon (AR, Aspendale); Emma Tompkins

Subject: Barrier Reef icon project

Dear Mike and Neil (copied to Simon Torok and Emma Tompkins for feedback)

In a meeting within the last few months Mike mentioned the idea of looking at the implications of climate change for what he termed "global icons", ie areas, regions and features that had a high profile in the public consciousness globally. One of these suggested icons was the Great Barrier Reef.

I've just come back from Queensland, where I made two trips out to the reef, visiting 3 sites for snorkelling. All three sites were in a pretty poor condition,

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with some 80 percent of the coral being dead. On each trip I spoke to the people running the boat about this, and on one occasion was told that it was due to cyclone damage, and on the other was informed that the destruction was caused by crown of thorns starfish. Speaking to other tourists it became apparent that this sort of destruction is widespread - everyone who had dived or snorkelled before said they were either shocked or disappointed by the state of the reef. It also became apparent that there was always a local "reason" for the destruction - cyclones, starfish, fertilizers from the sugar cane plantations (even 40 - 70 km offshore).

The person running the lodge where we stayed (who I believe used to be an environmental journalist) had a different explanation, putting the destruction down to bleaching resulting from anomalously hot summers. Of course damage from tourism could also be an explanation.

While my visit and various conversations represent only a very superficial survey, they suggest a couple of testable hypotheses that could be turned into a research project:

i) There is widespread systematic damage to the reef resulting principally from higher summer sea surface temperatures, combined in places with other hazards (in fact the former would make the coral more vulnerable to the latter).

ii) A perception of an "unchanging" environment leads people to generate local explanations for coral loss based on transient phenomena, while not acknowledging the possibility of systematic damage from long-term climatic/environmental change. (i) could be tested by examining the spatial distribution of damage and its relationship with sea surface temperature anomalies, cyclone tracks, recorded pest and pollution events and other such factors (eg frequency of tourist visits). Sites exhibiting different levels of damage could be used as training sites for characterising damage using satellite imagery - for example using mixture modelling with Thematic Mapper imagery.

(ii) would be a survey/questionnaire based study.

Such a project could do a lot to raise awareness of threats to the reef from climate change (depending on the outcome of the research!) and of the climate change issue generally, particularly in Australia, but also around the world. The project would be reasonably straightforward to design, and would be a good interdisciplinary study, and could strengthen links between Tyndall and Australian institutes such as CSIRO.

Simon - do you know if there is anything along these lines going on already? I'm sure there's been plenty done on the reef, and having just spoken to Emma it seems there is quite a lot of general reef literature out there. Not having any experience in this field it might be naïve of me to think I've come up with something original!

If this is viable it could be another means of keeping me employed if someone wants to act as PI and develop it with me, or could be turned into a PhD project, or both - there is potentially a lot of work here.

Your feedback is welcome.

Nick

--

Dr Nick Brooks
School of Environmental Sciences
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Tel: +44 1603 593904
Fax: +44 1603 593901

Email: nick.brooks@uea.ac.uk <http://www.cru.uea.ac.uk/~e118/welcome.htm> (personal site) <http://www.tyndall.ac.uk> (Tyndall Centre site) <http://www.uea.ac.uk/sahara> (Saharan Studies Programme)

--

1555. 2003-09-02

cc: André Berger <berger@astr.ucl.ac.be>, Urs Neu <urs.neu@sanw.unibe.ch>, Martin Hoffert <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, "Michael E. Mann" <mann@virginia.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes

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<mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, <jmahilman@ucar.edu>, <wuebbles@atmos.uiuc.edu>, <jto@u.arizona.edu>, <stocker@climate.unibe.ch>, Jürg Beer <beer@hermes.emp-eaw.ch>

date: Tue, 02 Sep 2003 15:06:06 -0400

from: Mike MacCracken <mmaccrac@comcast.net>

subject: Re: anti-CO2

to: Gabi Hegerl <hegerl@duke.edu>, Tom Wigley <wigley@ucar.edu>

To all--Before everyone gets into this anti-Vatican talk on climate change, I would urge you to read the statement of the US Bishops on Climate Change at <http://www.usccb.org/sdwp/international/globalclimate.htm>

I think it is one of the most thoughtful statements from the moral community on the issue.

In saying this, I need to mention my involvement, however. Several years ago the AAAS Committee on a Dialog between Science and Religion, which normally addresses issues dealing with biotechnology, etc., held a session on climate change. I was asked to give the science talk. The meeting also then had presentations by representatives of many of the major religions about their philosophical view toward the environment and human activities. Very interesting.

Although I am not Catholic (or much of anything), I was asked to serve as the science expert on a committee put together by the US Catholic Conference (under its Environmental Justice committee), which is the staff organization that supports the Bishops. It had representatives of various groups within the Catholic Church and often got rather theological, etc. (I kept asking how this would be understood more widely). In the end, their approach to the statement was to recognize that the Bishops are not experts in science (so they accepted the IPCC as representing the science), are not experts in technology (so they did not get into what technologies to pursue), are not experts in politics (so did not come out for or against Kyoto--quite a number of Protestant churches did come out specifically for Kyoto), etc.--but that their (the Bishops) expertise is in stating the moral underpinning on which the discussion should be based. As the statement indicates, they then focused on issues of equity and stewardship.

During our discussions, a representative of the equivalent part of the Vatican bureaucracy came over and spoke with us. Their representative basically said they did not have the necessary level of expertise to be the first part of the Church to address this issue and figure out they type of statement to mak--too much else on their plate. So, they were happy the US conference was developing its statement, and they followed along with it. I rather imagine they could have vetoed it if they had wanted. This statement, however, as I recall, passed unanimously. Unfortunately, its exposure has been overshadowed somewhat by other matters, but I am told they have gotten the word around about it.

So, I would suggest not pushing this idea of Zichichi adopting his position due to a view of the Catholic Church--having met him during nuclear winter discussions in the mid 1980s, I would guess the explanation lies elsewhere (and I have written Zichichi directly as did Andre expressing my concern about his views).

Mike MacCracken

> From: Gabi Hegerl <hegerl@duke.edu>

> Date: Tue, 02 Sep 2003 10:48:31 -0400

> To: Tom Wigley <wigley@ucar.edu>

> Cc: André Berger <berger@astr.ucl.ac.be>, Urs Neu <urs.neu@sanw.unibe.ch>,

> Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert

> <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera

> <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, "Michael E.

> Mann" <mann@virginia.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm

> Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin

cg2003

> Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott
> Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith
> Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael
> Oppenheimer <omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>,
> Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig
> <steig@ess.washington.edu>, jmahlman@ucar.edu, wuebbles@atmos.uiuc.edu,
> jto@u.arizona.edu, stocker@climate.unibe.ch, Jürg Beer
> <beer@hermes.emp-eaw.ch>
> Subject: Re: anti-CO2
>
> Such a committee would be very helpful, I agree.
> It could also address issues such as publicising who funds scientists
> with political influence
> (if its true the vatican sponsors "sceptics" (pun intended), that would
> be an outrage
> to lots of people, adding a few degrees to the already very hot water
> these guys are in
> with their base)!!
>
> by the way, the ultraconserveative guys would be those who would insist
> that if god would
> want to save the planet, HE would do it. The others would
> argue that people are the stewarts of the planet and have to act
> responsibly, there is lots of
> writings of catholic theologians and I am sure also theologians of other
> faiths on that...
>
> Gabi
>
>
> Tom Wigley wrote:
>
>> Andre,
>>
>> I agree. I heard that Zichichi has links with the Vatican. A number of
>> other greenhouse skeptics have extreme religious views. Perhaps they
>> believe that god would not let us do this to the planet, and that, if
>> we do, she will save us?
>>
>> Hmmm.
>>
>> Tom.
>> +++++
>>
>>
>> André Berger wrote:
>>
>>> Dear All,
>>>
>>> More I read your email about the "anti-CO2", more I am convinced that
>>> an International Committee on Ethics in Geo-Sciences is needed.
>>> Indeed either we do not answer their attacks or we lose time and
>>> money doing it. The third solution is an official statement telling
>>> what the members of such a Committee of Ethics think about
>>> irresponsible statements by such anti-CO2 fellows.
>>>
>>> Best Regards,
>>>
>>> André
>>>
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>>>
>>>

cg2003

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>>>
>>> *****
>>> Prof. A. BERGER
>>> Université catholique de Louvain
>>> Institut d'Astronomie et de Géophysique G. Lemaître
>>> 2 Chemin du Cyclotron
>>> B-1348 LOUVAIN-LA-NEUVE
>>> BELGIUM
>>> Tel. +32-10-47 33 03
>>> Fax +32-10-47 47 22
>>> E_mail: berger@astr.ucl.ac.be
>>> http://www.astr.ucl.ac.be
>>> *****
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> --
> -----
> Gabriele Hegerl
> Division of Earth and Ocean Sciences
> Nicholas School for the Environment and Earth Sciences
> Duke University, Durham NC 27708-90227
> Ph: 919 684 6167, fax 684 5833, email: hegerl@duke.edu
> http://www.nicholas.duke.edu/faculty/bios/hegerl.html
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1848. 2003-09-02

date: Tue, 02 Sep 2003 18:43:22 +0100
from: John Turnpenny <j.turnpenny@uea.ac.uk>
subject: Re: Adaptation paper and post-meeting thoughts
to: Nick Brooks <nick.brooks@uea.ac.uk>, John Schellnhuber
<H.J.Schellnhuber@uea.ac.uk>, Alex Haxeltine <alex.haxeltine@uea.ac.uk>,
dust-health <m.hulme@uea.ac.uk>, Emma Tompkins <e.tompkins@uea.ac.uk>, Neil Adger
<n.adger@uea.ac.uk>, Rachel Warren <r.warren@uea.ac.uk>, Jonathan Koehler
<J.Koehler@econ.cam.ac.uk>
<x-flowed>

dear Nick,
thank you for your analysis and passionate words, which are often missing
from academic discourse. As you know, I think it vital that the areas of
research you are suggesting be included within climate change research. We
need to be able to address the questions "what OUGHT we to do?" as well as
"what CAN we do?".

i think your characterisation of the two opposing camps certainly has some
truth in it, especially at the scale of world politics, but i would
describe it somewhat differently. Your difference is between the
'prometheans' and the 'green' approach, but i believe the true
'prometheans' are a small minority, with the vast majority having some
sympathy with the need for some type of sustainability. The dominance of
prometheans on the global stage at present is, i believe, not permanent,
and has skewed the picture about where the real battle front lies which is
in the divide within the 'sustainability' camp between "light" and "deep"
green.

Both types of green are opposed to the prometheans, but have very different
concepts of what is meant by sustainability. Many people and

cg2003

organisations, including the UK government, have a light green approach - for example, while considering environmental issues, they argue that economic growth is the overarching objective. Your greens, Nick, which you set up as in opposition to the prometheans, "seek to preserve the environment and foster stability, in which human beings are seen as one part of the natural world rather than as separate to it and dominating it". I would say this is a much deeper green position and is opposed to the lighter approach. In all seriousness, I have heard the precautionary principle explained as "we shouldn't stop development while the knowledge of damage is uncertain", and this from a hydrologist who considers himself green.

All in all, I think the trend in the west is slowly towards very weak sustainability and the real conflict will be between this and the deeper approach which is much more radical, more environmentally and socially aware, and would require major changes in the way societies are run.

By the way, I disagree that religion is a part of the promethean outlook. Perhaps one particular cultural brand of protestantism is so (for example, the woman I met in Santa Barbara last year who sincerely thanked God for having enough money to have a facelift), but this is the exception. Most religions (not least Islam and many many Christians) oppose the promethean ideology, not least because it is a form of idolatry. Cheers, John

At 11:14 30/08/03 +0100, Nick Brooks wrote:

>Dear all

>

>Attached is a working paper that will be coming out over the next few weeks
>(once Laura has cleared the admin backlog from the Sustainability Days). It
>is relevant to the discussions we had in the Tyndall-PIK meeting that some
>of you were at earlier today, and the rest of you might find it of interest
>and want to comment. One or two of you reviewed it so the final version
>might be of interest, if only for the records.

>

>Also in this morning's meeting the topic of economics and the need to move
>beyond the neo-classical model came up. I think it might be useful to
>explore the philosophical background to the current debate on economics and
>the environment, although I don't know whether this is the sort of thing it
>would be appropriate for Tyndall to engage in publicly. My own thoughts on
>the philosophical context are summarised below for anyone who is interested.

>

>I think the coming century will be characterised to a large extent by a
>clash between a "promethean" approach to the environment that seeks to push
>technology (and economic growth) forward with little or no attention to risk
>and environmental stability, counting on the same technology to get us out
>of any tight corners, and a "green" approach that seeks to preserve the
>environment and foster stability, in which human beings are seen as one part
>of the natural world rather than as separate to it and dominating it (as in
>the promethean view). I think this conflict is already underway, between the
>promethean extreme neo-classicists, and the sustainability movement. Climate
>change is the most obvious battleground between these two opposing
>philosophies, with the prometheans deploying all means at their disposal to
>oppose sustainability. Neo-classical economics is now a supporting ideology
>for the promethean view, and in the United States has effectively become a
>state religion deployed against those who challenge the economic or
>political orthodoxy. This orthodoxy has also to a certain extent merged with
>traditional religion and the enterprise culture to produce a philosophy in
>which any attempt to manage innovation, enterprise or economic growth is
>seen as morally wrong. I think this explains some of the vehemence of the
>opposition to processes such as Kyoto - it isn't just a question of costs
>and benefits, but rather a question of the struggle between good and evil,
>with any perceived interference with US policy being firmly identified with
>the latter. (See "America is a religion" by George Monbiot in the Guardian
>for an interesting discussion on this:

><http://www.guardian.co.uk/comment/story/0,3604,1007741,00.html>).

>
 >I think we should recognise that much of the argument against mitigation is
 >based on what I call "faith based economics", which starts from the certain
 >knowledge that growth is always good and any attempts to manage or constrain
 >economic activity are bad - thus anything argument that suggests such a
 >course of action must by definition be wrong. Economic models are wheeled
 >out to support this line, as long as they are the right models. The
 >promethean movement is absolutely certain that we will always be able to
 >solve any environmental problems through technology, and loathes the
 >precautionary principle in any form. An article in The Economist the other
 >week dismissed the precautionary principle as pseudo-philosophy, and I read
 >similar things in the Australian press while I was away - the precautionary
 >principle is the latest target of the prometheans.

>
 >There is a need to stand back from the debate and unmask it for what it is -
 >a battle between two ideological movements in which the science is often
 >lost or deliberately suppressed and/or manipulated in its interpretation. Of
 >course there are extremists on both sides, although I suspect many more on
 >the promethean side, as this is the side that tends to appeal more to base
 >self-interest, even though some of its faith in human ingenuity and
 >adaptability may be appealing for nobler reasons. We have to recognise that
 >neither extreme is likely to be viable or realisable in the long term, and
 >that, short of a radical decline in the human population, our future will be
 >one of constant adaptation to change coupled with environmental management
 >designed to make the world as sustainable as possible (adaptive
 >management?). There should be a place at least for a "soft" precautionary
 >principle that involves risk assessment - technological innovation will
 >continue but perhaps as a society we should be more conscious of what sort
 >of technological developments we think are desirable (these sorts of choices
 >are already being made at governmental level in connection with technologies
 >such as cloning, but the debate is curtailed when it comes to nuclear power,
 >GM foods and defence - the vested interests are stronger than the moral
 >objections).

>
 >A final thought - those that oppose the precautionary principle most
 >vigorously are often those that strongly support precautionary spending on
 >defence to guard against possible future attack by unidentified enemies -
 >odd eh? We are much more certain that the climate will change (with or
 >without human intervention) than we were that the Soviet Union would launch
 >a nuclear attack on the West. In the end it comes down to vested interests,
 >paranoia, ideology and machismo. I think someone should point this out to
 >the Washington "think tanks".

>
 >Does anyone think there is any mileage in Tyndall exploring some of the
 >issues above, in a more considered manner than I may have done here, as a
 >sort of meta-analysis of the climate change debate?

>
 >Cheers

>
 >Nick

>
 >--

>Dr Nick Brooks
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 >University of East Anglia
 >Norwich NR4 7TJ
 >Tel: +44 1603 593904
 >Fax: +44 1603 593901
 >Email: nick.brooks@uea.ac.uk
 ><http://www.cru.uea.ac.uk/~e118/welcome.htm> (personal site)
 ><http://www.tyndall.ac.uk> (Tyndall Centre site)
 ><http://www.uea.ac.uk/sahara> (Saharan Studies Programme)

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</x-flowed>

1862. 2003-09-02

date: Tue, 2 Sep 2003 11:18 -0400
 from: jgr-atmospheres@agu.org
 subject: Review Received by Journal of Geophysical Research - Atmospheres
 to: K.Briffa@uea.ac.uk

Dear Dr. Briffa:

Thank you for your review of "On reconciliation of borehole and proxy based temperature reconstructions over the last five centuries" by Shaopeng Huang [Paper #2003JD003856], which we have safely received. A copy of this review is attached for your reference.

Sincerely,
 Alan Robock
 Editor, JGR-Atmospheres

Assessment: Category 5
 Ranking: Poor
 Confidential Re-Review: No
 Annotated Manuscript: No
 Comments:

My overall opinion of the Huang manuscript is that it is not suitable for publication either as a stand-alone contribution, or even as a critical commentary relating to the Mann et al. (2003) paper. This is unfortunate in the sense that the Mann et al. paper is not without some shortcomings, but my task here is to review the contribution to the global warming debate represented by this manuscript and, overall, I feel that there is insufficient new information or insight to justify publication.

The title is a misrepresentation of the content and I must agree with referee 1 that there is no independent treatment of the borehole or any original paleoclimate data that results in any independent evidence of a better match between them. Rather, what the author has done is to combine the original Mann et al. (1999) final data composite series with the Huang et al. (2000) composite series, giving all weight to the former in the high-frequency domain and all weight to the latter in the low-frequency, and more equal weight to each in the mid-frequency range. This, in itself, tells us nothing new about the validity of the low-frequency components of either the Mann et al. or Huang et al. series. It certainly cannot be considered as in any way 'reconciling' them.

The supposed evidence for the validity of the Huang et al. long-timescale trend is apparently the better (in comparison to Mann et al.) match achieved between this and one particular radiative forcing series (shown in Fig. 4), incorporating a combination of solar irradiance, anthropogenic aerosols and greenhouse gases only. The volcanic component is excluded on subjective and unconvincing grounds: namely that "the long term effects ... on temperature change have not been well quantified"! The same can certainly be said of solar variability, and arguably also even CO2! What is sure is that if the volcanic component had been included, the shape of the forcing curve would differ.

The early level of mean temperature indicated by the combined borehole-based estimates of multiple local temperature are significantly warmer in the 16th and 17th centuries when the data are gridded prior to averaging: much warmer than implied in Huang et al. (2000) and the hemispheric mean based on these previously gridded records could have been used with equal justification, instead of the Huang et al. series, and the results (in terms of linear regression with the forcing history used here) would likely have been as good as those achieved here (and the implied sensitivity to forcing change would have been less).

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I have problems with other aspects of the manuscript but there is no point going into further detail. In several areas the author criticises the Mann et al. methodology but does not provide sufficient or warranted detail. This is unfortunate in that the Mann et al. so-called optimal approach is not entirely convincing, but, be that as it may, it is largely irrelevant when judging the merits of the manuscript before me, and its fundamental shortcomings as regards providing a 'reconciliation' of the various proxy temperature evidence or any convincing case that the Huang long-timescale trend is nearer the "truth". I must recommend rejection.

2132. 2003-09-02

cc: Urs Neu <urs.neu@sanw.unibe.ch>, Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, "Michael E. Mann" <mann@virginia.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenberth@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu, wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Jürg Beer <beer@hermes.emp-eaw.ch>
date: Tue, 02 Sep 2003 07:04:39 -0600
from: Tom Wigley <wigley@ucar.edu>
subject: Re: anti-CO2
to: André Berger <berger@astr.ucl.ac.be>
<x-flowed>

Andre,
I agree. I heard that Zichichi has links with the Vatican. A number of other greenhouse skeptics have extreme religious views. Perhaps they believe that god would not let us do this to the planet, and that, if we do, she will save us?

Hmmmm.

Tom.

+++++

André Berger wrote:

> Dear All,
>
> More I read your email about the "anti-CO2", more I am convinced that an
> International Committee on Ethics in Geo-Sciences is needed. Indeed
> either we do not answer their attacks or we lose time and money doing
> it. The third solution is an official statement telling what the members
> of such a Committee of Ethics think about irresponsible statements by
> such anti-CO2 fellows.
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> Best Regards,

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> *****

> Prof. A. BERGER
> Université catholique de Louvain
> Institut d'Astronomie et de Géophysique G. Lemaître
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cg2003

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4261. 2003-09-02

date: Tue, 2 Sep 2003 12:34 -0400
from: grlonline@agu.org
subject: Review Received by Geophysical Research Letters
to: K.Briffa@uea.ac.uk

Dear Dr. Briffa:

Thank you for your review of "Large-scale warming triggers Siberian treeline advances<" by Jan Esper, Fritz Schweingruber [Paper #2003GL018177], which we have safely received. A copy of this review is attached below for your reference.

Thank you for your time and effort!
Sincerely,

James Famiglietti
Editor
Geophysical Research Letters

Science Category: Science Category 4
Presentation Category: Presentation Category C
Annotated Manuscript: Yes
Anonymous: Yes
Referrals: No
Confidential Referrals:

Highlight: No
Highlight:

Formal Review:

This paper provides some new evidence of recent germination of trees near to tree line in northern Russia. Taken together with earlier published information it provides support that mid to late 20th century warming is initiating a potential advance of tree line, but the evidence shows (though the authors do not discuss) that this advance is variable in magnitude and timing. The information is, of itself, interesting and worthy of publication, but it is described in a cursory way here and it is not clear that this manuscript is appropriate for Geophysical Research Letters. My overall opinion is that the authors should develop the discussion further and submit it to a more ecologically orientated journal. The paper sets out to "test the assumption that large-scale temperature forcing has an effect on treeline dynamics" and to "compare the recent treeline positions with historic ones by documenting in situ remnants of relict stumps and logs". I feel that the paper goes some way to achieving the first aim but is too limited in its discussion of the evidence and makes little, if any, useful contribution to achieving the second aim. Without a more quantified analysis of the regional extent of treeline response, in terms of tree recruitment and migration, to absolute temperature change, and some explicit discussion of the implications for interpreting treeline changes with respect to large-scale temperature (or other climate) changes, the relevance to a geophysical journal is far less obvious than

its relevance to the more general ecological literature.

The paper points, in its introduction, to the numerous factors that exert some control over treeline dynamics, but the subsequent description does not discuss their relevance here, other than showing a Figure of germination dates and growth releases side by side with annual temperatures. There is mention of "correlation" between these, and it is stated that this is higher than for "summer" temperatures, but the discussion is very vague and no actual quantitative relationships are provided or discussed.

Similarly, the sampling strategy is said to be "a qualitative stratification of treeline forests by structure and tree form". This is a good idea, but unfortunately the results are not clearly differentiated with regard to the classes of forest, and it is not clear whether we are seeing latitudinal or elevational changes in tree line or how these are differentiated in the different types of ecotone. Again, though a "longitudinal and latitudinal structure" is referred to across the network, it is not clear what the climate controls (or specific temperature) influence is on this and how the germination or survival of new trees is influenced in the different parts of this structure.

The results summarised in Figure 4 are certainly interesting, but they suggest several questions not addressed by the authors. Why do the recruitment pulses commence prior to the major onset of warm periods (such as in 1939, 1949 and the early 1970s)? Similarly, why are there major episodes of abrupt growth increases (in the mid 1970s and mid 1980s) that do not correspond directly either to strong warming or germination phases? The period of strong warmth in the mid 1940s (and the years of extreme warmth ~1944, 1962, 1968, 1981, 1984), are not synchronous with germination peaks. Does this imply a requirement for cumulative warmth over several years or is there a confounding effect e.g. snow cover? More importantly, do these average results mask regional (or different ecotone) variability in response to warmth as is implied by Figure 3? This shows homogeneous responses in category B sites but significant differences in A and C. It is very interesting that URA1 shows no germination prior to the very recent period, while other regions (especially in C) show significant germination prior to 1950, but none in the most recent period! The question of mortality is crucial. No reference is made to this, yet the interpretation of past tree lines must be viewed in the light of the survival of new seedlings. Did the authors see any evidence of recently dead seedlings?

The information shown here, with a cursory inspection, clearly implies some response to mid 20th century and more recent warming, but the unprecedented accuracy in the timing of tree growth germination should allow a more detailed comparison of the influence of short-term temperature changes, and seems to show a complex relationship as regards germination and growth release in trees. The latter appear to be non-linearly related and the implication for dendroclimatic studies of such trees might usefully be mentioned.

The existence of large fossil remains is important but the authors do not give sufficient detail of the dates (or growth rates) of these samples to provide useful insight into the likely absolute temperatures that accompanied their growth. Did the authors also explore the possibility of absolute thresholds in the regional responses to local temperatures that may be more significant than the relative changes shown in their Figure 4 (especially given that their sites cover a range of elevations and mean climates)?

These data are without doubt of value and interest to the scientific community, but to publish these results, in this form, in this publication, is not the most appropriate way of presenting them. Finally, I do not find the brief allusion to the workings of the carbon cycle and implied importance for climate change, at all convincing.

Indeed, it is far more likely that changes in local albedo caused by treeline changes are likely to exert a dominating positive influence on rates of high latitude warming.

4357. 2003-09-02

cc: André Berger <berger@astr.ucl.ac.be>, Urs Neu <urs.neu@sanw.unibe.ch>, Mike

cg2003

MacCracken <mmaccrac@comcast.net>, Martin Hoffert <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, "Michael E. Mann" <mann@virginia.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu, wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Jürg Beer <beer@hermes.emp-eaw.ch>
date: Tue, 02 Sep 2003 10:48:31 -0400
from: Gabi Hegerl <hegerl@duke.edu>
subject: Re: anti-CO2
to: Tom Wigley <wigley@ucar.edu>
<x-flowed>

Such a committee would be very helpful, I agree.

It could also address issues such as publicising who funds scientists with political influence

(if its true the vatican sponsors "sceptics" (pun intended), that would be an outrage

to lots of people, adding a few degrees to the already very hot water these guys are in with their base)!!

by the way, the ultraconservative guys would be those who would insist that if god would

want to save the planet, HE would do it. The others would

argue that people are the stewards of the planet and have to act responsibly, there is lots of

writings of catholic theologians and I am sure also theologians of other faiths on that...

Gabi

Tom Wigley wrote:

> Andre,

>

> I agree. I heard that Zichichi has links with the Vatican. A number of
> other greenhouse skeptics have extreme religious views. Perhaps they
> believe that god would not let us do this to the planet, and that, if
> we do, she will save us?

>

> Hmmm.

>

> Tom.

> ++++++

>

>

> André Berger wrote:

>

>> Dear All,

>>

>> More I read your email about the "anti-CO2", more I am convinced that
>> an International Committee on Ethics in Geo-Sciences is needed.

>> Indeed either we do not answer their attacks or we lose time and
>> money doing it. The third solution is an official statement telling
>> what the members of such a Committee of Ethics think about
>> irresponsible statements by such anti-CO2 fellows.

>>

>> Best Regards,

>>

>> André

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cg2003

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>> *****
>> Prof. A. BERGER
>> Université catholique de Louvain
>> Institut d'Astronomie et de Géophysique G. Lemaître
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Gabriele Hegerl
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Ph: 919 684 6167, fax 684 5833, email: hegerl@duke.edu
<http://www.nicholas.duke.edu/faculty/bios/hegerl.html>

</x-flowed>

2. 2003-09-03

date: wed, 3 Sep 2003 11:13:21 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: forgot
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Hi Keith,
Glad to hear you are enthusiastically interested. The stuff related to low-frequency and RCS that you want to do with Tom and I is a bit of an extension of what I want to do right now. I mainly want to do a "state-of-the-art" comparison of existing reconstructions to determine where the greatest uncertainties currently lie. The work with Tom could build upon that very naturally because I am sure that the greatest uncertainties lie in the multi-centennial band where tree-ring standardization methods have the greatest impact.
>to say would prefer no involvement of Mann and Phil -
>and can you tell me what reconstruction Bradley did ever ? unless
>you mean the Bradley and Jones early decadal series?
I agree that Phil and Mike are best left out of this. Bradley? Yeah, he has done fuck-all except for the Bradley/Jones decadal series, which he maintains has withstood the test of time. Typical posturing on his part.
Cheers,
Ed

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>
>--
>Professor Keith Briffa,
>Climatic Research Unit
>University of East Anglia
>Norwich, NR4 7TJ, U.K.
>
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cg2003

>Phone: +44-1603-593909
>Fax: +44-1603-507784
>
><http://www.cru.uea.ac.uk/cru/people/briffa/>
--

=====
Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152
=====

</x-flowed>

435. 2003-09-03

cc: t.osborn@uea.ac.uk
date: Wed Sep 3 14:00:06 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: An idea to pass by you
to: Edward Cook <drdendro@ldeo.columbia.edu>

ED
without the slightest doubt , I do wish to be involved in this AND/OR something like it -
what I wanted to do (to be frank) myself, is to do a piece with you, Tim and Tom Melvin and Jan(?) , on the validity of the low frequency components of the family of reconstructions -
but with the emphasis on the tree-ring side . Tim is certainly (with me and you - remember)
doing a paper for The Holocene on the areas of uncertainty in these attempts (focusing on
calibration issues, spatial representation of predictors (spatial and time scale bias),
seasonal bias and relating these , ultimately. to the reliability of the reconstructions
{This is my version of what will be in it but he may disagree} . The basic point is that I
(and I think he) agree that Mike and Phil's latest contribution is a step backwards (in
time and understanding) - well in reality I do not believe it is a step forward. I need to
read you message in detail and then phone tomorrow (I HAVE to get this PhD report off to
New Zealand now) after talking to Tim . You know I desperately want to produce a new
temperature reconstruction from the various tree-ring data (and explore the Mann western US
PC correction - though Malcolm has ignored my request for the data) . At the least , all
this requires that I come to see you (and perhaps Tim too).
I WILL be in touch
Keith
At 08:32 AM 9/3/03 -0400, you wrote:
Hi Keith,
After the meeting in Norway, where I presented the Esper stuff as described in the
extended abstract I sent you, and hearing Bradley's follow-up talk on how everybody but
him has fucked up in reconstructing past NH temperatures over the past 1000

years (this is a bit of an overstatement on my part I must admit, but his air of papal infallibility is really quite nauseating at times), I have come up with an idea that I want you to be involved in. Consider the tentative title:
 "Northern Hemisphere Temperatures Over The Past Millennium: Where Are The Greatest Uncertainties?"
 Authors: Cook, Briffa, Esper, Osborn, D'Arrigo, Bradley(?), Jones (??), Mann (infinite?) - I am afraid the Mike and Phil are too personally invested in things now (i.e. the 2003 GRL paper that is probably the worst paper Phil has ever been involved in - Bradley hates it as well), but I am willing to offer to include them if they can contribute without just defending their past work - this is the key to having anyone involved. Be honest. Lay it all out on the table and don't start by assuming that ANY reconstruction is better than any other.
 Here are my ideas for the paper in a nutshell (please bear with me):
 1) Describe the past work (Mann, Briffa, Jones, Crowley, Esper, yada, yada, yada) and their data over-laps.
 2) Use the Briffa&Osborn "Blowing Hot And Cold" annually-resolved recons (plus Crowley?) (boreholes not included) for comparison because they are all scaled identically to the same NH extra-tropics temperatures and the Mann version only includes that part of the NH (we could include Mann's full NH recon as well, but he would probably go ballistic, and also the new Mann&Jones mess?)
 3) Characterize the similarities between series using unrotated (maybe rotated as well) EOF analysis (correlation for pure similarity, covariance for differences in amplitude as well) and filtering on the reconstructions - unfiltered, 20yr high-pass, 100-20 bandpass, 100 lowpass - to find out where the reconstructions are most similar and different - use 1st-EOF loadings as a guide, the comparisons of the power spectra could also be done I suppose
 4) Do these EOF analyses on different time periods to see where they differ most, e.g., running 100-year EOF windows on the unfiltered data, running 300-year for 20-1p data (something like that anyway), and plot the 1st-EOF loadings as a function of time
 5) Discuss where the biggest differences lie between reconstructions (this will almost certainly occur most in the 100 lowpass data), taking into account data overlaps
 6) Point out implications concerning the next IPCC assessment and EBM forcing experiments that are basically designed to fit the lower frequencies - if the greatest uncertainties are in the >100 year band, then that is where the greatest uncertainties will be in the forcing experiments
 7) Publish, retire, and don't leave a forwarding address
 Without trying to prejudice this work, but also because of what I almost think

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I know to
be the case, the results of this study will show that we can probably say a
fair bit
about <100 year extra-tropical NH temperature variability (at least as far as
we believe
the proxy estimates), but honestly know fuck-all about what the >100 year
variability
was like with any certainty (i.e. we know with certainty that we know
fuck-all).
Of course, none of what I have proposed has addressed the issue of seasonality
of
response. So what I am suggesting is strictly an empirical comparison of
published 1000
year NH reconstructions because many of the same tree-ring proxies get used in
both
seasonal and annual recons anyway. So all I care about is how the recons
differ and
where they differ most in frequency and time without any direct consideration
of their
TRUE association with observed temperatures.
I think this is exactly the kind of study that needs to be done before the
next IPCC
assessment. But to give it credibility, it has to have a reasonably broad
spectrum of
authors to avoid looking like a biased attack paper, i.e. like Soon and
Ballunas.
If you don't want to do it, just say so and I will drop the whole idea like a
hot
potato. I honestly don't want to do it without your participation. If you want
to be the
lead on it, I am fine with that too.
Cheers,
Ed
--

=====
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=====

--
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2022. 2003-09-03

cc: Keith Briffa <k.briffa@uea.ac.uk>
date: Wed, 3 Sep 2003 11:24:18 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: Re: Fwd: Re: Fwd: Soon & Baliunas
to: Tim Osborn <t.osborn@uea.ac.uk>
<x-flowed>

Hi Tim,
Thanks for the info on what you and Keith have been thinking about. I

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don't think that we are all that far apart on what we want to do. It might be worthwhile for me to come over to see you guys first (both cheaper for one, and you are probably more organized and farther along than me just now). What has me jammed up is my trip to Bhutan in Nov for a month and AGU in Dec.

I will indeed keep this whole thing quiet. I haven't contacted anyone else so far. I would like to get Jan involved however.

Cheers,

Ed

>Hi Ed,

>

>first all, yes I agree that we need a paper that takes a more
>objective look at where we are now and how we can take things
>forward in terms of NH temperature reconstructions (and possibly
>global, SH, spatial etc.).

>

>As Keith said, we (mainly I so far) have been planning our version
>of this (hopefully) "objective assessment", and by chance I was
>sketching out a vague outline of its possible content. We've been
>keeping this fairly close to our chests for now, so please keep our
>plans/ideas to yourself for the moment. There is partial overlap
>between our ideas and yours, so it might be good to do this jointly.
>Anyway, my current ideas are a number of forum articles, the first
>comparing existing reconstructions but without going into more
>depth, and the other three looking at the way forward (i.e. what
>should we attempt to do to improve them):

>

>Forum piece (1): Comparison of existing reconstructions

>

>This has most overlaps with your ideas, though I hadn't thought of
>it being so comprehensive. I was thinking more of:

>

>(a) comparing original series.

>(b) comparing them after our recalibration to common target data,
>including discussion of why some things don't change much (e.g.
>relative positioning of reconstructions), though amplitudes can
>change - and of course the comparison of Mann et al. with and
>without oceans/tropics.

>(c) maybe a bit on comparison with boreholes, though maybe not.

>(d) uncertainty estimates and how these may decrease with time scale
>and hence not all reconstructions lie in the Mann et al. uncertainty
>ranges.

>

>Forum piece (2): Selection of predictand and predictor data

>

>(a) what to try to reconstruct and why it matters - e.g. will we get
>the wrong spectral shape if we reconstruct ocean SST from land-based
>proxies. Plus some on seasonality, though Jones, Osborn and Briffa
>cover part of that issue (are you aware of that paper, in press with
>JGR?).

>(b) What proxies should be used - e.g. does throwing in "poor"
>proxies cause a problem with simple averaging, weighted averaging
>and multivariate regression approaches. Plus does using
>precipitation proxies to reconstruct temperature result in the wrong
>spectral shape?

>

>Forum piece (3): Reconstruction methods

>

>Something here on different methods (simple averaging, multivariate
>regression type approaches) and different implementation choices
>(e.g. calibration against trends/filtered data). Not entirely sure
>about this, but it would not be new work, just would critically
>appraise the methods used to date and what their

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>theoretical/potential problems/advantages might be.
>
>Forum piece (4): Estimating uncertainty
>
>Again, not entirely sure yet, but this must emphasise the absolute
>requirement to estimate AND USE uncertainty when comparing
>reconstructions against observations or simulations etc. Then
>something about how to do it, contrasting using calibration
>residuals, verification residuals, parameter uncertainty, with the
>type of approach that you've taken (bootstrap uncertainty, or
>measures of the EPS) to look at the common signal, with additional
>uncertainty of how the common signal differs from the predictand.
>
>So that's it!! Perhaps rather ambitious, so maybe a reduction to
>certain key points might be required. I was deliberately avoiding
>any review of tree-ring contributions and low-frequency per se,
>thinking that you and Keith would be taking the lead on that kind of
>review.
>
>One final think to mention, is that the emails copied below and the
>attached file might be of interest to you as an example of something
>that *might* go in a comparison paper of existing reconstructions.
>It's shows how the recalibrated average of existing reconstructions
>differs from the average of existing calibrated reconstructions.
>You'll see from Mike Mann's initial request below that he was
>thinking of it as a contribution to the EOS rebuttal of Soon and
>Baliunas, but I've not heard much from him since. Also Tom Crowley
>was very interests in this composite of the reconstructions, and I
>started to converse with him about it but never finished estimating
>the uncertainty range on the composite series and kind of stopped
>emailing him. But I guess either of them might reproduce this idea
>sometime, if it suits them.
>
>A visit to talk face to face about all these things would be good.
>Keith and I have been talking about how to fit a visit in.
>
>Cheers
>
>Tim
>
>
>>Date: Wed, 12 Mar 2003 16:16:16 +0000
>>To: "Michael E. Mann" <mann@virginia.edu>, Tom Crowley
>><tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>
>>From: Tim Osborn <t.osborn@uea.ac.uk>
>>Subject: Re: Fwd: Soon & Baliunas
>>Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>,
>>rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu,
>>srutherford@gso.uri.edu, k.briffa@uea.ac.uk, mann@virginia.edu
>>
>>This is an excellent idea, Mike, IN PRINCIPLE at least. In
>>practise, however, it raises some interesting results (as I have
>>found when attempting this myself) that may be difficult to avoid
>>getting bogged down with discussing.
>>
>>The attached .pdf figure shows an example of what I have produced
>>(NB. please don't circulate this further, as it is from work that
>>is currently being finished off - however, I'm happy to use it here
>>to illustrate my point).
>>
>>I took 7 reconstructions and re-calibrated them over a common
>>period and against an observed target series (in this case,
>>land-only, Apr-Sep, >20N - BUT I GET SIMILAR RESULTS WITH OTHER

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>>CHOICES, and this re-calibration stage is not critical). You will
>>have seen figures similar to this in stuff Keith and I have
>>published. See the coloured lines in the attached figure.
>>
>>In this example I then simply took an unweighted average of the
>>calibrated series, but the weighted average obtained via an EOF
>>approach can give similar results. The average is shown by the
>>thin black line (I've ignored the potential problems of series
>>covering different periods). This was all done with raw,
>>unsmoothed data, even though 30-yr smoothed curves are plotted in
>>the figure.
>>
>>The thick black line is what I get when I re-calibrate the average
>>record against my target observed series. THIS IS THE IMPORTANT
>>BIT. The *re-calibrated* mean of the reconstructions is nowhere
>>near the mean of the reconstructions. It has enhanced variability,
>>because averaging the reconstructions results in a redder time
>>series (there is less common variance between the reconstructions
>>at the higher frequencies compared with the lower frequencies, so
>>the former averages out to leave a smoother curve) and the
>>re-calibration is then more of a case of fitting a trend (over my
>>calibration period 1881-1960) to the observed trend. This results
>>in enhanced variability, but also enhanced uncertainty (not shown
>>here) due to fewer effective degrees of freedom during calibration.
>>
>>Obviously there are questions about observed target series, which
>>series to include/exclude etc., but the same issue will arise
>>regardless: the analysis will not likely lie near to the middle of
>>the cloud of published series and explaining the reasons behind
>>this etc. will obscure the message of a short EOS piece.
>>
>>It is, of course, interesting - not least for the comparison with
>>borehole-based estimates - but that is for a separate paper, I
>>think.
>>
>>My suggestion would be to stick with one of these options:
>>(i) a single example reconstruction;
>>(ii) a plot of a cloud of reconstructions;
>>(iii) a plot of the "envelope" containing the cloud of
>>reconstructions (perhaps also the envelope would encompass their
>>uncertainty estimates), but without showing the individual
>>reconstruction best guesses.
>>
>>How many votes for each?
>>
>>Cheers
>>
>>Tim
>>
>>At 15:32 12/03/03, Michael E. Mann wrote:
>>>p.s. The idea of both a representative time-slice spatial plot
>>>emphasizing the spatial variability of e.g. the MWP or LIA, and an
>>>EOF analysis of all the records is a great idea. I'd like to
>>>suggest a small modification of the latter:
>>>
>>>I would suggest we show 2 curves, representing the 1st PC of two
>>>different groups, one of empirical reconstructions, the other of
>>>model simulations, rather than just one in the time plot.
>>>
>>>Group #1 could include:
>>>
>>>1) Crowley & Lowery
>>>2) Mann et al 1999

>>>3) Bradley and Jones 1995
>>>4) Jones et al, 1998
>>>5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD
>>>reconstruction]
>>>6) Esper et al [yes, no?--one series that differs from the others
>>>won't make much of a difference]
>>>
>>>I would suggest we scale the resulting PC to the CRU 1856-1960
>>>annual Northern Hemisphere mean instrumental record, which should
>>>overlap w/ all of the series, and which pre-dates the MXD decline
>>>issue...
>>>
>>>Group #2 would include various model simulations using different
>>>forcings, and with slightly different sensitivities. This could
>>>include 6 or so simulation results:
>>>
>>>1) 3 series from Crowley (2000) [based on different solar/volcanic
>>>reconstructions],
>>>2) 2 series from Gerber et al (Bern modeling group result) [based
>>>on different assumed sensitivities]
>>>1) Bauer et al series (Claussen group EMIC result) [includes
>>>19th/20th century land use changes as a forcing].
>>>
>>>I would suggest that the model's 20th century mean is aligned with
>>>the 20th century instrumental N.Hem mean for comparison (since
>>>this is when we know the forcings best).
>>>
>>>
>>>I'd like to nominate Scott R. as the collector of the time series
>>>and the performer of the EOF analyses, scaling, and plotting,
>>>since Scott already has many of the series and many of the
>>>appropriate analysis and plotting tools set up to do this.
>>>
>>>We could each send our preferred versions of our respective time
>>>series to Scott as an ascii attachment, etc.
>>>
>>>thoughts, comments?
>>>
>>>thanks,
>>>
>>>mike
>>>
>>>At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:
>>>>Thanks Tom,
>>>>
>>>>Either would be good, but Eos is an especially good idea. Both
>>>>Ellen M-T and Keith Alverson are on the editorial board there, so
>>>>I think there would be some receptiveness to such a submission.t
>>>>
>>>>I see this as complementary to other pieces that we have written
>>>>or are currently writing (e.g. a review that Ray, Malcolm, and
>>>>Henry Diaz are doing for Science on the MWP) and this should
>>>>proceed entirely independently of that.
>>>>
>>>>If there is group interest in taking this tack, I'd be happy to
>>>>contact Ellen/Keith about the potential interest in Eos, or I'd
>>>>be happy to let Tom or Phil to take the lead too...
>>>>
>>>>Comments?
>>>>
>>>>mike
>>>>
>>>>At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:

>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>Phil et al,
>>>>>
>>>>>I suggest either BAMS or Eos - the latter would probably be
>>>>>better because it is shorter, quicker, has a wide distribution,
>>>>>and all the points that need to be made have been made before.
>>>>>
>>>>>rather than dwelling on Soon and Baliunas I think the message
>>>>>should be pointedly made against all of the standard claptrap
>>>>>being dredged up.
>>>>>
>>>>>I suggest two figures- one on time series and another showing
>>>>>the spatial array of temperatures at one point in the Middle
>>>>>Ages. I produced a few of those for the Ambio paper but already
>>>>>have one ready for the Greenland settlement period 965-995
>>>>>showing the regional nature of the warmth in that figure. we
>>>>>could add a few new sites to it, but if people think otherwise
>>>>>we could of course go in some other direction.
>>>>>
>>>>>rather than getting into the delicate question of which paleo
>>>>>reconstruction to use I suggest that we show a time series that
>>>>>is an eof of the different reconstructions - one that emphasizes
>>>>>the commonality of the message.
>>>>>
>>>>>Tom
>>>>>
>>>>>Dear All,
>>>>> I agree with all the points being made and the
>>>>>multi-authored article would be a good idea,
>>>>> but how do we go about not letting it get buried somewhere.
>>>>>Can we not address the
>>>>> misconceptions by finally coming up with definitive dates for
>>>>>the LIA and MWP and
>>>>> redefining what we think the terms really mean? With all of us
>>>>>and more on the paper, it should
>>>>> carry a lot of weight. In a way we will be setting the agenda
>>>>>for what should be being done
>>>>> over the next few years.
>>>>> We do want a reputable journal but is The Holocene the
>>>>>right vehicle. It is probably the
>>>>> best of its class of journals out there. Mike and I were
>>>>>asked to write an article for the EGS
>>>>> journal of Surveys of Geophysics. You've not heard of this -
>>>>>few have, so we declined. However,
>>>>> it got me thinking that we could try for Reviews of
>>>>>Geophysics. Need to contact the editorial
>>>>> board to see if this might be possible. Just a thought, but it
>>>>>certainly has a high profile.
>>>>> What we want to write is NOT the scholarly review a la
>>>>>Jean Grove (bless her soul) that
>>>>> just reviews but doesn't come to anything firm. We want a
>>>>>critical review that enables
>>>>> agendas to be set. Ray's recent multi-authored piece goes a
>>>>>lot of the way so we need
>>>>> to build on this.
>>>>>

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>>>>> Cheers
>>>>> Phil

>>>>>
>>>>>

>>>>>At 12:55 11/03/03 -0500, Michael E. Mann wrote:

>>>>>>Hi Malcolm,

>>>>>>

>>>>>>Thanks for the feedback--I largely concur. I do, though, think
>>>>>>there is a particular problem with "Climate Research". This
>>>>>>is where my colleague Pat Michaels now publishes exclusively,
>>>>>>and his two closest colleagues are on the editorial board and
>>>>>>review editor board. So I promise you, we'll see more of this
>>>>>>there, and I personally think there *is* a bigger problem with
>>>>>>the "messenger" in this case...

>>>>>>

>>>>>>But the Soon and Baliunas paper is its own, separate issue
>>>>>>too. I too like Tom's latter idea, of a more hefty
>>>>>>multi-authored piece in an appropriate journal
>>>>>>(Paleoceanography? Holocene?) that seeks to correct a number
>>>>>>of misconceptions out there, perhaps using Baliunas and Soon
>>>>>>as a case study ('poster child?'), but taking on a slightly
>>>>>>greater territory too.

>>>>>>

>>>>>>Question is, who would take the lead role. I *know* we're all very busy,

>>>>>>

>>>>>>mike

>>>>>>

>>>>>> At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

>>>>>>>I'm with Tom on this. In a way it comes back to a rant of mine
>>>>>>>to which some of you have already been victim. The general
>>>>>>>point is that there are two arms of climatology:
>>>>>>> neoclimatology - what you do based on instrumental records
>>>>>>>and direct, systematic observations in networks - all set in a
>>>>>>>very Late Holocene/Anthropocene time with hourly to decadal
>>>>>>>interests.

>>>>>>>paleoclimatology - stuff from rocks, etc., where major changes
>>>>>>>in the Earth system, including its climate, associated with
>>>>>>>major changes in boundary conditions, may be detected by
>>>>>>>examination of one or a handful of paleo records.

>>>>>>>Between these two is what we do - "mesoclimatology" -

>>>>>>>dealing with many of the same phenomena as neoclimatology,
>>>>>>>using documentary and natural archives to look at phenomena
>>>>>>>on interannual to millennial time scales. Given relatively small
>>>>>>>changes in boundary conditions (until the last couple of
>>>>>>>centuries), mesoclimatology has to work in a way that is very
>>>>>>>similar to neoclimatology. Most notably, it depends on heavily
>>>>>>>replicated networks of precisely dated records capable of
>>>>>>>being either calibrated, or whose relationship to climate may
>>>>>>>be modeled accurately and precisely.

>>>>>>>Because this distinction is not recognized by many (e.g.
>>>>>>>Sonnechkin, Broecker, Karlen) we see an accumulation of
>>>>>>>misguided attempts at describing the climate of recent
>>>>>>>millennia. It would be better to head this off in general, rather
>>>>>>>than draw attention to a bad paper. After all, as Tom rightly
>>>>>>>says, we could all nominate really bad papers that have been
>>>>>>>published in journals of outstanding reputation (although there
>>>>>>>could well be differences between our lists).

>>>>>>>End of rant, Cheers, Malcolm

>>>>>>> Hi guys,

>>>>>>>

>>>>>>> junk gets published in lots of places. I think that what could be
>>>>>>> done is a short reply to the authors in Climate Research OR

>>>>>>>a SLIGHTLY
>>>>>>> longer note in a reputable journal entitled something like
>>>>>>>"Continuing
>>>>>>> Misconceptions About interpretation of past climate change." I kind
>>>>>>> of like the more pointed character of the latter and submitting it as
>>>>>>> a short note with a group authorship carries a heft that a reply to a
>>>>>>> paper, in no matter what journal, does not.
>>>>>>>
>>>>>>> Tom
>>>>>>>
>>>>>>>
>>>>>>> > Dear All,
>>>>>>> > Apologies for sending this again. I was expecting a stack of
>>>>>>> >>emails this morning in
>>>>>>> > response, but I inadvertently left Mike off (mistake in pasting)
>>>>>>> >and picked up Tom's old
>>>>>>> > address. Tom is busy though with another offspring !
>>>>>>> > I looked briefly at the paper last night and it is appalling -
>>>>>>> >worst word I can think of today
>>>>>>> > without the mood pepper appearing on the email ! I'll have time to
>>>>>>> >read more at the weekend
>>>>>>> > as I'm coming to the US for the DoE CCPP meeting at Charleston.
>>>>>>> >Added Ed, Peck and Keith A.
>>>>>>> > onto this list as well. I would like to have time to rise to the
>>>>>>> >bait, but I have so much else on at
>>>>>>> > the moment. As a few of us will be at the EGS/AGU meet in Nice, we
>>>>>>> >should consider what
>>>>>>> > to do there.
>>>>>>> > The phrasing of the questions at the start of the paper
>>>>>>> >determine the answer they get. They
>>>>>>> > have no idea what multiproxy averaging does. By their logic, I
>>>>>>> >could argue 1998 wasn't the
>>>>>>> > warmest year globally, because it wasn't the warmest everywhere.
>>>>>>> >with their LIA being 1300-
>>>>>>> >1900 and their MWP 800-1300, there appears (at my quick first
>>>>>>> >reading) no discussion of
>>>>>>> > synchronicity of the cool/warm periods. Even with the instrumental
>>>>>>> >record, the early and late
>>>>>>> > 20th century warming periods are only significant locally at
>>>>>>> >between 10-20% of grid boxes.
>>>>>>> > Writing this I am becoming more convinced we should do
>>>>>>> >something - even if this is just
>>>>>>> > to state once and for all what we mean by the LIA and MWP. I think
>>>>>>> >the skeptics will use
>>>>>>> > this paper to their own ends and it will set paleo back
>>>>>>>a number of
>>>>>>> >
>>>>>>> >years if it goes
>>>>>>> > unchallenged.
>>>>>>> >
>>>>>>> > I will be emailing the journal to tell them I'm having
>>>>>>> >nothing more to do with it until they
>>>>>>> > rid themselves of this troublesome editor. A CRU person is on the
>>>>>>> >editorial board, but papers
>>>>>>> > get dealt with by the editor assigned by Hans von Storch.
>>>>>>> >
>>>>>>> > Cheers
>>>>>>> > Phil
>>>>>>> >
>>>>>>> > Dear all,
>>>>>>> > Tim Osborn has just come across this. Best to ignore
>>>>>>> >probably, so don't let it spoil your

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>>>>>>> > day. I've not looked at it yet. It results from this journal
>>>>>>> > having a number of editors. The
>>>>>>> > responsible one for this is a well-known skeptic in NZ.
>>>>>>> He has let
>>>>>>> >
>>>>>>> > a few papers through by
>>>>>>> > Michaels and Gray in the past. I've had words with Hans
>>>>>>> von Storch
>>>>>>> >
>>>>>>> > about this, but got nowhere.
>>>>>>> > Another thing to discuss in Nice !
>>>>>>> >
>>>>>>> > Cheers
>>>>>>> > Phil
>>>>>>> >
>>>>>>> >>X-Sender: f055@pop.uea.ac.uk
>>>>>>> >>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>>>>>> >>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>>>>>> >>To: p.jones@uea
>>>>>>> >>From: Tim Osborn <t.osborn@uea.ac.uk>
>>>>>>> >>Subject: Soon & Baliunas
>>>>>>> >>
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>>>>>>> >-----
>>>>>>> >-----
>>>>>>> >
>>>>>>> >
>>>>>>> >Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
>>>>>>> >/CARO) (00016021)
>>>>>>>
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>>>>>>

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>>>>>>Professor Michael E. Mann
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>>>>>>

>
>

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>Attachment converted: Macintosh HD:synth1.pdf (PDF /CARO) (0009D506)

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>

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=====
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=====

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2398. 2003-09-03

date: wed, 03 Sep 2003 15:54:41 +0100

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Fwd: Re: Fwd: Soon & Baliunas

to: Keith Briffa <k.briffa@uea.ac.uk>, Edward Cook <drdendro@ldeo.columbia.edu>

<x-flowed>

Hi Ed,

first all, yes I agree that we need a paper that takes a more objective look at where we are now and how we can take things forward in terms of NH temperature reconstructions (and possibly global, SH, spatial etc.). As Keith said, we (mainly I so far) have been planning our version of this (hopefully) "objective assessment", and by chance I was sketching out a vague outline of its possible content. We've been keeping this fairly close to our chests for now, so please keep our plans/ideas to yourself for the moment. There is partial overlap between our ideas and yours, so it might be good to do this jointly. Anyway, my current ideas are a number of forum articles, the first comparing existing reconstructions but without going into more depth, and the other three looking at the way forward (i.e. what should we attempt to do to improve them):

Forum piece (1): Comparison of existing reconstructions

This has most overlaps with your ideas, though I hadn't thought of it being so comprehensive. I was thinking more of:

(a) comparing original series.

(b) comparing them after our recalibration to common target data, including discussion of why some things don't change much (e.g. relative positioning of reconstructions), though amplitudes can change - and of course the comparison of Mann et al. with and without oceans/tropics.

(c) maybe a bit on comparison with boreholes, though maybe not.

(d) uncertainty estimates and how these may decrease with time scale and hence not all reconstructions lie in the Mann et al. uncertainty ranges.

Forum piece (2): Selection of predictand and predictor data

(a) what to try to reconstruct and why it matters - e.g. will we get the wrong spectral shape if we reconstruct ocean SST from land-based proxies. Plus some on seasonality, though Jones, Osborn and Briffa cover part of that issue (are you aware of that paper, in press with JGR?).

(b) what proxies should be used - e.g. does throwing in "poor" proxies cause a problem with simple averaging, weighted averaging and multivariate regression approaches. Plus does using precipitation proxies to

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reconstruct temperature result in the wrong spectral shape?

Forum piece (3): Reconstruction methods

Something here on different methods (simple averaging, multivariate regression type approaches) and different implementation choices (e.g. calibration against trends/filtered data). Not entirely sure about this, but it would not be new work, just would critically appraise the methods used to date and what their theoretical/potential problems/advantages might be.

Forum piece (4): Estimating uncertainty

Again, not entirely sure yet, but this must emphasise the absolute requirement to estimate AND USE uncertainty when comparing reconstructions against observations or simulations etc. Then something about how to do it, contrasting using calibration residuals, verification residuals, parameter uncertainty, with the type of approach that you've taken (bootstrap uncertainty, or measures of the EPS) to look at the common signal, with additional uncertainty of how the common signal differs from the predictand.

So that's it!! Perhaps rather ambitious, so maybe a reduction to certain key points might be required. I was deliberately avoiding any review of tree-ring contributions and low-frequency per se, thinking that you and Keith would be taking the lead on that kind of review.

One final think to mention, is that the emails copied below and the attached file might be of interest to you as an example of something that *might* go in a comparison paper of existing reconstructions. It's shows how the recalibrated average of existing reconstructions differs from the average of existing calibrated reconstructions. You'll see from Mike Mann's initial request below that he was thinking of it as a contribution to the EOS rebuttal of Soon and Baliunas, but I've not heard much from him since. Also Tom Crowley was very interests in this composite of the reconstructions, and I started to converse with him about it but never finished estimating the uncertainty range on the composite series and kind of stopped emailing him. But I guess either of them might reproduce this idea sometime, if it suits them.

A visit to talk face to face about all these things would be good. Keith and I have been talking about how to fit a visit in.

Cheers

Tim

>Date: Wed, 12 Mar 2003 16:16:16 +0000

>To: "Michael E. Mann" <mann@virginia.edu>, Tom Crowley

><tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>

>From: Tim Osborn <t.osborn@uea.ac.uk>

>Subject: Re: Fwd: Soon & Baliunas

>Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, rbradley@geo.umass.edu,

>mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, k.briffa@uea.ac.uk,

>mann@virginia.edu

>

>This is an excellent idea, Mike, IN PRINCIPLE at least. In practise, however, it raises some interesting results (as I have found when attempting this myself) that may be difficult to avoid getting bogged down with discussing.

>

>The attached .pdf figure shows an example of what I have produced (NB. please don't circulate this further, as it is from work that is currently being finished off - however, I'm happy to use it here to illustrate my point).

>

>I took 7 reconstructions and re-calibrated them over a common period and against an observed target series (in this case, land-only, Apr-Sep, >20N >- BUT I GET SIMILAR RESULTS WITH OTHER CHOICES, and this re-calibration stage is not critical). You will have seen figures similar to this in stuff Keith and I have published. See the coloured lines in the attached figure.

>

>In this example I then simply took an unweighted average of the calibrated series, but the weighted average obtained via an EOF approach can give

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>similar results. The average is shown by the thin black line (I've
>ignored the potential problems of series covering different
>periods). This was all done with raw, unsmoothed data, even though 30-yr
>smoothed curves are plotted in the figure.
>
>The thick black line is what I get when I re-calibrate the average record
>against my target observed series. THIS IS THE IMPORTANT BIT. The
>*re-calibrated* mean of the reconstructions is nowhere near the mean of
>the reconstructions. It has enhanced variability, because averaging the
>reconstructions results in a redder time series (there is less common
>variance between the reconstructions at the higher frequencies compared
>with the lower frequencies, so the former averages out to leave a smoother
>curve) and the re-calibration is then more of a case of fitting a trend
>(over my calibration period 1881-1960) to the observed trend. This
>results in enhanced variability, but also enhanced uncertainty (not shown
>here) due to fewer effective degrees of freedom during calibration.
>
>Obviously there are questions about observed target series, which series
>to include/exclude etc., but the same issue will arise regardless: the
>analysis will not likely lie near to the middle of the cloud of published
>series and explaining the reasons behind this etc. will obscure the
>message of a short EOS piece.
>
>It is, of course, interesting - not least for the comparison with
>borehole-based estimates - but that is for a separate paper, I think.
>
>My suggestion would be to stick with one of these options:
>(i) a single example reconstruction;
>(ii) a plot of a cloud of reconstructions;
>(iii) a plot of the "envelope" containing the cloud of reconstructions
>(perhaps also the envelope would encompass their uncertainty estimates),
>but without showing the individual reconstruction best guesses.
>
>How many votes for each?
>
>Cheers
>
>Tim
>
>At 15:32 12/03/03, Michael E. Mann wrote:
>>p.s. The idea of both a representative time-slice spatial plot
>>emphasizing the spatial variability of e.g. the MWP or LIA, and an EOF
>>analysis of all the records is a great idea. I'd like to suggest a small
>>modification of the latter:
>>
>>I would suggest we show 2 curves, representing the 1st PC of two
>>different groups, one of empirical reconstructions, the other of model
>>simulations, rather than just one in the time plot.
>>
>>Group #1 could include:
>>
>>1) Crowley & Lowery
>>2) Mann et al 1999
>>3) Bradley and Jones 1995
>>4) Jones et al, 1998
>>5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD
>>reconstruction]
>>6) Esper et al [yes, no?--one series that differs from the others won't
>>make much of a difference]
>>
>>I would suggest we scale the resulting PC to the CRU 1856-1960 annual
>>Northern Hemisphere mean instrumental record, which should overlap w/ all
>>of the series, and which pre-dates the MXD decline issue...

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>>
>>Group #2 would include various model simulations using different
>>forcings, and with slightly different sensitivities. This could include 6
>>or so simulation results:
>>
>>1) 3 series from Crowley (2000) [based on different solar/volcanic
>>reconstructions],
>>2) 2 series from Gerber et al (Bern modeling group result) [based on
>>different assumed sensitivities]
>>1) Bauer et al series (Claussen group EMIC result) [includes 19th/20th
>>century land use changes as a forcing].
>>
>>I would suggest that the model's 20th century mean is aligned with the
>>20th century instrumental N.Hem mean for comparison (since this is when
>>we know the forcings best).
>>
>>
>>I'd like to nominate Scott R. as the collector of the time series and the
>>performer of the EOF analyses, scaling, and plotting, since Scott already
>>has many of the series and many of the appropriate analysis and plotting
>>tools set up to do this.
>>
>>We could each send our preferred versions of our respective time series
>>to Scott as an ascii attachment, etc.
>>
>>thoughts, comments?
>>
>>thanks,
>>
>>mike
>>
>>At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:
>>>Thanks Tom,
>>>
>>>Either would be good, but Eos is an especially good idea. Both Ellen M-T
>>>and Keith Alverson are on the editorial board there, so I think there
>>>would be some receptiveness to such a submission.t
>>>
>>>I see this as complementary to other pieces that we have written or are
>>>currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are
>>>doing for Science on the MWP) and this should proceed entirely
>>>independently of that.
>>>
>>>If there is group interest in taking this tack, I'd be happy to contact
>>>Ellen/Keith about the potential interest in Eos, or I'd be happy to let
>>>Tom or Phil to take the lead too...
>>>
>>>Comments?
>>>
>>>mike
>>>
>>>At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>Phil et al,
>>>>
>>>>I suggest either BAMS or Eos - the latter would probably be better

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>>>>because it is shorter, quicker, has a wide distribution, and all the
>>>>points that need to be made have been made before.

>>>>

>>>>rather than dwelling on Soon and Baliunas I think the message should be
>>>>pointedly made against all of the standard claptrap being dredged up.

>>>>

>>>>I suggest two figures- one on time series and another showing the
>>>>spatial array of temperatures at one point in the Middle Ages. I
>>>>produced a few of those for the Ambio paper but already have one ready
>>>>for the Greenland settlement period 965-995 showing the regional nature
>>>>of the warmth in that figure. we could add a few new sites to it, but
>>>>if people think otherwise we could of course go in some other direction.

>>>>

>>>>rather than getting into the delicate question of which paleo
>>>>reconstruction to use I suggest that we show a time series that is an
>>>>eof of the different reconstructions - one that emphasizes the
>>>>commonality of the message.

>>>>

>>>>Tom

>>>>

>>>>

>>>>>Dear All,

>>>>> I agree with all the points being made and the multi-authored
>>>>> article would be a good idea,
>>>>> but how do we go about not letting it get buried somewhere. Can we
>>>>> not address the

>>>>> misconceptions by finally coming up with definitive dates for the
>>>>> LIA and MWP and
>>>>> redefining what we think the terms really mean? With all of us and
>>>>> more on the paper, it should
>>>>> carry a lot of weight. In a way we will be setting the agenda for
>>>>> what should be being done
>>>>> over the next few years.

>>>>> We do want a reputable journal but is The Holocene the right
>>>>> vehicle. It is probably the
>>>>> best of its class of journals out there. Mike and I were asked to
>>>>> write an article for the EGS
>>>>> journal of Surveys of Geophysics. You've not heard of this - few
>>>>> have, so we declined. However,
>>>>> it got me thinking that we could try for Reviews of Geophysics. Need
>>>>> to contact the editorial
>>>>> board to see if this might be possible. Just a thought, but it
>>>>> certainly has a high profile.

>>>>> What we want to write is NOT the scholarly review a la Jean
>>>>> Grove (bless her soul) that
>>>>> just reviews but doesn't come to anything firm. We want a critical
>>>>> review that enables
>>>>> agendas to be set. Ray's recent multi-authored piece goes a lot of
>>>>> the way so we need
>>>>> to build on this.

>>>>>

>>>>> Cheers

>>>>> Phil

>>>>>

>>>>>

>>>>>

>>>>>At 12:55 11/03/03 -0500, Michael E. Mann wrote:

>>>>>>HI Malcolm,

>>>>>>

>>>>>>Thanks for the feedback--I largely concur. I do, though, think there
>>>>>>is a particular problem with "Climate Research". This is where my
>>>>>>colleague Pat Michaels now publishes exclusively, and his two closest
>>>>>>colleagues are on the editorial board and review editor board. So I

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>>>>>promise you, we'll see more of this there, and I personally think
>>>>>there *is* a bigger problem with the "messenger" in this case...
>>>>>
>>>>>But the Soon and Baliunas paper is its own, separate issue too. I too
>>>>>like Tom's latter idea, of a more hefty multi-authored piece in an
>>>>>appropriate journal (Paleoceanography? Holocene?) that seeks to
>>>>>correct a number of misconceptions out there, perhaps using Baliunas
>>>>>and Soon as a case study ('poster child?'), but taking on a slightly
>>>>>greater territory too.
>>>>>
>>>>>Question is, who would take the lead role. I *know* we're all very busy,
>>>>>
>>>>>mike
>>>>>
>>>>> At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:
>>>>>>I'm with Tom on this. In a way it comes back to a rant of mine
>>>>>>to which some of you have already been victim. The general
>>>>>>point is that there are two arms of climatology:
>>>>>> neoclimatology - what you do based on instrumental records
>>>>>>and direct, systematic observations in networks - all set in a
>>>>>>very Late Holocene/Anthropocene time with hourly to decadal
>>>>>>interests.
>>>>>>paleoclimatology - stuff from rocks, etc., where major changes
>>>>>>in the Earth system, including its climate, associated with
>>>>>>major changes in boundary conditions, may be detected by
>>>>>>examination of one or a handful of paleo records.
>>>>>>Between these two is what we do - "mesoclimatology" -
>>>>>>dealing with many of the same phenomena as neoclimatology,
>>>>>>using documentary and natural archives to look at phenomena
>>>>>>on interannual to millennial time scales. Given relatively small
>>>>>>changes in boundary conditions (until the last couple of
>>>>>>centuries), mesoclimatology has to work in a way that is very
>>>>>>similar to neoclimatology. Most notably, it depends on heavily
>>>>>>replicated networks of precisely dated records capable of
>>>>>>being either calibrated, or whose relationship to climate may
>>>>>>be modeled accurately and precisely.
>>>>>>Because this distinction is not recognized by many (e.g.
>>>>>>Sonnechkin, Broecker, Karlen) we see an accumulation of
>>>>>>misguided attempts at describing the climate of recent
>>>>>>millennia. It would be better to head this off in general, rather
>>>>>>than draw attention to a bad paper. After all, as Tom rightly
>>>>>>says, we could all nominate really bad papers that have been
>>>>>>published in journals of outstanding reputation (although there
>>>>>>could well be differences between our lists).
>>>>>>End of rant, Cheers, Malcolm
>>>>> > Hi guys,
>>>>> >
>>>>> > junk gets published in lots of places. I think that what could be
>>>>> > done is a short reply to the authors in Climate Research OR a
>>>>> > SLIGHTLY
>>>>> > longer note in a reputable journal entitled something like
>>>>> > "Continuing
>>>>> > Misconceptions About interpretation of past climate change." I kind
>>>>> > of like the more pointed character of the latter and submitting it as
>>>>> > a short note with a group authorship carries a heft that a reply to a
>>>>> > paper, in no matter what journal, does not.
>>>>> >
>>>>> > Tom
>>>>> >
>>>>> >
>>>>> >
>>>>> > > Dear All,
>>>>> > > Apologies for sending this again. I was expecting a stack of

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>>>>>> > >emails this morning in
>>>>>> > > response, but I inadvertently left Mike off (mistake in pasting)
>>>>>> > >and picked up Tom's old
>>>>>> > > address. Tom is busy though with another offspring !
>>>>>> > > I looked briefly at the paper last night and it is appalling -
>>>>>> > >worst word I can think of today
>>>>>> > > without the mood pepper appearing on the email ! I'll have time to
>>>>>> > >read more at the weekend
>>>>>> > > as I'm coming to the US for the DoE CCPP meeting at Charleston.
>>>>>> > >Added Ed, Peck and Keith A.
>>>>>> > > onto this list as well. I would like to have time to rise to the
>>>>>> > >bait, but I have so much else on at
>>>>>> > > the moment. As a few of us will be at the EGS/AGU meet in Nice, we
>>>>>> > >should consider what
>>>>>> > > to do there.
>>>>>> > > The phrasing of the questions at the start of the paper
>>>>>> > >determine the answer they get. They
>>>>>> > > have no idea what multiproxy averaging does. By their logic, I
>>>>>> > >could argue 1998 wasn't the
>>>>>> > > warmest year globally, because it wasn't the warmest everywhere.
>>>>>> > >With their LIA being 1300-
>>>>>> > >1900 and their MWP 800-1300, there appears (at my quick first
>>>>>> > >reading) no discussion of
>>>>>> > > synchronicity of the cool/warm periods. Even with the instrumental
>>>>>> > >record, the early and late
>>>>>> > > 20th century warming periods are only significant locally at
>>>>>> > >between 10-20% of grid boxes.
>>>>>> > > Writing this I am becoming more convinced we should do
>>>>>> > >something - even if this is just
>>>>>> > > to state once and for all what we mean by the LIA and MWP. I think
>>>>>> > >the skeptics will use
>>>>>> > > this paper to their own ends and it will set paleo back a
>>>>>> number of
>>>>>> > >
>>>>>> > >years if it goes
>>>>>> > > unchallenged.
>>>>>> > >
>>>>>> > > I will be emailing the journal to tell them I'm having
>>>>>> > >nothing more to do with it until they
>>>>>> > > rid themselves of this troublesome editor. A CRU person is on the
>>>>>> > >editorial board, but papers
>>>>>> > > get dealt with by the editor assigned by Hans von Storch.
>>>>>> > >
>>>>>> > > Cheers
>>>>>> > > Phil
>>>>>> > >
>>>>>> > > Dear all,
>>>>>> > > Tim Osborn has just come across this. Best to ignore
>>>>>> > >probably, so don't let it spoil your
>>>>>> > > day. I've not looked at it yet. It results from this journal
>>>>>> > >having a number of editors. The
>>>>>> > > responsible one for this is a well-known skeptic in NZ. He
>>>>>> has let
>>>>>> > >
>>>>>> > >a few papers through by
>>>>>> > > Michaels and Gray in the past. I've had words with Hans von
>>>>>> Storch
>>>>>> > >
>>>>>> > >about this, but got nowhere.
>>>>>> > > Another thing to discuss in Nice !
>>>>>> > >
>>>>>> > > Cheers
>>>>>> > > Phil

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>>>>>> > >
>>>>>> > >>X-Sender: f055@pop.uea.ac.uk
>>>>>> > >>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>>>>> > >>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>>>>> > >>To: p.jones@uea
>>>>>> > >>From: Tim Osborn <t.osborn@uea.ac.uk>
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3253. 2003-09-03

date: Wed, 3 Sep 2003 08:32:11 -0400
from: Edward Cook <drdendro@ldeo.columbia.edu>
subject: An idea to pass by you
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Hi Keith,

After the meeting in Norway, where I presented the Esper stuff as described in the extended abstract I sent you, and hearing Bradley's follow-up talk on how everybody but him has fucked up in reconstructing past NH temperatures over the past 1000 years (this is a bit of an overstatement on my part I must admit, but his air of papal infallibility is really quite nauseating at times), I have come up with an idea that I want you to be involved in. Consider the tentative title:

"Northern Hemisphere Temperatures Over The Past Millennium: Where Are The Greatest Uncertainties?"

Authors: Cook, Briffa, Esper, Osborn, D'Arrigo, Bradley(?), Jones (??), Mann (infinite?) - I am afraid the Mike and Phil are too personally invested in things now (i.e. the 2003 GRL paper that is probably the worst paper Phil has ever been involved in - Bradley hates it as well), but I am willing to offer to include them if they can contribute without just defending their past work - this is the key to having anyone involved. Be honest. Lay it all out on the table and don't start by assuming that ANY reconstruction is better than any other.

Here are my ideas for the paper in a nutshell (please bear with me):

1) Describe the past work (Mann, Briffa, Jones, Crowley, Esper, yada, yada, yada) and their data over-laps.

2) Use the Briffa&Osborn "Blowing Hot And Cold" annually-resolved recons (plus Crowley?) (boreholes not included) for comparison because they are all scaled identically to the same NH extra-tropics temperatures and the Mann version only includes that part of the NH (we could include Mann's full NH recon as well, but he would probably go ballistic, and also the new Mann&Jones mess?)

3) Characterize the similarities between series using unrotated (maybe rotated as well) EOF analysis (correlation for pure similarity, covariance for differences in amplitude as well) and filtering on the reconstructions - unfiltered, 20yr high-pass, 100-20 bandpass, 100 lowpass - to find out where the reconstructions are most similar and different - use 1st-EOF loadings as a guide, the comparisons of the power spectra could also be done I suppose

4) Do these EOF analyses on different time periods to see where they differ most, e.g., running 100-year EOF windows on the unfiltered data, running 300-year for 20-lp data (something like that anyway), and plot the 1st-EOF loadings as a function of time

5) Discuss where the biggest differences lie between reconstructions (this will almost certainly occur most in the 100 lowpass data), taking into account data overlaps

6) Point out implications concerning the next IPCC assessment and EBM forcing experiments that are basically designed to fit the lower frequencies - if the greatest uncertainties are in the >100 year band, then that is where the greatest uncertainties will be in the forcing experiments

7) Publish, retire, and don't leave a forwarding address without trying to prejudice this work, but also because of what I almost think I know to be the case, the results of this study will show that we can probably say a fair bit about <100 year extra-tropical NH temperature variability (at least as far as we believe the proxy estimates), but honestly know fuck-all about what the >100 year variability was like with any certainty (i.e. we know with certainty that we know fuck-all).

Of course, none of what I have proposed has addressed the issue of

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seasonality of response. So what I am suggesting is strictly an empirical comparison of published 1000 year NH reconstructions because many of the same tree-ring proxies get used in both seasonal and annual recons anyway. So all I care about is how the recons differ and where they differ most in frequency and time without any direct consideration of their TRUE association with observed temperatures.

I think this is exactly the kind of study that needs to be done before the next IPCC assessment. But to give it credibility, it has to have a reasonably broad spectrum of authors to avoid looking like a biased attack paper, i.e. like Soon and Balliunas.

If you don't want to do it, just say so and I will drop the whole idea like a hot potato. I honestly don't want to do it without your participation. If you want to be the lead on it, I am fine with that too.

Cheers,

Ed

--

=====
Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152
=====

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4495. 2003-09-03

cc: Urs Neu <urs.neu@sanw.unibe.ch>, Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert <marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera <kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, "Michael E. Mann" <mann@virginia.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin Trenberth <trenberth@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve Schneider <shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>, jmahlman@ucar.edu, wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Jürg Beer <beer@hermes.emp-eaw.ch>
date: Wed, 03 Sep 2003 10:13:43 +0200

from: André Berger <berger@astr.ucl.ac.be>

subject: Re: anti-CO2

to: Tom Wigley <wigley@ucar.edu>

Dear Tom,

My explanation is the following:

Zichichi is primarily a theoretical physicist involved in neutrino research. So any theory

involving the Sun ... and any opportunity to show that they (physicists) are better than us

(climatologists) is welcome.

Best Regards,

André

At 07:04 2/09/2003 -0600, Tom Wigley wrote:

Andre,

I agree. I heard that Zichichi has links with the Vatican. A number of other greenhouse

skeptics have extreme religious views. Perhaps they believe that god would not let us do

cg2003

this to the planet, and that, if we do, she will save us?

Hmmmm.

Tom.

+++++

André Berger wrote:

Dear All,

More I read your email about the "anti-CO2", more I am convinced that an International

Committee on Ethics in Geo-Sciences is needed. Indeed either we do not answer their

attacks or we lose time and money doing it. The third solution is an official statement

telling what the members of such a Committee of Ethics think about irresponsible

statements by such anti-CO2 fellows.

Best Regards,

André

Prof. A. BERGER

Université catholique de Louvain

Institut d'Astronomie et de Géophysique G. Lemaître

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[2]http://www.astr.ucl.ac.be

5036. 2003-09-03

date: Wed Sep 3 14:10:35 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: forgot

to: drdendro@ldeo.columbia.edu

to say would prefer no involvement of Mann and Phil -
and can you tell me what reconstruction Bradley did ever ? unless you mean the
Bradley and

Jones early decadal series?

--

Professor Keith Briffa,

Climatic Research Unit

University of East Anglia

Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[1]http://www.cru.uea.ac.uk/cru/people/briffa[2]/

1556. 2003-09-04

cg2003

cc: david.roberts@metoffice.com, andy.jones@metoffice.com,
jonathan.gregory@metoffice.com, jason.lowe@metoffice.com,
richard.betts@metoffice.com, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk,
margaret.woodage@metoffice.com, p.jones@uea.ac.uk

date: Thu, 04 Sep 2003 14:20:01 +0100

from: Simon Tett <simon.tett@metoffice.com>

subject: Re: Abstract for AGU

to: Tom Crowley <tcrowley@duke.edu>

Hi Tom,

the model may be too sensitive! I just havn't yet done the detailed analysis. I will over the next month...

Simon

Tom Crowley wrote:

>

> Simon,

>

> sounds very good EXCEPT -- an alternate explanation is that the model

> sensitivity may be too high. come on, we must keep a balanced view

> of the sources of discrepancies! tom

>

> >Dear All,

> >

> > I have submitted an abstract(see below) on our simulation/analysis of
> > the last 500 years to AGU session PP11. Phil Jones has been
> > co-opted through his early instrumental data. You have up to 1400
> > UTC to scream! Sorry -- I left things till late in the day.

> >

> >Simon

> >

> >=====

> >

> >Simulating the Last Half-Millennium

> >

> >S. Tett (1), R. Betts (2), D. Roberts(2), M. Woodage

> >(2), A. Jones (2), T. Crowley (3), K. Briffa (4), T. Osborn (4), J.

> >Gregory (5),

> >J. Lowe (1) and P. Jones (3).

> >

> >(1) Hadley Centre -- Reading, Meteorology Building University of

> > Reading, Reading Berkshire RG6 6BB UK

> >

> >(2) Hadley Centre, Met Office, FitzRoy Road, Exeter, EX1 3PB, UK

> >

> >(3) Dept. of Earth and Ocean Sciences, Nicholas School of the

> > Environment and Earth Sciences, Duke University, NC, USA

> >

> >(4) Climatic Research Unit, School of Environmental Sciences,

> > University of East Anglia, Norwich NR4 7TJ UK

> >

> >(5) CGAM, Department of Meteorology, University of Reading, PO Box

> > 243, Reading RG6 6BB UK

> >

> >

> >To test simulated AOGCM variability and change against proxy

> >reconstructions we have simulated the last half-millennium using the

> >HadCM3 model forced with natural and anthropogenic forcings. The

> >natural forcings used were changes in orbital parameters, volcanic

> >aerosol forcings, and solar irradiance. A simulation (NATURAL) forced

> >with only natural factors and with land-surface characteristics set

> >to 1750 values and well-mixed greenhouse gases set to pre-industrial

> >concentrations was carried out. A second simulation (ALL) with both

> >anthropogenic and natural forcings was started in 1750 from

> >NATURAL. In ALL sulphate aerosols, greenhouse gases, ozone and land

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> > surface characteristics also change.
> >
> >
> > The natural simulation shows general agreement between the naturally
> > forced simulation and paleo-reconstructions until the mid- to
> > late-19th century. However the simulated response appears to be too
> > large while simulated decadal variability is significantly smaller
> > than that reconstructed. In the simulations there is an anthropogenic
> > impact on climate by the mid to late 19th century. Comparison with
> > early European instrumental data appears to qualitatively confirm the
> > simulated anthropogenic cooling during the 19th century.
> >
> >
> > After correcting for long-term drift, simulated sea-level falls
> > rapidly after large volcanic eruptions (such as Tambora) then recovers
> > over several decades to pre-eruption conditions. A simple diagnostic
> > model shows maximum glacier advance during the maunder minimum and the
> > mid-19th century. Twentieth century sea-level rise is dominated by
> > anthropogenic forcings mainly due to thermal expansion with a moderate
> > contribution from glacier retreat.
> >
> >
> >
> >--
> > Dr Simon Tett Managing Scientist, Data development and applications.
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> > Tel: +44 (0)1344 856886 Fax: +44 (0)1344 854898
> > E-mail: simon.tett@metoffice.com <http://www.metoffice.com>
> >
> --
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> Dept. of Earth and Ocean Sciences
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> Attachment Converted: "c:\eudora\attach\simon.tett9.vcf"

3901. 2003-09-04

date: Thu, 04 Sep 2003 08:27:02 -0600
from: Caspar Ammann <ammann@ucar.edu>
subject: Re: AGU session submission
to: Tim Osborn <t.osborn@uea.ac.uk>
<x-flowed>

Tim,
it seems to work now. Give it a shot.
Caspar
Tim Osborn wrote:
> Dear Caspar,
>

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> I have been trying for the past few hours to submit an abstract to the
> session that you're convening at the AGU Fall meeting. I've not
> managed to complete the process because their system is currently
> failing at the preview stage. It says to try again, which I will do,
> but because the official deadline is about to pass, I thought I'd
> email you my abstract just so that you know it is on the way (assuming
> the deadline is extended past when the system is fixed!).

> Best regards

> Tim

> -----
> Simulated and Observed Climate Signals in Borehole Temperature Profiles

> Tim Osborn
> Keith Briffa

> Climatic Research Unit, School of Environmental Sciences, University
> of East Anglia, Norwich NR4 7TJ, UK

> Simon Tett

> Hadley Centre - Reading, Meteorology Building, University of Reading,
> Reading, Berkshire RG6 6BB, UK

> Forward simulation of borehole temperature profiles are presented,
> using ground (soil) temperature variations simulated by the HadCM3
> climate model forced by natural and anthropogenic factors from AD 1500
> to 2000. Differences between soil temperature and air temperature
> variations are assessed, in terms of the driving influence of snow
> cover and vegetation cover changes, and in terms of their impact on
> the simulated borehole temperature profiles. The simulated profiles
> are then compared with observed temperature profile anomalies.

> The sensitivity of air temperature reconstructions to method of
> gridding the individual borehole records and to the profile sampling
> date will be demonstrated by analysis of the climate signal in the
> observed temperature profile data set. Comparison of the spatial
> signature of twentieth century borehole-derived trends with
> instrumental air temperature trends is used to assess confidence in
> the climate signal recoverable from the borehole data set.

> -----
>
> Dr Timothy J Osborn
> Climatic Research Unit
> School of Environmental Sciences, University of East Anglia
> Norwich NR4 7TJ, UK

> e-mail: t.osborn@uea.ac.uk
> phone: +44 1603 592089
> fax: +44 1603 507784
> web: <http://www.cru.uea.ac.uk/~timo/>
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

--
Caspar M. Ammann
National Center for Atmospheric Research
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Advanced Study Program
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Boulder, CO 80307-3000
email: ammann@ucar.edu
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fax: 303-497-1348

4496. 2003-09-04

date: Thu Sep 4 13:30:31 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Abstract for AGU
to: Simon Tett <simon.tett@metoffice.com>

Simon
though time short, thought it worth making the following suggested changes. The main ambiguity though is your meaning about the simulated variability being too large (see *Italic remarks*) - do you mean high-frequency? This sentence is not clear. Below changes shown in bold.

Keith and Tim
At 11:25 AM 9/4/03 +0100, you wrote:

Dear All,

I have submitted an abstract(see below) on our simulation/analysis of the last 500 years to AGU session PP11. Phil Jones has been co-opted through his early instrumental data. You have up to 1400 UTC to scream! Sorry -- I left things till late in the day.

Simon

=====

Simulating the Last Half-Millennium

S. Tett (1), R. Betts (2), D. Roberts(2), M. Woodage

(2), A. Jones (2), T. Crowley (3), K. Briffa (4), T. Osborn (4), J. Gregory

(5),

J. Lowe (1) and P. Jones (3).

(1) Hadley Centre -- Reading, Meteorology Building University of Reading, Reading Berkshire RG6 6BB UK

(2) Hadley Centre, Met Office, FitzRoy Road, Exeter, EX1 3PB, UK

(3) Dept. of Earth and Ocean Sciences, Nicholas School of the Environment and Earth Sciences, Duke University, NC, USA

(4) Climatic Research Unit, School of Environmental Sciences, University of East Anglia, Norwich NR4 7TJ UK

(5) CGAM, Department of Meteorology, University of Reading, PO Box 243, Reading RG6 6BB UK

To test simulated AOGCM variability and change against proxy reconstructions we have simulated the last half-millennium using the HadCM3 model forced with natural and anthropogenic forcings. The natural forcings used were changes in orbital parameters, volcanic aerosols, and solar irradiance. One simulation (NATURAL), was run from

A.D.1500 using

only natural forcing factors and with land-surface characteristics set to A.D.1750 values and well-mixed greenhouse gases set to pre-industrial concentrations. A second simulation (ALL), uses a combination of both anthropogenic and natural forcings starting in 1750. In ALL, sulphate

aerosols,

greenhouse gases, ozone and land

surface characteristics also change through time.

The natural simulation shows general agreement with the paleo-reconstructions until the

mid- to

late-19th century. However, the (is something missing here?) simulated response appears

to be too

large while simulated decadal variability is significantly smaller

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than that reconstructed. In the simulations there is an anthropogenic impact on climate by the mid to late 19th century. Comparison with early European instrumental data appears to confirm qualitatively the simulated anthropogenic (do you mean sulphate aerosol and what time?) cooling during the 19th century. After correcting for long-term drift (I would not put this previous phrase in here as the sea level response is not dependent on this correction) The simulated sea-level falls rapidly after large volcanic eruptions (such as Tambora), then recovers over several decades to pre-eruption levels. A simple diagnostic model shows maximum glacier advance occurring during the Maunder minimum and the mid-19th century. Twentieth century sea-level rise, which is dominated by anthropogenic forcings, is mainly due to ocean thermal expansion with a moderate contribution from glacier melting.

--
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E-mail: simon.tett@metoffice.com [1]<http://www.metoffice.com>

--
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[2]<http://www.cru.uea.ac.uk/cru/people/briffa>[3]/

643. 2003-09-05

cc: t.osborn@uea.ac.uk
date: Fri Sep 5 15:34:10 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: An idea to pass by you
to: Edward Cook <drdendro@ldeo.columbia.edu>
At 08:32 AM 9/3/03 -0400, Edward Cook wrote:

Hi Keith,
After the meeting in Norway, where I presented the Esper stuff as described in the extended abstract I sent you, and hearing Bradley's follow-up talk on how everybody but him has fucked up in reconstructing past NH temperatures over the past 1000 years so what more precisely was Bradley saying - we can discuss on phone (this is a bit of an overstatement on my part I must admit, but his air of papal infallibility is really quite nauseating at times), I have come up with an idea that I want you to be involved in. Consider the tentative title:
"Northern Hemisphere Temperatures Over The Past Millennium: Where Are The Greatest Uncertainties?"
prefer something like "where is the consensus" - doesn't imply an academic analysis of statistical (space and time) confidence levels
Authors: Cook, Briffa, Esper, Osborn, D'Arrigo, Bradley(?), Jones (??), Mann (infinite?) - I am afraid the Mike and Phil are too personally invested in

things now

(i.e. the 2003 GRL paper that is probably the worst paper Phil has ever been involved in

- Bradley hates it as well

interesting to know why - I too share this feeling though , again we can discuss on phone ,

but don't consider an

), but I am willing to offer to include them if they can contribute without just

defending their past work - this is the key to having anyone involved. Be honest. Lay it

all out on the table and don't start by assuming that ANY reconstruction is better than

any other.

Here are my ideas for the paper in a nutshell (please bear with me):

1) Describe the past work (Mann, Briffa, Jones, Crowley, Esper, yada, yada, yada) and

their data over-laps.

fine - plus a detailed breakdown of how they are produced (simple unweighted/weighted

averages) , and , most important what went into each

at different times - ie explicit how common data input increases back in time

2) Use the Briffa&Osborn "Blowing Hot And Cold" annually-resolved recons (plus Crowley?)

would only be latest Crowley (if annually resolved as I think it is)

(boreholes not included)

completely omit reference to Boreholes or it complicates all

for comparison because they are all scaled identically to the same NH

extra-tropics

temperatures and the Mann version only includes that part of the NH (we could include

Mann's full NH recon as well, but he would probably go ballistic, and also the new

Mann&Jones mess?)

3) Characterize the similarities between series using unrotated (maybe rotated as well)

EOF analysis (correlation for pure similarity, covariance for differences in amplitude

as well

coincidence - just suggested this to Pavla Fenwick as suggestion for exploring similarity

of chronology and core series in NZ (irrelevant comment other than it is unusual to see a

simple case study where this is done)

Also Tim has done some of this but I do not think it negates its inclusion here

) and filtering on the reconstructions - unfiltered, 20yr high-pass, 100-20 bandpass,

100 lowpass - to find out where the reconstructions are most similar and different - use

1st-EOF loadings as a guide, the comparisons of the power spectra could also be done I

suppose

yes of course is the crux of issue - but needs exploration of methods (eg using SSA ,

provided choice of prediction error filter length does not bias results. Fine to use

band-pass filters if can agree on bands - possibility also of calibrating against similarly

filtered temperatures (but maybe out of scope, though it needs doing more systematically -

though perhaps in Tim's paper). The power spectra (or coherency)

do need to be compared.

4) Do these EOF analyses on different time periods to see where they differ

most, e.g.,
 running 100-year EOF windows on the unfiltered data, running 300-year for
 20-1p data
 (something like that anyway), and plot the 1st-EOF loadings as a function of
 time
 agree
 5) Discuss where the biggest differences lie between reconstructions (this
 will almost
 certainly occur most in the 100 lowpass data), taking into account data
 overlaps
 yes - direction of discussion will have to wait on some results though
 6) Point out implications concerning the next IPCC assessment and EBM forcing
 experiments that are basically designed to fit the lower frequencies - if the
 greatest
 uncertainties are in the >100 year band, then that is where the greatest
 uncertainties
 will be in the forcing experiments
 yes , this is crucial issue regarding the significance of this agonising over
 what genuine
 independent confidence can be placed in variations at specific frequencies -
 seems to me
 that we could include a short mention of the work done so far (last Crowley ,
 recent Hegerl
 paper etc.) and illustrate this and perhaps show is their results are consistent
 (which
 they are not). We tried to allude to this in the Hot and Cold piece , but they
 cut it.
 7) Publish, retire, and don't leave a forwarding address
 Without trying to prejudice this work, but also because of what I almost think
 I know to
 be the case, the results of this study will show that we can probably say a
 fair bit
 about <100 year extra-tropical NH temperature variability (at least as far as
 we believe
 the proxy estimates), but honestly know fuck-all about what the >100 year
 variability
 was like with any certainty (i.e. we know with certainty that we know
 fuck-all).
 Of course, none of what I have proposed has addressed the issue of seasonality
 of
 response. So what I am suggesting is strictly an empirical comparison of
 published 1000
 year NH reconstructions because many of the same tree-ring proxies get used in
 both
 seasonal and annual recons anyway. So all I care about is how the recons
 differ and
 where they differ most in frequency and time without any direct consideration
 of their
 TRUE association with observed temperatures.
 I think this is exactly the kind of study that needs to be done before the
 next IPCC
 assessment. But to give it credibility, it has to have a reasonably broad
 spectrum of
 authors to avoid looking like a biased attack paper, i.e. like Soon and
 Ballunas.
 If you don't want to do it, just say so and I will drop the whole idea like a
 hot
 potato. I honestly don't want to do it without your participation. If you want
 to be the
 lead on it, I am fine with that too.
 The idea is a good one and consistent with what Tim and I are thinking (Tim
 Cheers,
 Ed

cg2003

--

=====

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=====

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1370. 2003-09-05

cc: Keith Briffa <k.briffa@uea.ac.uk>
date: Fri, 05 Sep 2003 13:51:08 -0400
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Something for the weekend !
to: Phil Jones <p.jones@uea.ac.uk>

sorry, meant "is just the minimum slope" constraint, in first sentence...
apologies for the multiple emails,
mike

At 01:47 PM 9/5/2003 -0400, Michael E. Mann wrote:

Actually,
I think Dave's suggestion "reflecting the data across the endpoints" is really
just the
"minimum norm" constraint, which insures zero slope near the boundary. In
other words,
he's probably only talking about reflecting about the time axis. I assert that
a
preferable alternative, when there is a trend in the series extending through
the
boundary is to reflect both about the time axis and the amplitude axis (where
the
reflection is with respect to the y value of the final data point). This
insures a point
of inflection to the smooth at the boundary, and is essentially what the
method I'm
employing does (I simply reflect the trend but not the variability about the
trend--they
are almost the same)...

mike

At 01:34 PM 9/5/2003 -0400, Michael E. Mann wrote:

mentioned
sorry phil, one more relevant item. I've cc'd in Keith on this, since you had
that you had discussed the issue w/ him.
This is from Dave Meko's (quite nice!) statistics lecture notes:
[1]http://www.ltrr.arizona.edu/~dmeko/notes_8.pdf
See page 2, section 8.1.
He provides two (in reality, as I mentioned before, there are really 3!) basic
boundary
constraints on a smooth (ie, in "filtering"). The first method he refers to is
what I
called the "minimum norm" constraint (assuming the long-term mean beyond the
boundary). The second, which he calls "reflecting the data across the

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endpoints", is
the constraint I have been employing which, again, is mathematically
equivalent to
insuring a point of inflection at the boundary. This is the preferable
constraint for
non-stationary mean processes, and we are, I assert, on very solid ground
(preferable
ground in fact) in employing this boundary constraint for series with
trends...
mike
At 05:20 PM 9/5/2003 +0100, Phil Jones wrote:
Mike,
Attached some more plots.
1. Figure 7 - Forcing. Guess this is it. Could cut the y scale to -6 and
say in
caption that
1258 or 1259 is the only event to go beyond this, then give value in
caption. Scale
will then widen out. OK to do ? Caspar's solar now there.
2. Fig 2a - first go at coverage. This is % coverage over 1856-2002 from
HadCRUT2v.
3. Fig 4 again. Moved legends and reduced scale. Talked to Keith and we both
think
that
the linear trend padding will get criticised. Did you use this in GRL and or
Fig 5 for
RoG
with Scott. If so we need to explain it.
On this plot all the series are in different units, so normalised over
1751-1950 (or
equiv for
decades) then smoothed. Again here I can reduce scale further and Law Dome
can go
out of the plot. Thoughts ? Think all should be same scale.
Have got GKSS model runs for Fig 8. Were you happy Hans' conditions. If so
I'll send
onto
Scott.
Next week I only have Fig 2b to do. This will be annual plot of NH, Europe
and CET,
smoothed in some way.
For the SOI I and Tim reckon that it won't work showing this at
interannual
timescale with
3 plots. It will then not be like the NAO plot.
Thoughts on colours as well.
Have a good weekend. Logging off once this has gone.
cheers
Phil
Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

cg2003
e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Department of Environmental Sciences, Clark Hall
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

996. 2003-09-08

date: Mon, 08 Sep 2003 13:09:24 +0100
from: Suraje Dessai <s.dessai@uea.ac.uk>
subject: Fwd: RE: Climate sensitivity PDF
to: Mike Hulme <m.hulme@uea.ac.uk>
some rather critical comments from Richard Tol on our paper ... I sent the paper around to the people who sent me their climate sensitivity PDFs, hence the e-mail.
Suraje

From: "Richard Tol" <tol@dkrz.de>
To: "Suraje Dessai" <s.dessai@uea.ac.uk>
Subject: RE: Climate sensitivity PDF
Date: Mon, 8 Sep 2003 14:10:12 +0200
X-Mailer: Microsoft Outlook IMO, Build 9.0.2416 (9.0.2910.0)
Importance: Normal
Hi Suraje,
I must say that I find the working paper disturbing. The main question "are probabilities necessary?" has been answered a long time ago: one can support decisions without probabilities, but the quality of the decision necessarily increases with the information available. (Probabilities are information.) This is undisputed with solitary decision makers, and the exceptions for moral hazards and public goods are well-documented. Climate change is not a special problem, so all this applies. Your title is misleading, because you write about adaptation rather than climate change in general. Your quotes in your "case against probabilities" are misinterpreted; these people argue that other types of research have a higher priority, not that probabilities would not be handy in adaptation research; besides, their proposed shift in emphasis is towards the type of research that they would like to do, and should therefore be discounted.

Best
Richard
Dr. Richard S.J. Tol
Michael Otto Professor of Sustainability and Global Change
Hamburg, Vrije and Carnegie Mellon Universities
ZMK, Tropelwitzstrasse 7, 22529 Hamburg, Germany
+49 40 428387007/8 (voice) +49 40 428387009 (fax) tol@dkrz.de

cg2003

[1]<http://www.uni-hamburg.de/wiss/FB/15/Sustainability/tol.html>

-----Original Message-----

From: Suraje Dessai [[2]<mailto:s.dessai@uea.ac.uk>]

Sent: Saturday, September 06, 2003 6:42 PM

To: jonathan.gregory@metoffice.com; ceforest@MIT.EDU; wigley@ucar.edu; knutti@climate.unibe.ch; tol@dkrz.de; schlesin@atmos.uiuc.edu

Cc: Sarah Raper

Subject: Fwd: Climate sensitivity PDF

Dear all,

published
time, as
published. I
would be
figure and I
If you

Following on from the e-mail below, I attach the working paper where we the climate sensitivity PDF figure. Of course this is only a snapshot in time, as Chris and Reto already have revised values since their papers were published. I assume IPCC or someone else will collect these values in the future. It would be interesting to have an article discussing just the climate sensitivity figure and I sent an outline to EOS (AGU's newsletter), but they never got back to me.

think this is worthwhile pursuing let me know.

Comments on the working paper are most welcome.

Best wishes,

Suraje

Date: Tue, 11 Mar 2003 18:22:18 +0000

To: jonathan.gregory@metoffice.com, ceforest@MIT.EDU, wigley@ucar.edu, knutti@climate.unibe.ch, tol@dkrz.de, schlesin@atmos.uiuc.edu

From: Suraje Dessai <s.dessai@uea.ac.uk>

Subject: Climate sensitivity PDF

Cc: Sarah Raper <s.raper@uea.ac.uk>

Dear all,

plotted
self-explanatory.
different
fingerprinting,
which
temperature,
change will
explicit about
data
on this
climate

Many thanks for the various climate sensitivity PDFs you sent me. I have them and attached it at the request of some of you. It should be self-explanatory. For the purposes of my literature review on "whether climate policy needs probabilities or not", I interpreted the figure as follows: Essentially, value judgements about which techniques to use (e.g., optimal bootstrapping or Bayesian techniques), which GCMS/models to employ or parameters to include (e.g., sulphate aerosols, solar forcing, ocean etc.) yield significantly different curves. Probabilities of climate change will remain subjective so it is extremely important for researchers to be explicit about their assumptions. I'm interested to know if you agree with this interpretation (of course constraints are also a major issue) and if you have any further thoughts on this comparison figure. Also, have I missed out any other major studies on sensitivity PDFs?

Best regards,
Suraje

Suraje Dessai

PhD Researcher

Tyndall Centre for Climate Change Research

cg2003
School of Environmental Sciences
University of East Anglia
Norwich, NR4 7TJ
United Kingdom
Tel: + 44 (0)1603 593911
Fax: + 44 (0)1603 593901
E-mail: s.dessai@uea.ac.uk
Web: [3]http://www.tyndall.ac.uk

3717. 2003-09-08

cc: lkeigwin@whoi.edu, plemke@awi-bremerhaven.de, ewwo@bas.ac.uk,
r.r.dickson@cefas.co.uk, maria.noguer@defra.gsi.gov.uk, mccave@esc.cam.ac.uk,
haugan@gfi.uib.no, studhope@glg.ed.ac.uk, B.Turrell@marlab.ac.uk,
rwood@meto.gov.uk, sfbtett@meto.gov.uk, p.j.valdes@bristol.ac.uk,
j.lowe@rhbnc.ac.uk, marotzke@dkrz.de, pc@soc.soton.ac.uk, a.j.watson@uea.ac.uk,
k.briffa@uea.ac.uk, m.hulme@uea.ac.uk, Philip Newton <ppn@nerc.ac.uk>, Meric
Srokosz <mas@soc.soton.ac.uk>, Jonathan Gregory <jonathan@met.reading.ac.uk>
date: Mon, 08 Sep 2003 09:06:54 +0100

from: Julia Slingo <j.m.slingo@reading.ac.uk>
subject: Re: Dates for RAPID model inter-comparison workshop
to: Christine Gommenginger <cgl@soc.soton.ac.uk>

Christine,

I have several commitments during this period but hope that Jonathan Gregory
will be able to represent me if necessary. Currently I am committed to the
following dates:

19/20 January: Royal Society Meeting on W. Indian Ocean

29/30 January: Seminar in Cambridge

14-29 February: CLIVAR Monsoon and Indian Ocean Panels, International Workshop
on Indian Ocean

Regards,

Julia

Christine Gommenginger wrote:

> Dear all,

>

> As part of the RAPID integrative modelling activities, we are planning a
> 1-day workshop for the wider community interested in participating in the
> RAPID model inter-comparison experiment. We are presently hoping to hold the
> workshop between mid-January and the end of February 2004, and I would like
> to pool for suitable dates over this period for those of you who plan to
> attend this event.

>

> I would also be grateful if you could point out any meetings/conferences
> during this period which might prevent interested people in the wider
> community from attending the workshop.

>

> Best wishes,
> Christine

>

>

>

Dr Christine Gommenginger

>

> Laboratory for Satellite Oceanography (LSO)
> James Rennell Division for Ocean Circulation and Climate
> Southampton Oceanography Centre (SOC)
> Southampton, SO14 3ZH, United Kingdom

>

> Tel (direct): +44 (0)2380 596411 Fax: +44 (0)2380 596400
> http://www.soc.soton.ac.uk/JRD/

>

> Assistant Science Co-ordinator for NERC RAPID Climate Change Programme
> http://rapid.nerc.ac.uk/

>
--

Prof. Julia Slingo
Director, NCAS Centre for Global Atmospheric Modelling
Department of Meteorology
University of Reading
Earley Gate
Reading RG6 6BB
Tel: +44 (0)118 378 8424
Fax: +44 (0)118 378 8316
Email: j.m.slingo@reading.ac.uk
Web: http://www.cgam.nerc.ac.uk/

3434. 2003-09-11

date: Thu Sep 11 10:53:46 2003
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: FW: Media coverage of the recent heatwave
to: cru.all@uea

Dear cru.all@uea
you might be interested in signing up to the letter below. I'm not sure of the
originator
(i.e. writer) of the letter, just gives an Oxford address. Personally I agree
with some of
the content but don't think it is worded carefully enough in places. Maybe you
feel
differently!

Tim

From: "Laura Middleton" <Laura.Middleton@uea.ac.uk>
To: <tyn.hq@uea.ac.uk>
You may or may not wish to respond to this.
Laura

~~~~~  
Laura Middleton  
Scientific Assistant  
The Tyndall Centre for Climate Change Research

-----Original Message-----

From: David Cromwell [[1]mailto:ddc@soc.soton.ac.uk]  
To: laura.middleton@uea.ac.uk

Please consider signing the following letter and perhaps sending round

Tyndall

Centre colleagues.

N.B. Do NOT reply to this email to sign up. Please email  
info@risingtide.org.uk as  
instructed below.

David Cromwell

James Rennell Division for Ocean Circulation and Climate  
Southampton Oceanography Centre

RISING TIDE SIGN ON LETTER TO PROTEST MEDIA COVERAGE OF THE HEATWAVE

\*\*\*\*\*

Dear Friend,

The news media's reporting of the heatwave was a disgrace, especially the  
coverage  
given to the climate change deniers. And it is dangerous. Ask around and  
you'll soon  
find that most people have absorbed the message that everything is going  
to get  
better in a greenhouse world.

This is a chance to challenge the underlying editorial values. Please  
SIGN this

cg2003

letter and distribute it on your own lists as far and wide as possible.  
Note the deadline of 12th September  
This letter will be sent to the news editors of all the national  
newspapers, the  
national evening television news, the main national radio news  
programmes, audience  
complaints programmes, and the Press Complaints Authority. In each case  
it will be  
personally addressed to the main contact. We will also use it to try to  
stimulate a  
wider debate about how climate change is being covered in the UK.  
TO SIGN THIS LETTER REPLY TO INFO@RISINGTIDE.ORG.UK AND PUT YOUR NAME IN  
THE SUBJECT  
LINE. PLEASE RESPOND BY FRIDAY 12TH SEPTEMBER.  
If you are signing on behalf of an organisation, or wish to give a few  
words about  
yourself such as your occupation or qualifications add the details to the  
top of the  
reply. We will keep your e-mail details confidential, pass them to no  
one, and will  
only write back to you to tell you about any response we receive.

\*\*\*\*\*  
\*

\*\*\*\*\*  
SIGN ON LETTER TO CHALLENGE THE UK MEDIA ON ITS COVERAGE OF CLIMATE  
CHANGE DURING  
THE RECENT HEAT WAVE  
Dear Sir or Madam  
We the undersigned individuals and groups are writing to you to express  
our deep  
concern and frustration over the recent coverage given to the August heat  
wave in  
the UK news media.  
The news media failed to truthfully report the heat wave in two important  
respects:  
it failed to accurately reflect the overwhelming consensus of the two  
thousand  
scientists involved in the Intergovernmental Panel on Climate Change and  
the  
endorsement they receive from governments and major scientific  
institutions.  
Secondly it  
failed to report the heat wave with value free objectivity.  
In the first case, newspapers and television regularly reported climate  
change as an  
unresolved debate between experts. Prominent and uncritical coverage was  
given to  
the tiny handful of confident self promoting contrarians who defy the  
scientific  
consensus. In the Times (August 8th) Philip Stott, who claims I do not  
believe in  
climate  
change was allowed a leader column to argue that global warming has  
morphed into an  
ancient style religion. Bjorn Lomborg was set up in debate formats on  
both  
Channel Four evening news and the Radio 4 Today Programme to argue that  
climate  
change is an exaggerated problem that is too expensive to counter.  
On the 10th August, when temperatures rose far above previous records,  
the BBC  
evening news introduced the news item as the climate debate. It carried

an interview with a so-called climate skeptic, Piers Corbyn saying it has nothing to do with global warming- it is correlated with particles from the sun. It was irresponsible and unprofessional to give such uncritical legitimacy to Corbyn, who has no scientific credibility and refuses to subject his eccentric climatic theories to peer review. On ITV these viewers heard the reporter signing off with the words- if and when we see fluke. scenes again will depend on whether it is down to global warming or just existed, Yet, if the public was being left in doubt as to whether climate change altogether they were left in no such doubt that higher temperatures were an desirable outcome. Smiling presenters made live link ups to reporters eating ice cream at the sea side. They spoke of the excellent weather, basking in sunshine and presented forecasts of continuing heat as more good news. The newspapers were similarly awash with bikinis, cheering holidaymakers and news of record grape harvests. Even the Guardian, which had otherwise faithfully reported climate change, called on readers to rejoice (Editorial 11th August). This reporting of national celebration was biased and value-driven. It allowed no consideration of the extreme discomfort and danger such extreme temperatures posed to the sick and old. We now know that there were over 900 additional deaths in Britain during the week ending 15th August. In France the heat wave killed at least 13,000 people. In a wider context climate change threatens the lives and livelihoods of millions of the worlds poorest and most vulnerable people. What is more, this coverage allowed a space for contrarians to promote the pernicious argument that climate change will be beneficial. Bjorn Lomborg argued that warmer winters would lead to fewer deaths overall. Philip Stott claimed that a little warming in the UK can only be a good thing (Times, 8th August). An extraordinary editorial in the Spectator attacked the mass of hot air generated by the climate change lobby and argued that Britain should not take a lead in international action because we would benefit more than anybody from climate change (9th August). We are therefore writing to you to express our deep frustration that, after two decades of consistent and growing evidence, the news media is still reporting climate change in this confused and superficial manner. Accurate and consistent

cg2003

reporting is all the more important to maintain focus against a problem which is relatively slow to develop and does not readily fit short term political and business cycles. We fear that yet another important opportunity has been lost to consolidate public opinion behind a wide ranging and timely response to this crisis.  
Yours sincerely,

\*\*\*\*\*  
\*

\*\*\*\*\*  
RISING TIDE Email: info@risingtide.org.uk Phone: 01865 241097 Address:  
16b Cherwell  
St, Oxford OX1 1BG. Web site [2]www.risingtide.org.uk  
PLEASE FORWARD THIS TO A FRIEND AND INVITE THEM TO JOIN THE LIST  
To subscribe to this list send a blank e-mail to  
news-subscribe@risingtide.org.uk To  
unsubscribe from this list send a blank e-mail to  
news-unsubscribe@risingtide.org.uk

4783. 2003-09-12

cc: <Chris.Pook@fco.gov.uk>, <Joshua.Mandell@fco.gov.uk>,  
<Christian.Turner@fco.gov.uk>, <m.hulme@uea.ac.uk>, <parrym1@aol.com>,  
<Cathy.Johnson@defra.gsi.gov.uk>  
date: Fri, 12 Sep 2003 17:23:36 +0100  
from: Hans.Verolme@fco.gov.uk  
subject: WSJ: New Global-Warming Study (Science: Vinnikov et al.)  
to: <david.warrilow@defra.gsi.gov.uk>, <maria.noguer@DEFRA.GSI.GOV.UK>,  
<peter.stott@metoffice.com>, <gjjenkins@meto.gov.uk>, <p.jones@uea.ac.uk> ,  
<simon.brown@metoffice.com>

This story will likely hit your shore on the weekend.  
Vinnikov and Grody write in today's issue of Science that tropospheric temperatures have recently increased .22-26 degrees per decade. The number contradicts earlier studies and has come under instant attack. Thought you would appreciate to see a copy of the offending article.

Which side of the temperature line do we come down?

HANS <<EXCHANGE(SE).RA-ATT>>

] Hans JH Verolme, senior environment adviser  
] British Embassy, 3100 Massachusetts Avenue, NW  
] Washington, DC 20008, USA  
] tel. +1 (202) 588-6879 fax. +1 (202) 588-7915  
] mobile +1 (202) 213-8768  
] FCO FTN 8430-6879

Please note, owing to security features, the Out-of-Office function does not work to those

users on a different security tier of the system or to any external [Internet] e-mail

recipients. If you feel you have not had a reply to an e-mail addressed to me within a

reasonable length of time, please telephone to check for any extended absence greeting on

my voicemail system (always kept up to date). Apologies for any inconvenience this may cause.

Submitted on June 11, 2003

Accepted on September 3, 2003

cg2003

Global Warming Trend of Mean Tropospheric Temperature Observed by Satellites

Konstantin Y. Vinnikov<sup>1</sup>\* Norman C. Grody<sup>2</sup>

<sup>1</sup> Department of Meteorology, University of Maryland, College Park, MD 20742, USA.

<sup>2</sup> NOAA/ NESDIS, 5200 Auth Road, Camp Spring, MD 20746, USA.

\* To whom correspondence should be addressed. E-mail: kostya@atmos.umd.edu

<[1]mailto:kostya@atmos.umd.edu>.

We have analyzed the global tropospheric temperature for 1978-2002<sup>1</sup> using passive microwave

sounding data from the NOAA series of<sup>2</sup> polar orbiters and the EOS/Aqua satellite. To

accurately retrieve<sup>3</sup> the climatic trend we combined the satellite data with an analytic<sup>4</sup>

model of temperature that contains three different time scales:<sup>5</sup> a linear trend and

functions that define the seasonal and diurnal<sup>6</sup> cycles. Our analysis shows a trend of

+0.22-0.26 K/10yr, consistent<sup>7</sup> with the global warming trend derived from surface

meteorological<sup>8</sup> stations.<sup>9</sup>

Wall Street Journal: New Global-Warming Study Sets Off a Scientific Dispute

By ANTONIO REGALADO

Staff Reporter of THE WALL STREET JOURNAL

A testy scientific dispute has broken out over a new study indicating significant signs of

global warming in the Earth's lower atmosphere.

The degree of warming in the troposphere -- the region where clouds form -- is a key

battleground in the highly politicized debate over global climate change.

While past studies had found little or no warming in the troposphere, a new analysis of

satellite observations being published Friday in the journal Science calculates that

temperatures in the lower atmosphere have increased about 0.5 degree Fahrenheit per decade

since 1978.

The findings, by Konstantin Vinnikov of the University of Maryland and Norman Grody of the

National Oceanic and Atmospheric Administration, are consistent with some warming

predictions but contradict two prior analyses of the satellite readings.

Scientists involved in the earlier work said they believe the Science report has glaring

errors, and questioned its publication. "It just adds noise to the whole debate," said

Frank Wentz of Remote Sensing Systems Inc., a Santa Rosa, Calif., company that analyzes

satellite data for the government. Remote Sensing previously found about half as much

warming.

The competing findings are based on identical measurements taken by orbiting weather

satellites, which can measure heat emitted by the atmosphere. However, the instrument's

readings are difficult to interpret, because of changing orbits and gradual degradation of

the instruments over time.

The conflicting results are caused by differences in how such effects are accounted for,

said John Christy, director of Earth systems science at the University of Alabama,

Huntsville. Dr. Christy went so far as to say he believed the journal had a strong bias in

cg2003

favor of global warming.

Ginger Pinholster, a spokesperson for Science, said "the allegation of an editorial bias is baseless and without merit" but indicated the magazine intended to invite critics to submit

a technical note identifying any errors.

Dr. Christy was the first to measure tropospheric warming using satellite data in 1990. His

analyses have indicated almost no warming, a result that has been widely cited by politicians and others opposed to new environmental rules to limit emissions of greenhouse gases.

Greenhouse gases, produced by the burning of fossil fuels by automobiles, factories and

other sources, are accumulating in the atmosphere where they trap heat, like a blanket.

Models of climate behavior predict the Earth could warm by several degrees over the next

century, although the troposphere results remain a major question mark in such predictions.

Solving the troposphere riddle has emerged as a priority for the Bush administration, which

cited the issue in a recent strategic plan for U.S. climate science.

Write to Antonio Regalado at [antonio.regalado@wsj.com](mailto:antonio.regalado@wsj.com)

<[2]mailto:antonio.regalado@wsj.com>

Updated September 12, 2003

\*\*\*\*\*

For more information on the Foreign & Commonwealth Office visit:

<http://www.fco.gov.uk> For

information about the UK visit: <http://www.i-uk.com>

Please note that all messages sent and received by members of the Foreign & Commonwealth

Office and its missions overseas may be monitored centrally. This is done to ensure the integrity of the system.

\*\*\*\*\*

Attachment Converted: "c:\eudora\attach\Science Vinnikov - Grody Sept 03.pdf"

1425. 2003-09-15

---

date: Mon, 15 Sep 2003 16:19:49 +0100

from: Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>

subject: Fwd: rural/urban paper

to: [t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk), [k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk), [simon.tett@metoffice.com](mailto:simon.tett@metoffice.com),

[peter.thorne@metoffice.com](mailto:peter.thorne@metoffice.com), [chris.folland@metoffice.com](mailto:chris.folland@metoffice.com), [david.parker@metoffice.com](mailto:david.parker@metoffice.com)

<x-flowed>

Dear All,

Link below is to a paper just out in the US. Could be some press coverage - as it says

there is no difference between urban and rural stations for temperature over the US !

Interesting to see if the skeptics pick up on this. They are probably still going through the

Vinnikov/Grody paper in Science showing MSU2 warming more than the surface, so

they have a lot to look at.

I reviewed Peterson's one with Chris and couldn't see anything wrong with the main message.

Cheers

Phil

cg2003

>Date: Mon, 15 Sep 2003 10:23:46 -0400  
>From: "Thomas C Peterson" <Thomas.C.Peterson@noaa.gov>  
>Organization: NOAA/NESDIS/NCDC  
>X-Mailer: Mozilla 4.79 [en] (Windows NT 5.0; U)  
>X-Accept-Language: en  
>To: Phil Jones <p.jones@uea.ac.uk>  
>Subject: rural/urban paper  
>  
>Hi, Phil.  
>  
>I was going to send you a copy of my rural/urban paper, but I didn't get  
>a .pdf before it was published. As it is 6 megs, I'll just give you the  
>link instead:  
>  
><http://ams.allenpress.com/pdfserv/i1520-0442-016-18-2941.pdf>  
>  
>Regards,  
>  
>Tom  
Prof. Phil Jones  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
UK

</x-flowed>

4634. 2003-09-15

---

date: Mon Sep 15 10:06:07 2003  
from: Mike Hulme <m.hulme@uea.ac.uk>  
subject: CONFIDENTIAL letter to John Lawton  
to: v.mcgregor  
Vanessa,  
Please prepare this letter for me to sign. It should be marked CONFIDENTIAL on the envelope.

---

15 September 2003  
Professor John Lawton  
NERC  
Swindon  
Dear John,  
It was useful to have the brief chat with you while you were here for the Zuckerman Institute opening. John and I very much appreciated it. We have put our heads together and would like to mention the names of a few people whom we would regard as appropriate for evaluating the Tyndall Centre next year. These names are of course not formal nominations, just people who have been mentioned by us in our discussions together. This list may or may not prove useful for you.  
Chris Rapley  
Bill Clark (Bill has indicated he would be happy to chair, if invited)  
Sir Crispin Tickell  
Steve Schneider  
Brian Walker  
Jan Rotmans, ICIS Maastricht  
Jacqui McGlade, European Environment Agency  
Nebosja Nakicenovic, IIASA  
and then some other names who would also have much to offer:  
Hal Mooney  
Tony McMichael  
John Mitchell



cg2003

Chris Anastasi, British Energy  
Sir Eric Ash  
Sir John Harman  
With best wishes,  
Professor Mike Hulme  
Professor John Schellnhuber

3337. 2003-09-17

cc: "John Schellnhuber" <H.J.Schellnhuber@uea.ac.uk>, "Mike Hulme"  
<m.hulme@uea.ac.uk>, "Robert Nicholls - work" <r.nicholls@mdx.ac.uk>,  
<j.koehler@econ.cam.ac.uk>  
date: Wed, 17 Sep 2003 11:11:05 +0100  
from: "John Schellnhuber" <H.J.Schellnhuber@uea.ac.uk>  
subject: Re: OECD Climate Policy Benefits 19-20 Sept (2)  
to: <Jan.CORFEE-MORLOT@oecd.org>

Dear Jan,

Unfortunately, I cannot come to the Paris meeting, because I have to go to  
Berlin instead  
for briefing the German government on the scientific evidence relevant to the  
incipient  
international post-Kyoto negotiations - an issue also relevant to OECD, I guess.  
Sorry for this extremely short notice, but my bi-national role warrants  
surprises of this  
type once in a while.

As for your meeting, I know that Bob Nicholls will be there who certainly can  
provide also  
the more general Tyndall perspective on the pertinent scientific agenda setting  
issue.

If you think it is possible at all and if he happens to be available, I would  
suggest to

invite also Jonathan Koehler who can bring the Cambridge climate economics  
wisdom on  
board.

I am looking forward to see you on a different occasion fairly soon!

Best regards,  
John Schellnhuber

----- Original Message -----

From: [1]Jan.CORFEE-MORLOT@oecd.org  
To: [2]hjacoby@mit.edu ; [3]Roger.Jones@csiro.au ; [4]r.nicholls@mdx.ac.uk ;  
[5]h.j.schellnhuber@uea.ac.uk ; [6]jsmith@stratusconsulting.com ;  
[7]leggett.jane@epa.gov ;  
[8]michele.pittini@defra.gsi.gov.uk ; [9]Lynda.Danquah@ec.gc.ca ;  
[10]Enno.Harders@bmu.bund.de ; [11]shreekant29@yahoo.com ;  
[12]ambrosi@centre-cired.fr  
Cc: [13]Carolyn.STURGEON@oecd.org ; [14]Shardul.AGRAWALA@oecd.org ;  
[15]Tom.JONES@oecd.org  
; [16]mark.hayden@cec.eu.int ; [17]anand@som.iitb.ac.in ;  
[18]jane1family@aol.com

Sent: Wednesday, September 17, 2003 10:38 AM

Subject: RE: OECD Climate Policy Benefits 19-20 Sept (2)

<<KV\_Art2-Sept04.doc>>

Steve Schneider has asked me to forward this more recent version of the IPCC  
CCT paper  
by Patwardhan, Scheider and Semenov - this should replace the earlier version  
(sent 12  
Sept).

Best regards,

Jan

From: Stephen H Schneider [shs@stanford.edu]

Sent: Wednesday, September 17, 2003 8:53 AM

To: CORFEE-MORLOT Jan, ENV/GSP

Subject: Re: OECD Climate Policy Benefits 19-20 Sept

cg2003

Hi again Jan. I attach the latest version of the CCT I am aware of--gave it to them when I left on Sept 4. It probably hasn't been put on the website yet. Cheers, Steve

-----  
Stephen H. Schneider, Professor  
Dept. of Biological Sciences  
Stanford University  
Stanford, CA 94305-5020 U.S.A.  
Tel: (650)725-9978  
Fax: (650)725-4387  
[19]shs@stanford.edu

2216. 2003-09-22

---

date: Mon, 22 Sep 2003 17:35:09 -0700  
from: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>  
subject: raw data probably found  
to: k.briffa@uea.ac.uk  
Keith - Fenbiao tells me he may well be able to find the raw data for the 27 long western conifers chronologies used to calculate the PCs in MBH 99, etc. More soon, Malcolm

.  
.  
.Malcolm Hughes  
Professor of Dendrochronology  
Laboratory of Tree-Ring Research  
University of Arizona  
Tucson, AZ 85721  
520-621-6470  
fax 520-621-8229

5304. 2003-09-22

---

date: Mon, 22 Sep 2003 12:54:13 -0700  
from: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>  
subject: Re: western US trees data  
to: Keith Briffa <k.briffa@uea.ac.uk>  
Dear Keith - sorry to have taken so long to reply. After a somewhat disrupted summer, the beginning of the semester was made all the more messy by my computer getting seriously "wormed". I'll answer your questions as best I can.  
1) As you didn't say, I assume that the western chronologies of interest are those used in Mann et al 1999. As the paper says, the first 3 PCs of these were used in that paper. Going back to the original files, I found 27 chronologies from the western US that reached back to AD 1000 or further at the time that work was done. I attach 3 MS Excel files that give some details of them, including the ITRDB identifiers, which is what Mike used to get the chronologies. The Excel files are abstracted from the many huge files Richard Holmes built for me during that search of the ITRDB, so each will contain stuff that may be irrelevant:  
vchron11000 contains, inter alia, the ITRDB ID, species code, first year, last year, collector's name  
vchron41000 contains the ITRDB ID then the first and last years with 5, 10, etc samples  
vchron81000 contains the ID, etc and then in the following cols: V mn sensitivity W chronology autocorrelation, AE number of series, AG mean correlation of series with chronology AH mean series autocorrelation, AI scores mean length, series median segment length.  
Please remember that this set ranges from lower forest border to upper forest border, so that various mixtures from all precip to precip plus temp locally apply.  
2) I have no idea of Mike and Phil used - I assume it was the PCs of those in this subset that go back to AD1, but I wasn't involved in any way, so I don't know.  
3) As for Gordon's series - the MBH 99 paper says which one was used, but it

cg2003

never passed through my hands - Mike dealt with this.

4) I don't know what alternative standardization methods you had in mind, but you should be aware that it would be completely unjustified to assume that the first measured ring was anywhere near the pith in many of these sites, especially as you go back in time, where the chronologies are based on remnants that have weathered on the inside and the outside. For this, and related, reasons, it would also be completely unjustified to assume any constant, or small, distance in years of the first measured rings from pith. That is, I can see no way of making a remotely reliable estimate of cambial age in the vast majority of these samples. I am sitting on the bones of a manuscript in which I had someone spend several months checking many hundreds of bristlecone and similar cross-sections and cores in our store. They found only a few dozen - less than 10%, were either pith was present, or the innermost ring could reasonably be described as 'near pith'. I have another manuscript in a slightly better state of preparation where we restandardized many of these series in the following way - identify the long, flat part of the sample ringwidth curve (i.e. remove the 'grand period of growth', if present) and then fit a straight line of no or negative slope.

5) Of course, I'd be happy to collaborate - what did you have in mind?

How are you doing these days?

Cheers, Malcolm

>

> Dear Malcolm

> just sending this again in case you did not get it last time (last  
> month) cheers Keith

>

> Malcolm

> I am exploring the role of the tree-ring data in the various  
> reconstructions of Northern Hemisphere temperatures, particularly,  
> as you know, the issue of standardisation of the original data and the  
> relative influence of specific chronologies on the estimates of  
> uncertainty relating to the reconstructions. I am anxious to  
> understand the fine details of the role of the "western US trees" and  
> the "correction" applied (by comparison with Gordon's Northern tree  
> line data). Can you point me to the detailed information on what  
> constitutes the western tree-ring data, as used in Mann et al and  
> Mann and Jones (in press), and to where we can pick these up to redo  
> some of the analyses on them (ditto the Jacoby data). We wish to try  
> alternative standardisation of both sets and explore the robustness of  
> the long-term trends etc. Of course I would be happy if you would  
> collaborate with us on this. Cheers Keith

>

> --

> Professor Keith Briffa,  
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>

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>

Malcolm Hughes  
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520-621-6470  
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4433. 2003-09-23

cc: [ian.burton@ec.gc.ca](mailto:ian.burton@ec.gc.ca), [crrhcr@racsa.co.cr](mailto:crrhcr@racsa.co.cr), [tom.downing@sei.se](mailto:tom.downing@sei.se),

cg2003

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Ravi Sharma <Ravi.Sharma@unep.org>, Mohamed Hassan::, Roland Fuchs  
<rfuchs@agu.org>, Hassan Virji <hvirji@agu.org>  
date: Tue, 23 Sep 2003 11:37:46 -0400  
from: aiacc <aiacc@agu.org>  
subject: IPCC WG2 Report Outline  
to: bscholes@csir.co.za, hewitson@egs.uct.ac.za, hcenr@sudanmail.net,  
goutbi@yahoo.com, esiegfried@tellus.org, atgaye@ucad.sn, jadejuwo@oauife.edu.ng,  
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wfer@ariel.efis.ucr.ac.cr, barros@at.fcen.uba.ar, agimenez@inia.org.uy,  
cgay@servidor.unam.mx, conde@servidor.unam.mx, gunab@glaucus.fcien.edu.uy,  
rawlinsa@carec.paho.org, achen@uwimona.edu.jm, koshy\_k@usp.ac.fj, abouhadid  
<ruafah@rusys.eg.net>, adepetua@unijos.edu.ng, <nyongao@hisen.org>,  
<rolph@seychelles.sc>, <knas@iconnect.co.ke>, suppakorn@start.or.th

To: AIACC project PIS

Dear Friends,

Attached for your information is the draft outline for the contribution of  
Working Group II  
to the 4th Assessment Report of the Intergovernmental Panel on Climate Change  
(IPCC). This

draft will be presented to a plenary meeting of government delegates to the IPCC  
in

November for approval -- and may change somewhat in the process. If/when I  
receive the

draft outlines for the reports of Working Groups I and III I will forward those  
as well.

If you are interested in being an author or review editor for the IPCC 4th  
assessment

report and have not yet informed me, please do so and send me the following:

- \* A short biography (half page) that
- \* highlights your expertise and experience relevant to the IPCC reports,
- \* identifies the Working Group (I, II or III) for which you wish to be  
considered,
- \* identifies the topics/chapter(s) of the WG report to which you would like to  
contribute

\* identifies the role(s) for which you wish to be considered: Coordinating

Lead Author,

Lead Author, Contributing Author, or Review Editor

\* Your full curriculum vitae

I will forward these materials to the Technical Support Unit of the relevant  
working group.

A formal request to governments to nominate persons to be authors and review  
editors will

be made by IPCC in November. Nomination by your government is not required for  
you to be

invited to be an author. But it would increase the likelihood. To increase your  
chances of

receiving an invitation, you should contact the IPCC focal point in your country  
to inform

him/her of your interest and qualifications and to request that your government  
nominate

you.

PLEASE CIRCULATE THIS EMAIL TO OTHER MEMBERS OF YOUR AIACC PROJECT TEAM!

Kind regards,

cg2003

Neil Leary

Attachment Converted: "c:\eudora\attach\1.\_WG2\_Outline.doc"

2189. 2003-09-25

cc: c.goodess@uea.ac.uk,k.briffa@uea.ac.uk,t.osborn@uea.ac.uk  
date: Thu, 25 Sep 2003 14:13:58 +0100  
from: Phil Jones <p.jones@uea.ac.uk>  
subject: Fwd: Re: new scientist  
to: m.hulme@uea.ac.uk,h.j.schellnhuber@uea.ac.uk

Dear All,

The issue has moved on a little. The editor of NS will not accept another piece, only a letter, which Stefan Rahmstorf has drafted. I've not had a chance to look at it, but if anyone wants to join Stefan can they get in touch with him directly.

I am going to sit this one out. I am a little alarmed by Mike Mann at times, but his comments are only ever in this friendly email context.

Cheers

Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu  
X-Mailer: QUALCOMM Windows Eudora Version 5.2.1  
Date: Thu, 25 Sep 2003 08:25:28 -0400  
To: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>  
From: "Michael E. Mann" <mann@virginia.edu>  
Subject: Re: new scientist  
Cc: Gavin Schmidt <gavin@isis.giss.nasa.gov>, cindy@stopesso.com,  
André Berger <berger@astr.ucl.ac.be>,  
Phil Jones <p.jones@uea.ac.uk>, Maraun <maraun@agnld.uni-potsdam.de>,  
mann@virginia.edu

Stefan,

It looks great to me, I wouldn't change anything except perhaps, the final clause of

sentence #1:

which received a disproportionate amount of media coverage due to the unfounded claims the authors made in their press releases ("Only about a third of the warming over the past century should be attributed to man").

to

which received a disproportionate amount of media coverage due to the unfounded claims the authors made in their press releases that "Only about a third of the warming over the past century should be attributed to man".

Your point about the problems in using a regression of empirical estimates of response against forcing is an important one. The main problem here is that the authors supposed

"global temperature" estimate is nothing of the sort. I actually did some research into

this issue and here are my comments:

Veizer's estimates are almost certainly not representative of the quantity claimed by

Veizer (i.e., tropical mean sea surface temperature). Going back to Veizer's original

(1999) "Chemical Geology" paper describing the data, I found some troubling issues in

the description of the data. The data were collected from a highly irregular and

inhomogenous spatial network of locations over the modern continents. The

authors

argue, based on paleogeographic reconstructions, that "most of the data come from the tropics". That is a disturbingly poor basis on which to define a composite of the data as a supposed estimate of tropical mean SST! No account seems to have been taken for whether or not a simple mean over the available sites is likely to represent a representative areal average of the tropical oceans (it can easily be shown that a similar random sampling of site-based SST measurements from the modern instrumental data base will generally give a substantially biased estimate of the true tropical mean SST variations). Climate scientists take great pains to insure that they average a set of site measurements in such a way that a meaningful areal (e.g. tropical, Northern Hemisphere, or global) average can be computed. A tropical SST estimate based mostly on tropical Pacific instrumental data, for example, would overly emphasize SST variations related to ENSO, and give a biased picture of global tropical SST. There is no evidence in anything I've read in Veizer's papers, that care was taken to insure a meaningful spatial mean estimate of tropical SST. Equally problematic is the changing distribution of sites and data sources over time, which may considerably bias the record. Veizer himself (2000) notes, in fact, that the Neogene estimates may be overly dominated by data from the North Pacific. These are all possible reasons for why the Veizer estimates may not be reliable estimates of the quantity (tropical mean SST) claimed. This may contribute to why they do not show good agreement with other (e.g. glacial) evidence (i.e., Figure 2A vs Figure 2C) even after correcting for the Ph effects, and thus cannot be used to infer (as in Shaviv and Veizer) an estimate of the sensitivity of the global climate to CO<sub>2</sub>. In fact any estimate of sensitivity from a regression analysis will in general underestimate the sensitivity (unless the forcing and response are self-consistently estimated, as in a forced model simulation). This has to do with the fact that the uncertainties in the forcing and response are independent, and while the uncertainties in the numerator of the expression used to derive the sensitivity from the data covariances cancel, the uncertainty in the forcing series artificially increases the estimated variance in the forcing series, which increases the denominator. I discussed this issue at some length in this paper: Waple, A., Mann, M.E., Bradley, R.S., [1]Long-term Patterns of Solar Irradiance Forcing in Model Experiments and Proxy-based Surface Temperature Reconstructions, Climate Dynamics, 18, 563-578, 2002. available here: [2]<ftp://holocene.evsc.virginia.edu/pub/mann/WMB2002.pdf>

cg2003

cheers,  
mike

At 01:37 PM 9/25/2003 +0200, Stefan Rahmstorf wrote:

Hi everyone,

I'm thinking of sending the following letter to New Scientist. Please check critically what I say to make sure it stands up under fire. Your comments will be most welcome.

Stefan

---

Stott claims that the paper by Shaviv & Veizer is important science that did not get enough attention from media and policy makers. The opposite is true: it is a paper of little scientific credibility, which received a disproportionate amount of media coverage due to the unfounded claims the authors made in their press releases (Only about a third of the warming over the past century should be attributed to man). Shaviv and Veizer claim to have found a correlation between cosmic ray flux and temperature. Even if we accept their (questionable) data, it should be noted that this correlation was constructed by arbitrarily stretching the time scale to shift the maxima of cosmic ray flux by up to 20 million years, to make them coincide with temperature minima. The unadulterated data show no significant correlation (we checked this). Shaviv and Veizer then proceed to estimate the climate sensitivity to a doubling of CO<sub>2</sub> concentration through regression analysis, which for a number of reasons is not possible. If it were, far better data could be used for this analysis: the Antarctic ice core data, which are more accurate, show variations on more relevant time scales (not tens of millions of years) and closer to present CO<sub>2</sub> levels, and apply to the present-day configuration of continents. This would yield a climate sensitivity exceeding 10°C, but no climatologist has suggested this is a viable method. Climatologists agree that doubling CO<sub>2</sub> concentration would heat global climate by ~2-4°C, not because this is a hegemonic myth but simply because this conclusion is based on sound science: the known radiative properties of CO<sub>2</sub> and an understanding of the key physical feedbacks in the climate system.

--

Stefan Rahmstorf

Potsdam Institute for Climate Impact Research (PIK)

For contact details, reprints, movies & general infos see:

[3]<http://www.pik-potsdam.de/~stefan>

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137

[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Prof. Phil Jones

cg2003  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
UK

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3398. 2003-09-25

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date: Thu Sep 25 07:59:46 2003  
from: Keith Briffa <k.briffa@uea.ac.uk>  
subject: Re: your submission to THE Holocene  
to: alana@unav.es

Dear Dr Gil-Alana

I am terribly sorry , but your email prompted me to check my files and I have now only just realised that I did not communicate with you following my last message. Your file was put in the wrong drawer.

I am sorry to say that we have decided not to publish your paper - the overwhelming reason being , not a criticism of its general scientific content , but rather the relatively low relevance weighting put on it by the referees, with specific regard to this journal. After reading their reports , one of which ( ironically the one that took a long time to secure), simply emphasised that the readership would not appreciate the significance of the work .

The other referee made potentially somewhat more substantive comments and these are copied below, but the question of relevance was also to the fore. I discussed this with our main editor, John Matthews, and we agreed that we would have to concur with this opinion, particularly given the current heavy load of submissions.

Of course this decision should have been communicated to you many weeks (even months ) ago , and for this I am truly sorry. I hope you accept this apology and will feel able to

submit the manuscript elsewhere.

Yours sincerely

Keith Briffa

referee 1 comments

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Review of manuscript "A Global Warming in the Temperatures in the Northern Hemisphere Using

Fractionally Integrated Techniques",

author: L.A. Gil-Alana

This manuscript describes some interesting statistical modeling experiments with the CRU

instrumental 'Northern hemisphere mean temperature' series of 1854-1989, building on

previous work by Bloomfield and others.

The primary problem with this, and other similar past papers of this kind, however, is that

the wrong null hypothesis is assumed, creating somewhat of a 'straw man' for the argument

in favor of a long-range dependent noise process. The null hypothesis invoked is that the

observed NH mean temperature series is a realization of a stationary noise process, and



that null hypothesis is subsequently rejected in favor of a non-stationary noise process

(i.e., a fractionally-integrated noise process). The null hypothesis thusly assumed is

inappropriate however, leading to false conclusions regarding the statistical character of

the series. It is very likely that at least 50% of the low-frequency variability in the

series in question is externally forced (by volcanic, solar, and in particular in the 20th

century, anthropogenic radiative forcing). See e.g.:

Crowley, T.J., Causes of Climate Change Over the Past 1000 Years, *Science*, 289 (14 July),

270-277, 2000.

The non-stationary (ie., the 20th century trends) in the series in large part arises from

the linear response of the climate to these forcings, and much of the apparent 'non-stationarity' is simply a result of the non-stationary nature of the

forcings, not the non-stationarity of the noise term. Moreover, this associated temporal dependence structure

is almost certain to change over time, as the emerging anthropogenic forcing increases the

relative importance of the forced vs. internal (noise) component of variance. See e.g.:

Wigley, T.M.L., R.L. Smith, and B.D. Santer, Anthropogenic Influence on the Autocorrelation Structure of Hemispheric-Mean Temperatures, *Science*, 282,

1676-1680, 1998.

The appropriate null hypothesis (and a challenging one to beat, in my opinion) would be

that the observed temperature series is the sum of an externally-forced component as

modeled e.g. by Crowley (the data is available here:

<http://www.ngdc.noaa.gov/paleo/pubs/crowley.html>) plus a simple autocorrelated AR(1)

internal noise process. This is the most physically-plausible model for the observed NH

mean temperature variations, so the fractionally-integrated process must at the very least

do better (in a statistical sense) than this model...

There are a number of other minor problems:

1) No account is taken of the obvious change in variance (and presumably, the temporal dependence structure as well) back in time with increased sampling uncertainty

(and potentially, bias due to limited spatial representation in the underlying data network) in

the sparser early observations. For some purposes that isn't a problem. However, in this

study, where it is precisely the variance and temporal dependence structure of the series

that is being analyzed, I believe this is a problem.

2) It looks as if an unnecessarily outdated version of the CRU NH series has been used. A

revised, and updated version through 2001 is available online here:

The author should also reference more recent work:

[1]<http://www.cru.uea.ac.uk/cru/data/temperature/>

Jones, P.D., M. New, D.E. Parker, S. Martin, and J.G. Rigor, Surface Air Temperature and

its Changes over the Past 150 Years, *Reviews of Geophysics*, 37 (2), 173-199, 1999.

see also the additional references and information in the website indicated above.

cg2003

3) It seems to me that a number of other papers on long-range dependence in surface temperature series have been published over the past 5 years (e.g. Smith, Nychka, others), and the author needs to do a far more thorough literature review. The reviewers' review looks, on the average, to be about 5 years or so out of date... I would thus suggest that the authors resubmit the paper for consideration after appropriately dealing with the issues outlined above.

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the short /late response

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-----  
I have finally read this paper and since you are so anxious to get a quick answer my opinion is that it is not the type of paper that paleo people would understand or be much interested in. This sort of thing has been looked at before and I do not think there is much to justify publishing it here. It would be better sent to a stats journal or climate journal that publishes statistical analysis of climate series. I think journal of climate would be a good option. I do not see anything glaringly wrong but I would suggest it is not your kind of thing.

-----  
-----  
At 04:29 PM 9/24/03 +0000, you wrote:

Dear Prof. Briffa,

I am writing you in connection with a paper submitted to The Holocene Research Papers a long time ago and titled: "A global warming in the temperatures in the Northern hemisphere using fractionally integrated techniques".

On 02 May 2003 you replied to me saying that you were still waiting for the comments of the second referee.

I would be very glad if you can inform me about the progress of the paper.

Sincerely

Dr. Luis A. Gil-Alana

On Fri, 02 May 2003 10:13:02 +0100 Keith Briffa wrote:

> Dear Dr Gil-Alana

> this is a brief note to say that I am still chasing up the second referee

> regarding your paper. I am away for a week now and hope to get some

> response by the time I return. Sorry about the delay but I will try

> to get

> a reply to you soon. Keith

>

> --

> Professor Keith Briffa,

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> University of East Anglia

> Norwich, NR4 7TJ, U.K.

>

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> Fax: +44-1603-507784

>

> [2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

Este mensaje ha sido enviado con Buzón - [3][www.unav.es](http://www.unav.es)

--

cg2003

Professor Keith Briffa,  
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Fax: +44-1603-507784  
[4][http://www.cru.uea.ac.uk/cru/people/briffa\[5\]/](http://www.cru.uea.ac.uk/cru/people/briffa[5]/)

4154. 2003-09-25

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date: Thu Sep 25 15:38:04 2003  
from: Keith Briffa <k.briffa@uea.ac.uk>  
subject: Re: western US trees data  
to: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

Malcolm  
thanks for this - I simply wanted to repeat the analysis of the tree-ring data as was done by Mike (?) to get the 3 PCs of the western US trees he used in his analysis, and explore the issue of the correction by comparison with the Jacoby data. I would love to be able to discuss this stuff with you and I am very interested in your papers on these data. I don't know what if anything will come out of it - but I would not consider writing anything on it without you. May I come back to you with any problems (data identification/extraction/interpretation etc) I don't suppose you have the exact set of data Mike used? I presume he had a set of 27 chronologies as a starting point? Thanks for your help.

Keith

At 12:54 PM 9/22/03 -0700, you wrote:

Dear Keith - sorry to have taken so long to reply. After a somewhat disrupted summer, the beginning of the semester was made all the more messy by my computer getting seriously "wormed". I'll answer your questions as best I can.

1) As you didn't say, I assume that the western chronologies of interest are those used in Mann et al 1999. As the paper says, the first 3 PCs of these were used in

that paper. Going back to the original files, I found 27 chronologies from the western US that reached back to AD 1000 or further at the time that work was done. I attach 3 MS Excel files that give some details of them, including the ITRDB identifiers, which is what Mike used to get the chronologies. The Excel files are abstracted from the many huge files Richard Holmes built for me during that search of the ITRDB, so each will contain stuff that may be irrelevant:

vchron11000 contains, inter alia, the ITRDB ID, species code, first year, last year, collector's name

vchron41000 contains the ITRDB IDm then the first and last years with 5, 10, etc samples

vchron81000 contains the ID, etc and then in the following cols: v mn sensitivity w chronology autocorrelation, AE number of series, AG mean correlation of series with chronology AH mean series autocorrelation, AI

scores

mean length, series median segment length.

Please remember that this set ranges from lower forest border to upper forest border, so that various mixtures from all precip to precip plus temp locally apply.

2) I have no idea of Mike and Phil used - I assume it was the PCs of those in this

subset that go back to AD1, but I wasn't involved in any way, so I don't know.

3) As for Gordon's series - the MBH 99 paper says which one was used, but it

cg2003

never passed through my hands - Mike dealt with this.

4) I don't know what alternative standardization methods you had in mind, but you should be aware that it would be completely unjustified to assume that the first measured ring was anywhere near the pith in many of these sites, especially

as you go back in time, where the chronologies are based on remnants that have weathered on the inside and the outside. For this, and related, reasons, it would

also be completely unjustified to assume any constant, or small, distance in years of the first measured rings from pith. That is, I can see no way of making a

remotely reliable estimate of cambial age in the vast majority of these samples. I

am sitting on the bones of a manuscript in which I had someone spend several months checking many hundreds of bristlecone and similar cross-sections and cores in our store. They found only a few dozen - less than 10%, were either

pith was present, or the innermost ring could reasonably be described as 'near pith'. I

have another manuscript in a slightly better state of preparation where we restandardized many of these series in the following way - identify the

long, flat part of the sample ringwidth curve (i.e. remove the 'grand period of growth',

if present) and then fit a straight line of no or negative slope.

5) Of course, I'd be happy to collaborate - what did you have in mind?

How are you doing these days?

Cheers, Malcolm

>

> Dear Malcolm

> just sending this again in case you did not get it last time (last

> month) cheers Keith

>

> Malcolm

> I am exploring the role of the tree-ring data in the various

> reconstructions of Northern Hemisphere temperatures, particularly,

> as you know, the issue of standardisation of the original data and the

> relative influence of specific chronologies on the estimates of

> uncertainty relating to the reconstructions. I am anxious to

> understand the fine details of the role of the "western US trees" and

> the "correction" applied (by comparison with Gordon's Northern tree

> line data). Can you point me to the detailed information on what

> constitutes the western tree-ring data, as used in Mann et al and

> Mann and Jones (in press), and to where we can pick these up to redo

> some of the analyses on them (ditto the Jacoby data). We wish to try

> alternative standardisation of both sets and explore the robustness of

> the long-term trends etc. Of course I would be happy if you would

> collaborate with us on this. Cheers Keith

>

> --

> Professor Keith Briffa,

> Climatic Research Unit

> University of East Anglia

> Norwich, NR4 7TJ, U.K.

>

> Phone: +44-1603-593909

> Fax: +44-1603-507784

>

> [1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

>

Malcolm Hughes

Professor of Dendrochronology

Laboratory of Tree-Ring Research

cg2003

University of Arizona  
Tucson, AZ 85721  
520-621-6470  
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--

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[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

5104. 2003-09-25

cc: Urs Neu <[urs.neu@sanw.unibe.ch](mailto:urs.neu@sanw.unibe.ch)>, Mike MacCracken <[mmaccrac@comcast.net](mailto:mmaccrac@comcast.net)>, Martin Hoffert <[marty.hoffert@nyu.edu](mailto:marty.hoffert@nyu.edu)>, Karl Taylor <[taylor13@llnl.gov](mailto:taylor13@llnl.gov)>, Ken Caldiera <[kenc@llnl.gov](mailto:kenc@llnl.gov)>, Curt Covey <[covey1@llnl.gov](mailto:covey1@llnl.gov)>, "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>, Raymond Bradley <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, Malcolm Hughes <[mhughes@lrr.arizona.edu](mailto:mhughes@lrr.arizona.edu)>, Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, Kevin Trenberth <[trenberth@ucar.edu](mailto:trenberth@ucar.edu)>, Scott Rutherford <[srutherford@gso.uri.edu](mailto:srutherford@gso.uri.edu)>, Caspar Ammann <[ammann@ucar.edu](mailto:ammann@ucar.edu)>, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, Michael Oppenheimer <[omichael@princeton.edu](mailto:omichael@princeton.edu)>, Steve Schneider <[shs@stanford.edu](mailto:shs@stanford.edu)>, Gabi Hegerl <[hegerl@duke.edu](mailto:hegerl@duke.edu)>, Ellen Mosley-Thompson <[thompson.4@osu.edu](mailto:thompson.4@osu.edu)>, Eric Steig <[steig@ess.washington.edu](mailto:steig@ess.washington.edu)>, jmahlman@ucar.edu, wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch, Jürg Beer <[beer@hermes.emp-eaw.ch](mailto:beer@hermes.emp-eaw.ch)>, Tom Wigley <[wigley@ucar.edu](mailto:wigley@ucar.edu)>

date: Thu, 25 Sep 2003 05:39:36 -0400

from: "Thomas Crowley, Ph.D." <[tcrowley@duke.edu](mailto:tcrowley@duke.edu)>

subject: Re: anti-CO2

to: André Berger <[berger@astr.ucl.ac.be](mailto:berger@astr.ucl.ac.be)>

Quoting André Berger <[berger@astr.ucl.ac.be](mailto:berger@astr.ucl.ac.be)>:

Andre, to illustrate the absurdity of the BS approach maybe one of us can volunteer to write a critique of stellar evolution models!

tom

> Dear Colleagues,

>

> I invite you to have a look on Progress in Physical Geography 27(3), pp.  
> 448-455, 2003, a paper by Soon W. & S. Balianus on "Global warming". I  
> always thought that review paper on modeling global warming must be made by  
> climate modeling experts. Please tell me what you think about this paper  
> which I found taking a very mixed position on GCM.

>

> Best Regards,

>

> André

>

>

>

>

>

> \*\*\*\*\*

> Prof. A. BERGER

> Université catholique de Louvain

> Institut d'Astronomie et de Géophysique G. Lemaître

> 2 Chemin du Cyclotron

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> Fax +32-10-47 47 22

> E\_mail: [berger@astr.ucl.ac.be](mailto:berger@astr.ucl.ac.be)

> <http://www.astr.ucl.ac.be>

> \*\*\*\*\*

>

&gt;

1096. 2003-09-26

---

cc: "'k.briffa@uea.ac.uk'" <k.briffa@uea.ac.uk>

date: Fri, 26 Sep 2003 09:10:05 +0100

from: "Young G.M." &lt;G.M.Young@Swansea.ac.uk&gt;

subject: RE: Holocene manuscript

to: 'Phil Camill' &lt;pcamill@carleton.edu&gt;

Dear Dr Camill

Your paper on a 672-year tree-ring drought reconstruction is listed as having been rejected by Keith Briffa earlier in the year. He should have informed you of this, although it is possible that there was a lack of communication when he was ill.

Yours sincerely

John A Matthews

Editor of The Holocene

-----Original Message-----

From: Phil Camill [mailto:pcamill@carleton.edu]

Sent: 24 July 2003 04:33

To: K.Briffa@uea.ac.uk

Cc: J.A.Matthews@swansea.ac.uk; G.M.Young@Swansea.ac.uk;

dhunzicker2002@yahoo.com; pcamill@carleton.edu

Subject: Holocene manuscript

Dear Keith,

I have not yet received an editorial response or reviews for the manuscript entitled "Using a new 672-year tree-ring drought reconstruction from west-central Montana to evaluate severe drought teleconnections in the western US and possible climatic forcing by the Pacific Decadal Oscillation" by Hunzicker and Camill. This manuscript has been in review for 14 months. Can you indicate when I can expect these materials?

Many thanks,

Phil

\*\*\*\*\*

Dr. Phil Camill

Assistant Professor of Biology and Environmental Studies Carleton College,

Department of Biology One North College St. Northfield, MN 55057

phone: (507) 646-5643

fax: (507) 646-5757

\*\*\*\*\*

3659. 2003-09-26

---

date: Fri Sep 26 12:08:56 2003

from: Mike Hulme &lt;m.hulme@uea.ac.uk&gt;

subject: Re: Request to review JAE 03/116

to: jae@harcourt.com

Sorry, I shall not be able to review this manuscript.

Mike

At 11:21 26/09/2003 +0100, you wrote:

Dear Dr Mike Hulme

Manuscript Reference Number: JAE 03/116

Title: Assessment of Desertification in Central Iran (Varamin Plain) Using GIS

Authors: A Emam, G Zehtabian, S Alavipanah, M Jafari

I am writing in the hope that you or one of your colleagues will consider reviewing the above paper

for the Journal of Arid Environments.

Please contact me at the Elsevier Editorial Office by fax or email as soon as possible if

you would like to receive the complete manuscript. I would need to have the report returned to

cg2003

me within three weeks.

It is equally important that you contact me if you are unable to review this paper. We would be grateful for any alternative referee suggestions you may have, including contact details if possible.

Please note that the manuscript will be sent to you by email as a single PDF unless you specifically request to receive it by mail.

Thank you very much for your help in this matter.

Yours sincerely

Annette Cooper

Administrative Editor

(on behalf of Professor Hutchinson)

Journal of Arid Environments

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We encourage authors of JAE to submit manuscripts electronically, preferably as a single file, to the following address: JAE@harcourt.com

4321. 2003-09-26

date: Fri Sep 26 18:13:01 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: letter to PiPG

to: p.jones,new\_Mark

Phil, Mark,

For your interest, this is the letter I am sending to PiPG on Monday. Phil - which issue

of EOS was Mike Mann's article in?

Thanks,

Mike

29 September 2003

Professor B.W. Atkinson

Department of ????????????

Queen Mary College

University of London

London ??????????????????

Dear Bruce,

I am writing to resign from my position as Editorial Adviser for the journal Progress in

Physical Geography. I do this reluctantly since I believe the journal continues to fulfil

a useful and important niche in the geographical sciences I remember my relying heavily

upon the journal as an undergraduate geographer more than 20 years ago.

I reached this decision after seeing the September 2003 issue of the journal in

cg2003

which I

noticed that Willie Soon and Sallie Baliunas have been asked to provide the annual progress reports for global warming for the journal and after reading their first contribution.

This choice of authorship truly baffles me. Both authors are in a department of astrophysics. Neither author is a geographer or climatologist by training.

Neither author

has published extensively in the field of human-induced climate change. And one of the relatively few scientific peer-reviewed articles they have published in the field of

climate change - Soon, W., and S.Baliunas, Proxy climatic and environmental changes of the

past 1000 years, Climate Research, 23, 89-110, 2003 seriously questions their credentials

to provide accurate and authoritative reviews in the area of global warming (see article published a few weeks ago in the AGU weekly EOS: On past temperatures and

anomalous late-20th century warmth by Mann,M.E., Ammann,C.M., Bradley,R.S., Briffa,K.R.,

Crowley,T.J., Jones,P.D., Oppenheimer,M., Osborn,T.J., Overpeck,J.T., Rutherford,S.,

Trenberth,K.E., Wigley,T.M.L.; and also the editorial from the publisher in the journal

Climate Research by Otto Kinne Climate Research: an article unleashed worldwide storms,

vol. 24:197-198; I attach copies of these articles for your interest).

You will gather that I strongly disagree with your choice of author(s) for this annual

review. Given that my views as an Editorial Adviser to the journal presumably invited

into that capacity to cover the general area of climate change, although maybe I presume

too much were not even sought, let alone listened to, I utterly fail to see the point of

my continuing in this role or my name being associated with the journal. I would of course

be interested to hear of your selection criteria and of your process that led to these two

authors being invited to provide the global warming review for the journal.

Might I ask that you copy my letter to the member of Arnold publishing staff who is

responsible for PiPG.

Yours sincerely,

Professor Mike Hulme

1457. 2003-09-29

---

date: Mon, 29 Sep 2003 09:29:00 +0100

from: "Mark New" <mark.new@geography.oxford.ac.uk>

subject: RE: letter to PiPG

to: "'Mike Hulme'" <m.hulme@uea.ac.uk>

Mike,

Thanks. I wrote to the Editor some time ago, saying...

Mark

-----  
I was alarmed to see the 'Global Warming' review by W. Soon and S. Baliunas in the latest

issue of Progress in Physical Geography (PiPG). You may be aware that these authors have

been the subject of heated debate in the climate science community, and in the public media



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(for example, Appell 2003). The recent publication of an article in Climate Research (Soon and Baliunas 2003) where the authors claimed that 20<sup>th</sup> century warming is not the largest climate anomaly in the last ~1200 years prompted (i) the resignation of several editors from Climate Research because they felt the publication of the article had violated the peer-review process, and (ii) a strong condemnation by leading scientists in EOS (Mann et al. 2003) who were concerned that the flawed conclusions in Soon and Baliunas (2003) had entered the public record in the US Senate as peer-reviewed science. There is ample scope for criticism of the extent to which of Soon and Baliunas's review accurately and fairly 'documents the quality' of General Circulation Models (GCMs) in PiPG, and the article may well stimulate comments from experts in the field. However, I am more concerned about the wider implications of appointing scientists who have consistently received criticism for the methodology and conclusions of their peer-reviewed work (see for example, Risbey 2002; Karoly et al. 2003; Mann et al. 2003) to publish review articles that are not subject to peer review. PiPG has a wide audience, most of whom are not specialists in climate science, and therefore unable to make critical judgements about the accuracy of a review such as this. Moreover, many of your readers are likely unaware that subject updates/reviews in PiPG are not subject to peer-review. Similarly, such articles can easily be portrayed to the uninformed as being a publication in a "peer-reviewed journal", which is substantially different to the article itself being peer-reviewed. Without prior knowledge of where Soon and Baliunas sit on the Global Warming issue, their PiPG review has the potential to seriously mislead a reader about the current capabilities and limitations of GCMs: their 'review' is a catalogue of real and perceived limitations in GCMs rather than a balanced review of achievements as well as problems in GCM modelling. I have no objection to minority and non-consensus views being published: healthy debate is to be encouraged and forces those involved to think more critically about their science. However, reviews should be balanced and reflect the full range of opinions, and Soon and Baliunas's article does not satisfy these requirements. For future reviews (and this may be appropriate for all subjects), I would suggest that at the very least you include a note from the editor stating that (i) the article is not peer-reviewed, and (ii) the article reflects the opinions of the authors rather than consensus in the discipline. A more rigorous approach would be to subject these articles to the normal peer-review process.

Sincerely,  
Dr Mark New  
Climatology Research Group

cg2003

School of Geography and the Environment  
University of Oxford

References

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Scientific American 289(2): 20-22.

Karoly, D., et al. (2003). "Comment on Soon et al. (2001) 'Modeling climatic effects of anthropogenic carbon dioxide emissions: unknowns and uncertainties'." Climate Research 24: 91-92.

Mann, M., et al. (2003). "On past temperatures and late-20th century warmth." EOS 84(27): 256-258.

Risbey, J. (2002). "Comment on Soon et al. (2001) 'Modeling climatic effects of anthropogenic carbon dioxide emissions: unknowns and uncertainties'." Climate Research 22(2): 185-186.

Soon, W. and S. Baliunas (2003). "Proxy climatic and environmental changes of the past 1000 years." Climate Research 23(2): 89-110.

-----Original Message-----

From: Mike Hulme [mailto:m.hulme@uea.ac.uk]

Sent: Friday, September 26, 2003 6:13 PM

To: p.jones@uea.ac.uk; mark.new@geog.ox.ac.uk

Subject: letter to PiPG

Phil, Mark,

For your interest, this is the letter I am sending to PiPG on Monday. Phil - which

issue of EOS was Mike Mann's article in?

Thanks,

Mike

---

29 September 2003

Professor B.W. Atkinson

Department of ?????????????

Queen Mary College

University of London

London ?????????????????

Dear Bruce,

I am writing to resign from my position as Editorial Adviser for the journal Progress in

Physical Geography. I do this reluctantly since I believe the journal continues to

fulfil a useful and important niche in the geographical sciences I remember my relying

heavily upon the journal as an undergraduate geographer more than 20 years ago.

I reached this decision after seeing the September 2003 issue of the journal in which I

noticed that Willie Soon and Sallie Baliunas have been asked to provide the annual

progress reports for "global warming" for the journal and after reading their first contribution.

This choice of authorship truly baffles me. Both authors are in a department of

astrophysics. Neither author is a geographer or climatologist by training. Neither

author has published extensively in the field of human-induced climate change. And one

of the relatively few scientific peer-reviewed articles they have published in the field

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of climate change - Soon, W., and S.Baliunas, "Proxy climatic and environmental changes of the past 1000 years", Climate Research, 23, 89-110, 2003 seriously questions their credentials to provide accurate and authoritative reviews in the area of "global warming" (see article published a few weeks ago in the AGU weekly EOS: "On past temperatures and anomalous late-20th century warmth" by Mann,M.E., Ammann,C.M., Bradley,R.S., Briffa,K.R., Crowley,T.J., Jones,P.D., Oppenheimer,M., Osborn,T.J., Overpeck,J.T., Rutherford,S., Trenberth,K.E., Wigley,T.M.L.; and also the editorial from the publisher in the journal Climate Research by Otto Kinne "Climate Research: an article unleashed worldwide storms", vol. 24:197-198; I attach copies of these articles for your interest). You will gather that I strongly disagree with your choice of author(s) for this annual review. Given that my views as an Editorial Adviser to the journal presumably invited into that capacity to cover the general area of climate change, although maybe I presume too much were not even sought, let alone listened to, I utterly fail to see the point of my continuing in this role or my name being associated with the journal. I would of course be interested to hear of your selection criteria and of your process that led to these two authors being invited to provide the "global warming" review for the journal. Might I ask that you copy my letter to the member of Arnold publishing staff who is responsible for PiPG.

Yours sincerely,  
Professor Mike Hulme

2907. 2003-09-30

---

date: Tue, 30 Sep 2003 16:27:27 +0100  
from: Tony Blair <response@new.labour.org.uk>  
subject: A future fair for all  
to: "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>

Dear Colleague,  
I wanted to take this opportunity to email you some thoughts about where we are as a Party as I make my annual speech to Labour Party Conference here in Bournemouth.

I have attached my conference speech here:

<http://www.labour.org.uk/tbbournemouth> You can send any comments to me at my Labour Party email address: [tony\\_blair@new.labour.org.uk](mailto:tony_blair@new.labour.org.uk)

There is no doubt, we are in uncharted waters. No Labour Government before has journeyed so far. No Labour Government has ever been in power for six and a half years.

So, we have already achieved a great deal - government and Party together - and there's a great deal in which we can take pride.

But the Labour Party now has the historic opportunity to change the landscape of our country for good.

With our economy strong and stable, with a million and a half more people in work, we can build a prosperous and just society for all.

Our great National Health Service can become once more the envy of the world delivering high quality care to all on the basis of need, not ability to

pay.

Our schools with the investment, extra teachers and reforms in place can ensure every child gets the best start in life.

Through tough action to tackle anti-social behaviour and increased investment, local people will have the power to build strong communities. We have already lifted half a million children out of poverty but can now end the shame of childhood poverty within a generation.

But to achieve these goals - goals which, if we are honest would have been celebrated by our party at any time in its history - won't be easy.

We'll need to show determination and courage, to tackle not just the challenges of today but of the future.

Here in Bournemouth we are discussing how we make Britain fairer and what more we must do to spread prosperity and opportunity to every family and community.

But while the challenge is great so is the prize within our grasp: a better, fairer more prosperous country; a future fair for all.

Tony Blair,

Prime Minister and Leader of the Labour Party

m.hulme@uea.ac.uk

1483397

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If you have received this email in error please notify the system manager by emailing labour.people@new.labour.org.uk

The Labour Party

3821. 2003-10-02

cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, ckfolland@meto.gov.uk, peter.stott@metoffice.com, d.viner@uea.ac.uk, m.hulme@uea.ac.uk

date: Thu, 02 Oct 2003 16:11:02 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re:

to: "Robert Matthews" <r.matthews@physics.org>

Dear Mr. Matthews,

Unfortunately Phil Jones is travelling and will probably be unable to offer a separate

reply. Since your comments involve work that is his as well, I have therefore taken the

liberty of copying your inquiry and this reply to several of his British colleagues.

The comparisons made in our paper are well explained therein, and your statements belie

the clearly-stated qualifications in our conclusions with regard to separate analyses of

the Northern Hemisphere, Southern Hemisphere, and globe.

An objective reading of our manuscript would readily reveal that the comments you refer to

are scurrilous. These comments have not been made by scientists in the peer-reviewed

literature, but rather, on a website that, according to published accounts, is run by

individuals sponsored by ExxonMobile corporation, hardly an objective source of

information.

Owing to pressures on my time, I will not be able to respond to any further inquiries from

you. Given your extremely poor past record of reporting on climate change issues, however,

I will leave you with some final words. Professional journalists I am used to dealing with

do not rely upon un-peer-reviewed claims off internet sites for their sources of

cg2003

information. They rely instead on peer-reviewed scientific research, and mainstream, rather than fringe, scientific opinion.

Sincerely,

Michael E. Mann

At 08:30 PM 10/2/2003 +0100, Robert Matthews wrote:

Dear Professor Mann

I'm putting together a piece on global warming, and I'll be making reference to your

paper in Geophysical Research Letters

with Prof Jones on "Global surface temperatures over the past two millennia".

When the paper came out, some critics argued that the paper actually showed that there

have been three periods in the last 2000 years which were warmer than today (one just

prior to AD 700, one just after, and one just prior to AD 1000). They also claimed that

the paper could only conclude that current temperatures were warmer if one compared the

proxy data with other data sets. (For an example of these arguments, see:

[1]<http://www.co2science.org/journal/2003/v6n34c4.htm>)

I'd be very interested to include your rebuttals to these arguments in the piece I'm

doing. I must admit to being confused by why proxy data should be compared to instrumental data for the last part of the data-set. Shouldn't the comparison

be a

consistent one throughout ?

With many thanks for your patience with this

Robert Matthews

-----  
Robert Matthews

Science Correspondent, The Sunday Telegraph

C/o: 47 Victoria Road, Oxford, OX2 7QF

Email: [2][r.matthews@physics.org](mailto:r.matthews@physics.org)

Homepage: [3][www.ncrg.aston.ac.uk/People/](http://www.ncrg.aston.ac.uk/People/)

Tel: (+44)(0)1865 514 004 / Mob: 0790-651 9126  
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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903  
-----

e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

654. 2003-10-03

cc: Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>,  
ckfolland@meto.gov.uk, peter.stott@metoffice.com, d.viner@uea.ac.uk,  
[m.hulme@uea.ac.uk](mailto:m.hulme@uea.ac.uk)

date: Fri, 03 Oct 2003 08:56:00 -0400

from: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>

subject: Re: Mann and Jones, climate of the last two millennia

to: Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>

Tim,

Many kind thanks for going out of your way to respond to this. Colleagues have increasingly

been warning me against "taking the bait" too often (which this seems another attempt at),

and so I resisted giving the detailed response that you have nicely provided (as well as I

could have myself, I might add). They tried to bog Ben Santer down with

cg2003

distractions,

they've been trying to do the same to me, and its supposed to be a warning to the rest of

us. So the trick is to find the middle ground between responding to most egregious and

potentially damaging accusations, and not swinging at every ball they throw your way. Its

thus very helpful if friends and colleagues can take up a bit of the slack now and then, as

you have so graciously done...

This guy has written such trash before on the subject, that I assume he's out to do a

hatchet job and there is little that we can do to change that. But your response was very

helpful. It will be interesting to see what comes of this,

thanks once again,

mike

p.s. I never saw the graph in Fred Pearce's piece, since the online version didn't show it.

But it does sound problematic from what you describe.

At 9:56 AM 10/3/2003 +0100, Tim Osborn wrote:

Dear Mr. Matthews,

I have not read the criticism on the website you refer to, but will add to Mike Mann's

response in a small, but hopefully helpful, way.

Comparison of the Mann and Jones proxy-based reconstruction with instrumental temperature data \*is\* a valid comparison to make, provided that the reconstruction is

\*calibrated\* to represent the instrumental record and provided that the \*uncertainties\*

in the calibration are taken into account when making the comparison.

That is, after all, the purpose of calibration - to allow two different data sets to be compared!

As is clear from their article, Mann and Jones do undertake a careful calibration and

only make comparisons after the calibration, and their comparison figure includes their

estimated uncertainty range. Thus the conclusions they draw (regarding whether recent

warming is unprecedented) are valid and are supported by their analysis.

This does not mean that future work, perhaps using new proxy records or different

methods for calibration or for estimating calibration uncertainties, will not change

those conclusions. But it remains true that their conclusions are supported by their

analysis.

As an example of a poor comparison, see the piece by Fred Pearce on page 5 of 12 July

2003 issue of New Scientist. This is a short news article about the Mann and Jones

paper, and it unfortunately shows a comparison figure without the associated calibration

uncertainties. That is not a good comparison. I mention this in case you were thinking

of including a diagram in your article, perhaps showing the Mann and Jones results. If

you do, then it will only be valid for comparing the recent instrumental temperatures

with the proxy-based reconstruction of earlier temperatures if the reconstruction

uncertainties are included. Try to avoid the mistake that Fred Pearce made.

cg2003

Regards

Tim

At 21:11 02/10/2003, Michael E. Mann wrote:

Dear Mr. Matthews,

Unfortunately Phil Jones is travelling and will probably be unable to offer a separate reply. Since your comments involve work that is his as well, I have therefore taken the liberty of copying your inquiry and this reply to several of his British colleagues.

The comparisons made in our paper are well explained therein, and your statements belie

the clearly-stated qualifications in our conclusions with regard to separate analyses of

the Northern Hemisphere, Southern Hemisphere, and globe.

An objective reading of our manuscript would readily reveal that the comments you refer

to are scurrilous. These comments have not been made by scientists in the peer-reviewed

literature, but rather, on a website that, according to published accounts, is run by

individuals sponsored by ExxonMobile corporation, hardly an objective source of information.

Owing to pressures on my time, I will not be able to respond to any further inquiries

from you. Given your extremely poor past record of reporting on climate change issues,

however, I will leave you with some final words. Professional journalists I am used to

dealing with do not rely upon un-peer-reviewed claims off internet sites for their

sources of information. They rely instead on peer-reviewed scientific research, and

mainstream, rather than fringe, scientific opinion.

Sincerely,

Michael E. Mann

At 08:30 PM 10/2/2003 +0100, Robert Matthews wrote:

Dear Professor Mann

I'm putting together a piece on global warming, and I'll be making reference to your

paper in Geophysical Research Letters

with Prof Jones on "Global surface temperatures over the past two millennia".

When the paper came out, some critics argued that the paper actually showed that there

have been three periods in the last 2000 years which were warmer than today (one just

prior to AD 700, one just after, and one just prior to AD 1000). They also claimed that

the paper could only conclude that current temperatures were warmer if one compared the

proxy data with other data sets. (For an example of these arguments, see:

<<http://www.co2science.org/journal/2003/v6n34c4.htm>><http://www.co2science.org/journal/2003/v6n34c4.htm>)

I'd be very interested to include your rebuttals to these arguments in the piece I'm

doing. I must admit to being confused by why proxy data should be compared to instrumental data for the last part of the data-set. Shouldn't the comparison

be a consistent one throughout ?

With many thanks for your patience with this

cg2003

Robert Matthews  
Dr Timothy J Osborn  
Climatic Research Unit  
School of Environmental Sciences, University of East Anglia  
Norwich NR4 7TJ, UK  
e-mail: t.osborn@uea.ac.uk  
phone: +44 1603 592089  
fax: +44 1603 507784  
web: [1]<http://www.cru.uea.ac.uk/~timo/>  
sunclock: [2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2072. 2003-10-03

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cc: "Michael E. Mann" <mann@virginia.edu>, "Phil Jones" <p.jones@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>, <ckfolland@meto.gov.uk>, <peter.stott@metoffice.com>, <d.viner@uea.ac.uk>, <m.hulme@uea.ac.uk>

date: Fri Oct 3 10:59:16 2003

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: Re: Mann and Jones, climate of the last two millennia

to: "Robert Matthews" <r.matthews@physics.org>

At 10:35 03/10/2003, you wrote:

Many thanks for this; it's much appreciated.

Could you comment on why only the NH temperature graph shows that the current warming is unprecedented?

thanks

Robert

The number (and possibly the quality of some) of proxy records is lower in the SH than the

NH. Thus there is greater uncertainty when using those records to reconstruct past SH temperature.

Also, warming during recent decades has been greater in the NH than the SH (e.g. 1976-2000

trends quoted by the latest IPCC report are 0.6 degrees C for the NH, and 0.3 degrees C for the SH).

Combining weaker recent warming with greater uncertainty in previous temperatures means

that the SH warming cannot be concluded to be unprecedented compared with the last 2000

years or so.

Regards

Tim

2622. 2003-10-05

---

cc: mann@virginia.edu

date: Sun, 05 Oct 2003 16:19:08 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Fwd: EOS: Soon et al reply

to: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa

<k.briffa@uea.ac.uk>, tom crowley <tom@ocean.tamu.edu>, "Malcolm Hughes"

<mhughes@ltrr.arizona.edu>, omichael@princeton.edu, Tim Osborn

<t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Scott Rutherford

<srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley



cg2003

<wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>

Dear Colleagues,

Sorry to have to bother you all with this-- I know how busy our schedules are, and this

comes at an unfortunately busy time for many of us I would guss. But I think we \*do\* have

to respond, and I'm hoping that the response can be, again, something we all sign our names

to.

I've asked Ellen for further guidance on the length limits of our response, and the due

date for our response. The criticisms are remarkably weak, and easy to reply to in my view.

S&B have thus unwittingly, in my view, provided us with a further opportunity to expose

the most egregious of the myths perpetuated by the contrarians (S&B have managed to cram

them all in there) in the format of a response to their comment.

Their comment includes a statement about how the article is all based on Mann et al [1999]

which is pretty silly given what is stated in the article, and what is shown in Figure 1.

It would be appropriate to begin our response by pointing out this obvious straw man.

Then there is some nonsense about the satellite record and urban heat islands that Phil,

Kevin, and Tom W might in particular want to speak to. And Malcolm and Keith might like to

speak to the comments on the supposed problems due to non-biological tree growth effects

(which even if they were correctly described, which they aren't, have little relevance to

several of the reconstructions shown, and all of the model simulation results shown). There

is one paragraph about Mann and Jones [2003] which is right from the Idsos' "Co2 science"

website, and Phil and I and Tim Osborn and others have already spoken too. I will draft a

short comment on that.

I'd like to solicit individual comments, sentences or paragraphs, etc. from each of you on

the various points raised, and begin to assimilate this into a "response". I'll let you

know as soon as I learn from Ellen how much space we have to work with.

Sorry for the annoyance. I look forward to any contributions you can each provide towards a

collective response.

Thanks,

mike

Date: Sun, 05 Oct 2003 08:23:03 -0400

To: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley, "Malcolm Hughes"

<mhughes@ltrr.arizona.edu>,

omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck

<jto@u.arizona.edu>, Scott Rutherford <srutherford@rwu.edu>, Kevin Trenberth

<trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Fwd: EOS: Soon et al reply

Comments?

Mike

Delivered-To: mem6u@virginia.edu

Date: Sat, 04 Oct 2003 12:33:04 -0400

From: Ellen Mosley-Thompson <thompson.4@osu.edu>

cg2003

Subject: EOS: Soon et al reply

X-Sender: ethompso@pop.service.ohio-state.edu

To: "Michael E. Mann" <mann@virginia.edu>

Cc: lzirke1@agu.edu, jjacobs@agu.org

X-Mailer: QUALCOMM Windows Eudora Version 6.0.0.22

Dear Dr. Mann (and co-authors of the Forum piece that appeared in EOS),

Dr. Willie Soon and his co-authors have submitted a reply to your Forum piece that I

have accepted. Let me outline below the official AGU procedure for replies so that you

know the options available. I have sent these same instructions to Dr. Soon.

As you wrote the original piece you now have the opportunity to see their comment

(attached) on your Forum piece. You may decide whether or not to send a reply. If you

choose not to reply - their reply will be published alone.

Should you decide to reply then your response will be published along with their comment

on your paper. One little twist is that if you submit a reply, they are allowed to see

the reply, but they can't comment on it. They have two options: they can let both

their and your comments go forward and be published together or (after viewing your

reply) they also have the option of withdrawing their comment. In the latter case, then

neither their comment or your reply to the comment will be published. Yes this is a

little contorted, but these are the instructions that I received from Judy Jacobs at

AGU.

I have attached the pdf of their comment. Please let me know within the next week

whether you and your colleagues plan to prepare a reply. If so, then you would have

several weeks to do this.

I have copied Lee Zirke1 and Judy Jacobs of AGU as this paper is out of the ordinary and

I want to be sure that I am handling all this correctly.

I look forward to hearing from you regarding your decision on a reply.

Best regards,

Ellen Mosley-Thompson

EOS, Editor

cc: Judy Jacobs and Lee Zirke1

attachment

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4902. 2003-10-05

date: Sun, 05 Oct 2003 16:24:28 -0400  
 from: "Michael E. Mann" <mann@virginia.edu>  
 subject: Re: Fwd: EOS: Soon et al reply  
 to: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, tom crowley <tom@ocean.tamu.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Scott Rutherford <srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, mann@virginia.edu  
 p.s. one other point that needs to be addressed in this: the supposed inconsistency w/ boreholes. There are two issues here: one involves whether the borehole GST estimates are representative of past SAT variations, and there are now numerous peer-reviewed studies that suggest the answer is "no". So its an 'apples and oranges' issue, which is a diplomatic way of putting it w/out getting into the specific disagreements between Pollack and coworkers, and many of the rest of us (which would detract from our message). The other point is that Pollack and others have indicated that they don't believe the boreholes have sensitivity to temperature changes more than 500 years ago or so, and that these longer-term estimates that S&B refer to that supposedly show a Medieval warm period, are not trusted by even Pollack and coworkers--I believe Tom C (Tom?) has written on this at some point? look forward to comments, mike  
 At 04:19 PM 10/5/2003 -0400, Michael E. Mann wrote:  
 Dear Colleagues,  
 Sorry to have to bother you all with this-- I know how busy our schedules are, and this comes at an unfortunately busy time for many of us I would guss. But I think we \*do\* have to respond, and I'm hoping that the response can be, again, something we all sign our names to.  
 I've asked Ellen for further guidance on the length limits of our response, and the due date for our response. The criticisms are remarkably weak, and easy to reply to in my view. S&B have thus unwittingly, in my view, provided us with a further opportunity to expose the most egregious of the myths perpetuated by the contrarians (S&B have managed to cram them all in there) in the format of a response to their comment. Their comment includes a statement about how the article is all based on Mann et al [1999] which is pretty silly given what is stated in the article, and what is shown in Figure 1. It would be appropriate to begin our response by pointing out this obvious straw man.  
 Then there is some nonsense about the satellite record and urban heat islands that Phil, Kevin, and Tom W might in particular want to speak to. And Malcolm and Keith might like to speak to the comments on the supposed problems due to non-biological tree growth

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effects (which even if they were correctly described, which they aren't, have little relevance to several of the reconstructions shown, and all of the model simulation results shown). There is one paragraph about Mann and Jones [2003] which is right from the Idsos' "Co2 science" website, and Phil and I and Tim Osborn and others have already spoken too. I will draft a short comment on that. I'd like to solicit individual comments, sentences or paragraphs, etc. from each of you on the various points raised, and begin to assimilate this into a "response". I'll let you know as soon as I learn from Ellen how much space we have to work with. Sorry for the annoyance. I look forward to any contributions you can each provide towards a collective response.

Thanks,  
mike

Date: Sun, 05 Oct 2003 08:23:03 -0400

To: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Scott Rutherford <srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>  
From: "Michael E. Mann" <mann@virginia.edu>  
Subject: Fwd: EOS: Soon et al reply  
Comments?

Mike

Delivered-To: mem6u@virginia.edu

Date: Sat, 04 Oct 2003 12:33:04 -0400

From: Ellen Mosley-Thompson <thompson.4@osu.edu>

Subject: EOS: Soon et al reply

X-Sender: ethompso@pop.service.ohio-state.edu

To: "Michael E. Mann" <mann@virginia.edu>

Cc: lzirkel@agu.edu, jjacobs@agu.org

X-Mailer: QUALCOMM Windows Eudora Version 6.0.0.22

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AGU.

I have attached the pdf of their comment. Please let me know within the next week

whether you and your colleagues plan to prepare a reply. If so, then you would have

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I have copied Lee Zirkel and Judy Jacobs of AGU as this paper is out of the ordinary and

I want to be sure that I am handling all this correctly.

I look forward to hearing from you regarding your decision on a reply.

Best regards,

Ellen Mosley-Thompson

EOS, Editor

cc: Judy Jacobs and Lee Zirkel

attachment

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4295. 2003-10-07

cc: j.palutikof@uea.ac.uk, mgrc@ceh.ac.uk, "N.W.Arnell" <N.W.Arnell@soton.ac.uk>,

n.adger@uea.ac.uk, mjb7@cam.ac.uk, "Grubb, Michael" <michael.grubb@ic.ac.uk>

date: Tue, 07 Oct 2003 12:33:19 +0100

from: "Jenkins, Geoff" <geoff.jenkins@metoffice.com>

subject: RE: DEFRA stabilisation - additions?

to: "Cox, Peter" <peter.cox@metoffice.com>, Mike Hulme <m.hulme@uea.ac.uk>

MIKE

i AGREE WITH PETERS POINT. in FACT i WOULD HAVE THOUGHT A MORE NATURAL (sorry about caps lock) progressions is:

How do we define dangerous interference and hence stabilisation level?

- thresholds in the climate system and the chem/eco/bio system (ie excluding people & money) (as in Pete Cox) (including irreverable ecological impacts from your later bullett)

- climate system changes (eg x% decrease in Gulf Stream, x% change in ability of natural sinks to upake human carbon, etc) or rate of change which may not be trigger points

- chnages at regiounal level (eg loss of 50% of amazon rain forest, loss of arctic sea ice, some ikons?)

- socioeconomic thresholds or changes

- bearing in mind the committment, not just the realised (ie dangerous = committed to dangerous in the future)

Emissions pathways and policies

- what is the range of emissions pathways, including uncertainties (eg due to c cycle)
- can least cost/most practicable (in terms of technology, timescale, etc) emissions profiles be found
- what burden sharing arrangements (from your bullett one)
- the rest as is

Other headings as

Other points: can we make use of other Defra progs in India and China to say something about cultural perceptions of risk etc?

Be worth relating the whole exercise to defra objectives, eg

Impacts and risks of stabilisation pathways is on eof defras top-ten science goals

GA Objective CC1 is to consider long term objectives on stab of ghg

GA Cross cutting priority is stabilisation of ghg in the atmos

GA Science Objectives

SO1 is assess impacts and risks assoc wirth stab levels and pathways

SO2 is economoc, env & social costs and befits assoc with stab levels

etc

Also would it worth mentioning the end of the UNFCCC dangerous sentence, ie about "enabling economic development to proced in a sustainable manner" etc?

Geoff

I am OK for 1430 today Tues but not 1030 Wed

PS: I have taken alex h off the ciruculation list as presumably in defra he has to maintain a distabnce from possible bidders

> -----Original Message-----

> From: Cox, Peter

> Sent: 07 October 2003 10:40

> To: 'Mike Hulme'

> Cc: Jenkins, Geoff; j.palutikof@uea.ac.uk; mgrc@ceh.ac.uk; N.W.Arnell; n.adger@uea.ac.uk; mjg7@cam.ac.uk; Grubb, Michael; alex.haxeltine@uea.ac.uk

> Subject: RE: DEFRA stabilisation - additions?

>

> Mike,

>

> thanks for this draft. It's really nicely written and contains a lot of the relevant elements of a potential programme on stabilisation.

>

> However, I think we could be a bit more explicit about the whole issue of defining "dangerous anthropogenic interference" in the context of Earth system dynamics. This is obviously a massive issue and one we couldn't hope to tackle alone, but we certainly ought to be able to make use of the findings of other government and EU funded programmes (such as RAPID, ENSEMBLES etc.).

>

> The bullet on "Ecological, economic and social thresholds" covers some of tis ground but more in the context of climate change impacts, rather than "interference in the climate system" per se.

>

> I would therefore like to see your first bullet point as:

> - Dangerous Interference in the Climate System - Are there thresholds in the Earth System which define dangerous climate change? How probable are abrupt changes in the Earth system in the next century and beyond (e.g. via the thermohaline circulation, the carbon cycle, the Amazonian rainforest, atmospheric chemistry changes)? How do these probabilities vary with the CO2 stabilisation level? Is there an "optimum" CO2 stabilisation level which would prevent the next ice-age but would minimise the risk of dangerous interference through greenhouse warming?

>

> The last question is obviously a bit "off the wall" so its down to you whether you include it or not.....!

>

> Unfortunately, I can't make either of the suggested TELECON times, but I should be able to respond to further drafts/emails in the next few days.>

>

> All the best,

>

cg2003

> Peter Cox

>

> -----Original Message-----

> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]

> Sent: 06 October 2003 12:36

> To: Jenkins, Geoff; j.palutikof@uea.ac.uk; mgrc@ceh.ac.uk; Cox, Peter;  
N.W.Arnell; n.adger@uea.ac.uk; mjg7@cam.ac.uk; Grubb, Michael;  
alex.haxeltine@uea.ac.uk

> Subject: DEFRA stabilisation - additions?

> Importance: High

>

> Dear All,

>

> As mentioned last week, here is a draft couple of pages on the issue itself

> to be inserted into the stabilisation tender. Your comments on this would  
> be valued, especially whether the sample of topics and questions I've  
> introduced is sufficiently representative (not exhaustive of course).

>

> My take on this is that we don't have to spell out the issues in more  
> detail than this in a tender; simply to give DEFRA a sense and flavour of  
> the questions and issues we \*will\* explore at length in the study - should

> we get it.

>

> Again, I am still hoping for a tele-conference with you all, either Tuesday

> 2.30pm or Wednesday 10.30am. Please let me know which you prefer.

>

> The bid has to be finalised by Friday.

>

> Mike

>

>

>

>

>

>

> << File: DEFRA stabilisation\_scope.doc >>

2607. 2003-10-09

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cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa  
<k.briffa@uea.ac.uk>, tcrowley@duke.edu, mhughes@lrr.arizona.edu,  
omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford  
<srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley  
<wigley@ucar.edu>, mann@virginia.edu

date: Thu, 09 Oct 2003 14:16:31 -0400

from: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

subject: Re: draft

to: Tom Crowley <tcrowley@duke.edu>

HI Tom,

My understanding of the papers from the borehole community ever since the 1997  
GRL article

by Huang et al is that they no longer believe that the data has proper  
sensitivity to

variations prior to about AD 1500--in fact, I don't believe anyone in that  
community now

feels they can meaningfully go farther back than that. Huang contributed the  
section on

boreholes in chapter 2 for IPCC (2001), and wrote the very words to that  
effect...

Now, the possible influences on boreholes might lead to inferred trends in GST

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that are  
different from those in SAT is a different one. A number of independent  
recently published  
papers by (Beltrami et al; Stiglitz et al; Mann and Schmidt) and others have  
demonstrated  
that there should be expectations for significant differences between past SAT  
(what we  
care about) and GST variations (what boreholes in the best case scenario see)  
due to  
snowcover influences, etc. We don't have time to discuss that in this very short  
piece, so

I tried, as briefly as possible, to cover our bases on this issue, in a way that  
doesn't

really stir up the pot w/ the borehole folks...

I'm interested in any further thoughts on the above,

mike

At 12:38 PM 10/9/03 -0400, Tom Crowley wrote:

Hi, I don't understand why we cannot cite the borehole data for the MWP - that  
in a

sense is the only legitimate data set that shows a ~1 C cooling from the MWP  
to the LIA

- forget the deforestation problem for the moment, that is later in time -

if the borehole data for the MWP are legitimate then there is still a case for  
concluding that the MWP was significantly warmer than the LIA

tom

Thanks Phil,

a few brief responses and inquiries below...

cheers,

mike

At 04:17 PM 10/9/03 +0100, Phil Jones wrote:

Mike,

Away Oct 11-16, so here are a few comments. A few times the tone could be  
a little

less

antagonistic. We don't want to inflame things any further. So remove the word  
laundry.

fair enough. You \*should\* have seen the first draft I wrote. This is quite  
toned down

now...

1. With the boreholes do we want to get one of the borehole group to sign up,  
eg Henry

Pollack?

would add a lot of weight to the last 500 year argument.

this has merit. unfortunately though I think it might open up a hornets nest  
of the

author list is not identical to the original list of authors on the Eos  
article. Other

thoughts on this...

2. On the UHI, there was a paper in a very recent issue of J. Climate by Tom  
Peterson,

arguing

for the USA that this is non-existent. Issue with UHI is one of large versus  
local

scale. One

station doesn't influence large-scale averages. All studies which look at the  
UHI

comprehensively

find very little effect (an order of magnitude smaller than the warming).

Also the

warming

in the 20th century is very similar between the NH and SH and between the  
land and

ocean



cg2003

components.  
let me see if I can fit one or two sentences in on this and keep the article under the length.  
Also, if we can't estimate temperature histories accurately, then SB can't say it was warmer in their MWP period. They believe the 20th century instrumental data when they want to.  
yes, one of a large number of amazing contradictions in their reasoning...  
3. Keith is away till next week. I doubt we will have the space to do the 'tree issues' justice.  
Best just to say that there are an (equal) number of non tree-based proxy series??  
I do think we need to address their spurious description of the putative biological effects. Any way that you can get in touch w/ Keith for a response, perhaps just to this one point? Also, Malcolm might want to comment on the current wording?  
4. Ray, Malcolm and Henry Diaz have a Science Perspectives piece coming out in the next couple of weeks on the MWP/E. This is also relevant.  
good!  
5. Don't think we will get away with the last paragraph. Whether we want it is an issue ??  
Shouldn't we be sticking to the science.  
ok, I wasn't sure myself--yet it is a powerful rebuke, and reminds people that the objection to the validity of their work goes beyond just our article--and that's important. Does someone want to try to rephrase this paragraph, maybe reducing it to a couple sentences?  
Cheers  
Phil  
At 21:37 08/10/2003 -0400, Michael E. Mann wrote:  
Dear co-authors,  
Attached is a draft response, incorporating suggestions Kevin, Tom W, and Michael. I've aimed to be as brief as possible, but hard to go much lower than 750 words and still address all the key issues. 750 words, by the way, is our allotted limit.  
Looking forward to any comments. Feel free to send an edited version if you prefer, and I'll try to assimilate all of the suggested edits and suggestions into a single revised draft. If you can get comments to me within the next couple days, that would be very helpful as we're working on a late October deadline for the final version.  
Thanks for your continued help,  
mike

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
Prof. Phil Jones

cg2003  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
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---

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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--  
Thomas J. Crowley  
Nicholas Professor of Earth Systems Science  
Dept. of Earth and Ocean Sciences  
Nicholas School of the Environment and Earth Sciences  
Box 90227  
103 Old Chem Building Duke University  
Durham, NC 27708  
tcrowley@duke.edu  
919-681-8228  
919-684-5833 fax

---

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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[3][http://www.evsc.virginia.edu/faculty/people/mann.\[4\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[4].shtml)

2794. 2003-10-09

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cc: "Mike Hulme" <m.hulme@uea.ac.uk>  
date: Thu, 9 Oct 2003 17:29:10 +0100  
from: "Elaine Jones" <E.L.Jones@uea.ac.uk>  
subject: Re: UNEP FI Global Roundtable 2003- "Sustaining Value" 20/21  
to: <Henry.THOMAS@unep.ch>

Dear Dr Thomas,  
How very kind you are; thank you for forwarding this extra information which I most appreciate. I had requested the brochure for the October Roundtable to keep up to date with the Initiatives. I attended the CDP Launch in London and understand that they are now embarking on a second phase. Andrew is just starting a Tyndall project - a strategic assessment on how best to involve the UK investment community in decarbonisation which will include a workshop in early 2004 - and I am in regular contact with him. I wish you a most successful meeting in Tokyo and would welcome any further information which may be available in the future.

Kind regards,

Elaine

Dr. Elaine Jones  
Business Liaison Manager  
Tyndall Centre for Climate Change Research  
School of Environmental Sciences  
University of East Anglia  
Norwich, NR4 7TJ, UK  
E.L.Jones@uea.ac.uk  
Tel. 01603-593907  
fax. 01603-593901

cg2003

www.tyndall.ac.uk

----- Original Message -----

From: <Henry.THOMAS@unep.ch>

To: <E.L.Jones@uea.ac.uk>

Sent: Thursday, October 09, 2003 3:24 PM

Subject: UNEP FI Global Roundtable 2003- "Sustaining Value" 20/21 October, 2003- Tokyo, Japan

Dear Dr. Elaine Jones,

I am replying to your request for further information on the planned October Meeting on Finance and Sustainability. I understand from a colleague that he has sent you the Tokyo Roundtable Brochure, that explains all the events. I thought I would add to this by forwarding you some information on the Climate Change Sessions, as you are from the Tyndall Centre for Climate Change Research, where Andrew Dlugolecki, a member of our Climate Change Working Group, has mentioned of some cooperation with yourselves beforehand. I have attached a further look into the Climate Change sessions, if you would like anymore information on the event please do not hesitate to contact me.

(See attached file: Toyko\_ClimateChange.doc)

Best Regards,

Henry Thomas

UNEP Finance Initiatives

Economics and Trade Branch

15 Chemin des Anémones

1219 Chatelaine, Genève

Switzerland

Tel: +41 22 917 8690

Fax: +41 22 796 92 40

Henry.Thomas@UNEP.ch

<http://www.unepfi.net>

-----  
UNEP FI Global Roundtable 2003- "Sustaining Value"

20/21 October, 2003- Tokyo, Japan

For more information visit, <http://www.unepfi.net/tokyo/>  
-----

1234. 2003-10-10

---

date: Fri, 10 Oct 2003 12:43:29 +0100

from: "Neil Adger" <N.Adger@uea.ac.uk>

subject: Re: GEC

to: "Andy Jordan" <A.Jordan@uea.ac.uk>, "'Mike Hulme'" <m.hulme@uea.ac.uk>

Andrew

Please go ahead and inform Martin or the publisher (as you think appropriate) that Mike and

I are interested in co-editing the journal. This is at least a starting position and of

course would be completely dependent on the right deal from the publisher.

Please also note that Mike and I would only negotiate with the publisher over this, not

with Martin.

Let us know if you want further information etc. Note that I am away till 22nd October

after today.

Thanks.

Neil

----- Original Message -----

From: [1]Andy Jordan

To: [2]'Mike Hulme' ; [3]'Neil Adger'

Sent: Monday, October 06, 2003 3:10 PM

Subject: FW: GEC

cg2003

Hi

Things have started moving in roughly the direction that I expected: see below.  
At this stage I will simply signal to Elsevier that I want out, but if you like  
I can look

for ways of involving you in the discussion with Martin/the publisher.

Please advise.

Cheers

Andy

---

Dr Andrew J. Jordan

Lecturer in Environmental Politics;

and Editor, Environment and Planning C

School of Environmental Sciences

University of East Anglia

Norwich

NR4 7TJ

United Kingdom

Tel: (00) (44) (0)1603 592552

Fax: (00) (44) (0)1603 593739

CSERGE website: <http://www.uea.ac.uk/env/cserge/>

Personal website: <http://www.uea.ac.uk/env/faculty/jordanaj.htm>

Environment and Planning C website: <http://www.envplan.com/>

---

-----Original Message-----

From: PARRYML@aol.com [mailto:PARRYML@aol.com]

Sent: 06 October 2003 14:54

To: A.Jordan@uea.ac.uk

Subject: GEC

Dear Andrew:

See below for my action on GEC. Mary Malin has been away until today I believe.

Regards,

Martin

Dear Mary:

I know you have been away. As soon as you return can you call me on my mobile,  
about the  
matter below?

Regards,

Martin

CC: Subj: Editorial handover for Global Environmental Change

Date: 26/09/2003

To: [4]M.Malin@elsevier.com

CC: [5]A.Healey@elsevier.co.uk, [6]g.brooks@elsevier.co.uk, [7]Cynparry

Dear Mary:

I would like to explore with you a change in Editor of Global Environmental  
Change, since I

am now coming up to my 12th year.

I suggest we aim to identify a new editor, who would start handling new papers  
from Jan 04,

with the first new issue being 4/04. If more time is needed to find a suitable  
successor,

then the dates, respectively, could be April 04 and 1/05.

I understand from Andrew Jordan that the Institutions would probably effect a  
change at the

same time.

Looking ahead, the schedule would then look like this:

1. Issue 1/04; due to publishers from Parry Oct 03

2. Extra (i.e. funded additional) special issue Water: papers received from  
Guest Ed (Dr

Adeel), currently being read by Parry; to be published early 04

3. Issue 2/04; due to publishers from Parry Jan/04

4. Extra (i.e. funded additional) special issue Climate Change (paid by DEFRA);  
edited by

Parry; papers to publishers November; to be published c. Feb 04

cg2003

5. Issue 3/04: Special issue on Co-Benefits (under guest editor, responsible to Parry)

6. Issue 4/04: first issue under new editor

7. Issue 1/05: Special Issue on Adaptation.

I am away next week. But perhaps you could call me either this afternoon, or on the

morning of 6th October. Best use my mobile: 07884 317108.

With kind regards,

Martin

Dr Martin Parry,

Co-Chair Working Group II (Impacts and Adaptation),

Intergovernmental Panel on Climate Change,

Hadley Centre,

UK Met Office,

London Road,

Bracknell RG12 2SY, UK.

Tel direct: +44 1986 781437

Tel switchboard: +44 1344 856888

direct e-mail: parrym1@aol.com

e-mail for WGII Technical Support Unit: ipccwg2@metoffice.com

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direct e-mail: parrym1@aol.com

e-mail for WGII Technical Support Unit: ipccwg2@metoffice.com

1428. 2003-10-10

---

date: Fri Oct 10 09:33:33 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Fwd: Reinventing Economics Coverage?

to: a.minns@uea.ac.uk

Asher,

An interesting question. I would have thought that a few of us here - Neil, TimO, myself,

plus one or two others - would make a useful group for Vicki to meet if she came up to UEA.

What do you think?

Mike

Subject: Reinventing Economics Coverage?

Date: Thu, 9 Oct 2003 16:51:12 +0100

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: Reinventing Economics Coverage?

Thread-Index: Ac0OfSnfziiEQXdvQByJUzCS6UCzXA==

From: "Vicki Barker" <vicki.barker@bbc.co.uk>

To: "Mike Hulme" <m.hulme@uea.ac.uk>

Dear Dr. Hulme,

My colleague Roger Harrabin suggested I contact you.

I am about to spend several months attempting to answer the following question for

senior BBC managers:

If we were to reinvent economics coverage from scratch, TODAY, incorporating what we now

know (or think we know) about global environmental and economic trends... what would it

look like?

cg2003

In recent years, I have watched an environmental undertow beginning to tug at economies around the world, even as the world's peoples have been awakening to the realities of an increasingly-globalized economy; and I have wondered if current newsgathering practices and priorities are conveying these phenomena as effectively as they could be. Is this a question you and some of your colleagues feel like pondering? I'd be delighted to come out to the Tyndall Centre, either during the first two weeks of November or in early January, when I return from an extended trip abroad. The report will be delivered in March or April. I will ring your office in a day or two to see whether or when it would be convenient for us to meet. Alternatively, you can reach me at this address.

Regards,  
Vicki Barker  
BBCi at [1]<http://www.bbc.co.uk/>  
This e-mail (and any attachments) is confidential and may contain personal views which are not the views of the BBC unless specifically stated.  
If you have received it in error, please delete it from your system.  
Do not use, copy or disclose the information in any way nor act in reliance on it and notify the sender immediately. Please note that the BBC monitors e-mails sent or received.  
Further communication will signify your consent to this.

1754. 2003-10-10

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date: Fri Oct 10 14:52:43 2003  
from: Mike Hulme <m.hulme@uea.ac.uk>  
subject: Fwd: Genie: deep ocean temperature & sea-level  
to: r.warren@uea.ac.uk

For info.

Mike

X-Sender: jgs@mail.soc.soton.ac.uk  
X-Mailer: QUALCOMM Windows Eudora Version 5.1.1  
Date: Thu, 09 Oct 2003 16:58:27 +0100  
To: Mike Hulme <m.hulme@uea.ac.uk>  
From: John Shepherd <j.g.shepherd@soc.soton.ac.uk>  
Subject: Genie: deep ocean temperature & sea-level

Mike

re using the Genie model for millennial studies, deep ocean temperature & sea-level, which we discussed, please see messages below... It sounds as though the tuned version (parameters chosen statistically to get best fit to the data, which we can do because the model is so fast) is doing pretty well, as good as (or better than) most GCM's, so I think this could be a runner....

John

From: Julia Hargreaves <jules@jamstec.go.jp>  
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.1; en-US; rv:1.0.2)  
Gecko/20030208  
Netscape/7.02  
X-Accept-Language: en-us, en, ja  
To: "James D. Annan" <jdannan@jamstec.go.jp>  
Cc: Tim Lenton <tlent@ceh.ac.uk>, jgre@bgs.ac.uk, genie-science@imperial.ac.uk  
Subject: Re: [genie-science] Re: Hydrate modelling  
X-Spam-Status: No, hits=-2.6 required=5.0

cg2003  
tests=EMAIL\_ATTRIBUTION,REFERENCES,SPAM\_PHRASE\_00\_01,USER\_AGENT,  
USER\_AGENT\_MOZILLA\_UA,X\_ACCEPT\_LANG  
version=2.44  
X-Spam-Level:  
Sender: genie-science-admin@ic.ac.uk  
X-BeenThere: genie-science@ic.ac.uk  
X-Mailman-Version: 2.0.8  
List-Help: <[1]mailto:genie-science-request@ic.ac.uk?subject=help>  
List-Post: <[2]mailto:genie-science@ic.ac.uk>  
List-Subscribe:  
<https://mailman.icpc.doc.ic.ac.uk/mailman/listinfo/genie-science>,  
<[3]mailto:genie-science-request@ic.ac.uk?subject=subscribe>  
List-Id: <genie-science.ic.ac.uk>  
List-Unsubscribe:  
<[4]https://mailman.icpc.doc.ic.ac.uk/mailman/listinfo/genie-science>,  
<[5]mailto:genie-science-request@ic.ac.uk?subject=unsubscribe>  
List-Archive:  
<[6]https://mailman.icpc.doc.ic.ac.uk/mailman/private/genie-science/>  
Date: Wed, 08 Oct 2003 18:24:06 +0900  
X-MailScanner-SOC: Found to be clean  
James has spurred me on to look at the results for the other deep ocean  
basins...  
The mean of our tuned ensemble is about 1.5 degrees too warm in the  
Atlantic and Southern Oceans, about 0.5 degrees in the Pacific and 0.25  
degrees in the Indian, The 1 std spread of the ensemble is of the order  
of one degree. Errors in the upper ocean are bigger!  
jules  
James D. Annan wrote:  
>Tim Lenton wrote:  
>  
>  
>  
>>1. Deep ocean / sea floor temperatures are notoriously badly predicted  
>>by global models, tending to be systematically too warm. Ours is no  
>>exception.  
>>  
>>  
>  
>Our tuned model (ensemble) actually seems to give pretty good deep ocean  
>temperatures, at least inasmuch as we've looked at them (ie deep Pacific  
>2.5C versus Levitus data 2C). However I cannot guarantee that other  
>aspects of the model state are in any way realistic!  
>  
>James  
>  
>  


---

genie-science mailing list  
genie-science@ic.ac.uk  
[7]https://mailman.icpc.doc.ic.ac.uk/mailman/listinfo/genie-science

2728. 2003-10-10

---

cc: Keith Briffa <k.briffa@uea.ac.uk>  
date: Fri, 10 Oct 2003 07:28:43 -0400  
from: Edward Cook <drdendro@ldeo.columbia.edu>  
subject: Re: data again  
to: Jan Esper <esper@wsl.ch>  
<x-flowed>

Jan,  
Did you finally get the raw ring-width data from Malcolm? Does Keith  
know about this? He asked Malcolm for the data as well, but did not  
receive a reply as far as I know.

Ed

>Dear Malcom

>

>thank you for the series of mails and attachements! I just came back  
>into office (and I am already close to leave for another fieldtrip  
>next week), and had no time yet to look in all the files you sent  
>me. As soon as I get an overview of what you sent, I will keep you  
>informed.

>

>About the Central Asian data, I am just putting another draft  
>together also describing some of the new data Kerstin Treydte (who  
>is now in our team) sampled. Kerstin herself started working on a  
>bigger analysis including her new ring width and stable isotope data  
>(she processed 1000-yr. records of carbon and oxygen stable  
>isotopes). This will be the major paper of her PhD, and once this  
>paper is accepted, we are intending to release data to the ITRDB.  
>Will keep you posted.

>

>Thank you again and take care

>Jan

>

>

>

>

>

>>Dear Jan - did you get the e-mail I sent on September 22? It may have caused  
>>problems, because there were 10 attachemnts. In fact, I include  
>>some that were  
>>missed with this message. In addition, you should be able to get  
>>the \*.rw1 files  
>>for the 27 western chronologies used in Mann, Bradley, Hughes 1998 at the  
>>following web location:  
>>[http://www.ltrr.arizona.edu/~fenbiao/For\\_Jan\\_27rw1/](http://www.ltrr.arizona.edu/~fenbiao/For_Jan_27rw1/)  
>>Please let me know if you experience any problems with this.  
>>I also omitted some of the attachments from the earlier message. They should  
>>be attached to this one. Good luck! Malcolm

>>

>>----- Forwarded message follows -----

>>From: Malcolm Hughes <mhughes@ltrr.arizona.edu>  
>>To: esper@wsl.ch  
>>Subject: data  
>>Copies to: fenbiao@ltrr.arizona.edu  
>>Date sent: Mon, 22 Sep 2003 17:30:24 -0700

>>

>>Dear Jan - I have recently started to clear up all outstanding  
>>business related to the next analysis by Mike Mann, Ray Bradley, et  
>>al., and found, to my horror, that I had not replied to your e-mail of  
>>last April 8 (copy at end of this message). In response to our  
>>request for access to the data on which your 2000 and 2002 papers were  
>>based, you indicated that you would need to check with a colleague at  
>>WSL. Have you been able to do this, and if so, what is the result?  
>>Obviously we are keen to include all important data already in the  
>>peer reviewed literature, such as yours, in our analyses. You also  
>>requested "the raw measurements of (y)our sequoia data and the western  
>>conifer data used in the Mann et al 1998, 1999 papers". 1) data used  
>>in Mann et al 1998 - these are all listed in the Nature on-line  
>>supplementary materials (attached), and were all from the ITRDB, so  
>>they may be downloaded from there. The same list is also attached. We  
>>think we can find the raw data (the \*.rw1 files) and send them to you  
>>if you would like - please let me know. 2) The western conifer data  
>>used in MBH 99 are a subset of these, as indicated in another set of  
>>attached MS-Excel files. These are a little bit repetitive, but  
>>contain the following particularly useful information for these 27



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>>longer chronologies: vchron11000 contains, inter alia, the ITRDB ID,  
>>species code, first year, last year, collector's name  
>>  
>>vchron41000 contains the ITRDB ID, then the first and last  
>>years with 5, 10, etc samples  
>>  
>>vchron81000 contains the ID, etc and then in the following  
>>cols: V mn sensitivity W chronology autocorrelation, AE  
>>number of series, AG mean correlation of series with  
>>chronology AH mean series autocorrelation, AI series mean  
>>length, series median segment length.  
>>Please remember that this set ranges from lower forest  
>>border to upper forest border, so that various mixtures from  
>>all precip to precip plus temp locally apply.  
>>  
>>As I recently told Keith Briffa, you should be aware that it  
>>would be completely unjustified to assume that the first  
>>measured ring was anywhere near the pith in many of these  
>>sites, especially as you go back in time, where the  
>>chronologies are based on remnants that have weathered on  
>>the inside and the outside. For this, and related, reasons, it  
>>would also be completely unjustified to assume any  
>>constant, or small, distance in years of the first measured  
>>rings from pith. That is, I can see no way of making a  
>>remotely reliable estimate of cambial age in the vast  
>>majority of these samples. I am sitting on the  
>>bones of a manuscript in which I had someone spend  
>>several months checking many hundreds of bristlecone and  
>>similar cross-sections and cores in our store. They found  
>>only a few dozen - less than 10%, where either pith was  
>>present, or the innermost ring could reasonably be described  
>>as 'near pith'. If you have seen these stripbark montane 5-  
>>needle pines, and ever tried to core them, you will  
>>understand why. A further problem arises from the  
>>observation that radial increment may increase rather  
>>dramatically in the period after most of the bark dies back,  
>>but of course we don't know when that was. Andy Bunn at  
>>Montana State University has, I think, a manuscript in  
>>preparation of review on this. I have a manuscript in  
>>preparation where we restandardized many of these series  
>>in the following way -  
>>identify the long, flat part of the sample ringwidth curve  
>>(i.e. remove the 'grand period of growth', if present) and  
>>then fit a straight line of no or negative slope.  
>>3) I attach \*rw1 and chronology files from three sequoia sites (those  
>>referred to by Hughes and Brown, 1992 Drought frequency in central  
>>California since 101 B.C. recorded in giant sequoia tree rings.  
>>Climate Dynamics, 6, 161-167 ) Please note the reasons given for the  
>>rather strong standardization used (explained in text) and for the  
>>splitting of the Mountain Home samples at AD 1297 (this explains my  
>>sending you 4 of each kind of file, even though there were only three  
>>sites in this case). We do not have pith dates for these samples, but  
>>it is important to note the following caution - most of the radials  
>>and cross- sections were from stumps, where we found that very slow  
>>growth near the pith was often an indicator of great age. This of  
>>course tells us that trees destined to be very old were often  
>>suppressed for many years in their early life (but not all of them).  
>>The tricky part comes from the observation that, although we could see  
>>slow growth on the top of the stump near the pith, the wood was often  
>>in too poor a state of preservation there to date and measure.  
>>Therefore, do not assume that the first ring measured was anywhere  
>>near pith - it could easily be off by centuries. There is a \*.crn and  
>>\*.rw1 for each of the four chronologies. Gfo is Giant Forest, CSX is

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>>Camp Six, and MH is Mountain Home, split into MH1 and MH 2 as  
>>indicated above. I'd be interested to know how you get on with this.  
>>Cheers, Malcolm . .  
>> ----- Forwarded message from Jan Esper <esper@wsl.ch> -----  
>>> Date: Tue, 8 Apr 2003 16:15:35 +0200  
>>> From: Jan Esper <esper@wsl.ch>  
>>> Reply-To: Jan Esper <esper@wsl.ch>  
>>> Subject: Re: from Malcolm Hughes  
>>> To: fenbiao@ltrr.arizona.edu  
>>>  
>>> Dear Fenbiao and Malcom  
>>>  
>>> Since I got funding from the Swiss Science Foundation to do some  
>>> similar research, I really like the idea to share our tree ring  
>>> data. However, I have to discuss this again with Kerstin Treydte who  
>>> now started to work at the WSL and is running a re-analysis  
>>> (including new samplings) for western central Asia.  
>>>  
>>> In principle, would it be possible to receive the raw measurements  
>>> of your Sequoia data and the western conifer data used in the Mann  
>>> et al. 1998, 1999 papers?  
>>>  
>>> What do you think?  
>>>  
>>> Take care  
>>> Jan  
>>>  
>>> CC  
>>> K Treydte  
>>> D Frank  
>>>  
>>> >Dear Jan,  
>>> >You may be familiar with our earlier attempts at very large scale  
>>> multi-proxy  
>>> >reconstruction of certain aspects of climate, (for example, Mann,  
>>> >Bradley  
>>> and  
>>> >Hughes, 1998, Nature, 392, 779-787). This work was possible because  
>>> >many colleagues made their data available. We are now assembling an  
>>> >updated and extended dataset for new work along similar lines. We  
>>> >hope to take advantage of data that were not available five years  
>>> >ago, and to use improved methods in our analyses.  
>>> >  
>>> >would you be willing to permit us to use the  
>>> >(chronologies/reconstruction?) reported in your paper (s) listed  
>> >below?  
>>> >  
>>> >Esper J. (2000). Long-term tree-ring variations in Juniperus at the  
>>> >upper timber-line in karakorum (Pakistan). Holocene 10 (2),  
>>> >253-260.  
>>> >  
>>> >Esper J., Schweingruber F.H., Winiger M. (2002). 1300 years of  
>>> >climatic history for western central Asia inferred from tree-rings.  
>>> >Holocene 12 (3),  
>>> >267-277.  
>>> >  
>>> >We are particularly interested in (1) the ring-width series of  
>>> >Juniperus excelsa M. Bieb and Juniperus turkestanica Kom. From 6  
>>> >different sites in  
>>> the  
>>> >Hunza-karakorum;  
>>> >(2) 20 individual sites ranging from the lower to upper local  
>>> >timber-lines

cg2003

```
>>> in
>>> >the Northwest karakorum of Pakistan and the Southern Tien Shan of
>>> Kirghizia.
>>> >
>>> >If at all possible, we would prefer to receive tree-ring data as
>>> >both raw
>>> data
>>> >(individual unmodified measurement series for all samples used) and
>>> >your
>>> final
>>> >chronologies used in the publication.
>>> >
>>> >If you are willing to share your data for the purposes of our
>>> >analyses, but
>>> do
>>> >not
>>> >wish them to be passed on to anyone else by us, please tell us, and
>>> >we will mark the data accordingly in our database. If data have
>>> >been marked as not being publicly available, we will pass on any
>>> >requests for them to you.
>>> >
>>> >Please reply to Dr. Fenbiao Ni's email address (this one). Many
>>> >thanks.
>>> >
>>> >Sincerely,
>>> >Malcolm K. Hughes
>>> >(team: Michael E. Mann, Ray Bradley, Malcolm Hughes, Scott
>>> >Rutherford,
>>> >Fenbiao
>>> >Ni)
>>> >
>>> >Malcolm Hughes
>>> >Professor of Dendrochronology
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>>Attachments:

>> D:\Projects\Bradley and Mann\Newest June 9 1997\westernforjan.xls  
>> D:\Projects\Bradley and Mann\Nature figures\naturesupmat.doc  
>> D:\Projects\SEQUOIA\for esper\csx.rwl D:\Projects\SEQUOIA\for  
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>> esper\MHF1ARS.CRN

>>----- End of forwarded message -----Malcolm

>>Hughes

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=====

</x-flowed>

3955. 2003-10-10

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date: Fri, 10 Oct 2003 12:16:50 +0100  
from: "Stephanie Ferguson" <stephanie.ferguson@ukcip.org.uk>  
subject: UKCIP news update  
to: "Stephanie Ferguson" <stephanie.ferguson@ukcip.org.uk>

Dear Colleagues

1. UKCIP risk training workshops - last chance to register!
2. UKCIP up and running with climateprediction.net
3. Paull Holme Strays Flood Defence Scheme - officially open
4. Rail Safety & Standards Board publish new report: Safety Implications of weather, Climate and Climate Change
5. Nature's Calendar - autumn 2002 data
6. Managing Risk and Liability in a Changing Climate
7. Other conferences
8. Yorkshire and Humber Region to appoint Regional Climate Change Co-ordinator
9. CEH-Wallingford to appoint Catchment Systems Modeller (including climate change)
10. UKCIP staff changes

1. UKCIP risk training workshops - last chance to register!

There are still some places left on the UKCIP risk training workshops in November and December (details below). UKCIP and the Environment Agency's

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Environmental Policy Risk and Forecasting team are running the free training workshops to demonstrate the application of the UKCIP risk framework (see [1][www.ukcip.org.uk/risk\\_uncert/risk\\_uncert.html](http://www.ukcip.org.uk/risk_uncert/risk_uncert.html)) and train attendees in its use. Places are limited, so email UKCIP now to avoid disappointment!

Built environment, 13 November 2003

How can a 1960s office building be modified to provide a comfortable internal environment over the next 20 years, while minimising energy use?

Town and Country Planning Association, 17 Carlton House Terrace, London  
Water resources, 19 November 2003

How should Silver Birches plc (a tree-growing business) adjust its long-term business strategy to better manage its climate change and water supply risks over the next 20 years?

Severn Trent Water, 2297 Coventry Road, Birmingham

Biodiversity, 27 November 2003

How should a National Nature Reserve Management Plan be revised to take account of climate change?

Scottish Executive, Victoria Quay, Edinburgh, EH6 6QQ.

Local authority plans, 4 December 2003

The Local Plan for Council A is due for review and one of those responsible wants to ensure that when it's revised, it is adapted to take account of climate change impacts. How should this be done? (This workshop will make use of an existing local plan in an area that includes flood risks.)

Sustainability North West, Giant's Basin, Potato Wharf, Manchester M15 4AY

2. UKCIP up and running with [climateprediction.net](http://climateprediction.net)

As we reported in the last e-news, [climateprediction.net](http://climateprediction.net) was launched in September to allow a state-of-the-art climate prediction model to be run as a background process on home, school and work computers, generating the world's largest climate prediction experiment. There are currently almost 37,000 registered users (over 11,000 in the UK) and we're pleased to report that all UKCIP computers are now participating and we've registered as group.

When the programme is running on your computer, you can monitor progress and there's a dinky visualisation of 'your' climate model to view. Once registered, you can also pander to your competitive instinct and view your personal and group ranking (UKCIP is 34th).

3. Paull Holme Strays Flood Defence Scheme - officially open

Elliot Morley MP officially opened the Environment Agency's innovative Paull Holme Strays Flood Defence Scheme on 9 October. The project uses managed realignment to strengthen the flood protection while creating 80 hectares of new inter-tidal habitat, therefore also meeting the requirements of the European Habitats Directive. Work on the project began in September 2001 and two years later the existing defences were breached in two places to allow the sea in. Thus the process of returning the land to its original habitat (mudflat and saltmarsh) began. The intention is to allow the site to develop naturally.

Visit the Environment Agency website for more information.

4. Rail Safety & Standards Board publish new report: Safety Implications of weather,

Climate and Climate Change

A new report commissioned by the Rail Safety and Standards Board from AEA Technology is now available on the RSSB website. The report makes use of the UKCIP climate change scenarios, to develop risk scenarios for the railway system. It outlines the relationship between weather and railway safety, for instance, the number of signals passed at danger (SPADS) due to weather events. It also identifies future research needs and proposes adaptation actions for the industry to address climate change risks.

5. Nature's Calendar - autumn 2002 data

The latest issue of Nature's Calendar News, published by the Woodland Trust, the Centre for Ecology & Hydrology and the UK Phenology Network, gives an analysis of events of autumn 2002. Thousands of volunteer recorders have monitored events such as bird migrations, leaf fall and fruit ripening, and their observations show that almost all these events occurred earlier in 2002 than in 2001. This seems to fly in the face of the usual message that

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autumn is getting later, but the picture is complex and factors such as low rainfall in September 2002 may play a part. It will be interesting to see how the data for 2003 compares!

For more information, visit the UK Phenology Network website.

#### 6. Managing Risk and Liability in a Changing Climate

Climate Change Management/Newzeeye

3 December 2003, Royal Geographical Society, London, UK

UKCIP Director, Chris West will be addressing this conference, along with speakers from the Carbon Trust, the Greater London Assembly and academics from the Oxford University, Middlesex University and University College London. Topics to be covered include: flood management, implications for planning and regeneration, climate change and the construction industry, climatic monitoring and prediction, climate change - obligations and liabilities and transport trends and policies.

For further details visit [2][www.climatecm.com/conferences](http://www.climatecm.com/conferences) or contact Selena Hannagan, tel + 44 (0)20 8969 1008 or email [3][selenahannagan@newzeeye.com](mailto:selenahannagan@newzeeye.com).

#### 7. Other conferences

Delivering climate technology - programmes, policies and politics

Royal Institute of International Affairs/Carbon Trust

4-5 November 2003, Chatham House, London, UK

Sessions include: technology strategies for a carbon-constrained world, de-carbonising utilities, fossil fuel transitions, delivering climate technology - the next phase.

For full details visit the conference website.

Climate Change: What needs to be done in North and South?

17-20 November 2003, Wilton Park, West Sussex, UK

What next for the Kyoto process? Can the US and major greenhouse gas emitters among transition and developing nations be drawn in? What role for alternative energy? How can we help entire societies soften the impacts through adaptation strategies?

For more information, visit the Wilton Park website.

#### 8. Yorkshire and Humber Region to appoint Regional Climate Change

Co-ordinator

The Government Office for Yorkshire and Humber region will shortly be advertising for a part-time Regional Climate Change Co-ordinator to carry forward the region's climate change agenda.

The position is a two-year fixed term, part-time post (18.5 hours per week).

The salary band for the position is Grade 7 (£35-£47k per annum, pro rata) and the position is based in the Government Office for Yorkshire & Humber in central Leeds.

For further details on this position or to discuss the post, please contact Les Saunders - phone 0113 283 5372 or email

[4][LJSAUNDERS.GOYH@go-regions.gsi.gov.uk](mailto:LJSAUNDERS.GOYH@go-regions.gsi.gov.uk)

#### 9. CEH-Wallingford to appoint Catchment Systems Modeller (including climate change)

The Risk Analysis and Trends Section within the Hydrological Risks & Resources Division at CEH-Wallingford are looking for a Catchment Systems Modeller to undertake research covering a number of different aspects of hydrological modelling, including climate change. Applicants should have a good honours degree and a relevant post-graduate qualification or at least three years' experience in research. Further details available from the Personnel Section, CEH Wallingford, Maclean Building, Crowmarsh Gifford, Wallingford Oxon OX10 8BB. Tel 01491 838800, email [5][wlreception@ceh.ac.uk](mailto:wlreception@ceh.ac.uk), quoting reference 10/03. Closing date - Friday 31st October 2003.

#### 10. UKCIP staff changes

Later this month we say goodbye to Sally Jeffery, who has been UKCIP's Administrator since May 2002. Sally has been at the heart of creating the efficient structures that keep the UKCIP office running smoothly. Everyone at UKCIP would like to say a big 'thank you' for her contribution to our progress and to wish her all the best for the future.

Best wishes

Chris West

Director, UKCIP

Based at the University of Oxford and funded by DEFRA, UKCIP helps organisations assess how they will be affected by climate change, so they can prepare for its impacts.

---

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965. 2003-10-13

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cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk, ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov  
 date: Mon, 13 Oct 2003 14:21:14 -0400  
 from: "Michael E. Mann" <mann@virginia.edu>  
 subject: Re: draft

to: Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>

Dear Keith,

Thanks a bunch for your comments, all of which are very helpful. I've attempted to incorporate these, along w/ those of Tim, Tom W, in the latest draft, as per my previous email.

There is one point I wanted to comment on further, regarding the issue the how a potentially non-stationary time series (e.g. one with a significant trend near the end)

should or should not be smoothed. The issue, happily, is not relevant to our Eos reply,

because the proxy reconstruction (which ends in 1980) was smoothed based on a procedure

that does not assume a continuation of the trend, the issue that seems controversial here.

However, I do think that this is particularly important in smoothing of the instrumental

surface temperature series. Those uninterested in this particular discussion need not read

any further, but I would encourage those interested (particular as it might involve

decisions about how to smooth time series in the next IPCC report) to read on... mike

comment on "minimum roughness" constraint in smoothing time series with significant trends

near the end of the series:

I favor a "minimum roughness" constraint (which tends to retain trends near the end of a

smooth) for smoothing of a series with a significant trend near (either) end for the

following reason. Smoothing of a time series with a zero phase (i.e., centered) filter

reflects a non-unique transformation of the data. It is non-unique because there is no information on one half of the filter center at either the beginning or end of the series.

Because of that lack of information, an additional a priori constraint has to be placed on

the filtering process. This constraint reflects an assumption about the data outside of the

available interval. This can be cast (as often it is in the signal processing literature) as an inverse problem w/ non--unique constraints. The typical constraints that are typically employed [see Park, J., Envelope estimation for quasi-periodic geophysical signals in noise: A multitaper approach, in Statistics in the Environmental and Earth Sciences, edited by A. T. Walden and P. Guttorp, pp. 189-219, Edward Arnold, London, 1992.] involve a (i) "minimum norm", (ii) "minimum slope", and (iii) "minimum roughness" solution for the underlying statistical model. A possible way of insuring a reasonably objective smoothing of the time series is to minimize, among all possible linear combinations of these 3 models, that which minimizes the mean-square-misfit with respect to the raw data [see Ghil, M., Allen, M.R., Dettinger, M.D., Ide, K., Kondrashov, D., Mann, M.E., Robertson, A.W., Tian, Y., Varadi, F., Yiou, P., Advanced Spectral Methods for Climatic Time Series, Reviews of Geophysics, 40 (1), 1003, doi: 10.1029/2000RG000092, 2002.]

Such an approach will favor constraints (i) or (ii) for a data series with stationary behavior in the mean near the end, and probably (iii) if there is non-stationary behavior (i.e. a long-term trend or, more specifically, a statistically significant trend over the last 1/2 smoothing filter window width).

(i) is called the "minimum norm" constraint because it chooses the smallest of all models for the smoothed data--it involves the minimization of the 0th derivative of the smooth. The implicit assumption is that the mean outside the available interval is equal to the mean of the available data. This is clearly wrong if there is a significant trend.

(ii) is called a "minimum slope" inversion. In this case, the solution involves the minimization of the mean-square first derivative of the model--the solution will favor a smooth that approaches the boundary with zero slope--the implicit assumption is that the mean outside the available interval may be different from the mean inside the available data, but that this reflects a step change in the mean value rather than any trend near the boundary. Ad hoc methods which pad the end of the series with e.g. the mean of the last 1/2 filter width, in essence, implement this constraint.

(iii) is called the "minimum roughness" solution because it minimizes the mean-square 2nd derivative among all possible models. It favors a smooth with an inflection point at the boundary. and is consistent with the assumption that a trend exists as one approaches the boundary. Mathematically, it is most simply implemented in the time domain by padding the series with an extension of the trend over the past 1/2 filter width. However, the constraint can be implemented directly in the frequency-domain inversion [Park,



1992; Ghil  
et al, 2002].

This is the proper choice if there is a statistically significant trend within the final

1/2 smoothing window width of the edge of the data. Objectively, it is defensible in those

situations where this choice minimizes the mean-square misfit with the raw data over all

possible linear combinations of choices (i), (ii), and (iii). For the global or hemispheric

mean instrumental series from 1856-present, that condition holds.

So my concern is the opposite of Keith's. I believe that smoothing routines that explicitly

invoke an assumption of stationarity are problematic when the series clearly is not

stationary.

This assertion has a rigorous foundation in the inverse theory literature [see Park, 1992

and other references therein] and is not a new or subjective approach.

Though the point is actually irrelevant to the discussion at hand for reasons mentioned

earlier, I would actually like to see this discussed, because I think that we (e.g. in IPCC

'01) may have underplayed the significance of recent warming by employing improper boundary

conditions on smooths of records like the global temperature or hemispheric temperature

series. I've cc'd Chris Folland and Tom Karl in on this discussion, for their comments...

Mike and all

Hi , just back from a trip and only now catching up with important emails.

Given

the restricted time and space available to furnish a response to SB comments , I offer the following mix of comment and specific wording changes:

I agree that the S+B response is designed to deflect criticism by confusing the issues

rather than answering our points.

In fact they fail to address any of the 3 specific

issues we raised Namely , 1. the need for critical evaluation of proxy inputs , 2. the

need for a consistent assimilation of widespread (dated and well resolved ) records,

3. the essential requirement for objective/quantitative calibration (scaling) of the

input

records to allow for assessment of the uncertainties when making comparisons of different reconstructions and when comparing early with recent temperatures.

Their own , ill-conceived and largely subjective approach did not take account of the uncertainties and problems in the use of palaeodata that they chose to

highlight in their opening remarks.

I would be in favour of stating something to this effect at the outset of our response.

Also , as regards the tree-ring bit , I fully concur with the sense of your text as

regards Section 1, but suggest the following wording (to replace ",rarely for annual

ring widths, and almost entirely at higher latitudes.")

"but in certain high-latitude regions only. Where this is the case , these relatively

recent

(ie post 1950) data are not used in calibrating temperature reconstructions.

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(even high-latitude) areas density or ring-width records display no bias."

In the spirit of healthy debate - I agree with Tim's remarks , warning against presenting a too

sanguine impression that the borehole debate is closed ( though I do think it

is

closing!).

I also believe , as you already know, that the use of a recent padding

algorithm to

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smoothed data to the present time, is inappropriate if it assumes the

continuation of a

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trend. This is likely to confuse , rather than inform, the wider public about

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current climate state .

Finally , I repeat my earlier remarks (made before EOS piece published) that

we are

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an opportunity to say that a warm Medieval period per se is not a refutation

of

anthropogenic

warming , {as its absence is no proof}, if we do not understand the role of

specific

forcings (natural

and anthropogenic) that influenced medieval and current climates.

Cheers

Keith

At 12:48 PM 10/9/03 -0600, Kevin Trenberth wrote:

Hi all

Here are my suggested changes: toned down in several places. Tracking turned

on

Kevin

Michael E. Mann wrote:

Dear co-authors,

Attached is a draft response, incorporating suggestions Kevin, Tom W, and

Michael. I've

aimed to be as brief as possible, but hard to go much lower than 750 words and

still

address all the key issues. 750 words, by the way, is our allotted limit.

Looking forward to any comments. Feel free to send an edited version if you

prefer, and

I'll try to assimilate all of the suggested edits and suggestions into a

single revised

draft. If you can get comments to me within the next couple days, that would

be very

helpful as we're working on a late October deadline for the final version.

Thanks for your continued help,

mike

---

Professor Michael E. Mann

Department of Environmental Sciences, Clark Hall

University of Virginia

Charlottesville, VA 22903

---

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cg2003

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[6]http://www.cru.uea.ac.uk/cru/people/briffa/

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1796. 2003-10-13

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mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk,  
jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley  
<wigley@ucar.edu>, p.jones@uea.ac.uk, mann@virginia.edu  
date: Mon, 13 Oct 2003 22:36:48 -0400  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Re: draft  
to: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>,  
Kevin Trenberth <trenbert@cgd.ucar.edu>

Dear All,

I plan to send out another revised version tomorrow. A few brief comments:

1) Re boreholes, my apologies. The statement on why the borehole estimates are confined to the past 500 years was not made in Huang et al (2000), but instead the earlier paper by Pollack et al (1998): "The combination of the predominant depth range of observations and the characteristic magnitude of noise has led us to choose five centuries as the practical interval over which to develop climate reconstructions". Will substitute in the correct reference, but keep the wording the same...

2) Regarding the final paragraph, I've shortened the discussion (e.g. removed the bit about the publisher) so as to downplay this, but haven't eliminated it all together. We can have an up or down vote on whether or not to keep it once we finalize the draft...

3) Malcolm's revision of the first paragraph would be great if we had 900 words, but we don't. The limit is 750, and they've indicated that they will be strict about this. So I'll try to reduce Malcolm's additions to one or two summarizing sentences.

More tomorrow. Thanks,  
mike

At 06:58 PM 10/13/2003 -0700, Malcolm Hughes wrote:  
<?xml version="1.0" ?>  
Dear Mike and all,

cg2003

Please find attached some small edits that I propose (MS-Word \*.doc file with track changes turned on). The first change you will find is designed to incorporate and give strong emphasis to Keith's very important point about keeping the focus on the inadequacies of the SB approach, and their failure to deal with our specific criticisms. This is far and away the most important point to make. The second main change I suggest concerns the words about boreholes. A careful reading for the papers they referred to simply does not justify the wording in Mike's most recent draft, since Huang, Pollack etc. never say exactly why they stop at 500 years rather than 400 or 1000. It is, however, the case that the Huang et al 1997 paper to which they refer has a multi-century hump about a thousand years ago, but it can't be compared with a period of 25 or 30 years. I don't see much point in the Folland et al citation, because it is not a primary source. Finally, I am in two minds about the last paragraph of Mike's draft. On the one hand, I understand the need to get the word out about the disquiet many feel about the circumstances surrounding publication of the SB et al papers, but I also suspect that our scientific arguments alone are more than enough to undermine their position. We may, in fact, be seen by many colleagues as making an ad hominem attack, and so arouse the suspicion that our scientific case is not strong enough to stand on its own. I suggest we think carefully before proceeding with the last paragraph as it stands.

Cheers, Malcolm

>

> Dear All,

>

> Thanks for all your comments, which are very helpful. I've done my best to address these within the pretty tight constraints (750 words) allotted. We come in now at 746 words, just inside the strict 750 word limit that has been imposed on us.

>

> We have 6 references now--I've asked folks at AGU if that's ok.

>

> If people have any final comments on the draft, please let me know ASAP. For those who haven't yet responded yet (Malcolm, Ray, Caspar, Scott, Peck), if you're happy w/ it as it currently stands, a simple "looks good as is now, sign my name to it too" would be great. I don't want to sign anyones name to this w/ out some indication of approval. I realize some of you are still travelling and have been unable to respond. I've asked AGU if we can have at least one more week before submitting...

>

> Thanks again for your continued help,

>

> mike

>

> At 04:36 PM 10/13/2003 +0100, Keith Briffa wrote:

>

> Mike and all

> Hi , just back from a trip and only now catching up with important emails. Given the restricted time and space available to furnish a response to SB comments , I offer the following mix of comment and

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>  
> Cheers  
> Keith

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>  
> Thanks for your continued help,  
>  
> mike

>  
>  
\_\_\_\_\_  
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> University of Virginia  
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>  
> \*\*\*\*\*  
> Kevin E. Trenberthe-mail:  
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> Climate Analysis Section,  
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> Boulder, CO 80307(303) 497 1333 (fax)  
>  
> Street address: 1850 Table Mesa Drive, Boulder, CO 80303

>  
--  
>  
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> University of East Anglia  
> Norwich, NR4 7TJ, U.K.  
>  
> Phone: +44-1603-593909  
> Fax: +44-1603-507784  
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> [6]http://www.cru.uea.ac.uk/cru/people/briffa/

>  
\_\_\_\_\_  
>  
> Professor Michael E. Mann  
> Department of Environmental Sciences, Clark Hall  
> University of Virginia  
> Charlottesville, VA 22903  
>  
\_\_\_\_\_  
> \_e-mail: mann@virginia.edu Phone: (434) 924-7770FAX: (434) 982-2137  
> [7]http://www.evsc.virginia.edu/faculty/people/mann.shtml

- -  
Malcolm K. Hughes  
Professor of Dendrochronology  
Laboratory of Tree-Ring Research  
W. Stadium 105  
University of Arizona  
Tucson, AZ 85721  
USA  
e-mail: mhughes@ltrr.arizona.edu  
telephone: 520-621-6470  
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cg2003  
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[9]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1965. 2003-10-13

---

date: Mon, 13 Oct 2003 14:24:07 +0100  
from: "Andy Jordan" <A.Jordan@uea.ac.uk>  
subject: RE: GEC  
to: "'Neil Adger'" <N.Adger@uea.ac.uk>, "'Mike Hulme'" <m.hulme@uea.ac.uk>

Hi  
I have set the wheels in motion by emailing (IN CONFIDENCE) Mary Malin at Elsevier.  
Hopefully she will get back to me soon as she is currently OOTO.  
I absolutely agree: you need to negotiate direct with Mary rather than via an intermediary.

Cheers  
Andy  
Cc MH

---

Dr Andrew J. Jordan  
Lecturer in Environmental Politics;  
and Editor, Environment and Planning C  
School of Environmental Sciences  
University of East Anglia  
Norwich  
NR4 7TJ  
United Kingdom  
Tel: (00) (44) (0)1603 592552  
Fax: (00) (44) (0)1603 593739  
CSERGE website: <http://www.uea.ac.uk/env/cserge/>  
Personal website: <http://www.uea.ac.uk/env/faculty/jordanaj.htm>  
Environment and Planning C website: <http://www.envplan.com/>

---

-----Original Message-----

From: Neil Adger [mailto:N.Adger@uea.ac.uk]  
Sent: 10 October 2003 12:43  
To: Andy Jordan; 'Mike Hulme'  
Subject: Re: GEC

Andrew

Please go ahead and inform Martin or the publisher (as you think appropriate) that Mike and

I are interested in co-editing the journal. This is at least a starting position and of

course would be completely dependent on the right deal from the publisher.

Please also note that Mike and I would only negotiate with the publisher over this, not

with Martin.

Let us know if you want further information etc. Note that I am away till 22nd October

after today.

Thanks.

Neil

----- Original Message -----

From: [1]Andy Jordan  
To: [2]'Mike Hulme' ; [3]'Neil Adger'  
Sent: Monday, October 06, 2003 3:10 PM  
Subject: FW: GEC

Hi

cg2003

Things have started moving in roughly the direction that I expected: see below.

At this stage I will simply signal to Elsevier that I want out, but if you like I can look for ways of involving you in the discussion with Martin/the publisher. Please advise.

Cheers

Andy

---

Dr Andrew J. Jordan  
Lecturer in Environmental Politics;  
and Editor, Environment and Planning C  
School of Environmental Sciences  
University of East Anglia  
Norwich  
NR4 7TJ  
United Kingdom  
Tel: (00) (44) (0)1603 592552  
Fax: (00) (44) (0)1603 593739  
CSERGE website: <http://www.uea.ac.uk/env/cserge/>  
Personal website: <http://www.uea.ac.uk/env/faculty/jordanaj.htm>  
Environment and Planning C website: <http://www.envplan.com/>

---

-----Original Message-----

From: PARRYML@aol.com [mailto:PARRYML@aol.com]

Sent: 06 October 2003 14:54

To: A.Jordan@uea.ac.uk

Subject: GEC

Dear Andrew:

See below for my action on GEC. Mary Malin has been away until today I believe.

Regards,

Martin

Dear Mary:

I know you have been away. As soon as you return can you call me on my mobile, about the matter below?

Regards,

Martin

CC: Subj: Editorial handover for Global Environmental Change

Date: 26/09/2003

To: [4]M.Malin@elsevier.com

CC: [5]A.Healey@elsevier.co.uk, [6]g.brooks@elsevier.co.uk, [7]Cynparry

Dear Mary:

I would like to explore with you a change in Editor of Global Environmental Change,

since I am now coming up to my 12th year.

I suggest we aim to identify a new editor, who would start handling new papers from Jan

04, with the first new issue being 4/04. If more time is needed to find a suitable

successor, then the dates, respectively, could be April 04 and 1/05.

I understand from Andrew Jordan that the Institutions would probably effect a change at the same time.

Looking ahead, the schedule would then look like this:

1. Issue 1/04; due to publishers from Parry Oct 03

2. Extra (i.e. funded additional) special issue Water: papers received from Guest Ed

(Dr Adeel), currently being read by Parry; to be published early 04

3. Issue 2/04; due to publishers from Parry Jan/04

4. Extra (i.e. funded additional) special issue Climate Change (paid by DEFRA); edited



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by Parry; papers to publishers November; to be published c. Feb 04

5. Issue 3/04: Special issue on Co-Benefits (under guest editor, responsible to Parry)

6. Issue 4/04: first issue under new editor

7. Issue 1/05: Special Issue on Adaptation.

I am away next week. But perhaps you could call me either this afternoon, or on the

morning of 6th October. Best use my mobile: 07884 317108.

with kind regards,

Martin

Dr Martin Parry,

Co-Chair Working Group II (Impacts and Adaptation),

Intergovernmental Panel on Climate Change,

Hadley Centre,

UK Met Office,

London Road,

Bracknell RG12 2SY, UK.

Tel direct: +44 1986 781437

Tel switchboard: +44 1344 856888

direct e-mail: parrym1@aol.com

e-mail for WGII Technical Support Unit: ipccwg2@metoffice.com

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2243. 2003-10-13

cc: Kevin Trenberth <trenbert@cgd.ucar.edu>, Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, p.jones@uea.ac.uk, mann@virginia.edu

date: Mon, 13 Oct 2003 12:59:15 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: draft

to: Tom Wigley <wigley@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>

Thanks Tom,

Working, at this very moment, on a way to broach the valid point raised by Keith w/out

otherwise conflicting w/ what we say. I think some careful wording can accomplish this.

More soon,

mike

At 10:37 AM 10/13/2003 -0600, Tom Wigley wrote:

Folks,

Keith makes a good point about the existence of the MWE. Its existence (or not) does not

have any \*direct\* bearing on the reality of anthro warming. But one must be careful here

not to appear to support the statement of S03 that we criticize at the start of our

response.

The past record \*does\* have a bearing on the confidence we place on anthro effects --

since it is an important aspect of model validation. So the key word here is 'direct'.

cg2003

I suggest looking again at the start of our response to make sure the issue here is clear.  
Tom.  
+++++=  
Keith Briffa wrote:  
Mike and all  
Hi , just back from a trip and only now catching up with important emails.  
Given the restricted time and space available to furnish a response to SB comments , I offer the following mix of comment and specific wording changes:  
I agree that the S+B response is designed to deflect criticism by confusing the issues rather than answering our points.  
In fact they fail to address any of the 3 specific issues we raised Namely , 1. the need for critical evaluation of proxy inputs , 2. the need for a consistent assimilation of widespread (dated and well resolved ) records, 3. the essential requirement for objective/quantitative calibration (scaling) of the input records to allow for assessment of the uncertainties when making comparisons of different reconstructions and when comparing early with recent temperatures.  
Their own , ill-conceived and largely subjective approach did not take account of the uncertainties and problems in the use of palaeodata that they chose to highlight in their opening remarks.  
I would be in favour of stating something to this effect at the outset of our response.  
Also , as regards the tree-ring bit , I fully concur with the sense of your text as regards Section 1, but suggest the following wording (to replace ",rarely for annual ring widths, and almost entirely at higher latitudes.")  
"but in certain high-latitude regions only. Where this is the case , these relatively recent (ie post 1950) data are not used in calibrating temperature reconstructions.  
In many other (even high-latitude) areas density or ring-width records display no bias."  
In the spirit of healthy debate - I agree with Tim's remarks , warning against presenting a too sanguine impression that the borehole debate is closed ( though I do think it is closing!).  
I also believe , as you already know, that the use of a recent padding algorithm to extend smoothed data to the present time, is inappropriate if it assumes the continuation of a recent trend. This is likely to confuse , rather than inform, the wider public about the current climate state .  
Finally , I repeat my earlier remarks (made before EOS piece published) that we are missing an opportunity to say that a warm Medieval period per se is not a refutation of anthropogenic

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warming , {as its absence is no proof}, if we do not understand the role of specific

forcings (natural and anthropogenic) that influenced medieval and current climates.

Cheers

Keith

At 12:48 PM 10/9/03 -0600, Kevin Trenberth wrote:

Hi all

Here are my suggested changes: toned down in several places. Tracking turned on

Kevin

Michael E. Mann wrote:

Dear co-authors,

Attached is a draft response, incorporating suggestions Kevin, Tom W, and Michael. I've

aimed to be as brief as possible, but hard to go much lower than 750 words and still

address all the key issues. 750 words, by the way, is our allotted limit.

Looking forward to any comments. Feel free to send an edited version if you prefer, and

I'll try to assimilate all of the suggested edits and suggestions into a single revised

draft. If you can get comments to me within the next couple days, that would be very

helpful as we're working on a late October deadline for the final version.

Thanks for your continued help,

mike

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

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(434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Kevin E. Trenberth

e-mail:

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-- Professor Keith Briffa,

Climatic Research Unit

University of East Anglia

Norwich, NR4 7TJ, U.K.

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[6]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[7]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2720. 2003-10-13

cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu, mhughes@lrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk

date: Mon Oct 13 16:36:52 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: Re: draft

to: Kevin Trenberth <trenbert@cgd.ucar.edu>, "Michael E. Mann" <mann@virginia.edu>  
Mike and all

Hi , just back from a trip and only now catching up with important emails. Given the restricted time and space available to furnish a response to SB comments , I offer the following mix of comment and specific wording changes:

I agree that the S+B response is designed to deflect criticism by confusing the issues

rather than answering our points.

In fact they fail to address any of the 3 specific

issues we raised Namely , 1. the need for critical evaluation of proxy inputs ,

2. the need for a consistent assimilation of widespread (dated and well resolved ) records,

3. the essential requirement for objective/quantitative calibration (scaling) of the input

records to allow for assessment of the uncertainties when making comparisons of different reconstructions and when comparing early with recent temperatures.

Their own , ill-conceived and largely subjective approach did not take account of the uncertainties and problems in the use of palaeodata that they chose to

highlight in their opening remarks.

I would be in favour of stating something to this effect at the outset of our response.

Also , as regards the tree-ring bit , I fully concur with the sense of your text as

regards Section 1, but suggest the following wording (to replace ",rarely for annual

ring widths, and almost entirely at higher latitudes.")

"but in certain high-latitude regions only. Where this is the case , these relatively

recent

(ie post 1950) data are not used in calibrating temperature reconstructions. In many other

(even high-latitude) areas density or ring-width records display no bias."

In the spirit of healthy debate - I agree with Tim's remarks , warning against presenting a

too

sanguine impression that the borehole debate is closed ( though I do think it is closing!).

I also believe , as you already know, that the use of a recent padding algorithm to extend

smoothed data to the present time, is inappropriate if it assumes the continuation of a

recent

trend. This is likely to confuse , rather than inform, the wider public about the current

climate state .

Finally , I repeat my earlier remarks (made before EOS piece published) that we are missing

an opportunity to say that a warm Medieval period per se is not a refutation of anthropogenic

warming , {as its absence is no proof}, if we do not understand the role of specific

forcings (natural

cg2003

and anthropogenic) that influenced medieval and current climates.

Cheers

Keith

At 12:48 PM 10/9/03 -0600, Kevin Trenberth wrote:

Hi all

Here are my suggested changes: toned down in several places. Tracking turned on

Kevin

Michael E. Mann wrote:

Dear co-authors,

Attached is a draft response, incorporating suggestions Kevin, Tom W, and Michael. I've

aimed to be as brief as possible, but hard to go much lower than 750 words and still

address all the key issues. 750 words, by the way, is our allotted limit.

Looking forward to any comments. Feel free to send an edited version if you prefer, and

I'll try to assimilate all of the suggested edits and suggestions into a single revised

draft. If you can get comments to me within the next couple days, that would be very

helpful as we're working on a late October deadline for the final version.

Thanks for your continued help,

mike

---

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---

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\*\*\*\*\*

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4114. 2003-10-13

---

cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu,  
mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk,  
jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley  
<wigley@ucar.edu>, p.jones@uea.ac.uk, mann@virginia.edu

date: Mon, 13 Oct 2003 23:04:43 -0700

from: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

subject: Re: draft

to: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>,  
Kevin Trenberth <trenbert@cgd.ucar.edu>, "Michael E. Mann" <mann@virginia.edu>

Dear Mike - please find attached a version that now has 763 words in the  
body of the text if counted after 'accepting all changes' (your last version had  
771). The only substantive difference is in the paragraph on boreholes, where  
I really think that the phrase you quote from Pollack et al (1998) in your e-

cg2003

mail message, and similar phrases in other papers from that group, cannot fairly be interpreted quite as you have it. They just made one of those judgements we too have all made about the balance of benefits and disadvantage of pushing back in time with a particular dataset given its limitations. In any case, the paper quoted by SO3 (Huang et al 97) was based on a somewhat different approach than Huang et al 2000, and we have neither time nor space to get into that here. It is, I think, important to note that they, once again, try to make an inappropriate comparison. Hence the words I propose. I have tried to pare off some words at various points in the draft, for example, "that follow" after "ensuing" is redundant.  
Cheers, Malcolm

>  
> Dear All,  
>  
> Attached is a revised version keeping some, but not all, of Malcolm's  
> helpful additions on the first paragraph (which address Keith and  
> Kevin's concerns), and making the other changes indicated. We're  
> presently about 20 words over--they might let us get away w/ that...  
>  
> Final comments? A "yes" or "no" from each author on keeping the final  
> paragraph intact (or largely getting rid of it) would be very  
> helpful...  
>  
> Thanks, and sorry for the multiple version. I think we're almost  
> there,  
>  
> mike

> At 10:36 PM 10/13/2003 -0400, Michael E. Mann wrote:

> Dear All,  
>  
> I plan to send out another revised version tomorrow. A few brief  
> comments:  
>  
> 1) Re boreholes, my apologies. The statement on why the borehole  
> estimates are confined to the past 500 years was not made in Huang  
> et al (2000), but instead the earlier paper by Pollack et al  
> (1998): "The combination of the predominant depth range of  
> observations and the characteristic magnitude of noise has led us  
> to choose five centuries as the practical interval over which to  
> develop climate reconstructions". Will substitute in the correct  
> reference, but keep the wording the same...  
>  
> 2) Regarding the final paragraph, I've shortened the discussion  
> (e.g. removed the bit about the publisher) so as to downplay this,  
> but haven't eliminated it all together. We can have an up or down  
> vote on whether or not to keep it once we finalize the draft...  
>  
> 3) Malcolm's revision of the first paragraph would be great if we  
> had 900 words, but we don't. The limit is 750, and they've  
> indicated that they will be strict about this. So I'll try to  
> reduce Malcolm's additions to one or two summarizing sentences.

> More tomorrow. Thanks,

> mike

> At 06:58 PM 10/13/2003 -0700, Malcolm Hughes wrote:

> <?xml version="1.0"?>  
> Dear Mike and all,  
> Please find attached some small edits that I propose (MS-word  
> \*.doc file with track changes turned on). The first change you  
> will find is designed to incorporate and give strong emphasis to

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> Keith's very important point about keeping the focus on the  
> inadequacies of the SB approach, and their failure to deal with  
> our specific criticisms. This is far and away the most important  
> point to make. The second main change I suggest concerns the words  
> about boreholes. A careful reading for the papers they referred to  
> simply does not justify the wording in Mike's most recent draft,  
> since Huang, Pollack etc. never say exactly why they stop at 500  
> years rather than 400 or 1000. It is, however, the case that the  
> Huang et al 1997 paper to which they refer has a multi-century  
> hump about a thousand years ago, but it can't be compared with a  
> period of 25 or 30 years. I don't see much point in the Folland et  
> al citation, because it is not a primary source. Finally, I am in  
> two minds about the last paragraph of Mike's draft. On the one  
> hand, I understand the need to get the word out about the disquiet  
> many feel about the circumstances surrounding publication of the  
> SB et al papers, but I also suspect that our scientific arguments  
> alone are more than enough to undermine their position. We may, in  
> fact, be seen by many colleagues as making an ad hominem attack,  
> and so arouse the suspicion that our scientific case is not strong  
> enough to stand on its own. I suggest we think carefully before  
> proceeding with the last paragraph as it stands. Cheers, Malcolm >  
> Dear All, > > Thanks for all your comments, which are very  
> helpful. I've done my > best to address these within the pretty  
> tight constraints (750 words) > allotted. We come in now at 746  
> words, just inside the strict 750 word > limit that has been  
> imposed on us. > > We have 6 references now--I've asked folks at  
> AGU if that's ok. > > If people have any final comments on the  
> draft, please let me know > ASAP. For those who haven't yet  
> responded yet (Malcolm, Ray, Caspar, > Scott, Peck), if you're  
> happy w/ it as it currently stands, a simple > "looks good as is  
> now, sign my name to it too" would be great. I don't > want to  
> sign anyone's name to this w/ out some indication of approval. > I  
> realize some of you are still travelling and have been unable to >  
> respond. I've asked AGU if we can have at least one more week  
> before >submitting... > > Thanks again for your continued help, >  
> > mike > > At 04:36 PM 10/13/2003 +0100, Keith Briffa wrote: >  
> >Mike and all >Hi , just back from a trip and only now catching up  
> with important >emails. Given the restricted time and space  
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> replace ",rarely for annual ring widths, and almost >entirely at  
> higher latitudes.") "but in certain high-latitude >regions only.  
> where this is the case , these relatively recent (ie >post 1950)  
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> warm >Medieval period per se is not a refutation of anthropogenic  
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> 12:48 PM 10/9/03 -0600, Kevin Trenberth wrote: >Hi all >Here are  
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> >Attached is a draft response, incorporating suggestions Kevin,  
> Tom >W, and Michael. I've aimed to be as brief as possible, but  
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> >Looking forward to any comments. Feel free to send an edited  
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> >suggested edits and suggestions into a single revised draft. If  
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> >would >be very helpful as we're working on a late October deadline  
> >for >the final version. > >Thanks for your continued help, > >mike  
> >

> >\_\_\_\_ >Professor Michael E. Mann >Department of Environmental  
> Sciences, Clark Hall >University of Virginia >Charlottesville, VA  
> 22903 >\_\_\_\_\_

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> >of East Anglia >Norwich, NR4 7TJ, U.K. > >Phone: +44-1603-593909  
> >Fax: +44-1603-507784 >  
> >http://www.cru.uea.ac.uk/cru/people/briffa/ >

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> >Malcolm K. Hughes Professor of Dendrochronology Laboratory of  
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> >85721 USA e-mail: mhughes@ltrr.arizona.edu telephone: 520-621-6470  
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4541. 2003-10-13

cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa  
<k.briffa@uea.ac.uk>, tcrowley@duke.edu, mhughes@ltrr.arizona.edu,  
omichael@princeton.edu, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>,  
Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk, Kevin Trenberth  
<trenbert@cgd.ucar.edu>  
date: Mon Oct 13 15:23:20 2003  
from: Tim Osborn <t.osborn@uea.ac.uk>  
subject: Re: draft  
to: "Michael E. Mann" <mann@virginia.edu>  
At 20:02 09/10/2003, Michael E. Mann wrote:  
Dear All,  
I like all of Kevin's changes. Please work with his version as a template for  
any additional suggested changes. I'll incorporate the additional comments  
received from Phil and Tom W and others afterwards...  
thanks,  
mike  
Dear Mike and co-authors,  
I've now had a chance to go through the drafts and comments etc. Working from  
Kevin's version, here are some suggestions to consider:  
(1) Are you sure that what we saw is the final version of S03, after any EOS  
editing, etc.? Wouldn't want any of the S03 quotes used here to get changed if they had  
to edit to reduce the length of their piece!  
(2) Suggested re-ordering of the end of point (1): 'it holds in some cases for  
tree-ring density measurements at higher latitudes, but rarely for annual ring widths.'  
(3) Suggested re-wording near start of point (2): '"clearly shows temperatures  
in the MWP that are as high as those in the 20th century" is misleading because it is true  
for only the early 20th century. The hemispheric warmth of the late 20th century is  
anomalous in a long-term context.' (with underlining of either 'late' or 'is' for emphasis).  
Of course, this suggestion needs to be checked carefully (e.g., is it only the 'early' 20th  
century that is exceeded by some earlier temperatures?). But it is an important change  
because it is not actually 'false' or 'untrue' if some part of the 20th century was  
exceeded earlier -

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they don't specify which part, so their statement is (probably deliberately) vague rather than wrong. The above suggestion simply points this out.

(4) Related to this comment, is the question of whether the actual reconstruction (not instrumental observations) in the late 20th century exceeds all reconstructed values (central estimates) prior to the 20th century. My copy of Mann and Jones (2003) has poor quality figures, so this is hard for me to tell. It appears that it might be true, but only right at the end - i.e. the 1980 value of the filtered series. If it is really only at the end, and a 40-year smoothing filter is used, then I would be concerned about this statement appearing in the response if it depends upon applying the filter right up to the end of the record. Doing so requires some assumption about values past the end of the series. This in itself is problematic, but especially so if the assumption were that the trend was extrapolated to produce values for input to the filter. Of course, if the straight 40-year mean from 1941-1980 of the reconstruction exceeds all other 40-year means of the reconstruction, then I'd be happy with the statement.

(5) I don't like point (3) on the boreholes. It relies on the "optimal" borehole series of Mann et al. (2003), a result that I have some concerns about and which is being used here to imply less uncertainty than really exists over this issue. In the EOS paper we included this and the "non-optimal" gridded borehole series, so we were leaving open some uncertainty. I'm not saying that I prefer/believe the Huang et al. series either, since I agree that extracting the temperature signal from the borehole data is very difficult. I just don't like to imply it has been solved when it hasn't.

(6) Can we provide a supporting reference for the statement in point (4) about land use changes leading to an overall cooling?

(7) I like the final paragraph as it is, possibly dropping the last "We feel it is time to move on" line.

Cheers  
Tim

4545. 2003-10-13

cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk, mann@virginia.edu

date: Mon, 13 Oct 2003 18:58:30 -0700

from: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

subject: Re: draft

to: Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, "Michael E. Mann" <mann@virginia.edu>

Dear Mike and all,

Please find attached some small edits that I propose (MS-Word \*.doc file with track changes

turned on). The first change you will find is designed to incorporate and give strong

cg2003

emphasis to Keith's very important point about keeping the focus on the inadequacies of the SB approach, and their failure to deal with our specific criticisms. This is far and away

the most important point to make.

The second main change I suggest concerns the words about boreholes. A careful reading for the papers they referred to simply does not justify the wording in Mike's most recent

draft, since Huang, Pollack etc. never say exactly why they stop at 500 years rather than

400 or 1000. It is, however, the case that the Huang et al 1997 paper to which they refer

has a multi-century hump about a thousand years ago, but it can't be compared with a period

of 25 or 30 years. I don't see much point in the Folland et al citation, because it is not

a primary source.

Finally, I am in two minds about the last paragraph of Mike's draft. On the one hand, I

understand the need to get the word out about the disquiet many feel about the circumstances surrounding publication of the SB et al papers, but I also

suspect that our

scientific arguments alone are more than enough to undermine their position. We may, in

fact, be seen by many colleagues as making an ad hominem attack, and so arouse the

suspicion that our scientific case is not strong enough to stand on its own. I suggest we

think carefully before proceeding with the last paragraph as it stands.

Cheers, Malcolm

>

> Dear All,

>

> Thanks for all your comments, which are very helpful. I've done my  
> best to address these within the pretty tight constraints (750 words)  
> allotted. We come in now at 746 words, just inside the strict 750 word  
> limit that has been imposed on us.

>

> We have 6 references now--I've asked folks at AGU if that's ok.

>

> If people have any final comments on the draft, please let me know  
> ASAP. For those who haven't yet responded yet (Malcolm, Ray, Caspar,  
> Scott, Peck), if you're happy w/ it as it currently stands, a simple  
> "looks good as is now, sign my name to it too" would be great. I don't  
> want to sign anyones name to this w/ out some indication of approval.  
> I realize some of you are still travelling and have been unable to  
> respond. I've asked AGU if we can have at least one more week before  
> submitting...

>

> Thanks again for your continued help,

>

> mike

>

> At 04:36 PM 10/13/2003 +0100, Keith Briffa wrote:

>

> Mike and all

> Hi , just back from a trip and only now catching up with important  
> emails. Given the restricted time and space available to furnish a  
> response to SB comments , I offer the following mix of comment and  
> specific wording changes:

>

> I agree that the S+B response is designed to deflect criticism by

cg2003

> confusing the issues rather than answering our points. In fact  
> they fail to address any of the 3 specific issues we raised Namely  
> , 1. the need for critical evaluation of proxy inputs , 2. the  
> need for a consistent assimilation of widespread (dated and well  
> resolved ) records, 3. the essential requirement for  
> objective/quantitative calibration (scaling) of the input records  
> to allow for assessment of the uncertainties when making  
> comparisons of different reconstructions and when comparing early  
> with recent temperatures. Their own , ill-conceived and largely  
> subjective approach did not take account of the uncertainties and  
> problems in the use of palaeodata that they chose to highlight in  
> their opening remarks. I would be in favour of stating something  
> to this effect at the outset of our response.

>  
> Also , as regards the tree-ring bit , I fully concur with the  
> sense of your text as regards Section 1, but suggest the following  
> wording (to replace ",rarely for annual ring widths, and almost  
> entirely at higher latitudes.") "but in certain high-latitude  
> regions only. Where this is the case , these relatively recent (ie  
> post 1950) data are not used in calibrating temperature  
> reconstructions. In many other (even high-latitude) areas density  
> or ring-width records display no bias."

>  
> In the spirit of healthy debate - I agree with Tim's remarks ,  
> warning against presenting a too sanguine impression that the  
> borehole debate is closed ( though I do think it is closing!). I  
> also believe , as you already know, that the use of a recent  
> padding algorithm to extend smoothed data to the present time, is  
> inappropriate if it assumes the continuation of a recent trend.  
> This is likely to confuse , rather than inform, the wider public  
> about the current climate state .

>  
> Finally , I repeat my earlier remarks (made before EOS piece  
> published) that we are missing an opportunity to say that a warm  
> Medieval period per se is not a refutation of anthropogenic  
> warming , {as its absence is no proof}, if we do not understand  
> the role of specific forcings (natural and anthropogenic) that  
> influenced medieval and current climates.

> Cheers  
> Keith

>  
> At 12:48 PM 10/9/03 -0600, Kevin Trenberth wrote:  
> Hi all  
> Here are my suggested changes: toned down in several  
> places.Tracking turned on Kevin

>  
> Michael E. Mann wrote:  
> Dear co-authors,

>  
> Attached is a draft response, incorporating suggestions Kevin, Tom  
> W, and Michael. I've aimed to be as brief as possible, but hard to  
> go much lower than 750 words and still address all the key issues.  
> 750 words, by the way, is our allotted limit.

>  
> Looking forward to any comments. Feel free to send an edited  
> version if you prefer, and I'll try to assimilate all of the  
> suggested edits and suggestions into a single revised draft. If  
> you can get comments to me within the next couple days, that would  
> be very helpful as we're working on a late October deadline for  
> the final version.

> Thanks for your continued help,

cg2003

>  
> mike  
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4618. 2003-10-13

date: Mon, 13 Oct 2003 15:45:00 +0100

cg2003

from: Nick Brooks <nick.brooks@uea.ac.uk>

subject: Re: BBL tomorrow - UNDERSTANDING OUR CONTEXT, part III

to: S Cornell <S.Cornell@uea.ac.uk>, <zicer.all@uea.ac.uk>

I'd like to encourage people to engage with these discussions.

Here are my suggestions as to how to break down this particular session into particular questions:

1. How can those of us concerned with climate change persuade others of the need to invest in alternative sources of energy? Is this sort of advocacy role appropriate for institutions such as UEA? (There is a suite of issues here ranging through energy technology, political considerations and vested interests, what constitutes dangerous climate change and the sensitivity of the climate system, whether we can and should "stabilise" the climate in the short term - longer term stabilisation being impossible without large-scale planetary engineering and so on)

2. To what extent are the aspirations of economic growth, increased consumption (the latter necessary to drive the former) and increased affluence compatible with climate stabilisation and sustainability?

3. How inevitable is the process of economic globalisation and does it necessarily mean increased consumption and use of resources?

4. What are the key levers for influencing national and international policies on climate and sustainability?

5. To what extent are climate stabilisation and sustainable contingent of top-down social engineering (a corollary to this is to what extent social engineering is already occurring to serve the interests of those pursuing economic growth and economic globalisation?).

6. How can the gap in wealth, living standards, access to public services and vulnerability to environmental and economic change between North and South be bridged? Should we attempt to bridge this gap? What are the implications of bridging this gap for and economic growth in the wealthy industrialised nations?

We won't answer these in 90 minutes, but a discussion might generate some ideas for pursuing these themes in a manner appropriate to our objectives and interests as researchers. We could set up an email discussion list for pursuing these threads at a more leisurely pace. If people think this is a good idea I will speak to Laura (who manages the Tyndall lists) about it.

Nick

--

Dr Nick Brooks

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<http://www.cru.uea.ac.uk/~e118/welcome.htm> (personal site)

<http://www.tyndall.ac.uk> (Tyndall Centre site)

<http://www.uea.ac.uk/sahara> (Saharan Studies Programme)

--

On 13/10/03 2:58 pm, "S Cornell" <S.Cornell@uea.ac.uk> wrote:

> Dear all,

> We continue to look at our roles and stances in climate change research and sustainability, and tomorrow's discussion will be:

> 3. How should we address competing issues of lifestyle, energy/food > security, globalisation, economic growth targets, and social > engineering?

> There has to be something for everyone in this discussion! I will be > chairing the debate, but you are the experts, so bring your lunch, and > join in with your ideas. As usual, our Brown Bag will be in the ZICER > library from 12:30 - 2 pm.

>

> Sarah

>

5073. 2003-10-13

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date: Mon, 13 Oct 2003 16:53:29 +0100  
from: "Andy Jordan" <A.Jordan@uea.ac.uk>  
subject: GEC  
to: "'Neil Adger'" <N.Adger@uea.ac.uk>, "'Mike Hulme'" <m.hulme@uea.ac.uk>  
Hi  
Mary Malin of Elsevier just called me and we discussed life post Martin for about 20 mins.  
Just to update you on how things now stand:  
1. MP to depart in January; ideally AJ and TOR to depart same time (but see below)  
2. Elsevier are already drawing up a shortlist of new eds, and the UEA was already on it. I mentioned you two and she already knew a lot about TYN/ENV. She said she would contact you soon.  
3. She wanted the instits col. to continue as its was very popular. I said I would 'babysit' someone new for a few issues if it relocated to UEA but wanted out in the medium to long term (ie during 2003).  
4. The current impact factor is c. 0.94, having dropped from above 1.0 last year. Elsevier are keen to raise it back again and see fresh editorial faces/a relaunch as one (unexpected) way to do this.  
Mary's details:  
m.malin@elsevier.com  
Tel: 01865 843471  
Over to you two...  
Cheers  
Andy

---

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Personal website: <http://www.uea.ac.uk/env/faculty/jordanaj.htm>  
Environment and Planning C website: <http://www.envplan.com/>

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325. 2003-10-14

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cc: Tim Osborn <t.osborn@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, rbradley@geo.umass.edu, tcrowley@duke.edu, omichael@princeton.edu, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk, Kevin Trenberth <trenbert@cgd.ucar.edu>  
date: Tue, 14 Oct 2003 10:25:45 -0600  
from: Caspar Ammann <ammann@ucar.edu>  
subject: Re: Fwd: Re: draft  
to: "Michael E. Mann" <mann@virginia.edu>

Mike,  
looks good to me. It is one of these points where they can persuade journalists that they are 'correct' and it actually got into newspapers and finally to the senate

cg2003

floor this way.

The more we are able to explain why the first half of the 20th century warmed up naturally, the more confidence we get on the detection of the anthropogenic signal afterwards.

Caspar

Michael E. Mann wrote:

Dear All,

In response to Caspar's suggestion, which I agree with, I propose rephrasing item "2"

as follows:

2) The statement by S03 that the Mann and Jones [2003] reconstruction "clearly shows

temperatures in the MWP that are as high as those in the 20th century" is misleading if

not false. M03 emphasize that it is the late, and not the early or mid 20th century

warmth, that is outside the range of past variability. Mann and Jones

emphasize

conclusions for the Northern Hemisphere, noting that those for the Southern Hemisphere

(and globe) are indeterminate due to a paucity of southern hemisphere data.

Consistent

with M03, they conclude that, late 20th century Northern Hemisphere mean temperatures

are anomalous in a long-term (nearly two millennium) context.

Any comments?

Thanks,

mike

Delivered-To: [1]mem6u@virginia.edu

Date: Tue, 14 Oct 2003 09:18:37 -0600

From: Caspar Ammann [2]<ammann@ucar.edu>

Organization: NCAR

User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.4)

Gecko/20030624

Netscape/7.1 (ax)

X-Accept-Language: en-us, en

To: "Michael E. Mann" [3]<mann@virginia.edu>

Subject: Re: draft

Hi Mike,

it now looks good to me indeed including the new last paragraph following Tom's wording.

The only point I would highlight a little more is in point 2): Maybe it could be stated

that the early part of the 20th century is within the natural range whereas the late

20th century, the main point of the AGU position statement and also in M03, is clearly

outside. Please also add a second 'n' in my name...

Cheers, and thanks for your momentum on this,

Caspar

Michael E. Mann wrote:

Dear All,

I agree with each of Tom W's suggestions. Adopting them, by the way, brings us down to 738 words.

So pending any revised language from Keith/Malcolm in response to Michael O's comment on

paragraph 2, I'm putting out a last call for comments, sign-ons, etc...

Thanks,

mike

At 08:00 AM 10/14/2003 -0600, Tom Wigley wrote:

Some minor points ....



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para. 2 -- should it be 'an' ensuing rather than 'the' ensuing?

para. 2 -- I still think 'each' (line 3) is unnecessary

para. 4 -- no comma after '(and globe)'

re boreholes, does the point about comparing late 20th century with a 'much longer period' 1000 years ago help us? Given that the 1000 years ago data is highly filtered, if one \*did\* have a series with a temporal resolution that allowed a legitimate comparison, then the likelihood of a warmer interval 1000 years ago must be higher.

In any event, the time scale issue will not be meaningful to most readers. The key point

is the data reliability/uncertainty. I would just say something like ...

".... taken into account. For times more than 500 years ago, uncertainties in the borehole reconstructions preclude any useful quantitative comparison."

Finally, I would like the last para. retained, but I suggest shorter wording as ...

".... as indicating that SB03 misinterpreted and misrepresented the paleoclimatological literature. The controversy ....".

My problem here is twofold. First, they really say nothing directly about 'mainstream

scientific opinion' (except that they clearly disagree with it). At issue is not the

mainstream opinion, but their interpretation of the literature and their illogical

conclusions. Second, they may have misrepresented the results of their work, but we do

not address this issue so it comes here as a non sequitur. In fact, just what such

'misrepresentation' consists of, and why it might be judged as

'misrepresentation' is a subtle issue. Hence my revision -- which retains the word 'misrepresentation', but in a

different context.

Tom.

+++++=

Michael E. Mann wrote:

Thanks Tim and Malcolm,

The latest round of suggestions were extremely helpful. I've accepted them w/ a few

minor tweaks (attached). We're at 765 words--I think AGU will let us get away w/ that...

So, comments from others?

Thanks,

mike

At 02:11 PM 10/14/2003 +0100, Tim Osborn wrote:

SO3 argue that borehole data provide a conflicting view of past temperature histories.

To the contrary, the borehole estimates for recent centuries shown in M03 may be

consistent with other estimates, provided consideration is given to statistical

uncertainties, spatial sampling and possible influences on the ground surface [e.g.,

snow cover changes--Beltrami and Kellman, 2003]. It is not meaningful to compare the

late 20th century with a much longer period 1000 years ago [Bradley et al., 2003],

especially given the acknowledged limitations [Pollack et al., 1998] of borehole data.

cg2003

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Boulder, CO 80307-3000  
email: [11]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

3047. 2003-10-14

---

cc: Tim Osborn <t.osborn@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>,  
Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Caspar  
Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu,  
jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley  
<wigley@ucar.edu>, p.jones@uea.ac.uk  
date: Tue, 14 Oct 2003 10:09:04 -0400  
from: Michael Oppenheimer <omichael@Princeton.EDU>  
subject: Re: draft  
to: "Michael E. Mann" <mann@virginia.edu>

Michael:

I'm fine with the last paragraph. However, the section on latitude dependence  
of tree-growth data remains obscure. In particular, the sentence "In such cases,  
relatively recent (i.e. post 1950) data are not used in calibrating temperature  
reconstructions"  
leaves the impression that data is rejected because it doesn't fit expectation.  
For the

cg2003

uninitiated, you need a few words on why this procedure is acceptable, like perhaps

"because confounding influences obscure the response to temperature" or whatever.

Michael

Michael

"Michael E. Mann" wrote:

Thanks Tim and Malcolm,

The latest round of suggestions were extremely helpful. I've accepted them w/ a few minor tweaks (attached). We're at 765 words--I think AGU will let us get away w/ that...

So, comments from others?

Thanks,

mike

At 02:11 PM 10/14/2003 +0100, Tim Osborn wrote:

S03 argue that borehole data provide a conflicting view of past temperature histories.

To the contrary, the borehole estimates for recent centuries shown in M03 may be

consistent with other estimates, provided consideration is given to statistical

uncertainties, spatial sampling and possible influences on the ground surface [e.g.,

snow cover changes--Beltrami and Kellman, 2003]. It is not meaningful to compare the

late 20th century with a much longer period 1000 years ago [Bradley et al., 2003],

especially given the acknowledged limitations [Pollack et al., 1998] of borehole data.

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\omichael8.vcf"

4591. 2003-10-14

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date: Tue, 14 Oct 2003 17:27:24 -0400

from: "Michael E. Mann" <mann@virginia.edu>

subject: Fwd: Re: smoothing

to: Tom Wigley <wigley@ucar.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov, jto@u.arizona.edu, mann@virginia.edu

Sorry--one more error. The MSE values for "minimum norm" and "minimum roughness" are

switched in the figure legend. Obviously the former is a better fit...

mike

Date: Tue, 14 Oct 2003 17:08:49 -0400

To: Tom Wigley <wigley@ucar.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Keith Briffa

<k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov, jto@u.arizona.edu, mann@virginia.edu

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: smoothing

Bcc: Scott Rutherford <srutherford@rwu.edu>

correction '1)' should read:

'1) minimum norm: sets padded values equal to mean of available data beyond the

cg2003

available data (often the default constraint in smoothing routines)'

sorry for the confusion,

mike

At 05:05 PM 10/14/2003 -0400, Michael E. Mann wrote:

Dear All,

To those I thought might be interested, I've provided an example for discussion of smoothing conventions. Its based on a simple matlab script which I've written (and attached) that uses any one of 3 possible boundary constraints [minimum norm, minimum slope, and minimum roughness] on the 'late' end of a time series (it uses the default 'minimum norm' constraint on the 'early' end of the series). Warming: you needs some matlab toolboxes for this to run... The routines uses a simple butterworth lowpass filter, and applies the 3 lowest order constraints in the following way:

- 1) minimum norm: sets mean equal to zero beyond the available data (often the default constraint in smoothing routines)
- 2) minimum slope: reflects the data in x (but not y) after the last available data point. This tends to impose a local minimum or maximum at the edge of the data.
- 3) minimum roughness: reflects the data in both x and y (the latter w.r.t. to the y value of the last available data point) after the last available data point. This tends to impose a point of inflection at the edge of the data---this is most likely to preserve a trend late in the series and is mathematically similar, though not identical, to the more ad hoc approach of padding the series with a continuation of the trend over the past 1/2 filter width.

The routine returns the mean square error of the smooth with respect to the raw data. It is reasonable to argue that the minimum mse solution is the preferable one. In the particular example I have chosen (attached), a 40 year lowpass filtering of the CRU NH annual mean series 1856-2003, the preference is indicated for the "minimum roughness" solution as indicated in the plot (though the minimum slope solution is a close 2nd)...

By the way, you may notice that the smooth is effected beyond a single filter width of the boundary. That's because of spectral leakage, which is unavoidable (though minimized by e.g. multiple-taper methods).

I'm hoping this provides some food for thought/discussion, esp. for purposes of IPCC...

mike

---

Professor Michael E. Mann  
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---

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

cg2003

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

877. 2003-10-15

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cc: Tim Osborn <t.osborn@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>,  
Keith Briffa <k.briffa@uea.ac.uk>, rbradley@geo.umass.edu, tcrowley@duke.edu,  
omichael@princeton.edu, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>,  
Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk, Kevin Trenberth  
<trenbert@cgd.ucar.edu>, mann@virginia.edu  
date: wed, 15 Oct 2003 12:22:27 -0400  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Re: draft  
to: Tom Crowley <tcrowley@duke.edu>

Thanks Tom,  
I knew you'd been travelling, and appreciate you getting back to me as soon as  
you could.

I've attached a revised 'final' draft adopting your suggestion on the final  
sentence w/ a  
minor tweak.

Will await any comments/go ahead from Peck, and then I should be ready to  
submit.

Thanks to all, again, for the considerable help...

mike

At 10:16 AM 10/15/2003 -0400, Tom Crowley wrote:

Mike, I have been out of town and have not participated in the exchanges - the  
reply is

nice - some might raise eyebrows about the pointedness but I think BS deserve  
a bit of  
pointedness, so I am happy to sign off on it.

A couple of minor points:

last para., first line, a comma does not seem necessary after "Education"

like "we  
last para., last sentence - needs a bit more punch to close it out - something

believe these developments speak for themselves with respect to the quality of  
the soon  
and Baliunas criticisms."

you don't need another go-around on this last point if you don't like it -  
just think

about it and I will defer to whatever you decide without taking any longer on  
this.

thanks for all your work, tom

Great,

Peck Thanks a bunch Tom...Will make those changes. Awaiting word from Tom C and  
before

signing off,

mike

At 11:45 AM 10/14/2003 -0600, Tom Wigley wrote:

Good job Mike -- and everyone else. Reads smoothly and punchily.

cg2003

Still some grammar errors, like 'et al' instead of 'et al.'

Also, I think Caspar should just be 'NCAR', for consistency with me and Kevin.  
TOM.

=====

Michael E. Mann wrote:

Thanks Malcolm,

I agree, it would be nice to give this issue, which is nuanced and somewhat complex, proper justice. But we can't. I think your minor change of wording helps.  
We're at 755

words--I think that'll be ok. Latest (final?) draft attached. The sentiment has been

overwhelmingly to keep the final paragraph, but in the shortened and downplayed form as

suggested by Tom W and others.

At this point, we only need to hear from Tom C and Peck to sign off on this.  
Tom, Peck,

any comments". Again, I don't want to sign either of your names to this until I at least

have an "ok".

Thanks,

mike

At 09:39 AM 10/14/2003 -0700, Malcolm Hughes wrote:

Dear Mike - count me in. Regarding Mike O's question, it might help to insert 'recent' before 'bias' at the end of para 1). As it stands the paragraph shows that SB treat a problem related to a subset of the tree-ring data as if it applied

to all of them. It would probably take a couple of sentences more to adequately answer Mike's reasonable concern and that would skew our text too far towards defending our own work rather than our critique of SB, in my view.

Given more space, the crucial issue is that there is reason to believe that the

weakening of the relationship between temperature and tree-ring density and width in some extensive northern regions, largely restricted to decadal time scales, is indeed anomalous. For example, the stability of the density/summer temperature relationship has been demonstrated against instrumental records as far back as the early 18th century in the case of Scotland. Thus the justification for not using the post 1950 period for training of models for retrodiction is that some new factor or combination of factors has come into play since then in these cases.

Hope this helps! Keith can probably say it in 80% fewer words. Cheers,  
Malcolm

.

.

.

Malcolm Hughes

Professor of Dendrochronology

Laboratory of Tree-Ring Research

University of Arizona

Tucson, AZ 85721

520-621-6470

fax 520-621-8229

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Professor Michael E. Mann

Page 742

cg2003  
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Thomas J. Crowley  
Nicholas Professor of Earth Systems Science  
Dept. of Earth and Ocean Sciences  
Nicholas School of the Environment and Earth Sciences  
Box 90227  
103 Old Chem Building Duke University  
Durham, NC 27708  
tcrowley@duke.edu  
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919-684-5833 fax

---

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
Attachment Converted: "c:\eudora\attach\SoonReply-final.doc"

4035. 2003-10-15

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date: Wed, 15 Oct 2003 11:08:51 +0100  
from: F.Berkhout@sussex.ac.uk (Frans Berkhout)  
subject: Re: IPCC TGCIA for AR4  
to: "Mike Hulme" <m.hulme@uea.ac.uk>

Mike

I have filled the form in quickly. I hope this is enough evidence.  
All the best

Frans

----- Original Message -----

From: "Mike Hulme" <m.hulme@uea.ac.uk>  
To: "Frans Berkhout" <F.Berkhout@sussex.ac.uk>  
Sent: Tuesday, October 14, 2003 5:52 PM  
Subject: Re: IPCC TGCIA for AR4

> Frans, I would be happy to nominate you.

>

> The attached document summarises the role and has a short pro-forma which  
> you need to complete - just the usual CV stuff you can cut and paste.

>

> If you get it back to me by Thursday I will forward it to DEFRA.

>

> Thanks,

>

> Mike

>

> At 18:10 13/10/2003 +0100, you wrote:

> >Mike

> >I'd like to put my name forward. Have been to a couple of meetings and  
> >enjoyed them.

> >All the best

> >Frans

> >

> >----- Original Message -----

> >From: "Mike Hulme" <m.hulme@uea.ac.uk>

> >To: <f.berkhout@sussex.ac.uk>; <a.jordan@uea.ac.uk>;

cg2003

<robert14@mdx.ac.uk>;  
> > "N.W.Arnell" <N.W.Arnell@soton.ac.uk>; "Sari Kovats"  
> > <sari.kovats@lshtm.ac.uk>; <tsb1@econ.cam.ac.uk>  
> > Sent: Monday, October 13, 2003 5:21 PM  
> > Subject: IPCC TGCIA for AR4  
> >  
> >  
> > Dear Colleague,  
> > >  
> > > I think most of you have had some involvement in the past in the IPCC  
Task  
> > > Group on Scenarios for Climate Impact Assessment (TGCIA), perhaps via  
> > > Martin Parry. For AR4, the IPCC are renewing the membership of this  
> > > cross-cutting activity and DEFRA are now seeking nominees from the UK  
for  
> > > possible membership.  
> > >  
> > > Are any of you interested in being put forward by DEFRA for membership  
for  
> > > the period 2003-2006? Richard Moss from the US Global Change Research  
> > > Program nows chairs the TGCIA in place of Martin Parry. The TGCIA  
meets  
> > > about twice a year in different parts of the world and has a  
membership of  
> > > about 15-20 people.  
> > >  
> > > DEFRA need nominations by this Friday, 17th October, so let me know if  
you  
> > > are interested - they have me to make any suitable nominations. I  
have a  
> > > simple nomination form.  
> > >  
> > > Thanks,  
> > >  
> > > Mike  
> > >  
> > >  
> > >  
> > >  
> > >  
>  
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TGCIA-Berkhout.doc"

239. 2003-10-16

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date: Thu, 16 Oct 2003 16:43:41 -0400  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Fwd: Correspondence on Harvard Crimson coverage of Soon / Baliunas  
to: Malcolm Hughes <mhughes@lrrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>,  
Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Caspar  
Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, tcrowley@duke.edu,  
omichael@princeton.edu, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>,  
p.jones@uea.ac.uk, mann@virginia.edu, Tom Wigley <wigley@ucar.edu>  
Dear All,  
Thought you would be interested in this exchange, which John Holdren of Harvard  
has been  
kind enough to pass along...  
mike  
Delivered-To: mem6u@virginia.edu  
X-Sender: jholdren@camail2.harvard.edu  
X-Mailer: QUALCOMM windows Eudora Version 5.0.2  
Date: Thu, 16 Oct 2003 13:53:08 -0400



cg2003

To: "Michael Mann" <mem6u@virginia.edu>, "Tom Wigley" <wigley@ucar.edu>  
From: "John P. Holdren" <john\_holdren@harvard.edu>  
Subject: Correspondence on Harvard Crimson coverage of Soon / Baliunas  
views on climate  
Michael and Tom --

I'm forwarding for your entertainment an exchange that followed from my being  
quoted in  
the Harvard Crimson to the effect that you and your colleagues are right and  
my  
"Harvard" colleagues Soon and Baliunas are wrong about what the evidence shows  
concerning surface temperatures over the past millennium. The cover note to  
faculty  
and postdocs in a regular Wednesday breakfast discussion group on  
environmental science  
and public policy in Harvard's Department of Earth and Planetary Sciences is  
more or  
less self-explanatory.

Best regards,

John

Date: Thu, 16 Oct 2003 11:02:24 -0400

To: schrag@eps.harvard.edu, oconnell@eps.harvard.edu, holland@eps.harvard.edu,  
pearson@eps.harvard.edu, eli@eps.harvard.edu, ingalls@eps.harvard.edu,  
m1m@eps.harvard.edu, avan@fas.harvard.edu, moyer@huarp.harvard.edu,  
poussart@fas.harvard.edu, jshaman@fas.harvard.edu, sivan@fas.harvard.edu,  
bec@io.harvard.edu, saleska@fas.harvard.edu

From: "John P. Holdren" <john\_holdren@harvard.edu>

Subject: For the EPS Wednesday breakfast group: Correspondence on Harvard  
Crimson

coverage of Soon / Baliunas views on climate

Cc: jeremy\_bloxham@harvard.edu, william\_clark@harvard.edu,  
patricia\_mclaughlin@harvard.edu,

Bcc:

Colleagues--

I append here an e-mail correspondence I have engaged in over the past few  
days trying  
to educate a Soon/Baliunas supporter who originally wrote to me asking how I  
could think  
that Soon and Baliunas are wrong and Mann et al. are right (a view attributed  
to me,  
correctly, in the Harvard Crimson). This individual apparently runs a web  
site on which  
he had been touting the Soon/Baliunas position.  
While it is sometimes a mistake to get into these exchanges (because one's  
interlocutor  
turns out to be ineducable and/or just looking for a quote to reproduce out of  
context  
in an attempt to embarrass you), there was something about this guy's  
formulations that  
made me think, at each round, that it might be worth responding. In the end,  
a couple  
of colleagues with whom I have shared this exchange already have suggested  
that its  
content would be of interest to others, and so I am sending it to our  
"environmental  
science and policy breakfast" list for your entertainment and, possibly,  
future  
breakfast discussion.

The items in the correspondence are arranged below in chronological order, so  
that it  
can be read straight through, top to bottom.

Best,

John

At 09:43 PM 9/12/2003 -0400, you wrote:

cg2003

Dr. Holdren:

In a recent Crimson story on the work of Soon and Baliunas, who have written for my

website [1]www.techcentralstation.com, you are quoted as saying:

My impression is that the critics are right. It's unfortunate that so much attention is

paid to a flawed analysis, but that's what happens when something happens to support the

political climate in Washington.

Do you feel the same way about the work of Mann et al.? If not why not?

Best,

Nick

Nick Schulz

Editor

TCS

1-800-619-5258

From: John P. Holdren [[2]mailto:john\_holdren@harvard.edu]

Sent: Monday, October 13, 2003 11:06 AM

To: Nick Schulz

Subject: Harvard Crimson coverage of Soon / Baliunas controversy

Dear Nick Schultz --

I am sorry for the long delay in this response to your note of September 12.

I have

been swamped with other commitments.

As you no doubt have anticipated, I do not put Mann et al. in the same category with

Soon and Baliunas.

If you seriously want to know "why not?", here are three ways one might arrive at what I

regard as the right conclusion:

(1) For those with the background and patience to penetrate the scientific arguments,

the conclusion that Mann et al. are right and Soon and Baliunas are wrong

follows from

reading carefully the relevant Soon / Baliunas paper and the Mann et al.

response to it:

W. Soon and S. Baliunas, "Proxy climatic and environmental changes of the past 1000

years", Climate Research, vol. 23, pp 89ff, 2003.

M. Mann, C. Amman, R. Bradley, K. Briffa, P. Jones, T. Osborn, T. Crowley, M. Hughes, M.

Oppenheimer, J. Overpeck, S. Rutherford, K. Trenberth, and T. Wigley, "On past temperatures and anomalous late-20th century warmth", EOS, vol 84, no. 27, pp

256ff, 8

July 2003.

This is the approach I took. Soon and Baliunas are demolished in this comparison.

(2) Those lacking the background and/or patience to penetrate the two papers, and

seriously wanting to know who is more likely to be right, have the option of asking

somebody who does possess these characteristics -- preferably somebody outside the

handful of ideologically committed and/or oil-industry-linked professional climate-change skeptics -- to evaluate the controversy for them. Better yet,

one could

poll a number of such people. They can easily be found by checking the web pages of

earth sciences, atmospheric sciences, and environmental sciences departments at any

number of major universities.

(3) The least satisfactory approach, for those not qualified for (1) and lacking the

cg2003

time or initiative for (2), would be to learn what one can about the qualifications (including publications records) and reputations, in the field in question, of the authors on the two sides. Doing this would reveal that Soon and Baliunas are, essentially, amateurs in the interpretation of historical and paleoclimatological records of climate change, while the Mann et al. authors include several of the most published and most distinguished people in the world in this field. Such an investigation would also reveal that Dr. Baliunas' reputation in this field suffered considerable damage a few years back, when she put her name on an incompetent critique of mainstream climate science that was never published anywhere respectable but was circulated by the tens of thousands, in a format mimicking that of a reprint from the Proceedings of the National Academy of Sciences, in pursuit of signatures on a petition claiming that the mainstream findings were wrong. Of course, the third approach is the least satisfactory because it can be dangerous to assume that the more distinguished people are always right. Occasionally, it turns out that the opposite is true. That is one of several good reasons that it pays to try to penetrate the arguments, if one can, or to poll others who have tried to do so. But in cases where one is not able or willing to do either of these things -- and where one is able to discover that the imbalance of experience and reputation on the two sides of the issue is as lopsided as here -- one ought at least to recognize that the odds strongly favor the proposition that the more experienced and reputable people are right. If one were a policy maker, to bet the public welfare on the long odds of the opposite being true would be foolhardy. Sincerely, John Holdren

PS: I have provided this response to your query as a personal communication, not as fodder for selective excerpting on your web site or elsewhere. If you do decide that you would like to propagate my views on this matter more widely, I ask that you convey my response in its entirety.

At 11:16 AM 10/13/2003 -0400, you wrote:  
I have the patience but, by your definition certainly, not the background, so I suppose it's not surprising I came to a different conclusion. I guess my problem concerns what lawyers call the burden of proof. The burden weighs heavily much more heavily, given the claims on Mann et.al. than it does on Soon/Baliunas. Would you agree? Falsifiability for the claims of Mann et. al. requires but a few examples, does it not? Soon/Baliunas make claims that have no such burden. Isn't that correct?

Best,

cg2003

Nick

From: John P. Holdren [[3]mailto:john\_holdren@harvard.edu]

Sent: Tuesday, October 14, 2003 5:54 PM

To: Nick Schulz

Subject: RE: Harvard Crimson coverage of Soon / Baliunas controversy

Nick--

Yes, I can see how it might seem that, in principle, those who are arguing for a strong and sweeping proposition (such as that "the current period is the warmest in the last 1000 years") must meet a heavy burden of proof, and that, because even one convincing counter-example shoots the proposition down, the burden that must be borne by the critics is somehow lighter. But, in practice, burden of proof is an evolving thing -- it evolves as the amount of evidence relevant to a particular proposition grows.

To choose an extreme example, consider the first and second laws of thermodynamics.

Both of these are "empirical" laws. Our confidence in them is based entirely on observation; neither one can be "proven" from more fundamental laws. Both are very sweeping. The first law says that energy is conserved in all physical processes. The second law says that entropy increases in all physical processes. So, is the burden of proof heavier on somebody who asserts that these laws are correct, or on somebody who claims to have found an exception to one or both of them? Clearly, in this case, the burden is heavier on somebody who asserts an exception. This is in part because the two laws have survived every such challenge in the past. No exception to either has ever been documented. Every alleged exception has turned out to be traceable to a mistake of some kind. This burden on those claiming to have found an exception is so strong that the US Patent Office takes the position, which has been upheld in court, that any patent application for an invention that violates either law can be rejected summarily, without any further analysis of the details.

Of course, I am not asserting that the claim we are now in the warmest period in a millennium is in the same league with the laws of thermodynamics. I used the latter only to illustrate the key point that where the burden is heaviest depends on the state of prior evidence and analysis on the point in question -- not simply on whether a proposition is sweeping or narrow.

In the case actually at hand, Mann et al. are careful in the nature of their claim.

They write along the lines of "A number of reconstructions of large-scale temperature changes support the conclusion" that the current period is the warmest in the last millennium.

And they write that the claims of Baliunas et al. are "inconsistent with

the preponderance of scientific evidence". They are not saying that no

shred of evidence to the contrary has ever been produced, but rather that analysis of the available evidence as a whole tends to support their conclusion. This is often the case in science. That is, there are often "outlier" data points or apparent contradictions that are not yet adequately explained, but still are not given much weight by most of the scientists working on a particular issue if a strong preponderance of evidence points the other way. This is because the scientists judge it to be more probable that the outlier data point or apparent contradiction will ultimately turn out to be explainable as a mistake, or otherwise explainable in a way that is consistent with the preponderance of evidence, than that it will turn out that the preponderance of evidence is wrong or is being misinterpreted. Indeed, apparent contradictions with a preponderance of evidence are FAR more often due to measurement error or analysis error than to real contradiction with what the preponderance indicates. A key point, then, is that somebody with a PhD claiming to have identified a counterexample does not establish that those offering a general proposition have failed in their burden of proof. The counterexample itself must pass muster as both valid in itself and sufficient, in the generality of its implications, to invalidate the proposition. In the case at hand, it is not even a matter of an "outlier" point or other seeming contradiction that has not yet been explained. Mann et al. have explained in detail why the supposed contrary evidence offered by Baliunas et al. does NOT constitute a counterexample. To those with some knowledge and experience in studies of this kind, the refutation by Mann et al is completely convincing.

Sincerely,  
John Holdren

At 08:08 AM 10/15/2003 -0400, you wrote:  
Dr. Holdren:  
Thank you for your thoughtful reply. I genuinely appreciate you taking the time. You are quite right about the laws of thermodynamics. And you are quite right that Mann et al is not in the same league as those laws and that s not to take anything from their basic research. You write to those with knowledge and experience in studies of this kind, the refutation by Mann et all is completely convincing. Since I do not have what you would consider the requisite knowledge or experience, I can t speak to that. I ve read the Mann papers and the Baliunas Soon paper and the Mann rebuttal and find Mann s claims based on his research extravagant and beyond what he can legitimately claim to know. That said, I m willing to believe it is because I don t have the tools necessary to understand.

cg2003

But if you will indulge a lay person with some knowledge of the matter, perhaps you

could clear up a thing or two.

Part of the confusion over Mann et al it seems to me has to do not with the research

itself but with the extravagance of the claims they make based on their research.

And yet you write: Mann et al. are careful in the nature of their claim. They write

along the lines of A number of reconstructions of large-scale temperature changes

support the conclusion that the current period is the warmest in the last millennium.

And they write that the claims of Baliunas et al. are inconsistent with the preponderance of scientific evidence .

That makes it seem as if Mann s not claiming anything particularly extraordinary based

on his research.

But Mann claimed in the NYTimes in 1998 that in their Nature study from that year Our

conclusion was that the warming of the past few decades appears to be closely tied to

emission of greenhouse gases by humans and not any of the natural factors." Does that

seem to be careful in the nature of a claim? Respected scientists like Tom Quigley

responded at the time by saying "I think there's a limit to how far you can ever go." As

for using proxy data to detect a man-made greenhouse effect, he said, "I don't think

we're ever going to get to the point where we're going to be totally convincing." These

are two scientists who would agree on the preponderance of evidence and yet they make

different claims about what that preponderance means. There are lots of respected

climatologists who would say Mann has insufficient scientific basis to make that claim.

Would you agree? The Soon Baliunas research is relevant to that element of the debate

what the preponderance of evidence enables us to claim within reason. To that end, I

don t think claims of Soon Baliunas are inconsistent with the preponderance of scientific evidence.

I ll close by saying I m willing to admit that, as someone lacking a PhD, I could be

punching above my weight. But I will ask you a different but related question How much

hope is there for reaching reasonable public policy decisions that affect the lives of

millions if the science upon which those decisions must be made is said to be by

definition beyond the reach of those people?

All best,

Nick

Date: Thu, 16 Oct 2003 08:46:23 -0400

To: "Nick Schulz" <nschulz@techcentralstation.com>

From: "John P. Holdren" <john\_holdren@harvard.edu>

Subject: RE: Harvard Crimson coverage of Soon / Baliunas controversy

Nick--

You ask good questions. I believe the thoughtfulness of your questions and the progress

I believe we are making in this interchange contain the seeds of the answer to

your final question, which, if I may paraphrase just a bit, is whether there's any hope of reaching reasonable public-policy decisions when the details of the science germane to those decisions are impenetrable to most citizens. This is a hard problem. Certainly the difficulty is not restricted to climate science and policy, but applies also to nuclear-weapon science and policy, nuclear-energy science and policy, genetic science and policy, and much more. But I don't think the difficulties are insurmountable. That's why I'm in the business I'm in, which is teaching about and working on the intersection of science and technology with policy. Most citizens cannot penetrate the details of what is known about the how the climate works (and, of course, what is known even by the most knowledgeable climate scientists about this is not everything one would like to know, and is subject to modification by new data, new insights, new forms of analysis). Neither would most citizens be able to understand how a hydrogen bomb works (even if the details were not secret), or what factors will determine the leak rates of radioactive nuclides from radioactive-waste repositories, or what stem-cell research does and promises to be able to do. But, as Amory Lovins once said in addressing the question of whether the public deserved and could play a meaningful role in debates about nuclear-weapon policy, even though most citizens would never understand the details of how nuclear weapons work or are made, "You don't have to be a chicken to know what to do with an egg." In other words, for many (but not all) policy purposes, the details that are impenetrable do not matter. There CAN be aspects of the details that do matter for public policy, of course. In those cases, it is the function and the responsibility of scientists who work across the science-and-policy boundary to communicate the policy implications of these details in ways that citizens and policy makers can understand. And I believe it is the function and responsibility of citizens and policy makers to develop, with the help of scientists and technologists, a sufficient appreciation of how to reach judgments about plausibility and credibility of communications about the science and technology relevant to policy choices so that the citizens and policy makers are NOT disenfranchised in policy decisions where science and technology are germane. How this is best to be done is a more complicated subject than I am prepared to try to explicate fully here. (Alas, I have already spent more time on this interchange than I could really afford from other current commitments.) Suffice it to say, for now, that improving the situation involves increasing at least somewhat, over time, the scientific

literacy of our citizens, including especially in relation to how science works, how to distinguish an extravagant from a reasonable claim, how to think about probabilities of who is wrong and who is right in a given scientific dispute (including the question of burden of proof as you and I have been discussing it here), how consulting and polling experts can illuminate issues even for those who don't understand everything that the experts say, and why bodies like the National Academy of Sciences and the Intergovernmental Panel on Climate Change deserve more credibility on the question of where mainstream scientific opinion lies than the National Petroleum Council, the Sierra Club, or the editorial page of the Wall Street Journal.

Regarding extravagant claims, you continue to argue that Mann et al. have been guilty of this, but the formulation of theirs that you offer as evidence is not evidence of this at all. You quote them from the NYT in 1998, referring to a study Mann and co-authors published in that year, as saying "our conclusion was that the warming of the past few decades appears to be closely tied to emission of greenhouse gases by humans and not any of the natural factors."

and you ask "Does that seem to be careful in the nature of a claim?" My answer is: Yes, absolutely, their formulation is careful and appropriate. Please note that they did NOT say "Global warming is closely tied to emission of greenhouse gases by humans and not any of the natural factors." They said that THEIR CONCLUSION (from a particular, specified study, published in NATURE) was that the warming of THE PAST FEW DECADES (that is, a particular, specified part of the historical record) APPEARS (from the evidence adduced in the specified study) to be closely tied... This is a carefully specified, multiply bounded statement, which accurately reflects what they looked at and what they found. And it is appropriately contingent --"APPEARS to be closely tied" -- allowing for the possibility that further analysis or new data could later lead to a different perspective on what appears to be true.

With respect, it does not require a PhD in science to notice the appropriate boundedness and contingency in the Mann et al. formulation. It only requires an open mind, a careful reading, and a degree of understanding of the character of scientific claims and the wording appropriate to convey them that is accessible to any thoughtful citizen.

That is why I'm an optimist.

You go on to quote the respected scientist "Tom Quigley" as holding a contrary view to that expressed by Mann. But please note that: (1) I don't know of any Tom Quigley working in this field, so I suspect you mean to refer to the prominent climatologist Tom Wigley; (2) the statements you attribute to "Quiqley" do not directly



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contradict the  
careful statement of Mann (that is, it is entirely consistent for Mann to say  
that his  
study found that recent warming appears to be tied to human emissions and for  
Wigley to  
say that that there are limits to how far one can go with this sort of  
analysis, without  
either one being wrong); and (3) Tom Wigley is one of the CO-AUTHORS of the  
resounding  
Mann et al. refutation of Soon and Baliunas (see attached PDF file).  
I hope you have found my responses to be of some value. I now must get on  
with other  
things.

Best,  
John Holdren  
JOHN P. HOLDREN

---

Teresa and John Heinz Professor of Environmental Policy  
& Director, Program in Science, Technology, & Public Policy,  
Belfer Center for Science and International Affairs,  
John F. Kennedy School of Government

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Professor of Environmental Science and Public Policy,  
Department of Earth and Planetary Sciences

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mail: BCSIA, JFK School, 79 JFK St, Cambridge, MA 02138  
phone: 617 495-1464 / fax 617 495-8963  
email: john\_holdren@harvard.edu  
assistant: Patricia\_McLaughlin@ksg.harvard.edu, 617 495-1498

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JOHN P. HOLDREN

---

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Belfer Center for Science and International Affairs,  
John F. Kennedy School of Government

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assistant: Patricia\_McLaughlin@ksg.harvard.edu, 617 495-1498

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3137. 2003-10-16

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date: Thu Oct 16 17:53:52 2003  
from: Mike Hulme <m.hulme@uea.ac.uk>  
subject: Re: reinventing bbc economics coverage

Page 753

cg2003

to: "Asher Minns" <A.Minns@uea.ac.uk>

Fine for me right now. Alex might also be interested.

Mike

At 17:47 16/10/2003 +0100, you wrote:

From: "Asher Minns" <A.Minns@uea.ac.uk>

To: <i.bateman@uea.ac.uk>,  
<tim.oriordan@uea.ac.uk>,  
"Mike Hulme" <m.hulme@uea.ac.uk>,  
<j.kohler@uea.ac.uk>,  
"Bo Kjellen" <Bo.Kjellen@uea.ac.uk>,  
<k.turner@uea.ac.uk>

Subject: reinventing bbc economics coverage

Date: Thu, 16 Oct 2003 17:47:02 +0100

Organization: University of East Anglia

X-Mailer: Microsoft Outlook Express 6.00.2800.1158

Dear Ian, Tim, Mike, Jonathan, Bo, Kerry,

Are you available on Monday 10th November to talk with Vicki Barker from the BBC about how their news service could better represent global economics in their reporting? (There is a little more information below)

I'll nominate 1-2pm lunchtime as a suggestion, but the timing is totally flexible if I find that lunchtime is no good for most.

Neil Adger is away for that date, but please do let me know if I ought to include some other people.

Best wishes,

Asher

>From Vicki Barker:

If we were to reinvent economics coverage from scratch, TODAY, incorporating what we now know (or think we know) about global environmental and economic trends... what would it look like?

In recent years, I have watched an environmental undertow beginning to tug at economies around the world, even as the world's peoples have been awakening to the realities of an increasingly-globalized economy; and I have wondered if current newsgathering practices and priorities are conveying these phenomena as effectively as they could be.

-----  
Mr Asher Minns

Communication Manager

Tyndall Centre for Climate Change Research

[1]www.tyndall.ac.uk

Mob: 07880 547 843

Tel: +44 0 1603 593906

3141. 2003-10-16

cc: <hare@pik-potsdam.de>,<Martin.welp@pik-potsdam.de>, <morgan@wwf.de>

date: Thu, 16 Oct 2003 12:57:37 +0200

from: "Stephan Singer" <SSinger@wwfepo.org>

subject: Re: Strengthening the European Research Area - call for proposals

to: <hourcade@centre-cired.fr>,<grassl@dkrz.de>, <klaus.hasselmann@dkrz.de>,  
<latif@dkrz.de>, <Carlo.Jaeger@pik-potsdam.de>, <Bert.Metz@rivm.nl>,  
<m.hulme@uea.ac.uk>

dear colleagues from the scientific community

i wondered whether we can do something jointly (may it be with selected institutions within or outside of the ECF) to ask for cash from the EU to go for various scenarios reflection our joint NGO goal to not step over the global 2 degree temperature increase threshold from various reasons.

My hunch would be to develop impact scenarios (including monetary cost evaluation?) for differentiating between a global climate system staying below or overshooting 2 degree and also looking into what 2 degree would/could mean for emissions reductions based on various climate sensitivities and resulting required technology pushes and implementations including costs.

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we can talk about that in detail later. key is that WWF alone is not in a position to do this at all and a broader consortium of science/NGOs may be able to get something done whereas our particular role would be more in the public campaign/awareness work of the project and the results. Bill - i include you as a) I think it would be great to get GP on board and b) as you have done already quite remarkable and outstanding work on this one.

so please comment and of course - we are also open to other participants - i think in particular on some technology folks who are well equipped on the renewable/energy efficiency side.

thanks

stephan

Stephan Singer

Head of European Climate and Energy Policy Unit

WWF, the conservation organization

E-mail: [ssinger@wwfepo.org](mailto:ssinger@wwfepo.org)

\*\*\*\*\*

[www.panda.org/epo](http://www.panda.org/epo) - Stay up-to-date with WWF's policy work in the capital of Europe

[www.passport.panda.org](http://www.passport.panda.org) - take action on global conservation issues - have you got your Passport yet?

\*\*\*\*\*

WWF European Policy Office

36 avenue de Tervuren Box 12

1040 Brussels, Belgium

Tel: +32-2-743-8817

Fax: +32-2-743-8819

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Attachment Converted: "c:\eudora\attach\European Research Area call - energy\_1.pdf"

2300. 2003-10-17

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date: Fri, 17 Oct 2003 07:53:10 -0700

from: "Chris Baisan" <[cbaisan@dakotacom.net](mailto:cbaisan@dakotacom.net)>

subject: Re: help with an idea?

to: Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>

Keith,

I am inclined to forward your note to Tony Caprio - any objections?

He has the best temperature sensitive foxtail pine material I am aware of.

I have some sense that there is a change in regional climate patterns prior to 1000AD in the western US. Not sure what or why...

Matt Salzer and Malcolm Hughes are working on 3k yr material from temperature sensitive upper tree-line sites in the west.

John King knows a great deal about the Sierra collections and data.

MaryBeth Keifer and Andrea Loyd-Faste collected the Sierra

Foxtail you referred to.

Chris B.

> Hi Lisa and Chris and Ed

>

> The first point of this message is to ask for access to the raw data

> for the Boreal and Camp Hill Foxtail pine chronologies (Lisa) that I

> believe you and/or your students produced and similar data that you

> may have (Chris). for the area inland of the Santa Barbara Basin ,

> California. I am also trying to stimulate your interest and hopefully

> start a joint collaboration (Lisa , Chris and Ed). Please allow me to

> explain . I was reading some papers on the putative link between North

> Atlantic temperatures (oxygen isotope record from Greenland) and

> climate (bio-turbation index) in the Santa Barbara basin , on the

> 1000-year time scale (papers by Boyle and Leuschner et al. in the

> PAGES QSR Volume published in 2000). It got me to thinking whether a

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> robust regional temperature chronology for North west Scandinavia  
> might show any associations with any climate factors as represented in  
> either high or low elevation tree-ring chronologies in western  
> California , at higher temporal resolution (perhaps decades to  
> century) - and hence whether there is any evidence for a thermohaline  
> link (or other more direct dynamic atmospheric connection) operating  
> on various time scales. Of course there are problems with what  
> specific climate response one would investigate (in terms of season  
> and variable). However, as a first look I compared our Tornetrask  
> temperature reconstruction (JJA in Northern Sweden) with a (very) few  
> series I had for the west US - among which were the chronologies  
> mentioned above from AD 800 that Jan Esper and Ed produced for their  
> Science paper, using data supplied by Lisa I believe .  
> Now I don't actually like the general way they applied the RCS ( -  
> using  
> a very large scale standardisation curve based on disparate data from  
> a very wide expanse of sites across the Northern Hemisphere - but as  
> Ed might say " it seems to work "). However, the association between  
> the Tornetrask series and the curves for Boreal/ Upper Wright have  
> stimulated me to try to look deeper and solicit your interest and  
> help. In my opinion, for the 600-year period between AD 1100 and 1700  
> the similarity in the 5 circa 120-year cycles that make up these  
> series certainly warrant serious further study. The similarity is not  
> apparent before this but the two California series themselves show  
> little agreement in the earlier 300 years of data that I have seen,  
> implying that the common signal at the regional level may not be well  
> represented in either anyway. This could be a standardisation issue  
> though. By producing more robust mean series and especially by  
> extending the series back before the post Christian era we could  
> significantly extend the power of the comparison. I would like to  
> establish well replicated series (using more-local RCS curves based  
> applied to more, and longer, data) for both the Tornetrask (and  
> possibly Northern Finnish) region and the combined set from Upper  
> Wright and Boreal and any other nearby Foxtail data ( from the region  
> of the 118 degrees west 36 degrees north) . We have earlier (than  
> circa AD 800 ) data for Tornetrask and Finland , showing good inter  
> region coherence . If we can establish stronger evidence of a North  
> Atlantic/Eastern Pacific link (at different time scales perhaps) we  
> can look at other high resolution records to establish the nature of  
> the likely forcing and the possible climate dynamic mechanisms. What  
> do you think? Can I play with your data to this end ? Whatever you  
> think , I would appreciate it if you would treat this as confidential  
> and any thoughts on the idea , or pointers to relevant data sets are  
> still welcome.  
> All the very best  
> Keith

> --  
> Professor Keith Briffa,  
> Climatic Research Unit  
> University of East Anglia  
> Norwich, NR4 7TJ, U.K.  
>  
> Phone: +44-1603-593909  
> Fax: +44-1603-507784  
>  
> <http://www.cru.uea.ac.uk/cru/people/briffa/>

> :)) ) )) )) ) )))) ) )) ) )) ) ))

Christopher Baisan  
Sr. Research Specialist  
Laboratory of Tree-Ring Research  
University of Arizona, Tucson 85721

cg2003

email: cbaisan@ltrr.arizona.edu  
tel: 520-621-7681  
Fax: 520-621-8229  
) .)) ) )) ) ) )) ) .) )) ) )) ) ) )) .) ) )) )))

4421. 2003-10-17

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cc: "'Stephen T. Gray'" <sgray@montana.edu>  
date: Fri, 17 Oct 2003 13:57:08 -0600  
from: "Graumlich, Lisa" <lisa@montana.edu>  
subject: RE: help with an idea?  
to: 'Keith Briffa' <k.briffa@uea.ac.uk>, "'cbaisan@ltrr.arizona.edu'"  
<cbaisan@ltrr.arizona.edu>, "'drdendro@ldeo.columbia.edu'"  
<drdendro@ldeo.columbia.edu>

Dear Keith, Chris, and Ed,  
Keith, thanks for getting this conversation going. I would be happy to contribute the CA data to this effort.  
I should also mention that my post-doc, Steve Gray, and I have been exploring some similar ideas and are very interested in contributing to a larger project. Attached is our AGU abstract as well as our reconstruction of the AMO. In particular, Figure A shows North Atlantic (0-70N) SST anomalies extracted from the Kaplan et al. 1998 reanalysis data (in SD units). The bottom graph (Fig B) is our tree-ring based reconstruction of SSTA spanning 1567-1990 AD. The black lines represent annual values and the red line shows a 10-yr spline. The data for the reconstruction come from Eastern North America, Europe, Scandinavia and the Middle East. We are combining these results with our new tree ring data from the inner Mountain West to explore PDO-AMO interactions.  
It strikes me that we might want to have a conference call, or at least some spirited email, to discuss some ideas for collaboration.

With best wishes, Lisa  
Lisa J. Graumlich  
Executive Director, Big Sky Institute  
406/994-5320

-----Original Message-----

From: Keith Briffa [mailto:k.briffa@uea.ac.uk]  
Sent: Friday, October 17, 2003 8:24 AM  
To: lisa@montana.edu; cbaisan@ltrr.arizona.edu; drdendro@ldeo.columbia.edu  
Subject: help with an idea?

Hi Lisa and Chris and Ed

The first point of this message is to ask for access to the raw data for the Boreal and Camp Hill Foxtail pine chronologies (Lisa) that I believe you and/or your students produced and similar data that you may have (Chris). for the area inland of the Santa Barbara Basin, California. I am also trying to stimulate your interest and hopefully start a joint collaboration (Lisa, Chris and Ed). Please allow me to explain.

I was reading some papers on the putative link between North Atlantic temperatures (oxygen isotope record from Greenland) and climate (bio-turbation index) in the Santa Barbara basin, on the 1000-year time scale (papers by Boyle and Leuschner et al. in the PAGES QSR Volume published in 2000).

It got me to thinking whether a robust regional temperature chronology for North west Scandinavia might show any associations with any climate factors as represented in either high or low elevation tree-ring chronologies in western California, at higher temporal resolution (perhaps decades to century) - and hence whether there is any evidence for a thermohaline link (or other more direct dynamic atmospheric connection) operating on various time scales. Of course there are problems with what specific climate response one would investigate (in terms of season and variable). However, as a first look I compared our Tornetrask temperature reconstruction (JJA in Northern Sweden) with a (very) few series I had for the west US - among which were the chronologies mentioned above from AD 800

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that Jan Esper and Ed produced for their Science paper, using data supplied by Lisa I believe .

Now I don't actually like the general way they applied the RCS ( - using a very large scale standardisation curve based on disparate data from a very wide expanse of sites across the Northern Hemisphere - but as Ed might say " it seems to work "). However, the association between the Tornetrask series and the curves for Boreal/ Upper Wright have stimulated me to try to look deeper and solicit your interest and help. In my opinion, for the 600-year period between AD 1100 and 1700 the similarity in the 5 circa 120-year cycles that make up these series certainly warrant serious further study. The similarity is not apparent before this but the two California series themselves show little agreement in the earlier 300 years of data that I have seen, implying that the common signal at the regional level may not be well represented in either anyway. This could be a standardisation issue though. By producing more robust mean series and especially by extending the series back before the post Christian era we could significantly extend the power of the comparison. I would like to establish well replicated series (using more-local RCS curves based applied to more, and longer, data) for both the Tornetrask (and possibly Northern Finnish) region and the combined set from Upper Wright and Boreal and any other nearby Foxtail data ( from the region of the 118 degrees west 36 degrees north) . We have earlier (than circa AD 800 ) data for Tornetrask and Finland , showing good inter region coherence . If we can establish stronger evidence of a North Atlantic/Eastern Pacific link (at different time scales perhaps) we can look at other high resolution records to establish the nature of the likely forcing and the possible climate dynamic mechanisms. What do you think? Can I play with your data to this end ? Whatever you think , I would appreciate it if you would treat this as confidential and any thoughts on the idea , or pointers to relevant data sets are still welcome.

All the very best  
Keith

--

Professor Keith Briffa,  
Climatic Research Unit  
University of East Anglia  
Norwich, NR4 7TJ, U.K.  
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Attachment Converted: "c:\eudora\attach\gary AGU Abstract 2003.doc"

1566. 2003-10-20

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cc: k.briffa@uea.ac.uk

date: Mon, 20 Oct 2003 11:32:32 +0100

from: Phil Jones <p.jones@uea.ac.uk>

subject: Re: Fwd: Re: MBH98

to: "Michael E. Mann" <mann@virginia.edu>, rbradley@geo.umass.edu,  
mhughes@ltrr.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, hfd@ncdc.noaa.gov

Dear All,

I've had several emails from Steve McIntyre. He comes across in these as friendly,

but

then asks for more and more. I have sent him some station temperature data in the past,

but eventually had to stop replying to me. Last time he emailed me directly was in

relation

to the Mann/Jones GRL paper. That time he wanted the series he used. I suspect that

cg2003

he is the person who sent the email around about only 7 of the 23 series used by Ray et al.

being in WDC-Paleo. I told him then that he needs to get in contact with the relevant paleo

people. It seems only Mike, Ray and me got this email from Timo, so I'll forward it.

He names the worst offenders (ie those not putting data on WDC-Paleo) as being

Cook, Mosley-Thompson, Hughes and Briffa !! He clearly should go to a few paleo meetings

to find out what is really out there. Last week I saw the Patzold Bermuda coral record again.

It is now 1000 years long and all there is an unwritten paper !

The second email I'm forwarding is one from Bill Kininmonth. I've met Bill several

times

at WMO meetings and in Australia. Bill has retired now. When I knew him he knew very

little about paleo. I wouldn't bother replying, unless you want to go into chapter and

verse

and don't think through Timo. I would like to believe Bill would be receptive, but it

would take

time. You could suggest, Ray, he reads your book rather than Lamb's, but from his tone

that

might not go down too well ! Both Hubert's books in the early 1990s are basically updates

of his 1974/77 books, with more references and in a chattier style.

Cheers

Phil

At 11:14 19/10/2003 -0400, Michael E. Mann wrote:

FYI--thought you guys should have this (below). This guy "McIntyre" appears to be yet

another shill for industry--he appears to be the one who forwarded the the scurrilous

"climateskeptic" criticisms of the recent Bradley et al Science paper.

Here is an email I sent him a few weeks ago in response to an inquiry. It appears, by

the way, that he has been trying to break into our machine ("multiproxy"). Obviously,

this character is looking for any little thing he can get ahold of. The irony here, of

course, is that simple composites of proxy records (e.g. Bradley and Jones; Mann and

Jones, etc) give very similar results to the pattern reconstruction approaches (Mann et

al EOF approach, Rutherford et al RegEM approach), so anyone looking to criticize the

basic NH temperature history based on details of e.g. the Mann et al '98 methodology

are misguided in their efforts...

The best that can be done is to ignore their desperate emails and, if they manage to

slip something into the peer-reviewed literature, as in the case of Soon & Baliunas,

deal w/ it as we did in that case--i.e., the Eos response to Soon et al---they were

stung badly by that, and the bad press that followed. For those of you who

cg2003

haven't seen

it, I'm forwarding an interesting email exchange from John Holdren of Harvard that I got the other day. He summarized the whole thing very nicely, from an independent perspective...

Cheers,

mike

p.s. I'm setting up my email server so that it automatically rejects emails from the "usual suspects". You might want to do the same. As they increasingly get automatic

reject messages from the scientists, they'll start to get the picture...

Date: Thu, 25 Sep 2003 18:53:33 -0400

To: "Steve McIntyre" <smcintyre@cgxenergy.com>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: MBH98

Bcc: Scott Rutherford <srutherford@gso.uri.edu>, mann@virginia.edu

Dear Mr. McIntyre,

A few of the series terminate prior to the nominal 1980 termination date of the calibration period (the earliest such instance, as you note, is 1971). In such cases, the data were continued to the 1980 boundary by persistence of the final available value. These details in fact, were provided in the supplementary information that accompanied the Nature article. That information is available here (see first paragraph):

[1]<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/MultiProxy/data-supp.html> and here:

[2][http://www.ngdc.noaa.gov/paleo/ei/data\\_supp.html](http://www.ngdc.noaa.gov/paleo/ei/data_supp.html)

The results, incidentally, are insensitive to this step; essentially the same reconstruction is achieved if a calibration period terminating in 1970 (prior to the

termination of any of the proxy series) was used instead.

Owing to numerous demands on my time, I will not be able to respond to further inquiries.

Other researchers have successfully implemented our methodology based on the information

provided in our articles [see e.g. Zorita, E., F. Gonzalez-Rouco, and S.

Legutke,

Testing the Mann et al. (1998) approach to paleoclimate reconstructions in the context

of a 1000-yr control simulation with the ECHO-G Coupled Climate Model, J.

Climate, 16,

1378-1390, 2003.]. I trust, therefore, that you will find (as in this case)

that all

necessary details are provided in the papers we have published or the supplementary

information links provided by those papers.

Best of luck with your work.

Sincerely,

Michael E. Mann

At 05:28 PM 9/25/2003 -0400, Steve McIntyre wrote:

Dear Prof Mann, <?xml:namespace prefix = o ns =

"urn:schemas-microsoft-com:office:office"

/>

Here is the pcproxy.txt file sent to me last April by Scott Rutherford at your direction. It contains some missing data after 1971. Your 1998 paper does not describe

how missing data in this period is treated and I wanted to verify that it is



cg2003

the correct

file. How did you handle missing data in this period? In earlier periods, it looks like you changed the roster of proxies in each of the periods described in the Supplementary Information using only proxies available throughout the entire period. I have obtained quite close replication of the rpc1 in the 20th century by calculating coefficients for the proxies and then calculating the rpc's using the minimization procedures described in MBH98 and the selection of PCs in the Supplementary Information. The reconstruction is less close in earlier periods. I also don't understand the reasoning for reducing the roster of eigenvectors in earlier periods. The description in MBH98 was necessarily very terse and is still very terse in the Supplementary Information; is there any more detailed description of the reconstruction methodology to help me resolve this? Thank

you for your attention.

Yours truly,  
Steve McIntyre,  
Toronto, Canada

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Prof. Phil Jones  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
UK

-----  
1011. 2003-10-21

---

date: Tue, 21 Oct 2003 15:09:10 +0100  
from: Phil Jones <p.jones@uea.ac.uk>  
subject: Fwd: Manuscript.  
to: i.harris@uea.ac.uk, k.briffa@uea.ac.uk  
<x-flowed>

Something like this was submitted to The Holocene early September.  
Phil

>From: "Carolyn Scheurle" <scheurle@uni-bremen.de>  
>To: p.jones@uea.ac.uk  
>Date: Tue, 19 Aug 2003 18:21:06 +0200  
>Subject: Manuscript.

cg2003

>Priority: normal  
>X-mailer: Pegasus Mail for Win32 (v3.12c)  
>  
>Phil,  
>attached is the new version of the manuscript. Now, I took the  
>solar minima out, the periods are not called LIA and modern  
>period anymore, and I also moved or added some sections. I  
>hope that I was able to put the ideas into words correctly, and  
>there are no more misleading words or sentences. Please have a  
>look at the text again and let me know what you criticize, and  
>your decision (if you want to be a co-author or not). Hopefully  
>we can come to an agreement.  
>I'm looking forward to hearing from you within the next days.  
>Regards, Carolyn.  
>  
>  
>Carolyn Scheurle  
>FB 5 - Geowissenschaften  
>Universität Bremen  
>Postfach 33 04 40  
>D-28334 BREMEN  
>  
>Tel: (+49) 0421 218-9132  
>Fax: (+49) 0421 218-8916  
>email: scheurle@uni-bremen.de  
>  
>The following section of this message contains a file attachment  
>prepared for transmission using the Internet MIME message format.  
>If you are using Pegasus Mail, or any another MIME-compliant system,  
>you should be able to save it or view it from within your mailer.  
>If you cannot, please ask your system administrator for assistance.  
>  
> ---- File information -----  
> File: 800yr-Holocene-Aug03b.doc  
> Date: 19 Aug 2003, 18:09  
> Size: 1092608 bytes.  
> Type: Unknown  
Prof. Phil Jones  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
UK

---

</x-flowed>

Attachment Converted: "c:\eudora\attach\800yr-Holocene-Aug03b.doc"

3993. 2003-10-21

---

date: Tue, 21 Oct 2003 12:40:34 +0100 (BST)

from: Itsuki C Handoh <I.Handoh@uea.ac.uk>

subject: HadCM3

to: Tim Osborn <t.osborn@uea.ac.uk>

Dear Tim,

I guess you know that HadCM3 does very bad job over the tropical Atlantic sector. The appalling representation of the interannual variability and its seasonal dependency could be due to a suppressed seasonal phase-locking structure of the Pacific ENSO in the model (compared with observational evidence). We cannot possibly use HadCM3 for seasonal forecasting over the Atlantic-Europe sector, until this

cg2003

problem is sorted out. I provoked COAPEC committee members when I gave a short talk on this issue. Because I didn't get COAPEC core team job (for seasonal forecast), I no longer intend to directly contribute to the modification of HadCM3, but will write up a paper on my analysis of HadCM3 ctrl runs, before I move to Sheffield.

As Adam's project would heavily rely on the control runs, I have been worried about it. We hope that decadal variability produced by the model is not as poor as the interannual counterpart. I have to work on decadal modulation of the interannual variability, though....

Cheers,  
-Itsuki-

---

Dr. Itsuki C. Handoh, Senior Research Associate,  
School of Environmental Sciences, University of East Anglia,  
Norwich NR4 7TJ, United Kingdom.

E-mail: I.Handoh@uea.ac.uk  
Tel: +44(0)1603-592041 (ENV 01.41B)  
Fax: +44(0)1603-591327  
WWW: <http://www.uea.ac.uk/~e096>  
Mobile: 07751-513263

---

On Tue, 21 Oct 2003, Tim Osborn wrote:

> At 21:22 20/10/2003, you wrote:  
> >Adam and I are going to discuss tropical Atlantic variability and darkness  
> >of HadCM3 control runs, in order to minimise potential overlaps between his  
> >and my projects.

>  
> Dear Itsuki,

>  
> I was intrigued to know what "darkness of HadCM3 control runs"  
> means? Could you explain further?

>  
> Cheers

>  
> Tim

>  
>  
> Dr Timothy J Osborn  
> Climatic Research Unit  
> School of Environmental Sciences, University of East Anglia  
> Norwich NR4 7TJ, UK

>  
> e-mail: t.osborn@uea.ac.uk  
> phone: +44 1603 592089  
> fax: +44 1603 507784  
> web: <http://www.cru.uea.ac.uk/~timo/>  
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

>  
>

5162. 2003-10-24

---

date: Fri, 24 Oct 2003 11:37:12 -0400

from: Neil Leary <nleary@agu.org>

subject: Supplemental Grants

to: ian.burton@ec.gc.ca, crrhcr@racsa.co.cr, tom.downing@sei.se,  
saleemul.huq@iied.org, fuj.jaeger@nextra.at, richard.klein@pik-potsdam.de,  
isabelle@enda.sn, harasawa@nies.go.jp, PARRYML@aol.com, anand@cc.iitb.ernet.in,  
bscholes@csir.co.za, Rwatson@worldbank.org, nobre@cptec.inpe.br,  
lal321@hotmail.com, lindam@atd.ucar.edu, Peter.Whetton@csiro.au, m.hulme@uea.ac.uk,  
sberesford@agu.org

To: AIACC Technical Committee

cg2003

Dear Friends,

Attached are two new proposals from AIACC projects for a Supplemental Grant. They are:

AF04, Bob Scholes PI. \$17,000 requested for training workshop on conservation planning.

Funds would be used to bring to the workshop persons from outside South Africa who

have a role and background in conservation planning. Requested funds exceed the \$15,000

budgeted for each project. [Note: Bob is a member of the Technical Committee and is on the

distribution list for this email. Bear that in mind before using "reply all" to broadcast

your comments. If you have confidential comments, please send them to me at <nleary@agu.org>.]

SIS06, Anthony Chen PI. \$15,000 requested for training workshop on modeling epidemiology of

dengue fever and to carryout modeling analysis. The proposal does not identify either

capacity building or stakeholder engagement as an explicit objective. However, it does

include a training workshop and supervision of a student who would conduct a modeling

analysis. Is this satisfactory or should we ask the PI to revise the proposal to better

emphasize and describe the capacity building aspect?

In a week or two I will be drafting a report to USAID on the Supplemental Grant Program.

I'll circulate copies to you. This will give you an overview of what we've approved thus

far. There are still a large number of projects who have not applied for a grant. I plan

to contact them soon to inquire about their intentions for submitting a proposal.

I look forward to your comments on the attached proposals.

Cheers,

Neil

--

Neil A. Leary

Science Director

Assessments of Impacts and Adaptations to Climate Change (AIACC)

The International START Secretariat

2000 Florida Avenue NW, Suite 200

Washington, DC 20009 USA

Phone: 1 202 462 2213

Fax: 1 202 457 5859

Email: nleary@agu.org

Website: www.start.org

Attachment Converted: "c:\eudora\attach\Supplemental\_AF04.doc" Attachment  
Converted:

"c:\eudora\attach\Supplement\_SIS06.doc"

2527. 2003-10-26

---

cc: mann@virginia.edu

date: Sun, 26 Oct 2003 13:47:44 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: CONFIDENTIAL Fwd:

to: Ray Bradley <rbradley@geo.umass.edu>, "Malcolm Hughes"

<mhughes@lrrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Steve Schneider

<shs@stanford.edu>, Tom Crowley <tom@ocean.tamu.edu>, Tom Wigley

<wigley@meeker.UCAR.EDU>, Jonathan Overpeck <jto@u.arizona.edu>, asocci@cox.net,

Michael Oppenheimer <omichael@Princeton.EDU>, Keith Briffa <k.briffa@uea.ac.uk>,

cg2003

Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tim\_Profeta@lieberman.senate.gov, Ben Santer <santer1@llnl.gov>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "Lonnie G. Thompson" <thompson.3@osu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>

Dear All,

This has been passed along to me by someone whose identity will remain in confidence.

Who knows what trickery has been pulled or selective use of data made. Its clear that

"Energy and Environment" is being run by the baddies--only a shill for industry would have

republished the original Soon and Baliunas paper as submitted to "Climate Research" without

even editing it. Now apparently they're at it again...

My suggested response is:

1) to dismiss this as stunt, appearing in a so-called "journal" which is already known to

have defied standard practices of peer-review. It is clear, for example, that nobody we

know has been asked to "review" this so-called paper

2) to point out the claim is nonsense since the same basic result has been obtained by

numerous other researchers, using different data, elementary compositing techniques, etc.

Who knows what sleight of hand the authors of this thing have pulled. Of course, the usual

suspects are going to try to peddle this crap. The important thing is to deny that this has

any intellectual credibility whatsoever and, if contacted by any media, to dismiss this for

the stunt that it is..

Thanks for your help,

mike

two people have a forthcoming 'Energy & Environment' paper that's being unveiled tomoro

(monday) that -- in the words of one Cato / Marshall/ CEI type -- "will claim that Mann

arbitrarily ignored paleo data within his own record and substituted other data for

missing values that dramatically affected his results.

When his exact analysis is rerun with all the data and with no data substitutions, two very large warming spikes will appear that are greater than

the 20th century.

Personally, I'd offer that this was known by most people who understand Mann's

methodology: it can be quite sensitive to the input data in the early centuries.

Anyway, there's going to be a lot of noise on this one, and knowing Mann's very thin

skin I am afraid he will react strongly, unless he has learned (as I hope he has) from

the past...."

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

cg2003

cc: Ray Bradley <rbradley@geo.umass.edu>, Malcolm Hughes  
<mhughes@lrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, tom crowley  
<tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Jonathan Overpeck  
<jto@u.arizona.edu>, <asocci@cox.net>, Michael Oppenheimer  
<omichael@Princeton.EDU>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones  
<p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>,  
<Tim\_Profeta@lieberman.senate.gov>, Ben Santer <santer1@llnl.gov>, Gabi Hegerl  
<hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "Lonnie G. Thompson"  
<thompson.3@osu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>  
date: Sun, 26 Oct 2003 22:19:17 -0800 (PST)  
from: Stephen H Schneider <shs@stanford.edu>  
subject: Re: CONFIDENTIAL Fwd:  
to: "Michael E. Mann" <mann@virginia.edu>

Hang in there Mike, just take pride that you are hurting them and they need to dissemble to get attention. It will fade in time, but the timing is not accidental--all about the McCain-Lieberman climate bill to be voted on this week. It will quiet down soon thereafter, so don't take the bait--just point out soberly why they are wrong and that they have no credible analysis to substitute for yours and the many real scientific investigators who independently do the same kinds of work--we replicate to gain confidence--and come up with similar conclusions. I'll attach my "final" testimony and some answers to Senator McCain's questions motivated by Sen. Inhofe's July28 Senate floor diatribes against me, Tom, you and others--cleverly disguised to say if one reads us between the lines we support THEIR positions. That makes responding in short paragraphs impossible, so my answers are way too long for Congress, but to give a paragraph would leave them guessing who was right and what happened. If anyone has any edits to suggest, I need them by Monday afternoon at the latest as COB monday McCain staff puts it up on the record I understand. Even though I am virtually certain we shall lose on McCain-Lieberman, they are forcing Senators to go on record for for against sensible climate policy--a non trivial price some may pay politically if they guess wrong what it means for their re-election (another reason why CATO et al are so shrill right now because this is a real threat to them and anything goes for them right now, including lies, character assassinations etc--again, take no bait!). Such "fun", Cheers,  
Steve

PS Tom, I presume you got plenty of questions too? Send me yours when you get a chance.

On Sun, 26 Oct 2003, Michael E. Mann wrote:

> Dear All,

>

> This has been passed along to me by someone whose identity will remain in  
> confidence.

>

> who knows what trickery has been pulled or selective use of data made. Its clear  
> that "Energy and Environment" is being run by the baddies--only a shill for  
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> to "Climate Research" without even editing it. Now apparently they're at it  
> again...

>

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>

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> that nobody we know has been asked to "review" this so-called paper

>

> 2) to point out the claim is nonsense since the same basic result has been  
> obtained by numerous other researchers, using different data, elementary  
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>

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cg2003

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> any media, to dismiss this for the stunt that it is..

>  
> Thanks for your help,

> mike

>  
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> being unveiled tomoro (monday) that -- in the words of one Cato /  
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> data within his own record and substituted other data for missing  
> values that dramatically affected his results.

> When his exact analysis is rerun with all the data and with no  
> data substitutions, two very large warming spikes will appear that are  
> greater than the 20th century.

> Personally, I'd offer that this was known by most people who  
> understand Mann's methodology: it can be quite sensitive to the input  
> data in the early centuries. Anyway, there's going to be a lot of  
> noise on this one, and knowing Mann's very thin skin I am afraid he  
> will react strongly, unless he has learned (as I hope he has) from the  
> past...."

>  
-----  
> Professor Michael E. Mann  
> Department of Environmental Sciences, Clark Hall  
> University of Virginia  
> Charlottesville, VA 22903

> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
> http://www.evsc.virginia.edu/faculty/people/mann.shtml

-----  
Stephen H. Schneider, Professor  
Dept. of Biological Sciences  
Stanford University  
Stanford, CA 94305-5020 U.S.A.  
Tel: (650)725-9978  
Fax: (650)725-4387  
shs@stanford.edu

Attachment Converted: "c:\eudora\attach\McCainQuestions for Schneider.doc"  
Attachment Converted: "c:\eudora\attach\Schneider--McCain-LiebermanTestimony  
10-01-03.doc"

20. 2003-10-27

cc: "Michael E. Mann" <mann@virginia.edu>, <asocci@cox.net>, Gabi Hegerl  
<hegerl@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa  
<k.briffa@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Mike MacCracken  
<mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Ray Bradley  
<rbradley@geo.umass.edu>, Ben Santer <santer1@llnl.gov>, "Lonnie G. Thompson"  
<thompson.3@osu.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>,  
<Tim\_Profeta@lieberman.senate.gov>, tom crowley <tom@ocean.tamu.edu>, Kevin  
Trenberth <trenbert@cgd.ucar.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley  
<wigley@meeker.UCAR.EDU>

date: Mon, 27 Oct 2003 12:00:59 -0800 (PST)

from: Stephen H Schneider <shs@stanford.edu>

subject: Re: Fw: New Study Questions Kyoto Global Warming Data

to: apetsonk@environmentaldefense.org

Hello all. Ah ha--the latest idiot--McKittrick--reenters the scene. He and  
another incompetent had a book signing party at the US Capitol--Mike

cg2003

MacCracken went and he can tell you about it--last summer. Mckitrick also had an article--oped, highly refereed of course--in the Canadian National Post on June 4 this year. Here is the URL that worked back then:  
<http://www.nationalpost.com/search/site/story.asp?id=045D5241-FD00-4773-B816-76222A771778>

It was a scream. He argued there is no such thing as global temperature change, just local--all natural variability mostly. To prove this he had a graph of temperature trends in Erie Pennsylvania for the past 50 years (this is from memory) which showed a cooling. That alone proves nothing, but when reading the caption I noticed the trend was for temperature in October and November!! So one station for two months constituted his "refutation" of global warming--another even dumber than Lomborg economist way out of depth and polemicizing. I showed it to a class of Stanford freshman, and one of them said: "I wonder how many records for various combinations of months they had to run through to find one with a cooling trend?" The freshman was smarter than this bozo. It is important to get that op-ed to simply tell all reporters how unbelievably incompetent he is, and should not even be given the time of day over climate issues, for which his one "contribution" is laughably incompetent. By the way, the Henderson/Castles stuff he mentions is also mostly absurd, but that is a longer discussion you all don't need to get into--check it out in the UCS response to earlier Inhofe polemics with answers I gave them on Henderson/Castles if you want to know more about their bad economics on top of their bad climate science. "Enjoy", Cheers, Steve  
PS More on Henderson/Castles can be downloaded from my still password protected website--still being edited:  
[stanford.edu/~shs/](http://stanford.edu/~shs/)

ID: Please

Password: comment

On Mon, 27 Oct 2003 apetsonk@environmentaldefense.org wrote:

>  
> Michael, this was on the Heartland Institute's website - it is an article  
> by one of the coauthors of the new study, ross mckitrick. Perhaps you have  
> run across this before?

>  
> Annie Petsonk  
> International Counsel  
> Environmental Defense  
> Tel: 202-387-3500 ext. 3323

>  
>  
> (See attached file: mckitrick.pdf)

-----  
Stephen H. Schneider, Professor  
Dept. of Biological Sciences  
Stanford University  
Stanford, CA 94305-5020 U.S.A.  
Tel: (650)725-9978  
Fax: (650)725-4387  
[shs@stanford.edu](mailto:shs@stanford.edu)

Attachment Converted: "c:\eudora\attach\RPC-UCS response-final.doc"

1283. 2003-10-27

cc: Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, Ray Bradley <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, Malcolm Hughes <[mhughes@ltrr.arizona.edu](mailto:mhughes@ltrr.arizona.edu)>, Mike MacCracken <[mmaccrac@comcast.net](mailto:mmaccrac@comcast.net)>, Steve Schneider <[shs@stanford.edu](mailto:shs@stanford.edu)>, tom crowley <[tom@ocean.tamu.edu](mailto:tom@ocean.tamu.edu)>, Tom Wigley <[wigley@meeker.UCAR.EDU](mailto:wigley@meeker.UCAR.EDU)>, Jonathan Overpeck <[jto@u.arizona.edu](mailto:jto@u.arizona.edu)>, asocci@cox.net, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, Tim Profeta <[timprofeta@lieberman.senate.gov](mailto:timprofeta@lieberman.senate.gov)>, Ben Santer <[santer1@llnl.gov](mailto:santer1@llnl.gov)>, Gabi Hegerl <[hegerl@duke.edu](mailto:hegerl@duke.edu)>, Ellen Mosley-Thompson <[thompson.4@osu.edu](mailto:thompson.4@osu.edu)>, "Lonnie G. Thompson" <[thompson.3@osu.edu](mailto:thompson.3@osu.edu)>, Kevin Trenberth <[trenbert@cgd.ucar.edu](mailto:trenbert@cgd.ucar.edu)>



cg2003

date: Mon, 27 Oct 2003 12:55:04 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Re: CONFIDENTIAL Fwd:  
to: Michael Oppenheimer <omichael@Princeton.EDU>  
Nick Katz of USA Today called me asking about the story. I gave him the low down  
as spelled  
out in the "response" I drafted earlier, and encouraged him to get second  
opinions by other  
scientists in the field, naming some of you. Hope you all don't mind...  
mike  
At 09:52 AM 10/27/2003 -0500, Michael Oppenheimer wrote:

Mike:  
From my point of view, the critical thing now is that you NOT spend too much  
time or  
psychological energy on this stuff. I know that's a tall order, but it could  
effectively take you away from your real business for much too long, and  
potentially  
exact an even higher price upon you. Your decision to let others do the heavy  
lifting to  
the extent that's feasible is wise. Also, at this point, it is unlikely that  
the vote  
would be affected later this week by these goings-on. All this sounds very  
familiar,  
reeking of what happened to Ben. Their real objective is to neutralize you by  
dragging  
you into time-consuming and eventually fruitless fighting, and to send a  
message to  
other scientists who might be tempted to enter the public arena.

Michael  
"Michael E. Mann" wrote:  
Thanks Phil,  
I guess we just have to wait and see what happens w/ this,  
mike  
At 01:50 PM 10/27/2003 +0000, Phil Jones wrote:

Mike,  
Steve McIntyre is the person who asked me for the series he couldn't get  
from the  
M+J  
article. Told him I couldn't send them - back in August. He's sent emails to  
the Finn  
(Timo)  
saying some of the series weren't available, blaming us for using data that  
aren't  
readily  
available.

Some years ago I sent him loads of temperature stations and discussed  
homogeneity  
issues,  
but never heard anything else.

From my recollection of the emails I suspect the article isn't likely to  
be up to  
much.

Cheers  
Phil

At 08:35 27/10/2003 -0500, Michael E. Mann wrote:  
Thanks Phil,  
Got your email just as I sent off my latest.  
I agree fully with what you say--it is very difficult to repeat such an  
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exactly, and the real point here is, who knows what this guy (Steven  
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know who the supposed 2nd author is) actually did. The Mann et al '99 paper  
was clear

cg2003

that the results were sensitive to a small number of skillful predictors prior to AD

1400, and that non-climate biases had to be corrected for in some of the longer series

to get a skillfully cross-validated reconstruction. Without knowing what the guy did,

I'm guessing that he doesn't even demonstrate that his alternative "reconstruction"

passes cross-validation. If not, its all moot...

But more fundamentally, this wasn't submitted to a legitimate peer-reviewed scientific

journal. Its a social science journal, and one that has shown a disdain for peer review

(e.g. in publishing the Soon et al Climate Research paper essentially in its original

unedited form--and see the recent documented comments of the editor).

I agree this might blow over, but the folks in DC, such as McCain and Lieberman, who

are fighting to represent what the legitimate scientific community has to say, need to

be prepared in case the special interests try to use this. Hence, the short response I

sent out.

cheers,

mike

At 01:23 PM 10/27/2003 +0000, Phil Jones wrote:

Mike,

Depending exactly on what it says I suggest we should do our best to ignore it. E&E

is

edited ( a very loose use of the word) by Sonia Boehmer-Christiansen, who's generally

involved, in some way, in all skeptic stuff here in Britain.

It is rather odd that the email said the two had rerun his (Mann's) exact analysis

and got

quite different results. I know I couldn't do this, as when Keith, Tim and me wanted to

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MBH98/99

then for M+J03 in GRL, there at least three series that are not available for use,

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So let's wait to see what it says. Suggested response would seem follow response 2.

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My suggested response is:

1) to dismiss this as stunt, appearing in a so-called "journal" which is already known

to have defied standard practices of peer-review. It is clear, for example, that nobody

we know has been asked to "review" this so-called paper

2) to point out the claim is nonsense since the same basic result has been obtained by

numerous other researchers, using different data, elementary compositing techniques, etc.

who knows what sleight of hand the authors of this thing have pulled. Of course, the

usual suspects are going to try to peddle this crap. The important thing is to deny that

this has any intellectual credibility whatsoever and, if contacted by any media, to

dismiss this for the stunt that it is..

Thanks for your help,

mike

two people have a forthcoming 'Energy & Environment' paper that's being unveiled tomorrow

(Monday) that -- in the words of one Cato / Marshall/ CEI type -- "will claim that Mann

arbitrarily ignored paleo data within his own record and substituted other data for

missing values that dramatically affected his results.

When his exact analysis is rerun with all the data and with no data substitutions, two very large warming spikes will appear that are greater than the 20th

century.

Personally, I'd offer that this was known by most people who understand Mann's

methodology: it can be quite sensitive to the input data in the early centuries.

Anyway, there's going to be a lot of noise on this one, and knowing Mann's very thin

skin I am afraid he will react strongly, unless he has learned (as I hope he has) from

the past...."

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Prof. Phil Jones

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University of East Anglia

Norwich

Email p.jones@uea.ac.uk

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cg2003  
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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1846. 2003-10-27

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cc: mann@virginia.edu  
date: Mon, 27 Oct 2003 13:50:06 +0000  
from: Phil Jones <p.jones@uea.ac.uk>  
subject: Re: CONFIDENTIAL Fwd:  
to: "Michael E. Mann" <mann@virginia.edu>, Ray Bradley <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Steve Schneider <shs@stanford.edu>, tom crowley <tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Jonathan Overpeck <jto@u.arizona.edu>, asocci@cox.net, Michael Oppenheimer <omichael@Princeton.EDU>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tim\_Profeta@lieberman.senate.gov, Ben Santer <santer1@llnl.gov>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "Lonnie G. Thompson" <thompson.3@osu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>

Mike,

Steve McIntyre is the person who asked me for the series he couldn't get from the M+J article. Told him I couldn't send them - back in August. He's sent emails to the Finn

(Timo)

saying some of the series weren't available, blaming us for using data that aren't readily available.

Some years ago I sent him loads of temperature stations and discussed homogeneity issues,

but never heard anything else.

From my recollection of the emails I suspect the article isn't likely to be up to much.

Cheers

cg2003

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cg2003

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2165. 2003-10-27

cc: "Michael E. Mann" <mann@virginia.edu>, <asocci@cox.net>, Gabi Hegerl  
<hegerl@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa  
<k.briffa@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Mike MacCracken  
<maccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Ray Bradley  
<rbradley@geo.umass.edu>, Ben Santer <santer1@llnl.gov>, "Lonnie G. Thompson"  
<thompson.3@osu.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>,  
<Tim\_Profeta@lieberman.senate.gov>, tom crowley <tom@ocean.tamu.edu>, Kevin  
Trenberth <trenbert@cgd.ucar.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley  
<wigley@meeker.UCAR.EDU>

date: Mon, 27 Oct 2003 18:04:09 -0800 (PST)

from: Stephen H Schneider <shs@stanford.edu>

subject: Re: Fw: New Study Questions Kyoto Global Warming Data

to: apetsonk@environmentaldefense.org

Hello all again. I found a copy of the Mckitrick thing where he "refutes"  
global warming with a 60 year record of Oct/Nov temperatures at Erie PA.

It should make for amusing reading and wonderful ammunition for reporters  
and congressional staffers who call any of us about Mckitrick and his

buffonery. Cheers, Steve

On Mon, 27 Oct 2003, Stephen H Schneider wrote:

> Hello all. Ah ha--the latest idiot--Mckitrick--reenters the scene. He and  
> another incompetent had a book signing party at the US Capitol--Mike  
> MacCracken went and he can tell you about it--last summer. Mckitrick also  
> had an article--oped, highly refereed of course--in the Canadian National  
> Post on June 4 this year. Here is the URL that worked back then:

>  
> <http://www.nationalpost.com/search/site/story.asp?id=045D5241-FD00-4773-B816-76222A771778>

>  
> It was a scream. He argued there is no such thing as global temperature  
> change, just local--all natural variability mostly. To prove this he had a  
> graph of temperature trends in Erie Pennsylvania for the past 50 years  
> (this is from memory) which showed a cooling. That alone proves nothing,  
> but when reading the caption I noticed the trend was for temperature in  
> October and November!! So one station for two months constituted his  
> "refutation" of global warming--another even dumber than Lomborg economist

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> way out of depth and polemicizing. I showed it to a class of Stanford  
> freshman, and one of them said: "I wonder how many records for various  
> combinations of months they had to run through to find one with a cooling  
> trend?" The freshman was smarter than this bozo. It is important to get  
> that op-ed to simply tell all reporters how unbelievably incompetent he  
> is, and should not even be given the time of day over climate issues, for  
> which his one "contribution" is laughably incompetent. By the way, the  
> Henderson/Castles stuff he mentions is also mostly absurd, but that is a  
> longer discussion you all don't need to get into--check it out in the UCS  
> response to earlier Inhofe polemics with answers I gave them on  
> Henderson/Castles if you want to know more about their bad economics on  
> top of their bad climate science. "Enjoy", Cheers, Steve  
> PS More on Henderson/Castles can be downloaded from my still password  
> protected website--still being edited:  
> stanford.edu/~shs/  
> ID: Please  
> Password: comment  
>  
> On Mon, 27 Oct 2003 apetsonk@environmentaldefense.org wrote:  
>  
> >  
> > Michael, this was on the Heartland Institute's website - it is an article  
> > by one of the coauthors of the new study, ross mckitrick. Perhaps you have  
> > run across this before?  
> >  
> > Annie Petsonk  
> > International Counsel  
> > Environmental Defense  
> > Tel: 202-387-3500 ext. 3323  
> >  
> >  
> > (See attached file: mckitrick.pdf)  
>  
> -----  
> Stephen H. Schneider, Professor  
> Dept. of Biological Sciences  
> Stanford University  
> Stanford, CA 94305-5020 U.S.A.  
>  
> Tel: (650)725-9978  
> Fax: (650)725-4387  
> shs@stanford.edu  
>  
> -----  
Stephen H. Schneider, Professor  
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Stanford University  
Stanford, CA 94305-5020 U.S.A.  
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shs@stanford.edu  
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3961. 2003-10-27

cc: Phil Jones <p.jones@uea.ac.uk>, Ray Bradley <rbradley@geo.umass.edu>, Malcolm  
Hughes <mhughes@ltrr.arizona.edu>, Steve Schneider <shs@stanford.edu>, tom crowley  
<tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Jonathan Overpeck  
<jto@u.arizona.edu>, <asocci@cox.net>, Keith Briffa <k.briffa@uea.ac.uk>, Tim  
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<thompson.4@osu.edu>, "Lonnie G. Thompson" <thompson.3@osu.edu>, Kevin Trenberth



cg2003

<trenbert@cgd.ucar.edu>

date: Mon, 27 Oct 2003 11:18:31 -0500

from: Mike MacCracken <maccrac@comcast.net>

subject: Re: CONFIDENTIAL Fwd:

to: "Michael E. Mann" <mann@virginia.edu>, Michael Oppenheimer

<omichael@Princeton.EDU>

Hi Mike--Back from a couple of weeks away and trying to catch up.

I thought it might be of interest that OMB has put out draft guidelines on what is supposed

to constitute peer review for agency reports or reports they rely on about major issues--and just to note that there is a sense (in at least one agency and some

CCSP

staff), supported by some discussions with the author of the guidelines, that most or all

climate materials would need to live up to the guidelines.

Two points on what is there:

a. Frankly, there are a lot of problems with the proposed guidelines in that they seek to

have all reviewers essentially be so unconflicted that no one who knows anything is likely

to qualify or be willing to be a reviewer. The whole notion of the content of the review

comment mattering more than its source is totally lost (much less offering any guidance on

how seriously agencies need to take any comments). I imagine those on journal review boards

or serving as editors (like Steve) might want to check out the proposal and see how their

guidelines compare--and how they think the OMB guidelines might work (or not work) for

them.

b. However, the guidelines do presume that journal peer review provides a challengeable

qualification to the paper. Interestingly, there is no indication that the journal must be

of any given quality or follow any approved procedures, so what is sure, if these

guidelines go through, is that there will be a rash of new journals created, all of little

stature.

I have made these and a number of related points to the OMB in response to their solicitation of comments. And now, the NRC is going to hold a meeting on them

(see email

notice below--though without form), as apparently I have not been alone in objecting. I'll

be on travel but did send in my letter to OMB (copy available on request--since it is

several pages long, I won't burden everyone with the letter). By the way, comments deadline

has been extended to Dec 15 to accommodate NRC workshop, I presume.

In any case, this matter of what constitutes "peer review" is coming up for attention by

this Admin--so perhaps this effort of skeptics to get things into what they call peer-reviewed journals is so they can be cited more directly by the Admin.

Mike MacCracken

Subject: PEER REVIEW OF REGULATORY SCIENCE WORKSHOP-November 18, 2003

Dear Colleague:

In light of expressions of interest and concern from within the research community regarding the newly issued "Proposed OMB Bulletin and Supplemental Information Quality Guidelines: Peer Review and Information Quality," and with the encouragement of U.S. Office of Management and Budget (OMB), The National Academies Science, Technology, and Law (STL) Program intends to hold a one-day public workshop on Tuesday, November 18, 2003, in Washington, D.C.,

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at which federal agencies subject to these new standards can share their views and hear ideas and concerns from each other and from external communities, including academic researchers, about the implications, merits, and practicality of the proposed bulletin. The workshop is intended to assist the agencies in developing their agency-specific comments on the bulletin and ultimately in developing their peer review procedures. Further details on the agenda will be sent out in late October. Please free to forward this announcement to other interested parties.

If you would like to attend the workshop, please fill out the attached registration form and fax to (202-334-2530).

For more information please contact:

Contact Name: Stacey Speer

Email: [sspeer@nas.edu](mailto:sspeer@nas.edu)

Phone: 202-334-1713

Fax: 202-334-2530

Attachment Converted: "c:\eudora\attach\peer\_review\_andnfo\_quality.pdf"

4327. 2003-10-27

cc: Ray Bradley <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, Malcolm Hughes <[mhughes@lrrr.arizona.edu](mailto:mhughes@lrrr.arizona.edu)>, Mike MacCracken <[mmacrac@comcast.net](mailto:mmacrac@comcast.net)>, tom crowley <[tom@ocean.tamu.edu](mailto:tom@ocean.tamu.edu)>, Tom Wigley <[wigley@meeker.UCAR.EDU](mailto:wigley@meeker.UCAR.EDU)>, Jonathan Overpeck <[jto@u.arizona.edu](mailto:jto@u.arizona.edu)>, <[asocci@cox.net](mailto:asocci@cox.net)>, Michael Oppenheimer <[omichael@Princeton.EDU](mailto:omichael@Princeton.EDU)>, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, <[Tim\\_Profeta@lieberman.senate.gov](mailto:Tim_Profeta@lieberman.senate.gov)>, Ben Santer <[santer1@llnl.gov](mailto:santer1@llnl.gov)>, Gabi Hegerl <[hegerl@duke.edu](mailto:hegerl@duke.edu)>, Ellen Mosley-Thompson <[thompson.4@osu.edu](mailto:thompson.4@osu.edu)>, "Lonnie G. Thompson" <[thompson.3@osu.edu](mailto:thompson.3@osu.edu)>, Kevin Trenberth <[trenbert@cgd.ucar.edu](mailto:trenbert@cgd.ucar.edu)>, <[wuebbles@gale.atmos.uiuc.edu](mailto:wuebbles@gale.atmos.uiuc.edu)>, <[m.hulme@uea.ac.uk](mailto:m.hulme@uea.ac.uk)>, <[rpomerance@aecs-inc.org](mailto:rpomerance@aecs-inc.org)>, <[j.salinger@niwa.co.nz](mailto:j.salinger@niwa.co.nz)>, <[mark.eakin@noaa.gov](mailto:mark.eakin@noaa.gov)>, <[solomon@a1.noaa.gov](mailto:solomon@a1.noaa.gov)>  
date: Mon, 27 Oct 2003 09:20:48 -0800 (PST)  
from: Stephen H Schneider <[shs@stanford.edu](mailto:shs@stanford.edu)>  
subject: Re: CONFIDENTIAL Fwd:  
to: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>

Hi all. All you had to say, Mike, was Sonja B-C, and it explains it all. She is an ideological zealot--not for the coal industry, but for anything "anti-establishment". She is one of the "deconstructionists" that seem to plague the UK--you know, that science is "socially constructed" a focus group of farmers and miners are as qualified to assess risk as the IPCC. She hates "elitism"--us that is--since we have entry barriers to join the technical debate (you have to know something, such a concept!) --and that is anti-democratic. They are decidedly non-empirical, referencing social theory rather than doing in-depth case study analyses. They wouldn't get tenure here as dog catcher, but some places that groove on post-modernism and other intellectually bankrupt fads actually hire such folks as professors. I once had a ten e-mail dialogue with her because she loves Timo H and his gang of retired closed minds and their little chat network, and I tried for weeks to explain to her why they were not cute and didn't deserve a forum until they had disciplined and competent arguments. All she could say is that they were "fresh thinkers" and the principle of contrarian welcoming was more important--democratic participation in science rather than elitist inside-the-club peer review etc. The problem isn't them--they're hopeless and intellectually miniscule--it is that they lend the imprimatur of peer reviewed legitimacy to trash. The bottom line is we can't make the world safe against polemics, and Mike O. is right--that you can't give yourselves ulcers trying to argue with the likes of Sonja, CATO etc. I agree you need a defense, but a well written rebuttal and a careful selection of who you spend time talking to--national media, not every backwater political reporter who calls and will turn it into a "he-said/she-said" circus--would be my advice. Save most of your energy for the high priority fights and, every once and again, doing more science--you know, the elitist stuff that we pretend should be done with

cg2003

rigor and standards before you earn the right to being heard. Cheers,  
Steve

On Mon, 27 Oct 2003, Michael E. Mann wrote:

> Thanks Steve,

>

> Yes, the timing is suspicious at best--this appears yet another act of  
> desperation by those losing the battle on the scientific front. I will,  
naturally,

> resist the bait, while nonetheless providing the material necessary to defend my  
> colleagues and me against the scurrilous claims. Any efforts that others can make  
> in confronting the claims helps to deny them what they're looking for (entraining  
> me into the fray).

>

> For this reason, I'm asking my friends and colleagues to consider responding on  
my

> behalf if contacted for their opinions on the matter.

>

> I've prepared a response (attached word file) to what I anticipate the paper  
> claims. I may expand upon this once a copy of the paper is available, but I  
> believe it may be important to have an initial response on hand.

>

> I anticipate that the mainstream media will ignore their attempts at promoting  
> this. But CATO, API, etc. will certainly be trying to promote this inside the  
> beltway as McCain-Lieberman grows near,

>

> Best regards,

>

> mike

>

> p.s. I've attached the official E&E "Mission Statement" written by Sonja  
> Boehmer-Christiansen, which I believe many of you will find eye opening...

>

> At 10:19 PM 10/26/2003 -0800, Stephen H Schneider wrote:

> Hang in there Mike, just take pride that you are hurting them and they  
> need to dissemble to get attention. It will fade in time, but the  
> timing

> is not accidental--all about the McCain-Lieberman climate bill to be  
> voted

> on this week. It will quiet down soon thereafter, so don't take the  
> bait--just point out soberly why they are wrong and that they have no  
> credible analysis to substitute for yours and the many real scientific  
> investigators who independently do the same kinds of work--we

> replicate to  
> gain confidence--and come up with similar conclusions. I'll attach my  
> "final" testimony and some answers to Senator McCain's questions  
> motivated

> by Sen. Inhofe's July28 Senate floor diatribes against me, Tom, you  
> and

> others--cleverly disguised to say if one reads us between the lines we  
> support THEIR positions. That makes responding in short paragraphs  
> impossible, so my answers are way too long for Congress, but to give  
> a paragraph would leave them guessing who was right and what happened.

>

> If  
> anyone has any edits to suggest, I need them by Monday afternoon at  
> the

> latest as COB monday McCain staff puts it up on the record I  
> understand.

> Even though I am virtually certain we shall lose on McCain-Lieberman,  
> they

> are forcing Senators to go on record for for against sensible climate  
> policy--a non trivial price some may pay politically if they guess  
> wrong

> what it means for their re-election (another reason why CATO et al are

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> so  
> shrill right now because this is a real threat to them and anything  
> goes  
> for them right now, including lies, character assassinations etc--again,  
> take no bait!). SUCH "fun", Cheers,  
> Steve  
> PS TOM, I presume you got plenty of questions too? Send me yours when  
> you  
> get a chance.

> On Sun, 26 Oct 2003, Michael E. Mann wrote:

> Dear All,  
>  
> This has been passed along to me by someone whose identity will  
> remain in  
> confidence.  
>  
> who knows what trickery has been pulled or selective use of data  
> made. Its clear  
> that "Energy and Environment" is being run by the baddies--only a  
> skill for  
> industry would have republished the original Soon and Baliunas paper  
> as submitted  
> to "Climate Research" without even editing it. Now apparently  
> they're at it  
> again...

> My suggested response is:

>  
> 1) to dismiss this as stunt, appearing in a so-called "journal"  
> which is already  
> known to have defied standard practices of peer-review. It is clear,  
> for example,  
> that nobody we know has been asked to "review" this so-called paper

>  
> 2) to point out the claim is nonsense since the same basic result  
> has been  
> obtained by numerous other researchers, using different data,  
> elementary  
> compositing techniques, etc.

>  
> who knows what sleight of hand the authors of this thing have  
> pulled. Of course,  
> the usual suspects are going to try to peddle this crap. The  
> important thing is to  
> deny that this has any intellectual credibility whatsoever and, if  
> contacted by  
> any media, to dismiss this for the stunt that it is..

>  
> Thanks for your help,

>  
> mike

>  
>  
> two people have a forthcoming 'Energy & Environment' paper  
> that's  
> being unveiled tomoro (monday) that -- in the words of one  
> Cato /  
> Marshall/ CEI type -- "will claim that Mann arbitrarily  
> ignored paleo  
> data within his own record and substituted other data for  
> missing

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> values that dramatically affected his results.  
> When his exact analysis is rerun with all the data and  
> with no  
> data substitutions, two very large warming spikes will appear  
> that are  
> greater than the 20th century.  
> Personally, I'd offer that this was known by most  
> people who  
> understand Mann's methodology: it can be quite sensitive to  
> the input  
> data in the early centuries. Anyway, there's going to be a lot  
> of  
> noise on this one, and knowing Mann's very thin skin I am  
> afraid he  
> will react strongly, unless he has learned (as I hope he has)  
> from the  
> past...."

---

> Professor Michael E. Mann  
> Department of Environmental Sciences, Clark Hall  
> University of Virginia  
> Charlottesville, VA 22903

---

> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434)  
> 982-2137  
> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>

> -----  
> Stephen H. Schneider, Professor  
> Dept. of Biological Sciences  
> Stanford University  
> Stanford, CA 94305-5020 U.S.A.

> Tel: (650)725-9978  
> Fax: (650)725-4387  
> shs@stanford.edu

---

> Professor Michael E. Mann  
> Department of Environmental Sciences, Clark Hall  
> University of Virginia  
> Charlottesville, VA 22903

---

> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>

> -----  
> Stephen H. Schneider, Professor  
> Dept. of Biological Sciences  
> Stanford University  
> Stanford, CA 94305-5020 U.S.A.  
> Tel: (650)725-9978  
> Fax: (650)725-4387  
> shs@stanford.edu

4854. 2003-10-27

---

cc: mann@virginia.edu  
date: Mon, 27 Oct 2003 08:35:36 -0500  
from: "Michael E. Mann" <mann@virginia.edu>

cg2003

subject: Re: CONFIDENTIAL Fwd:

to: Phil Jones <p.jones@uea.ac.uk>, Ray Bradley <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@lrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Steve Schneider <shs@stanford.edu>, tom crowley <tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Jonathan Overpeck <jto@u.arizona.edu>, asocci@cox.net, Michael Oppenheimer <omichael@Princeton.EDU>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tim\_Profeta@lieberman.senate.gov, Ben Santer <santer1@llnl.gov>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "Lonnie G. Thompson" <thompson.3@osu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>

Thanks Phil,

Got your email just as I sent off my latest.

I agree fully with what you say--it is very difficult to repeat such an analysis exactly,

and the real point here is, who knows what this guy (Steven McIntyre--I don't know who the

supposed 2nd author is) actually did. The Mann et al '99 paper was clear that the results

were sensitive to a small number of skillful predictors prior to AD 1400, and that

non-climate biases had to be corrected for in some of the longer series to get a skillfully

cross-validated reconstruction. Without knowing what the guy did, I'm guessing that he

doesn't even demonstrate that his alternative "reconstruction" passes cross-validation. If

not, its all moot...

But more fundamentally, this wasn't submitted to a legitimate peer-reviewed scientific

journal. Its a social science journal, and one that has shown a disdain for peer review

(e.g. in publishing the Soon et al Climate Research paper essentially in its original

unedited form--and see the recent documented comments of the editor).

I agree this might blow over, but the folks in DC, such as McCain and Lieberman, who are

fighting to represent what the legitimate scientific community has to say, need to be

prepared in case the special interests try to use this. Hence, the short response I sent

out.

cheers,

mike

At 01:23 PM 10/27/2003 +0000, Phil Jones wrote:

Mike,

Depending exactly on what it says I suggest we should do our best to ignore it. E&E

is

edited ( a very loose use of the word) by Sonia Boehmer-Christiansen, who's generally

involved, in some way, in all skeptic stuff here in Britain.

It is rather odd that the email said the two had rerun his (Mann's) exact analysis

and got

quite different results. I know I couldn't do this, as when Keith, Tim and me wanted to

do

some comparisons with MBH98 a few years ago a few of the series could not be made

available. I'm not trying to make any sort of point here, just to state that repeating

an

analysis with exactly the same data is normally very difficult. Missing

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values is an

odd

phrase also, as all the series used are complete from first to last year. If it isn't

MBH98/99

then for M+J03 in GRL, there at least three series that are not available for use,

without

contacting the authors of the original papers.

So let's wait to see what it says. Suggested response would seem follow response 2.

Cheers

Phil

At 13:47 26/10/2003 -0500, Michael E. Mann wrote:

Dear All,

This has been passed along to me by someone whose identity will remain in confidence.

who knows what trickery has been pulled or selective use of data made. Its clear that

"Energy and Environment" is being run by the baddies--only a shell for industry would

have republished the original Soon and Baliunas paper as submitted to "Climate Research"

without even editing it. Now apparently they're at it again...

My suggested response is:

1) to dismiss this as stunt, appearing in a so-called "journal" which is already known

to have defied standard practices of peer-review. It is clear, for example, that nobody

we know has been asked to "review" this so-called paper

2) to point out the claim is nonsense since the same basic result has been obtained by

numerous other researchers, using different data, elementary compositing techniques,

etc.

who knows what sleight of hand the authors of this thing have pulled. Of course, the

usual suspects are going to try to peddle this crap. The important thing is to deny that

this has any intellectual credibility whatsoever and, if contacted by any media, to

dismiss this for the stunt that it is..

Thanks for your help,

mike

two people have a forthcoming 'Energy & Environment' paper that's being unveiled tomoro

(monday) that -- in the words of one Cato / Marshall/ CEI type -- "will claim that Mann

arbitrarily ignored paleo data within his own record and substituted other data for

missing values that dramatically affected his results.

When his exact analysis is rerun with all the data and with no data substitutions, two very large warming spikes will appear that are greater than the 20th century.

Personally, I'd offer that this was known by most people who understand Mann's

methodology: it can be quite sensitive to the input data in the early centuries.

Anyway, there's going to be a lot of noise on this one, and knowing Mann's very thin

skin I am afraid he will react strongly, unless he has learned (as I hope he has) from

the past...."

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Prof. Phil Jones  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
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Charlottesville, VA 22903

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

5080. 2003-10-27

---

date: Mon, 27 Oct 2003 20:10:18 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: final for the night  
to: asocci@cox.net, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck  
<jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Malcolm Hughes  
<mhughes@lrrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Phil Jones  
<p.jones@uea.ac.uk>, Ray Bradley <rbradley@geo.umass.edu>, Ben Santer  
<santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, "Lonnie G. Thompson"  
<thompson.3@osu.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>,  
Tim Profeta@lieberman.senate.gov, tom crowley <tom@ocean.tamu.edu>, Kevin Trenberth  
<trenbert@cgd.ucar.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley  
<wigley@meeker.UCAR.EDU>, apetsonk@environmentaldefense.org, "Jim Salinger"  
<j.salinger@niwa.co.nz>, Annie\_Petsonk@environmentaldefense.org,  
rpomerance@aecs-inc.org, mann@virginia.edu

Dear All,

This is my final email for a while--hopefully this is the end of this. Attached is a revised and expanded statement of response to the paper, having seen more of the details

now. please feel free to use it in any way that may be helpful.

After having gone over their posted analysis and procedure, I've refined my comments

further--some of my previous conclusions about what they have done were not correct. The

main problem is a bit more subtle, probably having to do w/ how the Reconstructed Principal

Components are scaled. My attached comments deal with both the scientific problems w/ what

they have done, and the problems w/ the lack of any legitimate scientific peer review...

The mainstream media doesn't seem to be biting at this thing, but you can be sure there

will be an attempt to use this inside the beltway...

cheers,  
mike



cg2003

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
Attachment Converted: "c:\eudora\attach\EandEResponse-revised.doc"

4469. 2003-10-28

---

date: Tue, 28 Oct 2003 08:45:38 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Fwd: Re: Fw: New Study Questions Kyoto Global Warming Data  
to: asocci@cox.net, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck  
<jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Malcolm Hughes  
<mhughes@ltrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Phil Jones  
<p.jones@uea.ac.uk>, Ray Bradley <rbradley@geo.umass.edu>, Ben Santer  
<santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, "Lonnie G. Thompson"  
<thompson.3@osu.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>,  
Tim\_Profeta@lieberman.senate.gov, tom crowley <tom@ocean.tamu.edu>, Kevin Trenberth  
<trenbert@cgd.ucar.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley  
<wigley@meeker.UCAR.EDU>, apetsonk@environmentaldefense.org, "Jim Salinger"  
<j.salinger@niwa.co.nz>

Dear All,  
Hopefully, the last time. After considerable review of what the authors have  
done, and some  
feedback from others, I've revised and streamlined my "response". The authors  
convolution  
of incommensurate scaling factors with the posted Mann et al eigenvectors,  
resulting from  
their use of a different instrumental temperature data set and inconsistent  
normalization  
convention (standard deviation of detrended series was used in Mann et al while  
non-detrended gridpoint standard deviations have been used by the authors to  
unnormalize  
the Mann et al eigenvectors--this leads to a spatially-variable biased  
enhancement of  
variance) appears likely to be the culprit.  
Its clearly an error. There may be several others yet uncovered, but this alone  
certainly  
invalidates what they have done.  
The attached, hopefully final version of my "response" should suffice.  
Thanks,  
mike

Date: Mon, 27 Oct 2003 15:50:38 -0500  
To: mann@virginia.edu  
From: "Michael E. Mann" <mann@virginia.edu>  
Subject: Re: Fw: New Study Questions Kyoto Global Warming Data  
Cc: asocci@cox.net, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck  
<jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Malcolm Hughes  
<mhughes@ltrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Phil Jones  
<p.jones@uea.ac.uk>, Ray Bradley <rbradley@geo.umass.edu>, Ben Santer  
<santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, "Lonnie G. Thompson"  
<thompson.3@osu.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>,  
Tim\_Profeta@lieberman.senate.gov, tom crowley <tom@ocean.tamu.edu>, Kevin  
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<trenbert@cgd.ucar.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley  
<wigley@meeker.UCAR.EDU>, apetsonk@environmentaldefense.org, "Jim Salinger"  
<j.salinger@niwa.co.nz>

Dear All,  
One small correction upon further reading--they did appear to use subsets of

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eigenvectors corresponding to those nominally used by Mann et al (1998) upon further reading of their description.

However, the eigenvectors aren't the same, because the surface temperature dataset is a different one from that used by Mann et al! So the eigenvectors #s that they used are not the same as the corresponding eigenvectors used by Mann et al (1998).

Again, because they didn't apply the objective criterion themselves (they appear to have simply used the same nominal eigenvectors, but of a different(!) temperature dataset), they didn't appear to discover the problem...

mike

At 02:38 PM 10/27/2003 -0500, Michael E. Mann wrote:

Dear All,

The link below has been passed along to me by a colleague (whose name has been expunged to protect his identity):

Based on the description of data and method provided, which the authors claim is a complete (!) description [this is important], I have confidently concluded that they have neither used the same data (instrumental or proxy) but, more problematically, the same method as Mann et al (1998). In fact, there is a huge problem in the statistical calibration procedure they used, which omits an essential step used by Mann et al (1998) to protect against statistical overfitting in the presence of an increasingly sparse proxy data network back in time.

1) They used the wrong instrumental data set.

Mann et al (1998) used the older version of the CRU instrumental surface temperature data set that dates back to 1854; they apparently used the newer version of the data set that goes back to 1856 which Phil published and updated to in the mid 90s--the data sets are actually significantly different in places. They differ significantly, for example, in where they have missing data. Phil can provide people details. Thus, the eigenvectors are assured as being different, and the missing data are different. It would have been surprising if they had found otherwise!

2) They used the wrong proxy data.

The authors apparently used an excel spreadsheet version of the MBH98 data that my associate Scott Rutherford had sent them. It appears that the data got shifted and scrambled a bit in the process of being converted to an excel spreadsheet or upon being downloaded or opened. This would explain the numerous transcription errors the authors find in the file. Of course, we used the uncorrupted data in our study. These

ascii versions of the data have always been publicly available on our computer "holocene". Had

the authors used the (correct) ascii series on the data set, they wouldn't have encountered transcription errors. From what I But they appear to have had a

corrupted

version of the data. The authors then describe an elaborate effort to download suitable

approximate versions of the proxy data series they couldn't get ahold of. In many cases,

these appear to be substantially different versions of the proxy series than the ones

used by Mann et al (1998) But again, I don't think this matters.

3) They did not implement the Mann et al approach!!

From the description of the method provided, it appears that the authors skipped the

essential step of (1) applying an objective criterion (i.e., Presidendorffer's Rule N as

used in Mann et al, '98) to determine the optimal size N of the subset of the full (16)

candidate instrumental principal component series to retain in the calibration of the

proxy data and (2) optimizing the calibration resolved variance with respect to all

subsets of the leading PC series of size N. These crucial aspects of the procedure were

clearly layed out in Mann et al (1998), and is perhaps one of the most essential

steps---it is only the application of this objective criterion that prevents an obvious

statistical overfitting problem--the authors \*always\* appear to use a subset of all 16

PC series! However, the criterion used by Mann et al (1998) dictated the retention of

a maximum of 11 PC series, only a few PC series prior to AD 1600, and only one prior to

AD 1450. So the authors appear to have tried to fit 16 PC series to the reconstruction

from AD 1400-AD 1450, when an objective criterion would only dictate 1!

This is a really basic statistical error, and its likely this massive overfitting that

is responsible for the wild behavior in their reconstruction prior to about AD 1600.

Can't beleive they made such a basic error? See for yourself:

[1]<http://www.climate2003.com/computations.html>

Any statistical climatologists worth their salt would have picked this up. But I believe

the paper wasn't even reviewed!

4) Their result is, not surprisingly, wacky

The resulting reconstruction they show, with an enormous warm anomaly during the 15th

and 16th centuries, looks like nothing ever produced in any objective estimate of past

Northern Hemisphere temperature trends that I'm familiar with. I guess they could term

this the "Renaissance Warm Period", but of course no other model or empirical Northern

Hemisphere temperature reconstruction looks anything like this. Frankly, the ridiculous

result should have let them know they did something wrong.

Mike,

This seems to be the HTML version of McIntyre's paper. (I got this unsolicited from Gene Avrett, Soon & Baliunas's boss at Harvard.)

----- Original Message -----

From: "Eugene H. Avrett"

To:

Date: Mon, 27 Oct 2003 12:09:22 -0500

Subject: article in Energy and Environment

cg2003

-----

Dear

You may be interested in the article by McIntyre and McKittrick just published in Energy and Environment which questions the validity of the Mann et al. (1998) study that provided the basis for the claim that 20th century warming is unprecedented.

See [2]<http://www.uoguelph.ca/~rmckittri/research/trc.html>.

Yours sincerely,

Gene Avrett

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

---

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Charlottesville, VA 22903

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e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\EandEResponse-final.doc"

901. 2003-10-29

---

cc: raymond s.bradley <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, Steve Schneider <[shs@stanford.edu](mailto:shs@stanford.edu)>, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, mhughes@lrr.arizona.edu, [mann@multiproxy.evsc.virginia.edu](mailto:mann@multiproxy.evsc.virginia.edu), Scott Rutherford <[srutherford@rwu.edu](mailto:srutherford@rwu.edu)>

date: Wed, 29 Oct 2003 12:32:25 -0500

from: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>

subject: Re: FW: The Mann et al "Hockey Stick" Corrected II

to: Mike MacCracken <[mmacrac@comcast.net](mailto:mmacrac@comcast.net)>

p.s. Mike, any appearance of "gloating" over the mistake that had been made by Scott, was

incorrectly perceived. I was a bit excited, I suppose, about having established the fact

that I knew we hadn't, in fact, made a mistake--very relieved, and perhaps gloating a bit

with respect to that. Sorry this was wrongly taken by some...

But the authors were given the correct information, the publically available link, and I

think Scott is sending out a full transcript of the exchanges between them which will make

it clear that Scott's efforts were in good faith. Scott--I didn't receive this yet. Can you

make sure these folks get this...

mike

At 09:12 AM 10/29/2003 -0500, Mike MacCracken wrote:

A good example of why making sure they got good data would have relieved everyone of

having to try to catch up with this wildfire of misinformation.

Mike

-----  
 From: Timo Hämeranta <timo.hameranta@pp.inet.fi>  
 Date: Wed, 29 Oct 2003 11:31:13 +0200  
 To: "Akio Kitoh" <kitoh@mri-jma.go.jp>, "Alan Robock"  
 <robock@envsci.rutgers.edu>,  
 "Alexey Fedorov" <alexey@princeton.edu>, "Anders J. Noren"  
 <anders.noren@alumni.carleton.edu>, André L. Berger <berger@astr.ucl.ac.be>,  
 André W.  
 Droxler <andre@rice.edu>, Andreas Indermühle <indermuehle@climate.unibe.ch>,  
 "Andrei P.  
 Sokolov" <sokolov@MIT.EDU>, "Andrew M. Vogelmann" <avogelmann@ucsd.edu>,  
 "Andrew Weaver"  
 <weaver@ocean.seos.uvic.ca>, "Anil K. Gupta" <anilg@gg.iitkgp.ernet.in>,  
 "Antonio J.  
 Busalacchi Jr." <tonyb@essic.umd.edu>, "Atsumu Ohmura"  
 <ohmura@geo.umnw.ethz.ch>, "Atul  
 K. Jain" <jain@uiatma.atmos.uiuc.edu>, "Benjamin Giese" <b-giese@tamu.edu>,  
 "Bernhard  
 Stauffer" <stauffer@climate.unibe.ch>, "Bert Bolin"  
 <bert.r@osteraker.mail.telia.com>,  
 "Bette L. Otto-Bliesner" <ottobli@ucar.edu>, "Bo Nordell"  
 <Bo.Nordell@sb.luth.se>, "Bob  
 Thunell" <thunell@geol.sc.edu>, "C. D. Keeling" <cdkeeling@ucsd.edu>, "Carl  
 Wunsch"  
 <cwunsch@mit.edu>, "Carolus J. Schrijver" <schryver@lmsal.com>, "Caspar M.  
 Ammann"  
 <ammann@ucar.edu>, "Chris E. Forest" <ceforest@mit.edu>, "Christian Pfister"  
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Subject: The Mann et al "Hockey Stick" Corrected II

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McIntyre, Stephen and Ross Mckittrick, 2003. Corrections to the Mann et. al.

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Now, three interesting pictures:

- 1) The well-known Mann et al. <sup>3</sup>Hockey stick<sup>2</sup>, adapted by the IPCC
- 2) The original Mann et al. reconstruction and the correction made
- 3) Both, using 20 year running mean

The discrepancy is, well, astonishing .

The same data, but results differ?

I hope Mann et al will response revealing their methodologies and analysing,  
whether

there are differences in methodologies used.

All the best

Timo Hämeranta

xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx

Timo Hämeranta, M.LL.

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"To dwell only on horror scenarios of the future  
shows only a lack of imagination". (Kari Enqvist)  
"If the facts change, I'll change my opinion."  
What do you do, Sir" (John Maynard Keynes)  
xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx

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from: "Michael E. Mann" <mann@virginia.edu>  
subject: Re: Advising British Government on paleoreconstructions  
to: peter.stott@metoffice.com, peter.stott@metoffice.com

Hi Peter,  
Oops, sent the other emails to you w/out sending this explanation first...  
Thanks for getting in touch w/ me on this...  
its taken us a couple days to get to the bottom of their error, but we've got it! We're all  
drafting an op-ed reply for "USA Today" as I write this...  
Bottom line--they didn't use the correct proxy data. Rather than using the publically  
posted version of the proxy data, they used an excel file that they had requested from my  
associate Scott Rutherford in which multiple series were inadvertently overprinted into  
single data columns. This renders the proxy data series for the period prior to about 1700  
largely useless, and the series prior to 1600 or so completely meaningless. Thus, they're  
wacky result for the earlier centuries in particular.  
So, their results are completely wrong, and E&E will need to retract the paper. I'm forwarding more details now...

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we think of this scientifically.

Thanks !

Peter

--

~~~~~  
Dr. Peter Stott Climate Scientist
Met Office Hadley Centre for Climate Prediction and Research
London Road Bracknell Berkshire RG12 2SY United Kingdom
Tel: +44 (0)1344 854011 Fax: +44 (0)1344 854898
E-mail: peter.stott@metoffice.com [1]<http://www.metoffice.com>
~~~~~

From 10 November  
Address :  
Met Office Hadley Centre (Reading Unit)  
Meteorology Building, University of Reading  
Reading RG6 6BB  
Tel: +44 1189 378 5613  
NOTE WILL ALSO BE AT EXETER PART OF EACH WEEK  
Mobile: 07753880683

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1666. 2003-10-29

---

date: Wed, 29 Oct 2003 08:38:54 +0000  
from: Phil Jones <p.jones@uea.ac.uk>  
subject: Fwd: Re: STOP THE PRESS!  
to: k.briffa@uea.ac.uk  
<x-flowed>  
>Date: Tue, 28 Oct 2003 19:53:52 -0800 (PST)  
>From: Stephen H Schneider <shs@stanford.edu>  
>To: "Michael E. Mann" <mann@virginia.edu>  
>cc: Richard Kerr <rkerr@aaaas.org>, Andy Revkin <anrevk@nytimes.com>,  
> David Appell <appell@nasw.org>,  
> <Annie\_Petsonk@environmentaldefense.org>,  
> Mike MacCracken <mmaccrac@comcast.net>,  
> Michael Oppenheimer <omichael@Princeton.EDU>,  
> "Socci.Tony-epamail.epa.gov" <Socci.Tony@epamail.epa.gov>,  
> <Tim\_Profeta@lieberman.senate.gov>, <rbradley@geo.umass.edu>,  
> <mhughes@ltrr.arizona.edu>, Jonathan Overpeck <jto@u.arizona.edu>,  
> Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford  
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> Gabi Hegerl <hegerl@duke.edu>, tom crowley <tom@ocean.tamu.edu>,  
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> Stefan Rahmstorf <rahmstorf@pik-potsdam.de>,  
> Gavin Schmidt <gavin@isis.giss.nasa.gov>,  
> Rob Dunbar <dunbar@stanford.edu>, <zubeke@onid.orst.edu>,  
> Ross Gelbspan <ross@theworld.com>, Ben Santer <santer1@llnl.gov>,  
> <thompson.4@osu.edu>, <thompson.3@osu.edu>  
>Subject: Re: STOP THE PRESS!  
>  
>Good, Mike, we scientists need to work hard to find fair and effective  
>boiled down statments that convey both urgency and uncertainty and explain  
>complexity with simple methphors--as long as we have back up details in  
>books, websites, papers etc.  
> Speaking of available data, I note the USA Today column said you did not  
>make your data available--please be sure that charge is clarified in your

cg2003

>summary of this affair. Cheers, Steve  
>PS This is what Schulz wrote about you and data availability--if false it  
>gives the USA Today the obligation to give you a rebuttal letter:  
>\*\*\*\*\*  
>In an interview, Mckitrick said, "If a study is going to be the basis for  
>a major policy decision, then the original data must be disseminated and  
>the results have to be reproducible. That's why in our case we have posted  
>everything online and invite outside scrutiny."  
>  
>Mann never made his data available online - nor did many of the earlier  
>researchers whose data Mann relied upon for his research. That by itself  
>raises questions about the U.N. climate-change panel's scientific process.  
>\*\*\*\*\*  
>Hello all,  
> OK, back to me again. You also need to remind our audiences that IPCC is  
>not a research agency--IPCC does assessment of others work, and it is not  
>responsible to put data on websites etc. In fact, governments have  
>specifically told us NOT to do original research, just assesment of  
>research. It does not prohibit us from, as individual scientists,  
>publishing scientific research relevant to what IPCC would like to assess,  
>but then the IPCC process will subject such work to massive peer  
>review--with Review Editors watching. So there is no "scientific process"  
>at IPCC strictly speaking, just a scientific asesment process. This may  
>seem subtle, but the IPCC--a UN agency, with political baggage at least in  
>the US--is an assessment, not research, organization by design. Cheers,  
>Steve  
>  
>  
>On Tue, 28 Oct 2003, Michael E. Mann wrote:  
>  
> > Thanks Steve,  
> >  
> > I plan to work w/ the staffers to try boil this down to its most basic  
> > terms...  
> >  
> > Of course, the proxy data were available uncorrupted on our anonymous ftp  
> > site--the authors chose not to use that, and instead requested a  
> > spreadsheet version from my associated (Scott). Its not his fault that  
> > there were some problems with that particular file--the authors could  
> > have done numerous things to confirm the possible sources of the obvious  
> > problems w/ the file that they note in their 'paper'.  
> >  
> > This will be an important point to convey to folks.  
> >  
> > This is one of the worst examples yet (and we've had some good ones  
> > recently) of a disingenuous/deficient/absent peer review coupled with an  
> > irresponsible editor..  
> >  
> > mike  
> >  
> > At 07:10 PM 10/28/2003 -0800, Stephen H Schneider wrote:  
> > Hello all. Interesting tale--why we have competent peer  
> > review at  
> > competent journals, and why professional courtesy is always  
> > to run  
> > heterodox results by the orthodox for private comments before  
> > going  
> > public--unless the motivation isn't science, but a big  
> > spalsh. Too bad for  
> > them--the wrong guys will belly-flop (couldn't have happened  
> > to a nicer  
> > bunch of prevaricators!). By the way, I give it a 50%  
> > (Bayesian priors)

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> > subjective probability they will accuse you of deliberately  
> > misleading  
> > them or deliberately preventing replication by "independent"  
> > scientists  
> > and the only reason they did this was to smoke you out. From  
> > them, expect  
> > anything. Can you explain this to Senator McCain's folks so  
> > they  
> > understand the complexities and professional courtesy/peer  
> > review issues?  
> > This stuff is not very sound bite friendly and needs some  
> > prethinking to  
> > put it simply and clearly so it can be useful in the debate  
> > held by  
> > non-scientist debaters. Good luck, Steve  
> >  
> > On Tue, 28 Oct 2003, Michael E. Mann wrote:  
> >  
> > > Dear Friends and Colleagues,  
> > >  
> > > I've got a story with a very happy ending to tell. I't  
> > will take a bit  
> > > of patience to get through the details of the story, but I  
> > think its  
> > > worth it.  
> > >  
> > > By the way, please keep this information confidential for  
> > about the next  
> > > day or so.  
> > >  
> > > OK, well its about 48 hours since I first had the chance to  
> > review the  
> > > E&E paper by M&M. Haven't had a lot of sleep, but I have  
> > had a lot of  
> > > coffee, and my wife Lorraine has been kind enough to allow  
> > me to stay  
> > > perpetually glued to the terminal. So what has this effort  
> > produced?  
> > >  
> > > well, upon first looking at what the authors had done, I  
> > realized that  
> > > they had used the wrong CRU surface temperature dataset  
> > (post 1995  
> > > version) to calculate the standard deviations for use in  
> > un-normalizing  
> > > the Mann et al (1998) EOF patterns. Their normalization  
> > factors were  
> > > based on Phil's older dataset. The clues to them should  
> > have been that a)  
> > > our data set goes back to 1854 and theirs only back to 1856  
> > and (b) why  
> > > are 4 of the 1082 Mann et al (1998) gridpoints missing??  
> > [its because  
> > > the reference periods are different in the two datasets,  
> > which leads to a  
> > > different spatial pattern of missing values]. So they had  
> > used the wrong  
> > > temperature standard deviations to un-normalize our EOFs in  
> > the process  
> > > of forming the surface temperature reconstruction. And I  
> > thought to  
> > > myself, hmm--this could lead to some minor problems, but I  
> > don't see how

> > > they get this divergence from the Mann et al (1998)  
 > > estimate that  
 > > > increases so much back in time, and becomes huge before  
 > > 1500 or so. That  
 > > > can't be it, can it?  
 > > >  
 > > > Then I uncovered that they had used standard deviations of  
 > > the raw  
 > > > gridpoint temperature series to un-normalize the EOFs,  
 > > while we had  
 > > > normalized the data by the detrended standard deviations.  
 > > Either  
 > > > convention can be justified, but you can't mix and  
 > > match--which is what  
 > > > they effectively did by adopting our EOFs and PCs, and  
 > > using their  
 > > > standard deviations. And I thought, hmm--this could  
 > > certainly lead to an  
 > > > artificial inflation of the variance in the reconstruction  
 > > in general,  
 > > > and this could give an interesting spatial pattern of bias  
 > > as well (which  
 > > > might have an interesting influence on the areally-weighted  
 > > hemispheric  
 > > > mean). But I thought, hmm, this can't really lead to that  
 > > tremendous  
 > > > divergence before 1500 that the authors find. I was still  
 > > scratching my  
 > > > head a bit at this point.  
 > > >  
 > > > Then I read about the various transcription errors, values  
 > > being shifted,  
 > > > etc. that the authors describe as existing in the dataset.  
 > > And I thought,  
 > > > hmm, that sounds like an excel spread sheet problem, not a  
 > > problem w/ the  
 > > > MBH98 proxy data set. It started to occur to me at this  
 > > point that there  
 > > > might be some problems w/ the excel spreadsheet data that  
 > > my colleague  
 > > > Scott Rutherford had kindly provided the authors at their  
 > > request. But  
 > > > these problems sounded pretty minor from the authors'  
 > > description, and  
 > > > the authors described a procedure to try to fix any  
 > > obvious  
 > > > transcription errors, shifted cell values, etc. So I  
 > > thought, hmm, they  
 > > > might not have fixed things perfectly, and that could also  
 > > lead to some  
 > > > problems. But I still don't see how they get that huge  
 > > divergence back in  
 > > > time from this sort of error...  
 > > >  
 > > > Still scratching my head at this point...Then finally this  
 > > afternoon,  
 > > > some clues. After looking at their on-line description one  
 > > more time, I  
 > > > became disturbed at something I read. The data matrix  
 > > they're using has  
 > > > 112 columns! Well that can't be right! That's can't  
 > > constitute the Mann  
 > > > et al (1998) dataset. There are considerably more than that

> > number of  
 > > > independent proxy indicators necessary to reproduce the  
 > > stepwise Mann et  
 > > > al reconstruction. Something is amiss!  
 > > >  
 > > > well, 112 is the number of proxy indicators used back to  
 > > 1820. But some  
 > > > of these indicators are principal components of regional  
 > > sub-networks  
 > > > (e.g. the Western U.S. ITRDB tree-ring data) to make the  
 > > dataset more  
 > > > manageable in size, and those principal components (PCs) are  
 > > unique to the  
 > > > time interval analyzed. So there is some set of PC series  
 > > for the  
 > > > 1820-1980 period. Farther back in time, say, back to 1650  
 > > there are fewer  
 > > > data series the regional sub-networks. So we recalculate a  
 > > completely  
 > > > different EOF/PC basis set for that period, and that  
 > > constitutes an  
 > > > additional, unique set of proxy indicators that are  
 > > appropriate for a  
 > > > reconstruction of the 1650-1980 period. PC #1 from one  
 > > interval is not  
 > > > equivalent to PC#1 from a different interval. This turns  
 > > out to be the  
 > > > essential detail. A reconstruction back to 1820  
 > > calibrated against the  
 > > > 20th century needs to make use of the unique set of proxy  
 > > PCs available  
 > > > for the 1820-1980 period. A reconstruction back to 1650  
 > > calibrated  
 > > > against the 20th century needs to make use of the  
 > > independent (smaller)  
 > > > set of PC series available for the 1650-1980 period, and so  
 > > on, back to  
 > > > 1400.  
 > > >  
 > > > So there have to be significantly more than 112 series  
 > > available to  
 > > > perform the iterative, stepwise reconstruction approach of  
 > > Mann et al  
 > > > (1998), because each sub interval actually has a unique set  
 > > of PC series  
 > > > representations of various proxy sub-networks. Then it  
 > > started to hit  
 > > > me. The PC#1 series calculated for networks of similar  
 > > size (say, the  
 > > > network available back to 1820 and that available back to  
 > > 1750) should be  
 > > > similar. But as the sub-network gets sparser back in time,  
 > > the PC#1  
 > > > series will resemble less and less the PC#1 series of the  
 > > denser networks  
 > > > available at later times. PC#1 of the western ITRDB  
 > > tree-ring calculated  
 > > > for the 1400-1980 period will bear almost no resemblance  
 > > to the PC#1  
 > > > series of the western N.Amer ITRDB data calculated for the  
 > > 1820-1980  
 > > > period during their interval (1820-1980) of mutual overlap.  
 > > >



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> > > Then it really hit me. What--just what--if the proxy data  
> > had been  
> > > pigeonholed into a 112 column matrix by the following  
> > (completely  
> > > inappropriate!) procedure: What if it had been decided that  
> > there would  
> > > only be 1 column for "PC #1 of the Western ITRDB tree ring  
> > data", even  
> > > though that PC reflects something completely different over  
> > each  
> > > sub-interval. Well, that can't be done in a reasonable way.  
> > But it can be  
> > > done in an \*unreasonable\* way: by successively overprinting  
> > the data in  
> > > that column as one stores the PCs from later and later  
> > intervals. So a  
> > > given column would reflect PC#1 of the 1400-1980 data from  
> > 1400-1450,  
> > > PC#1 of the 1450-1980 from 1450-1500, PC#1 of the 1500-1980  
> > data for  
> > > 1500-1650, PC#1 of the 1650-1980 data for 1650-1750, etc.  
> > and so on. In  
> > > this process, the information necessary to calibrate the  
> > early PCs would  
> > > be obliterated with each successive overprint. The  
> > resulting 'series'  
> > > corresponding to that column of the data matrix, an amalgam  
> > of  
> > > increasingly unrelated information down the column, would  
> > be completely  
> > > useless for calibration of the earlier data. A  
> > reconstruction back to AD  
> > > 1400 would be reconstructing the PC#1 of the 1400-1450  
> > interval based on  
> > > calibration against the almost entirely unrelated PC#1 of  
> > the 1820-1980  
> > > interval. The reconstruction of the earliest centuries  
> > would be based on  
> > > a completely spurious calibration of an unrelated PC of a  
> > much later  
> > > proxy sub network. And I thought, gee, what if Scott (sorry  
> > Scott), had  
> > > \*happened\* to do this in preparing the excel file that the  
> > authors used.  
> > > Well it would mean that, progressively in earlier  
> > centuries, one would  
> > > be reconstructing an apple, based on calibration against  
> > an orange. It  
> > > would yield completely meaningless results more than a few  
> > centuries ago.  
> > > And then came the true epiphany--ahhh, this could lead to  
> > the kind of  
> > > result the authors produced. In fact, it seemed to me that  
> > this would  
> > > almost \*insure\* the result that the authors get--an  
> > increasing divergence  
> > > back in time, and total nonsense prior to 1500 or so. At  
> > this point, I  
> > > knew that's what Scott must have done. But I had to  
> > confirm.  
> > >  
> > > I simply had to contact Scott, and ask him: Scott, when you  
> > prepared that

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> > > excel file for these guys, you don't suppose by any chance  
> > that you might  
> > have....  
> > >  
> > > And, well, I think you know the answer.  
> > >  
> > > So the proxy data back to AD 1820 used by the authors may  
> > by-in-large be  
> > > correct (aside from the apparent transcription/cell shift  
> > errors which  
> > > they purport to have caught, and fixed, anyway). The data  
> > become  
> > > progressively corrupted in earlier centuries. By the time  
> > one goes back  
> > > to AD 1400, the 1400-1980 data series are, in many cases,  
> > entirely  
> > > meaningless combinations of early and late information, and  
> > have no  
> > > relation to the actual proxy series used by Mann et al  
> > (1998).  
> > >  
> > > And so, the authors results are wrong/meaningless/useless.  
> > The mistake  
> > > made insures, especially, that the estimates during the  
> > 15th and 16th  
> > > centuries are entirely spurious.  
> > >  
> > > So whose fault is this? Well, the full, raw ascii proxy  
> > data set has been  
> > > available on our anonymous ftp site  
> > > ftp://holocene.evsc.virginia.edu/pub/MBH98/  
> > > and the authors were informed of this in email  
> > correspondence. But they  
> > > specifically requested that the data be provided to them in  
> > excel format.  
> > > And Scott prepared it for them in that format, in good  
> > faith--but  
> > > overlooked the fact that all of the required information  
> > couldn't  
> > > possibly be fit into a 112 column format. So the file Scott  
> > produced was  
> > > a complete corruption of the actual Mann et al proxy data  
> > set, and  
> > > essentially useless, transcription errors, etc. aside. The  
> > authors had  
> > > full access to the uncorrupted data set. We therefore take  
> > no  
> > > reasonability for their use of corrupted data.  
> > >  
> > > One would have thought that the authors might have tried to  
> > reconcile  
> > > their completely inconsistent result prior to publication.  
> > One might have  
> > > thought that it would at least occur to them as odd that  
> > the Mann et al  
> > > (1998) reconstruction is remarkably similar to entirely  
> > independent  
> > > estimates, for example, by Crowley and Lowery (2000). Could  
> > both have  
> > > made the same supposed mistake, even though the data and  
> > method are  
> > > entirely unrelated. Or might M&M have made a mistake? Just  
> > possibly,

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> > > perhaps???

> > > Of course, a legitimate peer-review process would have caught this problem. In fact, in about 48 hours if I (or probably, many of my colleagues) had been given the opportunity to review the paper. But that isn't quite the way things work at "E&E" I guess. I guess there may just be some corruption of scientific objectivity when a journal editor seems more interested in politics than science.

> > > The long and short of this. I think it is morally incumbent upon E&E to publish a full retraction of the M&M article immediately. Its unlikely that they'll do this, but its reasonable to assert that it would be irresponsible for them not to if the issue arises.

> > > I think that's the end of the story. Please, again, keep this information under wraps for next day or two. Then, by all means, feel free to disseminate this information as widely as you like...

> > > Mike

> > >

---

> > > Professor Michael E. Mann  
> > > Department of Environmental Sciences, Clark Hall  
> > > University of Virginia  
> > > Charlottesville, VA 22903

---

> > > e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX:  
> > > (434) 982-2137  
> > > <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

> > > -----  
> > > Stephen H. Schneider, Professor  
> > > Dept. of Biological Sciences  
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> > > Stanford, CA 94305-5020 U.S.A.

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> > > Professor Michael E. Mann  
> > > Department of Environmental Sciences, Clark Hall  
> > > University of Virginia  
> > > Charlottesville, VA 22903

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> >

>

>-----

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University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

</x-flowed>

2211. 2003-10-29

---

date: Wed, 29 Oct 2003 08:35:33 +0000

from: Phil Jones <p.jones@uea.ac.uk>

subject: Fwd: Re: STOP THE PRESS!

to: k.briffa@uea.ac.uk

<x-flowed>

>Date: Tue, 28 Oct 2003 19:10:17 -0800 (PST)

>From: Stephen H Schneider <shs@stanford.edu>

>To: "Michael E. Mann" <mann@virginia.edu>

>cc: Richard Kerr <rkerr@aaaas.org>, Andy Revkin <anrevk@nytimes.com>,

> David Appell <appell@nasw.org>,

> <Annie\_Petsonk@environmentaldefense.org>,

> Mike MacCracken <mmaccrac@comcast.net>,

> Michael Oppenheimer <omichael@Princeton.EDU>,

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> <Tim\_Profeta@lieberman.senate.gov>, <rbradley@geo.umass.edu>,

> <mhughes@lrr.arizona.edu>, Jonathan Overpeck <jto@u.arizona.edu>,

> Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford

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> Stefan Rahmstorf <rahmstorf@pik-potsdam.de>,

> Gavin Schmidt <gavin@isis.giss.nasa.gov>,

> Rob Dunbar <dunbar@stanford.edu>, <zubeke@onid.orst.edu>,

> Ross Gelbspan <ross@theworld.com>, Ben Santer <santer1@l1n1.gov>,

> <thompson.4@osu.edu>, <thompson.3@osu.edu>

>Subject: Re: STOP THE PRESS!

>

>Hello all. Interesting tale--why we have competent peer review at  
>competent journals, and why professional courtesy is always to run  
>heterodox results by the orthodox for private comments before going  
>public--unless the motivation isn't science, but a big splash. Too bad for  
>them--the wrong guys will belly-flop (couldn't have happened to a nicer  
>bunch of prevaricators!). By the way, I give it a 50% (Bayesian priors)  
>subjective probability they will accuse you of deliberately misleading  
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>and the only reason they did this was to smoke you out. From them, expect  
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>put it simply and clearly so it can be useful in the debate held by  
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>  
>On Tue, 28 Oct 2003, Michael E. Mann wrote:  
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>> coffee, and my wife Lorraine has been kind enough to allow me to stay  
>> perpetually glued to the terminal. So what has this effort produced?  
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>> Well, upon first looking at what the authors had done, I realized that  
>> they had used the wrong CRU surface temperature dataset (post 1995  
>> version) to calculate the standard deviations for use in un-normalizing  
>> the Mann et al (1998) EOF patterns. Their normalization factors were  
>> based on Phil's older dataset. The clues to them should have been that a)  
>> our data set goes back to 1854 and theirs only back to 1856 and (b) why  
>> are 4 of the 1082 Mann et al (1998) gridpoints missing?? [its because  
>> the reference periods are different in the two datasets, which leads to a  
>> different spatial pattern of missing values]. So they had used the wrong  
>> temperature standard deviations to un-normalize our EOFs in the process  
>> of forming the surface temperature reconstruction. And I thought to  
>> myself, hmm--this could lead to some minor problems, but I don't see how  
>> they get this divergence from the Mann et al (1998) estimate that  
>> increases so much back in time, and becomes huge before 1500 or so. That  
>> can't be it, can it?  
>>  
>> Then I uncovered that they had used standard deviations of the raw  
>> gridpoint temperature series to un-normalize the EOFs, while we had  
>> normalized the data by the detrended standard deviations. Either  
>> convention can be justified, but you can't mix and match--which is what  
>> they effectively did by adopting our EOFs and PCs, and using their  
>> standard deviations. And I thought, hmm--this could certainly lead to an  
>> artificial inflation of the variance in the reconstruction in general,  
>> and this could give an interesting spatial pattern of bias as well (which  
>> might have an interesting influence on the areally-weighted hemispheric  
>> mean). But I thought, hmm, this can't really lead to that tremendous  
>> divergence before 1500 that the authors find. I was still scratching my  
>> head a bit at this point.  
>>  
>> Then I read about the various transcription errors, values being shifted,  
>> etc. that the authors describe as existing in the dataset. And I thought,  
>> hmm, that sounds like an excel spread sheet problem, not a problem w/ the  
>> MBH98 proxy data set. It started to occur to me at this point that there  
>> might be some problems w/ the excel spreadsheet data that my colleague  
>> Scott Rutherford had kindly provided the authors at their request. But  
>> these problems sounded pretty minor from the authors' description, and  
>> the authors described a procedure to try to fix any obvious  
>> transcription errors, shifted cell values, etc. So I thought, hmm, they  
>> might not have fixed things perfectly, and that could also lead to some  
>> problems. But I still don't see how they get that huge divergence back in  
>> time from this sort of error...  
>>  
>> Still scratching my head at this point...Then finally this afternoon,  
>> some clues. After looking at their on-line description one more time, I

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> > became disturbed at something I read. The data matrix they're using has  
> > 112 columns! Well that can't be right! That's can't constitute the Mann  
> > et al (1998) dataset. There are considerably more than that number of  
> > independent proxy indicators necessary to reproduce the stepwise Mann et  
> > al reconstruction. Something is amiss!  
> >  
> > Well, 112 is the number of proxy indicators used back to 1820. But some  
> > of these indicators are principal components of regional sub-networks  
> > (e.g. the Western U.S. ITRDB tree-ring data) to make the dataset more  
> > manageable in size, and those principal components (PCs) are unique to the  
> > time interval analyzed. So there is some set of PC series for the  
> > 1820-1980 period. Farther back in time, say, back to 1650 there are fewer  
> > data series the regional sub-networks. So we recalculate a completely  
> > different EOF/PC basis set for that period, and that constitutes an  
> > additional, unique set of proxy indicators that are appropriate for a  
> > reconstruction of the 1650-1980 period. PC #1 from one interval is not  
> > equivalent to PC#1 from a different interval. This turns out to be the  
> > essential detail. A reconstruction back to 1820 calibrated against the  
> > 20th century needs to make use of the unique set of proxy PCs available  
> > for the 1820-1980 period. A reconstruction back to 1650 calibrated  
> > against the 20th century needs to make use of the independent (smaller)  
> > set of PC series available for the 1650-1980 period, and so on, back to  
> > 1400.  
> >  
> > So there have to be significantly more than 112 series available to  
> > perform the iterative, stepwise reconstruction approach of Mann et al  
> > (1998), because each sub interval actually has a unique set of PC series  
> > representations of various proxy sub-networks. Then it started to hit  
> > me. The PC#1 series calculated for networks of similar size (say, the  
> > network available back to 1820 and that available back to 1750) should be  
> > similar. But as the sub-network gets sparser back in time, the PC#1  
> > series will resemble less and less the PC#1 series of the denser networks  
> > available at later times. PC#1 of the western ITRDB tree-ring calculated  
> > for the 1400-1980 period will bear almost no resemblance to the PC#1  
> > series of the western N.Amer ITRDB data calculated for the 1820-1980  
> > period during their interval (1820-1980) of mutual overlap.  
> >  
> > Then it really hit me. What--just what--if the proxy data had been  
> > pigeonholed into a 112 column matrix by the following (completely  
> > inappropriate!) procedure: What if it had been decided that there would  
> > only be 1 column for "PC #1 of the Western ITRDB tree ring data", even  
> > though that PC reflects something completely different over each  
> > sub-interval. Well, that can't be done in a reasonable way. But it can be  
> > done in an \*unreasonable\* way: by successively overprinting the data in  
> > that column as one stores the PCs from later and later intervals. So a  
> > given column would reflect PC#1 of the 1400-1980 data from 1400-1450,  
> > PC#1 of the 1450-1980 from 1450-1500, PC#1 of the 1500-1980 data for  
> > 1500-1650, PC#1 of the 1650-1980 data for 1650-1750, etc. and so on. In  
> > this process, the information necessary to calibrate the early PCs would  
> > be obliterated with each successive overprint. The resulting 'series'  
> > corresponding to that column of the data matrix, an amalgam of  
> > increasingly unrelated information down the column, would be completely  
> > useless for calibration of the earlier data. A reconstruction back to AD  
> > 1400 would be reconstructing the PC#1 of the 1400-1450 interval based on  
> > calibration against the almost entirely unrelated PC#1 of the 1820-1980  
> > interval. The reconstruction of the earliest centuries would be based on  
> > a completely spurious calibration of an unrelated PC of a much later  
> > proxy sub network. And I thought, gee, what if Scott (sorry Scott), had  
> > \*happened\* to do this in preparing the excel file that the authors used.  
> > Well it would mean that, progressively in earlier centuries, one would  
> > be reconstructing an apple, based on calibration against an orange. It  
> > would yield completely meaningless results more than a few centuries ago.  
> > And then came the true epiphany--ahhhh, this could lead to the kind of

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> > result the authors produced. In fact, it seemed to me that this would  
> > almost \*insure\* the result that the authors get--an increasing divergence  
> > back in time, and total nonsense prior to 1500 or so. At this point, I  
> > knew that's what Scott must have done. But I had to confirm.  
> >  
> > I simply had to contact Scott, and ask him: Scott, when you prepared that  
> > excel file for these guys, you don't suppose by any chance that you might  
> > have....  
> >  
> > And, well, I think you know the answer.  
> >  
> > So the proxy data back to AD 1820 used by the authors may by-in-large be  
> > correct (aside from the apparent transcription/cell shift errors which  
> > they purport to have caught, and fixed, anyway). The data become  
> > progressively corrupted in earlier centuries. By the time one goes back  
> > to AD 1400, the 1400-1980 data series are, in many cases, entirely  
> > meaningless combinations of early and late information, and have no  
> > relation to the actual proxy series used by Mann et al (1998).  
> >  
> > And so, the authors results are wrong/meaningless/useless. The mistake  
> > made insures, especially, that the estimates during the 15th and 16th  
> > centuries are entirely spurious.  
> >  
> > So whose fault is this? Well, the full, raw ascii proxy data set has been  
> > available on our anonymous ftp site  
> > <ftp://holocene.evsc.virginia.edu/pub/MBH98/>  
> > and the authors were informed of this in email correspondence. But they  
> > specifically requested that the data be provided to them in excel format.  
> > And Scott prepared it for them in that format, in good faith--but  
> > overlooked the fact that all of the required information couldn't  
> > possibly be fit into a 112 column format. So the file Scott produced was  
> > a complete corruption of the actual Mann et al proxy data set, and  
> > essentially useless, transcription errors, etc. aside. The authors had  
> > full access to the uncorrupted data set. We therefore take no  
> > reasonability for their use of corrupted data.  
> >  
> > One would have thought that the authors might have tried to reconcile  
> > their completely inconsistent result prior to publication. One might have  
> > thought that it would at least occur to them as odd that the Mann et al  
> > (1998) reconstruction is remarkably similar to entirely independent  
> > estimates, for example, by Crowley and Lowery (2000). Could both have  
> > made the same supposed mistake, even though the data and method are  
> > entirely unrelated. Or might M&M have made a mistake? Just possibly,  
> > perhaps???  
> >  
> > Of course, a legitimate peer-review process would have caught this  
> > problem. In fact, in about 48 hours if I (or probably, many of my  
> > colleagues) had been given the opportunity to review the paper. But that  
> > isn't quite the way things work at "E&E" I guess. I guess there may just  
> > be some corruption of scientific objectivity when a journal editor seems  
> > more interested in politics than science.  
> >  
> > The long and short of this. I think it is morally incumbent upon E&E to  
> > publish a full retraction of the M&M article immediately. Its unlikely  
> > that they'll do this, but its reasonable to assert that it would be  
> > irresponsible for them not to if the issue arises.  
> >  
> > I think that's the end of the story. Please, again, keep this information  
> > under wraps for next day or two. Then, by all means, feel free to  
> > disseminate this information as widely as you like...  
> >  
> > Mike  
> >

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</x-flowed>

2637. 2003-10-29

cc: Gabi Hegerl <hegerl@duke.edu>, tom crowley <tom@ocean.tamu.edu>, mhughes@ltrr.arizona.edu, "raymond s.bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, Steve Schneider <shs@stanford.edu>, peter.stott@metoffice.com, Gavin Schmidt <gavin@isis.giss.nasa.gov>, mann@multiproxy.evsc.virginia.edu

date: Wed, 29 Oct 2003 13:05:07 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: some info you'll want to have...

to: stocker@climate.unibe.ch, joos@climate.unibe.ch, knutti@climate.unibe.ch

Dear Thomas, Fortunat, Reto:

You might have wanted to check w/ us first, but thanks anyway for responding to this. We've

uncovered the error in what they did. They didn't use the proxy data available on our

public ftp site, which I had pointed them too--instead they used a spreadsheet file that my

associate Scott Rutherford had prepared. In this file, most of the early series were

overprinted at later years. This resulted in the reconstruction becoming increasingly

spurious as one goes further back in time--the estimates prior to 1700 or so were rendered

meaningless. There were also some other methodological errors that will be detailed

shortly, but this was the big one.

So they will probably have to retract the paper. You can find out more about this here, on

journalist David Appell's "blog":

[1]<http://www.davidappell.com/>

We also have an op-ed piece going out this afternoon, further detailing the



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problems. will

send that as soon as its available. I've attached a few other relevant documents, and I'm forwarding another email I sent out to colleagues yesterday, just after I had discovered the main problem in what they've done...  
mike

---

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---

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\Journalists.re.EandEfin-revised.doc"

3738. 2003-10-29

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date: wed, 29 Oct 2003 08:05:09 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Fwd: draft

to: Phil Jones <p.jones@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, mhughes@lrr.arizona.edu, jto@u.arizona.edu, tom crowley <tom@ocean.tamu.edu>, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, crowley@duke.edu

Dear All,

Particularly the British among us--what's the latest you guys will have access to email

today (Eastern Standard Time US please, since my brain is not working quick as well after

all the sleep deprivation). I'm going to try to work w/ Annie Petsonk at EDF to incorporate their suggestions w/ those you guys have provided, but we'll probably need to

finalize this and confirm authors by early afternoon east coast U.S. time...

will keep you posted of any developments as they occur.

Thanks for all the wonderful advice, and your critical support at this particular time,

mike

At 09:16 AM 10/29/2003 +0000, Phil Jones wrote:

Mike,

I'm happy to sign up for this and Keith and Tim may like to as well, so cc'ing this

reply

to them as well. I'm off this afternoon to Newcastle so will be out of contact till I

get there.

I will have a chance to check email tomorrow am.

Here are a few thoughts in the meantime:

1. Text needs a little fine tuning as Malcolm says and getting in dates of emails etc between

you, Scott and them would be good. I doubt that such details will make it into the

final piece,

but they are useful background evidence.

2. I would really have a go at Schulz's second sentence --- 'If it withstands scrutiny

.....

This is what the whole peer-review process is about and E&E have clearly failed to get

the

paper adequately reviewed. Papers do get scrutinized after publication, but this is

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almost always  
about the interpretation of results, not simple methodological flaws or clear mistakes.

Perhaps, something like, The authors did not seem to stop to think why their results

were  
so different from MBH. Any respectable scientists attempting to repeat or reanalyze  
earlier

work would want to fully understand why the results were different. Any scientist

wanting to  
publish such differences would want to check, double-even-triple check their results.

The  
study here seems to have accepted the results, possibly because they appear at first  
glance

to be the results they wanted. They should have stopped to think why they were so

different, especially as several other groups have obtained essentially the same basic  
results

as MBH, with different proxy networks and different methods of combining the results.

Also, would the authors have published the results if the 'random' data had showed

the  
opposite result. I guess it could have by chance, but I suspect they would have been  
more

cautious as the result did not agree with their preconceptions.

3. Related to the above there is the fact that their results just don't look right. I

always say  
that data analysts need to have a feel for the data. Here, the result just looks plain  
wrong.

I try to drum this into my students and post-docs - saying go back and find the

mistake,  
the results aren't right !

4. Also need to cover the issue of Scott's inadvertent mistake. I've no idea how to do

anything  
in Excel - except get any data out of it ! I'm told it is quite difficult to write out

data in excel  
spreadsheet format. Back to the post-grads - they often come and say 'Excel

can't do  
it' to

which I retort then program the method from scratch in Fortran. I may be a dinosaur in

this  
respect, but this helps understand the technique being used, as you have to go through

it  
step by step.

Need to fully cover any accusations of making the mistake deliberately.

Anyway, have a few other things to do before going off at 11  
Cheers

Phil

At 00:10 29/10/2003 -0500, Michael E. Mann wrote:

oops, my draft op-ed was pasted at the end of that previous email. here it is up front,

mike  
DRAFT REPLY TO USA TODAY OPINION PIECE

The opinion piece "Researchers question key global-warming study" published in USA Today by Nick Schulz, describes a deeply flawed article published in a discredited journal "Energy and Environment" by two individuals with no scientific expertise. The article is deceptive on multiple accounts.

It was not revealed that TechCentralStation.com, the website that the author Nick Shultz edits, receives considerable funding from Exxon-Mobile--this makes Schulz hardly disinterested matter in discussions of human-induced climate change and climate change policy.

Schulz makes the blatantly false claim: Mann never made his data available online nor did many of the earlier researchers whose data Mann relied upon for his research. That by itself raises questions about the U.N. climate-change panel's scientific process.

The data used by Mann and colleagues have been in the public domain for nearly two years, at the readily accessible website:  
[1]<ftp://holocene.evsc.virginia.edu/pub/MBH98/>

Had the authors of the study in question used the publicly available data provided by Mann and colleagues, they would have reproduced their overall results, and those of numerous other paleoclimatologists who have produced statistically indistinguishable results to those of Mann and colleagues. Instead, the authors requested from an associate of Mann and coworkers a specially formatted, spreadsheet version of the data set. There appear to have been some significant errors in that version of the dataset.

Even though the authors detected some problems, they did not contact the associate who sent them the data to inquire about them. The spreadsheet version inadvertently appears to have overprinted much of the early data, rendering the proxy data set prior to about 1600 erroneous. It is the use of the incorrect early values in the proxy series that lead to the wide divergence of the authors estimates from nearly all previously published estimates during the 15th and 16th centuries. The anomalous warmth they claim to reconstruct in those centuries is nothing more than an artifact of their having used scrambled early data in place of the correct data.

There are other more minor sources of error. The authors misapplied the methodology of Mann et al by convoluting their previous estimated temperature patterns from one dataset with an inconsistent set of temperature estimates from an entirely different dataset.

However, it is the use of scrambled estimates of the proxy data that is responsible for

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the huge errors in their estimates during the 15th-16th centuries.  
Had this paper been submitted to reputable scientific journal, such as Nature  
(where the original paper by Mann and colleagues was published) or Science, where high  
quality paleoclimatic work has often been published, the deep flaws would have quickly  
been uncovered in their method. Instead, the authors published their article in a  
social science journal, "Energy and Environment", with questionable editorial  
practices (as detailed in an article last September in the Chronicle of Higher Education).  
The journal "Energy and Environment" if it has any editorial integrity, will  
demand a retraction of the paper by McKittrick and McIntyre's, as the results presented  
are entirely spurious, and the conclusions wholly without merit.  
The assertion in dozens of more mainstream, scientific publications that late  
20th century Northern Hemisphere average warmth is unprecedented not only in the  
past six centuries (as shown by Mann and colleagues in 1998), but at least the past  
millennium or longer is the conclusion of more than a dozen independent studies published in  
reputable scientific journals over the past several years and this latest deeply flawed  
study does nothing whatsoever to change those conclusions.  
At 12:03 AM 10/29/2003 -0500, Michael E. Mann wrote:  
I know how sick you guys are of this routine by now. hopefully, this is the  
last time.  
EDF wants to try to help me get a response to the USA Today opinion piece by  
Nick Schulz into tomorrows edition. She thinks we could use several co-authors from the  
paleo community, and Steve S thinks they'll have to print it, because Schulz  
completely lied about us supposedly not having provided our data in the public domain (they've  
been on a public website on our machine holocene since March '02 according to the dates  
on the files)...  
We need to finalize this by tomorrow afternoon.  
Can I get any/all of you to sign on w/ me. We'll work on revising and  
finalizing tomorrow morning/afternoon.  
let me know. thanks,  
mike  
p.s. the op-ed piece is pasted in below:  
Researchers question key global-warming study  
By Nick Schulz  
An important new paper in the journal Energy & Environment upsets a key  
scientific claim about climate change. If it withstands scrutiny, the collective scientific  
understanding of recent global warming might need an overhaul.  
A little background is needed to understand the importance of the new research  
behind this paper by Stephen McIntyre, a statistics expert who works in the mining  
industry, and Ross McKittrick, a professor of economics at the University of Guelph,  
Ontario. As scientists and governments have tried to understand mankind's influence on the

environment, global warming has become a primary concern. Do mankind's activities especially burning fossil fuels to create energy affect climate? If so, how? what should be done?

These questions were so important that in 1988 the United Nations, along with the world Meteorological Organization, formed the U.N. Intergovernmental Panel on Climate Change (IPCC) to study "human-induced climate change."

Ten years after IPCC's founding, a paper from Michael Mann, now an assistant professor of environmental sciences at the University of Virginia, and his colleagues in the journal *Nature* shook scientific and political circles. It reconstructed temperatures dating back to the year 1400 by looking at tree rings, ice cores and other so-called proxy records to derive a temperature signature. This was before the sophisticated climate-measuring equipment we use today.

What Mann claimed to find was startling: The late-20th century was unusually warm warmer than at any time in the previous six centuries. (Later research by Mann extended the climate history back 1,000 years.) The reason? "It really looks like (the recent warming) can only be explained by greenhouse gases," Mann said then. His clear implication: The Earth's climate was changing dramatically, and mankind was responsible.

Earth heats up?

The U.N. used Mann's research to declare the 1990s "the warmest decade and 1998 the warmest year of the millennium." Countless news stories picked up on this idea that the past few years have been unusually warm.

Efforts to limit the emission of the greenhouse gases blamed for this warming were bolstered by Mann's research. In fact, this week the Senate plans to consider legislation co-sponsored by Sens. John McCain, R-Ariz., and Joe Lieberman, D-Conn., to reduce the emission of greenhouse gases. McCain's Web site says, "Global warming is a growing problem. ... The 10 warmest years (on record) have all occurred since 1987." The statement is based on Mann's research.

But what if it's not true?

When McIntyre and Mckitrick audited Mann's data to see whether its conclusions could be replicated, they discovered significant problems. Once they corrected the errors, the two researchers made a remarkable conclusion: The late 20th century was not unusually warm by historical standards.

Not alone in his conclusion when asked about the paper, which had undergone review by other scientists before being published, Mann said he had heard about it but had not seen it. He called it a "political stunt" and said "dozens of independent studies published by leading journals" had come to conclusions similar to his.

What's to guarantee Mckitrick and McIntyre's research will withstand the kind of

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scrutiny they gave Mann's research?

In an interview, Mckitrick said, "If a study is going to be the basis for a major policy decision, then the original data must be disseminated and the results have to be reproducible. That's why in our case we have posted everything online and invite outside scrutiny."

Mann never made his data available online nor did many of the earlier researchers whose data Mann relied upon for his research. That by itself raises questions about the U.N. climate-change panel's scientific process.

It remains to be seen whether the Mckitrick and McIntyre study will withstand the "outside scrutiny" they have asked for and will no doubt receive. But given the implications of the errors and problems they apparently have unearthed within the Mann study, the two researchers have done a tremendous service to science and the public,

which should rely on facts to make informed public policy decisions. Nick Schulz is editor of TechCentralStation.com, a science, technology and public policy web site.

Date: Tue, 28 Oct 2003 23:58:21 -0500  
To: Annie\_Petsonk@environmentaldefense.org  
From: "Michael E. Mann" <mann@virginia.edu>  
Subject: draft

Cc: mann@virginia.edu  
Before midnight as promised :)  
here is a rough draft of an op-ed. Any help I can get from you or any associates of yours in refining this and getting this published will be very helpful.

I can work on co-authors tomorrow morning. iPerhaps we can send something similar on to other newswire journalists such as Joan Lowey, etc...

DRAFT REPLY TO USA TODAY OPINION PIECE  
The opinion piece "Researchers question key global-warming study" published in USA Today by Nick Schulz, describes a deeply flawed article published in a discredited journal "Energy and Environment" by two individuals with no scientific expertise. The article is deceptive on multiple accounts.

It was not revealed that TechCentralStation.com, the website that the author Nick Shultz edits, receives considerable funding from Exxon-Mobile--this makes Schulz hardly disinterested matter in discussions of human-induced climate change and climate change policy.

Schulz makes the blatantly false claim: Mann never made his data available online nor did many of the earlier researchers whose data Mann relied upon for his research. That by itself raises questions about the U.N. climate-change panel's scientific process.

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[2]ftp://holocene.evsc.virginia.edu/pub/MBH98/

Had the authors of the study in question used the publicly available data

provided by Mann and colleagues, they would have reproduced their overall results, and those of numerous other paleoclimatologists who have produced statistically indistinguishable results to those of Mann and colleagues. Instead, the authors requested from an associate of Mann and coworkers a specially formatted, spreadsheet version of the data set. There appear to have been some significant errors in that version of the dataset. Even though the authors detected some problems, they did not contact the associate who sent them the data to inquire about them. The spreadsheet version inadvertently appears to have overprinted much of the early data, rendering the proxy data set prior to about 1600 erroneous. It is the use of the incorrect early values in the proxy series that lead to the wide divergence of the authors estimates from nearly all previously published estimates during the 15th and 16th centuries. The anomalous warmth they claim to reconstruct in those centuries is nothing more than an artifact of their having used scrambled early data in place of the correct data. There are other more minor sources of error. The authors misapplied the methodology of Mann et al by convoluting their previous estimated temperature patterns from one dataset with an inconsistent set of temperature estimates from an entirely different dataset. However, it is the use of scrambled estimates of the proxy data that is responsible for the huge errors in their estimates during the 15th-16th centuries. Had this paper been submitted to reputable scientific journal, such as Nature (where the original paper by Mann and colleagues was published) or Science, where high quality paleoclimatic work has often been published, the deep flaws would have quickly been uncovered in their method. Instead, the authors published their article in a social science journal, "Energy and Environment", with questionable editorial practices (as detailed in an article last September in the Chronicle of Higher Education). The journal "Energy and Environment" if it has any editorial integrity, will demand a retraction of the paper by McKittrick and McIntyre's, as the results presented are entirely spurious, and the conclusions wholly without merit. The assertion in dozens of more mainstream, scientific publications that late 20th century Northern Hemisphere average warmth is unprecedented not only in the past six centuries (as shown by Mann and colleagues in 1998), but at least the past millennium or longer is the conclusion of more than a dozen independent studies published in reputable scientific journals over the past several years and this latest deeply flawed study does nothing whatsoever to change those conclusions.

Professor Michael E. Mann

cg2003  
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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3874. 2003-10-29

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date: wed, 29 Oct 2003 08:34:51 +0000

from: Phil Jones <p.jones@uea.ac.uk>

subject: Fwd: STOP THE PRESS!

to: k.briffa@uea.ac.uk

X-Sender: mem6u@multiproxy.evsc.virginia.edu

X-Mailer: QUALCOMM Windows Eudora Version 5.2.1

Date: Tue, 28 Oct 2003 21:43:33 -0500

To: "Richard Kerr" <rkerr@aaaas.org>,

Andy Revkin <anrevk@nytimes.com>, David Appell <appell@nasw.org>,

Stephen H Schneider <shs@stanford.edu>,

Annie\_Petsonk@environmentaldefense.org,

Mike MacCracken <mmaccrac@comcast.net>,

Michael Oppenheimer <omichael@Princeton.EDU>,

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Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, mann@virginia.edu,

Gavin Schmidt <gavin@isis.giss.nasa.gov>,



cg2003

Rob Dunbar <dunbar@stanford.edu>, zubeke@onid.orst.edu,  
ross@theworld.com, Ben Santer <santer1@llnl.gov>, thompson.4@osu.edu,  
thompson.3@osu.edu

From: "Michael E. Mann" <mann@virginia.edu>

Subject: STOP THE PRESS!

Cc: mann@virginia.edu

Dear Friends and Colleagues,

I've got a story with a very happy ending to tell. I't will take a bit of  
patience to  
get through the details of the story, but I think its worth it.  
By the way, please keep this information confidential for about the next day  
or so.

OK, well its about 48 hours since I first had the chance to review the E&E  
paper by M&M.

Haven't had a lot of sleep, but I have had a lot of coffee, and my wife  
Lorraine has  
been kind enough to allow me to stay perpetually glued to the terminal. So  
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Well, upon first looking at what the authors had done, I realized that they  
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Then I uncovered that they had used standard deviations of the raw gridpoint  
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standard deviations. Either convention can be justified, but you can't mix and  
match--which is what they effectively did by adopting our EOFs and PCs, and  
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standard deviations. And I thought, hmm--this could certainly lead to an  
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interesting spatial pattern of bias as well (which might have an interesting  
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Then I read about the various transcription errors, values being shifted, etc.  
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excel spread sheet problem, not a problem w/ the MBH98 proxy data set. It started to occur to me at this point that there might be some problems w/ the excel spreadsheet data that my colleague Scott Rutherford had kindly provided the authors at their request. But these problems sounded pretty minor from the authors' description, and the authors described a procedure to try to fix any obvious transcription errors, shifted cell values, etc. So I thought, hmm, they might not have fixed things perfectly, and that could also lead to some problems. But I still don't see how they get that huge divergence back in time from this sort of error... Still scratching my head at this point...Then finally this afternoon, some clues. After looking at their on-line description one more time, I became disturbed at something I read. The data matrix they're using has 112 columns! Well that can't be right! That's can't constitute the Mann et al (1998) dataset. There are considerably more than that number of independent proxy indicators necessary to reproduce the stepwise Mann et al reconstruction. Something is amiss! Well, 112 is the number of proxy indicators used back to 1820. But some of these indicators are principal components of regional sub-networks (e.g. the Western U.S. ITRDB tree-ring data) to make the dataset more managable in size, and those principal components (PCs) are unique to the time interval analyzed. So there is some set of PC series for the 1820-1980 period. Farther back in time, say, back to 1650 there are fewer data series the regional sub-networks. So we recalculate a completely different EOF/PC basis set for that period, and that constitutes an additional, unique set of proxy indicators that are appropriate for a reconstruction of the 1650-1980 period. PC #1 from one interval is not equivalent to PC#1 from a different interval. This turns out to be the essential detail. A reconstruction back to 1820 calibrated against the 20th century needs to make use of the unique set of proxy PCs available for the 1820-1980 period. A reconstruction back to 1650 calibrated against the 20th century needs to make use of the independent (smaller) set of PC series available for the 1650-1980 period, and so on, back to 1400. So there have to be significantly more than 112 series available to perform the iterative, stepwise reconstruction approach of Mann et al (1998), because each sub interval actually has a unique set of PC series representations of various proxy sub-networks. Then it started to hit me. The PC#1 series calculated for networks of similar size (say, the network available back to 1820 and that available back to 1750)

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should be similar. But as the sub-network gets sparser back in time, the PC#1 series will resemble less and less the PC#1 series of the denser networks available at later times. PC#1 of the western ITRDB tree-ring calculated for the 1400-1980 period will bear almost no resemblance to the PC#1 series of the western N.Amer ITRDB data calculated for the 1820-1980 period during their interval (1820-1980) of mutual overlap. Then it really hit me. What--just what--if the proxy data had been pigeonholed into a 112 column matrix by the following (completely inappropriate!) procedure: What if it had been decided that there would only be 1 column for "PC #1 of the Western ITRDB tree ring data", even though that PC reflects something completely different over each sub-interval. Well, that can't be done in a reasonable way. But it can be done in an \*unreasonable\* way: by successively overprinting the data in that column as one stores the PCs from later and later intervals. So a given column would reflect PC#1 of the 1400-1980 data from 1400-1450, PC#1 of the 1450-1980 from 1450-1500, PC#1 of the 1500-1980 data for 1500-1650, PC#1 of the 1650-1980 data for 1650-1750, etc. and so on. In this process, the information necessary to calibrate the early PCs would be obliterated with each successive overprint. The resulting 'series' corresponding to that column of the data matrix, an amalgam of increasingly unrelated information down the column, would be completely useless for calibration of the earlier data. A reconstruction back to AD 1400 would be reconstructing the PC#1 of the 1400-1450 interval based on calibration against the almost entirely unrelated PC#1 of the 1820-1980 interval. The reconstruction of the earliest centuries would be based on a completely spurious calibration of an unrelated PC of a much later proxy sub network. And I thought, gee, what if Scott (sorry Scott), had \*happened\* to do this in preparing the excel file that the authors used. Well it would mean that, progressively in earlier centuries, one would be reconstructing an apple, based on calibration against an orange. It would yield completely meaningless results more than a few centuries ago. And then came the true epiphany--ahhh, this could lead to the kind of result the authors produced. In fact, it seemed to me that this would almost \*insure\* the result that the authors get--an increasing divergence back in time, and total nonsense prior to 1500 or so. At this point, I knew that's what Scott must have done. But I had to confirm. I simply had to contact Scott, and ask him: Scott, when you prepared that excel file for these guys, you don't suppose by any chance that you might have.... And, well, I think you know the answer. So the proxy data back to AD 1820 used by the authors may by-in-large be

correct (aside from the apparent transcription/cell shift errors which they purport to have caught, and fixed, anyway). The data become progressively corrupted in earlier centuries. By the time one goes back to AD 1400, the 1400-1980 data series are, in many cases, entirely meaningless combinations of early and late information, and have no relation to the actual proxy series used by Mann et al (1998). And so, the authors results are wrong/meaningless/useless. The mistake made insures, especially, that the estimates during the 15th and 16th centuries are entirely spurious. So whose fault is this? Well, the full, raw ascii proxy data set has been available on our anonymous ftp site [1]ftp://holocene.evsc.virginia.edu/pub/MBH98/ and the authors were informed of this in email correspondence. But they specifically requested that the data be provided to them in excel format. And Scott prepared it for them in that format, in good faith--but overlooked the fact that all of the required information couldn't possibly be fit into a 112 column format. So the file Scott produced was a complete corruption of the actual Mann et al proxy data set, and essentially useless, transcription errors, etc. aside. The authors had full access to the uncorrupted data set. We therefore take no responsibility for their use of corrupted data. One would have thought that the authors might have tried to reconcile their completely inconsistent result prior to publication. One might have thought that it would at least occur to them as odd that the Mann et al (1998) reconstruction is remarkably similar to entirely independent estimates, for example, by Crowley and Lowery (2000). Could both have made the same supposed mistake, even though the data and method are entirely unrelated. Or might M&M have made a mistake? Just possibly, perhaps??? Of course, a legitimate peer-review process would have caught this problem. In fact, in about 48 hours if I (or probably, many of my colleagues) had been given the opportunity to review the paper. But that isn't quite the way things work at "E&E" I guess. I guess there may just be some corruption of scientific objectivity when a journal editor seems more interested in politics than science. The long and short of this. I think it is morally incumbent upon E&E to publish a full retraction of the M&M article immediately. Its unlikely that they'll do this, but its reasonable to assert that it would be irresponsible for them not to if the issue arises. I think that's the end of the story. Please, again, keep this information under wraps for next day or two. Then, by all means, feel free to disseminate this information as widely as you like...

Mike

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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4600. 2003-10-29

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date: Wed, 29 Oct 2003 08:38:40 +0000  
from: Phil Jones <p.jones@uea.ac.uk>  
subject: Fwd: Re: STOP THE PRESS!  
to: k.briffa@uea.ac.uk

X-Sender: mem6u@multiproxy.evsc.virginia.edu  
X-Mailer: QUALCOMM Windows Eudora Version 5.2.1  
Date: Tue, 28 Oct 2003 22:23:02 -0500  
To: Stephen H Schneider <shs@stanford.edu>  
From: "Michael E. Mann" <mann@virginia.edu>  
Subject: Re: STOP THE PRESS!  
Cc: Richard Kerr <rkerr@aaaas.org>, Andy Revkin <anrevk@nytimes.com>,  
David Appell <appell@nasw.org>,  
<Annie\_Petsonk@environmentaldefense.org>,  
Mike MacCracken <mmaccrac@comcast.net>,  
Michael Oppenheimer <omichael@Princeton.EDU>,  
"Socci.Tony-epamail.epa.gov" <Socci.Tony@epamail.epa.gov>,  
<Tim\_Profeta@lieberman.senate.gov>, <rbradley@geo.umass.edu>,  
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Scott Rutherford <srutherford@rwu.edu>,  
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Stefan Rahmstorf <rahmstorf@pik-potsdam.de>,  
Gavin Schmidt <gavin@isis.giss.nasa.gov>,  
Rob Dunbar <dunbar@stanford.edu>, <zubeke@onid.orst.edu>,  
Ross Gelbspan <ross@theworld.com>, Ben Santer <santer1@linl.gov>,  
<thompson.4@osu.edu>, <thompson.3@osu.edu>

Thanks Steve,

I plan to work w/ the staffers to try boil this down to its most basic terms...  
Of course, the proxy data were available uncorrupted on our anonymous ftp site--the authors chose not to use that, and instead requested a spreadsheet version from my associated (Scott). Its not his fault that there were some problems with that particular file--the authors could have done numerous things to confirm the possible sources of the obvious problems w/ the file that they note in their 'paper'.  
This will be an important point to convey to folks.  
This is one of the worst examples yet (and we've had some good ones recently) of a disingenuous/deficient/absent peer review coupled with an irresponsible

editor..

mike

At 07:10 PM 10/28/2003 -0800, Stephen H Schneider wrote:

Hello all. Interesting tale--why we have competent peer review at competent journals, and why professional courtesy is always to run heterodox results by the orthodox for private comments before going public--unless the motivation isn't science, but a big splash. Too bad for them--the wrong guys will belly-flop (couldn't have happened to a nicer bunch of prevaricators!). By the way, I give it a 50% (Bayesian priors) subjective probability they will accuse you of deliberately misleading them or deliberately preventing replication by "independent" scientists and the only reason they did this was to smoke you out. From them, expect anything. Can you explain this to Senator McCain's folks so they understand the complexities and professional courtesy/peer review issues? This stuff is not very sound bite friendly and needs some prethinking to put it simply and clearly so it can be useful in the debate held by non-scientist debaters. Good luck, Steve

On Tue, 28 Oct 2003, Michael E. Mann wrote:

> Dear Friends and Colleagues,

>

> I've got a story with a very happy ending to tell. I't will take a bit  
> of patience to get through the details of the story, but I think its  
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> E&E paper by M&M. Haven't had a lot of sleep, but I have had a lot of  
> coffee, and my wife Lorraine has been kind enough to allow me to stay  
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> they had used the wrong CRU surface temperature dataset (post 1995  
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> Mike

>  
> -----  
> Professor Michael E. Mann  
> Department of Environmental Sciences, Clark Hall  
> University of Virginia  
> Charlottesville, VA 22903  
> -----  
> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
> [2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>

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Stanford, CA 94305-5020 U.S.A.  
Tel: (650)725-9978  
Fax: (650)725-4387  
shs@stanford.edu

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University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
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4742. 2003-10-29

cc: Gabi Hegerl <hegerl@duke.edu>, tom crowley <tom@ocean.tamu.edu>,  
mhughes@ltrr.arizona.edu, "raymond s.bradley" <rbradley@geo.umass.edu>, Keith  
Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Stefan  
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peter.stott@metoffice.com, Gavin Schmidt <gavin@isis.giss.nasa.gov>,  
mann@multiproxy.evsc.virginia.edu  
date: Wed, 29 Oct 2003 13:05:19 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Fwd: STOP THE PRESS!  
to: stocker@climate.unibe.ch, joos@climate.unibe.ch, knutti@climate.unibe.ch  
Delivered-To: mem6u@virginia.edu  
X-Sender: mem6u@multiproxy.evsc.virginia.edu  
X-Mailer: QUALCOMM Windows Eudora Version 5.2.1  
Date: Tue, 28 Oct 2003 21:43:33 -0500

cg2003

To: "Richard Kerr" <rkerr@aaaas.org>, Andy Revkin <anrevk@nytimes.com>, David Appell <appell@nasw.org>, Stephen H Schneider <shs@stanford.edu>, Annie\_Petsonk@environmentaldefense.org, Mike MacCracken <mmaccrac@comcast.net>, Michael Oppenheimer <omichael@Princeton.EDU>, "Socci.Tony-epamail.epa.gov" <Socci.Tony@epamail.epa.gov>, Tim\_Profeta@lieberman.senate.gov, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, Jonathan Overpeck <jto@u.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford <srutherford@rwu.edu>, Gabi Hegerl <hegerl@duke.edu>, tom crowley <tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Tim Osborn <t.osborn@uea.ac.uk>, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, mann@virginia.edu, Gavin Schmidt <gavin@isis.giss.nasa.gov>, Rob Dunbar <dunbar@stanford.edu>, zubeke@onid.orst.edu, ross@theworld.com, Ben Santer <santer1@llnl.gov>, thompson.4@osu.edu, thompson.3@osu.edu

From: "Michael E. Mann" <mann@virginia.edu>

Subject: STOP THE PRESS!

Cc: mann@virginia.edu

Dear Friends and Colleagues,

I've got a story with a very happy ending to tell. I't will take a bit of patience to

get through the details of the story, but I think its worth it.

By the way, please keep this information confidential for about the next day or so.

OK, well its about 48 hours since I first had the chance to review the E&E paper by M&M.

Haven't had a lot of sleep, but I have had a lot of coffee, and my wife Lorraine has

been kind enough to allow me to stay perpetually glued to the terminal. So what has this

effort produced?

Well, upon first looking at what the authors had done, I realized that they had used

the wrong CRU surface temperature dataset (post 1995 version) to calculate the standard

deviations for use in un-normalizing the Mann et al (1998) EOF patterns. Their normalization factors were based on Phil's older dataset. The clues to them

should have been that a) our data set goes back to 1854 and theirs only back to 1856 and (b) why are

4 of the 1082 Mann et al (1998) gridpoints missing?? [its because the reference periods

are different in the two datasets, which leads to a different spatial pattern of missing

values]. So they had used the wrong temperature standard deviations to un-normalize our

EOFs in the process of forming the surface temperature reconstruction. And I thought to

myself, hmm--this could lead to some minor problems, but I don't see how they get this

divergence from the Mann et al (1998) estimate that increases so much back in time, and

becomes huge before 1500 or so. That can't be it, can it?

Then I uncovered that they had used standard deviations of the raw gridpoint temperature

series to un-normalize the EOFs, while we had normalized the data by the detrended

standard deviations. Either convention can be justified, but you can't mix and match--which is what they effectively did by adopting our EOFs and PCs, and

using their standard deviations. And I thought, hmm--this could certainly lead to an artificial

inflation of the variance in the reconstruction in general, and this could

give an interesting spatial pattern of bias as well (which might have an interesting influence on the areally-weighted hemispheric mean). But I thought, hmm, this can't really lead to that tremendous divergence before 1500 that the authors find. I was still scratching my head a bit at this point.

Then I read about the various transcription errors, values being shifted, etc. that the authors describe as existing in the dataset. And I thought, hmm, that sounds like an excel spread sheet problem, not a problem w/ the MBH98 proxy data set. It started to occur to me at this point that there might be some problems w/ the excel spreadsheet data that my colleague Scott Rutherford had kindly provided the authors at their request. But these problems sounded pretty minor from the authors' description, and the authors described a procedure to try to fix any obvious transcription errors, shifted cell values, etc. So I thought, hmm, they might not have fixed things perfectly, and that could also lead to some problems. But I still don't see how they get that huge divergence back in time from this sort of error...

Still scratching my head at this point...Then finally this afternoon, some clues. After looking at their on-line description one more time, I became disturbed at something I read. The data matrix they're using has 112 columns! Well that can't be right! That's can't constitute the Mann et al (1998) dataset. There are considerably more than that number of independent proxy indicators necessary to reproduce the stepwise Mann et al reconstruction. Something is amiss!

Well, 112 is the number of proxy indicators used back to 1820. But some of these indicators are principal components of regional sub-networks (e.g. the Western U.S. ITRDB tree-ring data) to make the dataset more manageable in size, and those principal components (PCs) are unique to the time interval analyzed. So there is some set of PC series for the 1820-1980 period. Farther back in time, say, back to 1650 there are fewer data series the regional sub-networks. So we recalculate a completely different EOF/PC basis set for that period, and that constitutes an additional, unique set of proxy indicators that are appropriate for a reconstruction of the 1650-1980 period.

PC #1 from one interval is not equivalent to PC#1 from a different interval. This turns out to be the essential detail. A reconstruction back to 1820 calibrated against the 20th century needs to make use of the unique set of proxy PCs available for the 1820-1980 period. A reconstruction back to 1650 calibrated against the 20th century needs to make use of the independent (smaller) set of PC series available for the 1650-1980

period,  
 and so on, back to 1400.  
 So there have to be significantly more than 112 series available to perform  
 the iterative, stepwise reconstruction approach of Mann et al (1998), because each  
 sub interval actually has a unique set of PC series representations of various  
 proxy sub-networks. Then it started to hit me. The PC#1 series calculated for  
 networks of similar size (say, the network available back to 1820 and that available back  
 to 1750) should be similar. But as the sub-network gets sparser back in time, the PC#1  
 series will resemble less and less the PC#1 series of the denser networks available  
 at later times. PC#1 of the western ITRDB tree-ring calculated for the 1400-1980 period  
 will bear almost no resemblance to the PC#1 series of the western N.Amer ITRDB  
 data calculated for the 1820-1980 period during their interval (1820-1980) of  
 mutual overlap.  
 Then it really hit me. What--just what--if the proxy data had been pigeonholed  
 into a 112 column matrix by the following (completely inappropriate!) procedure: What  
 if it had been decided that there would only be 1 column for "PC #1 of the Western ITRDB  
 tree ring data", even though that PC reflects something completely different over each  
 sub-interval. Well, that can't be done in a reasonable way. But it can be done  
 in an \*unreasonable\* way: by successively overprinting the data in that column as  
 one stores the PCs from later and later intervals. So a given column would reflect PC#1  
 of the 1400-1980 data from 1400-1450, PC#1 of the 1450-1980 from 1450-1500, PC#1 of  
 the 1500-1980 data for 1500-1650, PC#1 of the 1650-1980 data for 1650-1750, etc.  
 and so on.  
 In this process, the information necessary to calibrate the early PCs would be  
 obliterated with each successive overprint. The resulting 'series'  
 corresponding to that column of the data matrix, an amalgam of increasingly unrelated  
 information down the column, would be completely useless for calibration of the earlier data.  
 A reconstruction back to AD 1400 would be reconstructing the PC#1 of the  
 1400-1450 interval based on calibration against the almost entirely unrelated PC#1 of  
 the 1820-1980 interval. The reconstruction of the earliest centuries would be  
 based on a completely spurious calibration of an unrelated PC of a much later proxy sub  
 network.  
 And I thought, gee, what if Scott (sorry Scott), had \*happened\* to do this in  
 preparing the excel file that the authors used. Well it would mean that, progressively  
 in earlier centuries, one would be reconstructing an apple, based on calibration against  
 an orange. It would yield completely meaningless results more than a few  
 centuries ago. And then came the true epiphany--ahhh, this could lead to the kind of result the

authors

produced. In fact, it seemed to me that this would almost \*insure\* the result that the authors get--an increasing divergence back in time, and total nonsense prior to 1500 or

so. At this point, I knew that's what Scott must have done. But I had to confirm.

I simply had to contact Scott, and ask him: Scott, when you prepared that excel file for

these guys, you don't suppose by any chance that you might have....

And, well, I think you know the answer.

So the proxy data back to AD 1820 used by the authors may by-in-large be correct (aside

from the apparent transcription/cell shift errors which they purport to have caught, and

fixed, anyway). The data become progressively corrupted in earlier centuries. By the

time one goes back to AD 1400, the 1400-1980 data series are, in many cases, entirely

meaningless combinations of early and late information, and have no relation to the

actual proxy series used by Mann et al (1998).

And so, the authors results are wrong/meaningless/useless. The mistake made insures,

especially, that the estimates during the 15th and 16th centuries are entirely spurious.

So whose fault is this? Well, the full, raw ascii proxy data set has been available on

our anonymous ftp site [1]ftp://holocene.evsc.virginia.edu/pub/MBH98/

and the authors were informed of this in email correspondence. But they specifically

requested that the data be provided to them in excel format. And Scott prepared it for

them in that format, in good faith--but overlooked the fact that all of the required

information couldn't possibly be fit into a 112 column format. So the file Scott

produced was a complete corruption of the actual Mann et al proxy data set, and

essentially useless, transcription errors, etc. aside. The authors had full access to

the uncorrupted data set. We therefore take no reasonability for their use of corrupted

data.

One would have thought that the authors might have tried to reconcile their completely

inconsistent result prior to publication. One might have thought that it would at least

occur to them as odd that the Mann et al (1998) reconstruction is remarkably similar to

entirely independent estimates, for example, by Crowley and Lowery (2000). Could both

have made the same supposed mistake, even though the data and method are entirely

unrelated. Or might M&M have made a mistake? Just possibly, perhaps???

Of course, a legitimate peer-review process would have caught this problem. In fact, in

about 48 hours if I (or probably, many of my colleagues) had been given the opportunity

to review the paper. But that isn't quite the way things work at "E&E" I guess. I guess

there may just be some corruption of scientific objectivity when a journal editor seems

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more interested in politics than science.

The long and short of this. I think it is morally incumbent upon E&E to publish a full

retraction of the M&M article immediately. Its unlikely that they'll do this, but its

reasonable to assert that it would be irresponsible for them not to if the issue arises.

I think that's the end of the story. Please, again, keep this information under wraps

for next day or two. Then, by all means, feel free to disseminate this information as

widely as you like...

Mike

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Professor Michael E. Mann  
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University of Virginia  
Charlottesville, VA 22903

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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5225. 2003-10-29

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date: wed, 29 Oct 2003 09:24:45 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Fwd: Re: Proxies in MBH

to: Stephen H Schneider <shs@stanford.edu>, Richard Kerr <rkerr@aaaas.org>, Andy

Revkin <anrevk@nytimes.com>, David Appell <appell@nasw.org>,

Annie\_Petsonk@environmentaldefense.org, Mike MacCracken <mmaccrac@comcast.net>,

Michael Oppenheimer <omichael@Princeton.EDU>, "Socci.Tony-epamail.epa.gov"

<Socci.Tony@epamail.epa.gov>, Tim\_Profeta@lieberman.senate.gov,

rbradley@geo.umass.edu, mhughes@lrr.arizona.edu, Jonathan Overpeck

<jto@u.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford

<srutherford@rwu.edu>, Gabi Hegerl <hegerl@duke.edu>, tom crowley

<tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Tim Osborn

<t.osborn@uea.ac.uk>, Gavin Schmidt <gavin@isis.giss.nasa.gov>, Rob Dunbar

<dunbar@stanford.edu>, zubeke@onid.orst.edu, Ross Gelbspan <ross@theworld.com>, Ben

Santer <santer1@llnl.gov>, thompson.4@osu.edu, thompson.3@osu.edu, Keith Briffa

<k.briffa@uea.ac.uk>, peter.stott@metoffice.com

Below is the vindicating email,

mike

Date: wed, 09 Apr 2003 05:58:29 -0400

To: Steve McIntyre <smcintyre@cgxenergy.com>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Proxies in MBH

Cc: Scott Rutherford <srutherford@deschutes.gso.uri.edu>

Dear Mr. McIntyre,

These data are available on an anonymous ftp site we have set up. I've forgotten the

exact location, but I've asked my colleague Dr. Scott Rutherford if he can provide you

with that information.

best regards,

Mike Mann

cg2003

At 01:47 PM 4/8/2003 -0400, Steve McIntyre wrote:

Dear Dr. Mann,

I have been studying MBH98 and 99. I located datasets for the 13 series used in 99 at

[1]<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/Millennium/DATA/PROXIES/> (the

convenience of the ftp: location being excellent) and was intereseted in locating

similar information on the 112 proxies referred to in MBH98, as well as listing (the

listing at [2][http://www.ngdc.noaa.gov/paleo/ei/data\\_supp.html](http://www.ngdc.noaa.gov/paleo/ei/data_supp.html) is for 390 datasets, and

I gather/presume that many of these listed datasets have been condensed into PCs, as

mentioned in the paper itself. Thank you for your attention.

Yours truly,

Stephen McIntyre,  
Toronto, Canada

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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1258. 2003-10-30

cc: Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, "raymond s.bradley" <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, Scott Rutherford <[srutherford@rwu.edu](mailto:srutherford@rwu.edu)>

date: Thu, 30 Oct 2003 20:04:06 -0700

from: [mhughes@ltrr.arizona.edu](mailto:mhughes@ltrr.arizona.edu)

subject: Re: Can you believe it???

to: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>

Mike - my ability to take part in this discussion is extremely limited - I will probably not see e-mail again until November 5, and I am today unable to open attachments here, so I cannot see and comment on the latest version of your draft rebuttal. I think we are eight hours ahead of the UK and 14 ahead of the US east coast so I will probably not see any replies from any of you before Wednesday (unless you're working late on Thursday). This means my name cannot go on anything new before next Wednesday.

I think we need to have a detailed rebuttal document on file, for use on a case-by-case basis, but I do not think it can be used as a "press release". I am in strong sympathy with the tone of both Keith's remarks and Ray's suggestion. Even in the "file copy" of the rebuttal document, there should be not the slightest reference to MM's motives or expertise, nor to the history of the journal E and E. Rather, it should detail the flaws in their article. In fact, it might best be viewed as a briefing document for scientific colleagues performing the role Ray suggests for CRU.

On Ray's suggestion, it might be good, if the colleagues concerned were willing to get involved in this lousy business and give of their time, to broaden the base beyond Keith, Phil and Tim. Perhaps Tom Wigley, with his NCAR base, could

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be of help in pulling this off. His name, and perhaps a couple of other quantitative big shots who have not been involved in reconstructions on this time scale would extend the "authority" beyond we reconstructers. I remember the way Rick Anthes rallied people around Ben Santer in the previous incarnation of this fight and I think the statement he and others issued played a big part in showing the scientific community what was going on

Mike - any more news about USA Today?

Cheers, Malcolm

Quoting "Michael E. Mann" <mann@virginia.edu>:

>

>

> sorry, some typos fixed and minor changes in the attachment.

>

> please work w/ this if you care to makes

> edits/additions/suggestions/etc....

>

>

> thanks,

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> mike

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> At 04:23 PM 10/30/2003 +0000, Tim Osborn wrote:

>

> Mike,

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> in case you're worried by a lack of response from this side of the  
> Atlantic, then it's because Phil is away today and Keith and I have just  
> been in a project progress meeting all afternoon.

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> Cheers

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> Tim

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> At 16:16 30/10/2003, Michael E. Mann wrote:

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> Dear Tim et al,

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> Attached is my response, after several days of looking at what they've  
> done.

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> I think this will speak for itself.

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> I look forward to your comments and thoughts,

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> mike

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> At 02:13 PM 10/30/2003 +0000, Tim Osborn wrote:

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> Mike et al.,

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cg2003

> have you seen the update that McIntyre has put on  
> <http://www.climate2003.com/>  
> - including copies of all emails etc. regarding obtaining the data. He's got  
> wind of your reply from David Appell, and responded in some way. They're  
> getting into the argument of who's fault it was that the data they used were  
> wrong - whereas the thing to focus on is that their results are wrong, rather  
> than who's fault this was.

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> Cheers

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> Tim

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> At 14:02 30/10/2003, you wrote:

> Guys, can you take a look at this.

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> I think that everything I say here is true! But we've got to be sure.

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> There are more technical things they did wrong that I want to add, but this  
> is the critical bit--what do you think. Comments? Thanks...

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> mike

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> \_\_\_\_\_  
> The recent paper by McIntyre and Mckitrick (Energy and Environment, 14,  
> 751-771) claims to be an "audit" of the analysis of Mann, Bradley and Hughes  
> (1998) or "MBH98". An audit involves a careful examination, using the same  
> data and following the exact procedures used in the report or study being  
> audited. McIntyre and Mckitrick ("MM") have done no such thing, having used  
> neither the data nor the procedures of MBH98. Their analysis is notable only  
> in how deeply they have misrepresented the data, methods, and results of  
> MBH98. Journals that receive critical comments on a previously published  
> papers always provide the authors who are being criticized an opportunity to  
> review the study prior to publication, and offer them the chance to respond.  
> This is standard operating procedure in any legitimate peer-reviewed  
> scientific journal. Mann and colleagues were never given this opportunity,  
> nor were any other leading paleoclimate scientists that we're familiar with.  
> It is unfortunate that the profound errors, and false and misleading  
> statements, and entirely spurious results provided in the McIntyre and  
> Mckitrick article were ever allowed to see the light of day by those would  
> have been able to detect them. . We suspect the extremely checkered history  
> of "Energy and Environment" has some role to play in this. The authors should  
> retract their article immediately, and issue a public apology to the climate  
> research community for the injustice they have done in publishing and  
> promoting this deeply deceptive and flawed analysis.

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> Not only were critical errors made in their analysis that render it  
> thoroughly invalid, but there appear to have been several strikingly  
> subjective decisions made to remove key indicators of the original MBH98  
> network prior to AD 1600, with a dramatic impact on the resulting  
> reconstruction. It is precisely the over which the numerous indicators were

> removed (pre 1600 period) during which MM reconstruct anomalous warmth that  
 > is in sharp opposition to the cold conditions observed in MBH98 and nearly  
 > all other independent published estimates that we know of.

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> while the authors dutifully cite the small inconsistency between the number  
 > of proxy indicators reported by, and found in the public data archive, of  
 > Mann et al back in time (there indeed appear to have been some minor typos in  
 > the MBH98 paper), it is odd that they do not cite the number of indicators in  
 > their putative version of the Mann et al network based on the independent  
 > collection of data, back time. The reader is literally left to do a huge  
 > amount of detective work, based on the tables in their pages 20-23, to  
 > determine just what data have been eliminated from the original Mann et al  
 > network. It seems odd, indeed, that their "substitutions" of other versions  
 > (or in some case, only apparent, and not actual, versions) of proxy data  
 > series for those in the original Mann et al (1998) network has the selective  
 > effect of deleting key proxy indicators that contribute dramatic cooling  
 > during the 16th century, when the MM reconstruction shows an anomalous  
 > warming departure from the Mann et al (1998) and all other published Northern  
 > Hemisphere temperature reconstructions.

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> In point of fact, not only were the individual WNA data all available on the

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> public ftp site provided by Mann and colleagues:  
> ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/, but they were  
> also available, despite the claims to the contrary by MM, on NOAA's website  
> as well: ftp://ftp.ngdc.noaa.gov/paleo/treering/chronologies/northamerica/usa

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> The deletion of this critical (see Mann et al, 1999) dataset appears to one  
> of the more important censorings performed by MM that allows them to achieve  
> their spurious result of apparent 15th-16th century warmth.

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> We have not, as yet, finished determining just how many important indicators  
> were subtly censored from the MBH98 dataset by the various subjective  
> substitutions described on pages 20-23. However, given the relatively small  
> number of indicators available between 1400-1500 in the MBH98 network (22-24)  
> and their elimination of some of the more critical ones, it would appear that  
> this subjective censoring of data, alone, explains the spurious, misleading,  
> and deceptive result achieved by the authors.

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> Incidentally, MBH98 go to great depths to perform careful cross-validation  
> experiments as a function of increasing sparseness of the candidate  
> predictors back in time, to demonstrate statistically significant  
> reconstructive skill even for their earlier (1400-1450) reconstruction  
> interval. MM describe no cross-validation experiments. We wonder what the  
> verification resolved variance is for their reconstruction based on their  
> 1400-1450 available network, during the independent latter 19th century  
> period?

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> There are numerous other serious problems that would render the MM analysis  
> completely invalid, even in the absence of the serious issue raised above,  
> and these are detailed below

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>  
> Professor Michael E. Mann

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> sunclock:  
> <http://www.cru.uea.ac.uk/~timo/sunclock.htm>\_\_\_\_\_

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> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>  
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>  
> Professor Michael E. Mann  
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> Department of Environmental Sciences, Clark Hall  
>  
> University of Virginia  
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> Charlottesville, VA 22903  
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>  

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>  
> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>  
>

1689. 2003-10-30

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cc: [mhughes@ltrr.arizona.edu](mailto:mhughes@ltrr.arizona.edu)  
date: Thu, 30 Oct 2003 12:44:45 -0500  
from: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>  
subject: Re: One way out....  
to: Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, "raymond s. bradley"  
<[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, p.jones@uea.ac.uk  
Thanks Keith,  
I see the mutual collaboration as well underway now, w/ the Rutherford et al  
paper sort of  
representing our pilot effort. I much look forward to increasingly closer  
interaction among  
the group-that can only lead to good things, better science, a better  
understanding of the  
science--so this all sounds good to me.  
I think its appropriate to note that are still legitimate differences and  
uncertainties (as  
indicated in the spread of different empirical and model estimates shown in the  
various  
spaghetti plots we've all produced ). But that \*this\* is not one of them--I  
think all of  
the errors I've documented in MM are correct, in particular the very convenient  
censoring  
of ITRDB PC #1 and one of the oldest Jacoby tree-ring series of the network, and  
that's how  
they get that ridiculous result...But if you think some details aren't clear,  
I'd like to  
discuss them/try to clarify them. I'd like to hear what everyone thinks about  
the facts.  
We can soften the tone. I'm pretty darned sure of the facts, having spent about  
4 days  
pouring over this, looking at the data series, re-reading their descriptions,  
looking at  
their codes, etc...So I'd like to discuss any questions in what I've written,  
after you all  
have had time to read over the paper, my response, etc...  
thanks,

cg2003

mike

At 05:11 PM 10/30/2003 +0000, Keith Briffa wrote:

Ray et al

I agree with this idea in principle . Whatever scientific differences and fascination with the nuances of techniques we may /may not share, this whole process represents the most despicable example of slander and down right deliberate perversion of the scientific process , and bias (unverified) work being used to influence public perception and due political process. It is , however, essential that you (we) do not get caught up in the frenzy that these people are trying to generate, and that will more than likely lead to error on our part or some premature remarks that we might regret. I do think the statement re Mike's results needs making , but only after it can be based on repeated work and in full collaboration of us all. I am happy to push Tim to take the lead and collaborate in this - and I feel we could get sanction very quickly from the DEFRA if needed. BUT this must be done calmly , and in the meantime a restrained statement but out saying we have full confidence in Mike's objectivity and independence - which we can not say of the sceptics. In fact I am moved tomorrow to contact Nature and urge them to do an editorial on this . The political machinations in Washington should NOT dictate the agenda or scheduling of the work - but some cool statement can be made saying we believe the "prats have really fucked up someway" - and that the premature publication of their paper is reprehensible . Much of the detail in Mikes response though is not sensible (sorry Mike) and is rising to their bate.

Keith

At 11:55 AM 10/30/03 -0500, raymond s. bradley wrote:

Tim, Phil, Keef:

I suggest a way out of this mess. Because of the complexity of the arguments involved, to an unformed observer it all might be viewed as just scientific nit-picking by "for" and "against" global warming proponents. However, if an "independent group" such as you guys at CRU could make a statement as to whether the M&M effort is truly an "audit", and if they did it right, I think that would go a long way to defusing the issue. It's clear from the figure that Reno Knuti sent yesterday that something pretty whacky happened in their analysis prior to ~AD1600, and this led Mike to figure out the problem. See:  
[1]file:///c:/eudora/attach/nh\_temp\_rec.jpg  
If you are willing, a quick and forceful statement from The Distinguished CRU Boys would help quash further arguments, although here, at least, it is already quite out of control.....yesterday in the US Senate the debate opened on the McCain-Lieberman bill to control CO2 emissions from power plants. Sen Inhofe stood up & showed the M & M figure and stated that Mann et al--& the IPCC assessment --was now disproven and so

cg2003

there was  
no reason to control CO2 emissions.....I wonder how many times a "scientific"  
paper gets  
reported on in the Senate 3 days after it is published....

Ray

--

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---

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1838. 2003-10-30

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cc: [mann@multiproxy.evsc.virginia.edu](mailto:mann@multiproxy.evsc.virginia.edu), [mhughes@ltrr.arizona.edu](mailto:mhughes@ltrr.arizona.edu)  
date: Thu, 30 Oct 2003 11:55:18 -0500  
from: "raymond s. bradley" <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>  
subject: One way out....  
to: Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, [p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk), [k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)  
<x-flowed>  
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gets reported on in the Senate 3 days after it is published....  
Ray  
</x-flowed>

2209. 2003-10-30

---

cc: Scott Rutherford <[srutherford@rwu.edu](mailto:srutherford@rwu.edu)>, Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, Keith  
Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, Phil Jones <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>  
date: Thu, 30 Oct 2003 09:38:07 -0500  
from: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>  
subject: Re: the usual stuff  
to: "raymond s. bradley" <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>  
Ray, that relates to another issue that I'll discuss among the more technical  
problems.

cg2003

Ray, this relates to a different problem. They didn't calculate PCs of the networks stepwise like we did, they calculated on the full available interval--so our PCs and their

PCs aren't PCs of the same thing! A minor point, but yet something else they did wrong (or at least, different)...

mike

Incidentally, do you have an explanation for this statement:

Indeed it was the

observation of the unusually poor fit between the MBH98 Texas-Mexico PCs and

the

underlying ITRDB data that led to the detailed audit undertaken in this paper.

Ray

At 04:44 AM 10/30/2003 -0500, you wrote:

Malcolm, Ray, Scott...

It looks like they've severely misrepresented the Mann et al proxy data in their

supposed recreation of the dataset (what give the result Figure 6d and their Fig 7

(bottom) that look so ridiculous.

I'll need your help to confirm this isn't my imagine. I believe this is what they've

done:

Look at table 7.5 in their paper (attached). If I'm reading correctly, they've completely misrepresented the PC series. They've obliterated most of our data

prior to

1600 based on their inability to find the same versions of the data underlying

our PCs

on the WDCP (even though we clearly have those individual series that make up the PCs in

the appropriate subdirectory of our public ftp site:

<ftp://holocene.evsc.virginia.edu/pub/MBH98/>

If so, the pre-1600 proxy data set they have created has nothing whatsoever to do w/ the

MBH proxy data set. They've eliminated all of the early ITRDB and Stahle PC series,

because they couldn't find the corresponding series on the WDCP site! Is this really

what they've done???

Please all read and let me know if this is your interpretation too. If so, this is

scandalous, absolutely scandalous. A brazen act of intellectual dishonesty.

But I need

some 2nd, 3rd, etc. opinions as to whether or not they've really done

something so

alarming here!!!

thanks, now back to sleep for me...

mike

At 12:22 AM 10/30/2003 -0700, mhughes@lrr.arizona.edu wrote:

Mike - I don't know if I've been getting all the correspondence, but I've certainly got plenty! I did see the one with the submitted version of the riposte to USA Today, but didn't see anything about whether they will carry

it.

As for the other point, we might want to consider sending the response to Energy and Environment - after all, their turn-round time is fast - in the expectation that they will not publish it. Maybe there are other possibilities as well as web sites? My gut feeling is to avoid advocacy outlets even though they are sympathetic - in the long run that would damage our credibility with, for example, the lieberman/McCains of this world. How about Scientific

American,

or the Chronicle? Dick Kerr and his ilk could also be viable too. I hope you are getting some sleep and rest now - adrenaline cold turkey is a horrible



cg2003

all

sensation so be careful! Also - heed Mike Oppenheimer's wise and kind words!  
These guys (or their allies) will hit back in some way, so let's not shoot

our bullets at once.

Back to the middle taiga.... Cheers, Malcolm

Quoting "Michael E. Mann" <mann@virginia.edu>:

>

>

> Hi Malcolm,

>

>

> have you been receiving all the correspondences? There is still the  
> possibility that the op-ed will run in USA Today tomorrow. We took the  
> liberty of signing you on, even though we couldn't get confirmation from  
> you on the final draft...

>

>

> Meanwhile, I've already discovered numerous major errors, and still  
> finding some more. I'm taking the initial stab that you and Ray took at  
> drafting a more formal response, and turning it into a detailed  
> description of their mistakes.

>

>

> I'm still open to thoughts about what to do with this. I personally don't  
> think that we should submit a response to E&E--that implicitly would  
> recognize it as a legitimate forum. And we can't do that.

>

>

> We could post the response on an appropriate website, and broadcast its  
> availability to the community. I'm guessing that David Appell would be  
> more than happy to provide a link from his blog to this...

>

>

> mike

>

>

> At 08:33 PM 10/29/2003 -0700, mhughes@ltrr.arizona.edu wrote:

>

>

>

>

> Mike - I assume you were dealing  
> with the following all along -

>

> [1]<http://www.uoguelph.ca/~rmckitri/research/trc.html>

>

> Cheers, malcolm

>

>

>

>

> Professor Michael E. Mann

>

> Department

> of Environmental Sciences, Clark Hall

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>

> University of Virginia

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> Charlottesville, VA 22903

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cg2003

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\*Climate System Research Center: 413-545-0659  
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Paleoclimatology Book Web Site: [5]<http://www.geo.umass.edu/climate/paleo/html>

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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2707. 2003-10-30

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cc: mhughes@ltrr.arizona.edu  
date: Thu, 30 Oct 2003 14:21:47 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Re: One way out....  
to: Keith Briffa <k.briffa@uea.ac.uk>, "raymond s. bradley"  
<rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk>, p.jones@uea.ac.uk  
Thanks a bunch Keith,  
Edited version looks great, we're definitely on track...  
I'll work on things tonight, though I'll probably crash very soon (going on  
about 48 hours  
no sleep now).  
The example I sent out is probably a slightly extreme scenario, but it makes the  
basic  
point.  
I need to revise the analysis to be just a bit to be closer to what I think  
they did (I  
probably shouldn't eliminate all the Jacoby series, just some of them).  
Will try to produce a nice version of the plot and send out before crashing  
tonight...  
thanks for the help!  
mike  
At 06:53 PM 10/30/2003 +0000, Keith Briffa wrote:  
Things obviously moving over there - this result looks good. Just thought I'd

cg2003

send this

first bit (up to dotted line) of edited version , to illustrate possible toning down?

Have to go now and feed daughter . Will wait til see your joint version first thing

tomorrow - rest assured, that am entirely with you on this and still appalled by the MM

stuff - but keeping your distance and calm stance is still urged.

all the best to all

any objections if I talk to Nature tomorrow?

Keith

At 01:31 PM 10/30/03 -0500, Michael E. Mann wrote:

Guys,

So the verification RE for the "censored" NH mean reconstruction? -6.64

The verification RE for the original MBH98 NH mean reconstruction: 0.42

I think the case is really strong now!

What if were to eliminate the discussion of all the other technical details

(and just

say they exist), and state more nicely that these series were effectively

censored by

their substitutions, and that by removing those series which they censored, I

get a

similar result, with a dismal RE.

And most people would keep the RE of 0.42 over the RE of -6, right? So this would make

that point. I think we also need to say something about the process, etc. (the intro was

based on something that Malcolm/Ray had originally crafted).

Thoughts, comments? Thanks,

mike

I'm thinking of a note saying basically this, and attaching this figure.

Could everybody sign on to something like this?

Thanks for all your help,

mike

At 05:11 PM 10/30/2003 +0000, Keith Briffa wrote:

Ray et al

I agree with this idea in principle . Whatever scientific differences and fascination

with the nuances of techniques we may /may not share, this whole process represents the

most despicable example of slander and down right deliberate perversion of the scientific process , and bias (unverified) work being used to influence public perception and due political process. It is , however, essential that you (we)

do not

get caught up in the frenzy that these people are trying to generate, and that

will more

than likely lead to error on our part or some premature remarks that we might

regret. I

do think the statement re Mike's results needs making , but only after it can

be based

on repeated work and in full collaboration of us all. I am happy to push Tim

to take the

lead and collaborate in this - and I feel we could get sanction very quickly

from the

DEFRA if needed. BUT this must be done calmly , and in the meantime a

restrained

statement but out saying we have full confidence in Mike's objectivity and

independence

- which we can not say of the sceptics. In fact I am moved tomorrow to contact

Nature

and urge them to do an editorial on this . The political machinations in

washington

should NOT dictate the agenda or scheduling of the work - but some cool

statement can be made saying we believe the "prats have really fucked up someway" - and that the premature publication of their paper is reprehensible . Much of the detail in Mikes response though is not sensible (sorry Mike) and is rising to their bate. Keith  
At 11:55 AM 10/30/03 -0500, raymond s. bradley wrote:  
Tim, Phil, Keef:  
I suggest a way out of this mess. Because of the complexity of the arguments involved, to an unformed observer it all might be viewed as just scientific nit-picking by "for" and "against" global warming proponents. However, if an "independent group" such as you guys at CRU could make a statement as to whether the M&M effort is truly an "audit", and if they did it right, I think that would go a long way to defusing the issue. It's clear from the figure that Reno Knuti sent yesterday that something pretty whacky happened in their analysis prior to ~AD1600, and this led Mike to figure out the problem. See:  
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Ray

--

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2802. 2003-10-30

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cc: Annie\_Petsonk@environmentaldefense.org, "raymond s.bradley"  
<rbradley@geo.umass.edu>, mhughes@lrr.arizona.edu, Tim Osborn  
<t.osborn@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>, Keith Briffa  
<k.briffa@uea.ac.uk>  
date: Thu, 30 Oct 2003 07:02:45 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: RE: Re: Fwd: Re: Editor's comments  
to: "Loschnigg, Johannes (Govt Affairs)" <Johannes\_Loschnigg@govt-aff.senate.gov>  
Dear Annie, Johannes,  
There is a late breaking development. It now looks, upon closer and closer  
reading, as if  
M&M, very subtly, dropped the key indicators of the Mann et al (1998) network  
from the  
period AD 1400-1600 in the reconstruction that they performed based on their own  
supposed  
'version' of the Mann et al network--thats the version that has the huge spike  
between  
1400-1600 (recall that the authors analysis using the Mann et al data network is  
wrong  
because of the data merge/scramble problems we've discussed before). The authors  
appear to  
generate the erroneous early warming spike by dropping out the key proxy data  
from the  
Mann et al network that gives that reconstruction its characteristic shape  
prior to 1600  
or so.  
They appear to have eliminated the pre-1600 Western North American and  
Texas/Mexico data  
used by Mann et al (1998) based on the argument they couldn't find the older  
data in the  
public domain. This despite that fact the data is on NOAA's website and our  
public site. I'm  
working to confirm that w/ a 2nd opinion/read from various colleagues, but I'm  
almost sure  
this is true.  
If so, it constitutes intellectual dishonesty most foul indeed!  
will update ASAP,  
mike

---

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3904. 2003-10-30

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cc: "Griggs, Dave" <dave.griggs@metoffice.com>, ocanz@ciudad.com.ar,  
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allali@mailcity.com, edeaa@servidor.unam.mx, lucka.kajfez.bogataj@bf.uni-lj.si  
date: Thu, 30 Oct 2003 16:30:55 +0000

cg2003

from: "Pritchard, Norah" <norah.pritchard@metoffice.com>  
subject: Request for information about potential authors for IPCC AR4  
to: sbrown@winrock.org, j.skea@psi.org.uk, ittekkot@zmt.uni-bremen.de,  
lbijlsma@worldbank.org, l.bijlsma@rikz.rws.minvenw.nl, rexcruz@laguna.net,  
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kandadji@intnet.ne, g37dieudonne@hotmail.com, j.skea@psi.org.uk,  
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Dear colleague

Request for urgent action - please reply by 21/11/2003

It is now time to begin planning the writing of the AR4 WG2 component of the 2007 reports.

Our first job is to work with you and your colleagues to generate an optimal set of

authors, contributors and editors to generate a top quality team.

1. Please can you help us update our lists by suggesting new people (not in TAR) you know

who have appropriate skills and experience and who you feel might be able and wish to be

involved. Please indicate in what capacity you feel they might best serve. (N.B.

Contact

information for the names is needed as well.)

[Please note that for 2007, we are especially keen to bring in new talent, a stronger

people-livelihoods component and a significantly higher proportion of talented authors and

editors from the South and from economies in transition. Please bear these strongly in mind

when replying.]

2. Also, at this stage we need to ask you to consider whether you would want to be involved

again and if so, in what capacity (naturally with no commitment yet, on either side).

We need your reply within three weeks (by 21/11/2003).

Thanks and best regards from your co-chairs and your new WG2 TSU team.

Martin Parry and Osvaldo Canziani

IPCC WGII TSU

Met Office, Fitzroy Road

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United Kingdom

Tel: + 44 (0) 1392 88 6888

Fax: + 44 (0) 1392 88 5681

[1]www.ipcc-wg2.org/index.htm

norah.pritchard@metoffice.com

4438. 2003-10-30

cc: "raymond s. bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@lrrr.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Mike MacCracken <mmaccrac@comcast.net>, Michael Oppenheimer <omichael@Princeton.EDU>, mann@multiproxy.evsc.virginia.edu, Stephen H Schneider <shs@stanford.edu>

date: Thu, 30 Oct 2003 15:56:28 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Fwd: check out who he cc's these to... Fwd: Proposal that Nature

to: mann@multiproxy.evsc.virginia.edu

Keith/Tim/Ray/Malcolm/Phil: Our email response will have to go out ASAP (we're preparing

for a mass emailing tomorrow).

cg2003

To those not yet in the know (please keep it confidential), we can now show that M&M censored most of our early data in their "improved" data set, by replacing longer series we had used (that go back to the 15th century), with "better" shorter versions that only go back to the 17th century. By doing so, they selectively deleted all of our proxy series that indicate significant 15th-16th century cooling. NOT KIDDING! They justified this by claiming they couldn't find the older data in the public domain, though we can cite two public sources where all these data were available. Removing the proxy data that they removed, we reproduce the anomalous warm spike result--but we can show that the resulting reconstruction completely fails the standard statistical verification tests, while our original reconstruction of course passed them fairly well. Its pretty serious stuff, and we're going to talk to Nature about doing a story on this. And there may be a need for a formal investigation into scientific dishonesty--but not quite the one the authors have in mind...

mike

X-Sender: sepp@mail.his.com  
X-Mailer: QUALCOMM Windows Eudora Version 5.2.1  
Date: Thu, 30 Oct 2003 08:49:56 -0500  
To: smcintyre@cgxenergy.com, rmckitri@uoguelph.ca  
From: "S. Fred Singer" <singer@sepp.org>  
Subject: Proposal that Nature consider withdrawing Mann,Bradley, Hughes 1998  
Cc: seitz@rockvax.rockefeller.edu, cstarr@epri.com, art@oism.org, rlindzen@mit.edu, wsoon@cfa.harvard.edu, sbaliunas@cfa.harvard.edu, pabelson@aaaas.org, dek@uclink4.berkeley.edu, fspilhaus@agu.org, jmarburg@ostp.eop.gov, James.R.Mahoney@noaa.gov, Vicki.Horton@noaa.gov

Gentlemen

I have now studied yr rejoinder to the rather inadequate reply from Michael Mann to yr devastating critique (in Energy & Environment) of the underlying data relating to the "Hockeystick" (the temperature history that has been used by the IPCC and others to suggest that the 20th century was the warmest in 1000 years). [See

<<http://www.uoguelph.ca/~rmckitri/research/trc.html>>[\[earch/trc.html\]\(http://www.uoguelph.ca/~rmckitri/research/trc.html\) \]](http://www.uoguelph.ca/~rmckitri/res</a></p></div><div data-bbox=)

[I had earlier served as a referee of yr basic paper published in E&E (Oct 2003), and subsequently spent several hours with Steve McIntyre to carefully review its main

points. See [www.climate2003.com/index.html](http://www.climate2003.com/index.html) ]

I propose that NATURE be asked to appoint an independent panel of statisticians, econometricians, (and others NOT connected in any way with climate studies) to conduct an investigation of the MBH98 paper and its critique by McIntyre and Mckittrick.

The purpose would be to determine the need to formally withdraw the paper.

This request to Nature should be signed by a large number of scientists, including, if possible, members of the Royal Society and other academies, editors of scientific



cg2003

journals, and public figures, such as scientific advisers to presidents and prime ministers.

Pls note that I am not suggesting culpability on the part of Mann or his coauthors.

They might not even have been aware of the gross mishandling of the data used in their

publications. Nor can one fault individual scientists connected to the IPCC -- since

IPCC accepts publication in a peer-reviewed journal as prima facie endorsement of its

correctness. The chief responsibility now lies with the editors of NATURE. Yr comments on this proposal are most welcome.

Fred Singer

S. Fred Singer, Ph.D.

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\*\*\*\*\*

"The improver of natural knowledge absolutely refuses to acknowledge authority, as such. For him, scepticism is the highest of duties; blind faith the one unpardonable sin."

> Thomas H. Huxley

\*\*\*\*\*

"If the facts change, I'll change my opinion. What do you do, sir? "

>J. M. Keynes

\*\*\*\*\*

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4451. 2003-10-30

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cc: mhughes@ltrr.arizona.edu

date: Thu, 30 Oct 2003 13:58:23 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: One way out....

to: Keith Briffa <k.briffa@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk>, p.jones@uea.ac.uk

Guys,

actually, that isn't quite yet a fair comparison, because I didn't do the stepwise

reconstruction using the eigenvector subsets they did--I just used 1 eigenvector and did

the whole 1400-1980 period. So stay tuned for an even more appropriate comparison...

thanks,

mike

At 05:11 PM 10/30/2003 +0000, Keith Briffa wrote:

Ray et al

I agree with this idea in principle . Whatever scientific differences and fascination

with the nuances of techniques we may /may not share, this whole process represents the

most despicable example of slander and down right deliberate perversion of the

cg2003

scientific process , and bias (unverified) work being used to influence public perception and due political process. It is , however, essential that you (we) do not get caught up in the frenzy that these people are trying to generate, and that will more than likely lead to error on our part or some premature remarks that we might regret. I do think the statement re Mike's results needs making , but only after it can be based on repeated work and in full collaboration of us all. I am happy to push Tim to take the lead and collaborate in this - and I feel we could get sanction very quickly from the DEFRA if needed. BUT this must be done calmly , and in the meantime a restrained statement but out saying we have full confidence in Mike's objectivity and independence - which we can not say of the sceptics. In fact I am moved tomorrow to contact Nature and urge them to do an editorial on this . The political machinations in Washington should NOT dictate the agenda or scheduling of the work - but some cool statement can be made saying we believe the "prats have really fucked up someway" - and that the premature publication of their paper is reprehensible . Much of the detail in Mikes response though is not sensible (sorry Mike) and is rising to their bate.

Keith  
At 11:55 AM 10/30/03 -0500, raymond s. bradley wrote:  
Tim, Phil, Keef:  
I suggest a way out of this mess. Because of the complexity of the arguments involved, to an uniformed observer it all might be viewed as just scientific nit-picking by "for" and "against" global warming proponents. However, if an "independent group" such as you guys at CRU could make a statement as to whether the M&M effort is truly an "audit", and if they did it right, I think that would go a long way to defusing the issue. It's clear from the figure that Reno Knuti sent yesterday that something pretty whacky happened in their analysis prior to ~AD1600, and this led Mike to figure out the problem. See:  
[1]file:///c:/eudora/attach/nh\_temp\_rec.jpg  
If you are willing, a quick and forceful statement from The Distinguished CRU Boys would help quash further arguments, although here, at least, it is already quite out of control.....yesterday in the US Senate the debate opened on the McCain-Lieberman bill to control CO2 emissions from power plants. Sen Inhofe stood up & showed the M & M figure and stated that Mann et al--& the IPCC assessment --was now disproven and so there was no reason to control CO2 emissions.....I wonder how many times a "scientific" paper gets reported on in the Senate 3 days after it is published....

Ray  
--  
Professor Keith Briffa,  
Climatic Research Unit

cg2003

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
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e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4509. 2003-10-30

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date: Thu, 30 Oct 2003 08:50:31 -0500  
from: Edward Cook <[drdendro@ldeo.columbia.edu](mailto:drdendro@ldeo.columbia.edu)>  
subject: NINO3 SST recon  
to: Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>

Hi Keith,  
Here is the Nino3 DJF SST recon back to 1408. I have attached the mean estimates that are online at NGDC. The mean is the average of several split early/late calibration/verification runs with successively longer subsets of Tex-Mex chronologies. The plot below shows the way in which the mean was put together through overlay plots of the individual model recons. As you can see, there is a high degree of coherence between the estimates. The attached \*.tabs file has all of the subset model estimates plus the mean and the actual data used for calibration/verification. So, you can look at the individual subset model recons as well. All of the models verify very well, by the way. I probably should have published this stuff years ago. I did it for Mark Cane. I have taken a quick look at that deconstruction of the MBH paper by McIntyre and Mckittrick. They claim to show a number of errors in the data Mike used. I know that you and Tim have worked with Mike's data as well. Did you find the same things? I'm just curious. I don't plan on weighing in on this mess other than to suggest that Mike, Ray, and Malcolm are living in glass houses when they criticize the Esper work in the way they do. One needs to be very careful about criticizing the analyses of others because turn-around is fair play and payback is a bitch. That is all I have to say.  
Cheers,  
Ed  
[cid:a05200f00bbc6c3a359fa@[10.0.1.201].1.0]

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Dr. Edward R. Cook  
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Director, Tree-Ring Laboratory  
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Email: [drdendro@ldeo.columbia.edu](mailto:drdendro@ldeo.columbia.edu)  
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Fax: 845-365-8152

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Embedded Content: NINO3\_PLTS.pdf: 00000001,0ce56f72,00000000,00000000 Attachment  
Converted:  
"c:\eudora\attach\NGDC\_NINO3\_RECON" Attachment Converted:  
"c:\eudora\attach\NINO3\_DJF\_RECONS.tabs.txt"

4768. 2003-10-30

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cc: k.briffa@uea.ac.uk, "Phil Jones" <p.jones@uea.ac.uk>, "raymond s.bradley"  
<rbradley@geo.umass.edu>, mhughes@ltrr.arizona.edu  
date: Thu, 30 Oct 2003 06:22:50 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: Re: Fwd: op ed for USA Today  
to: Tim Osborn <t.osborn@uea.ac.uk>

Thanks a bunch Tim,  
Well, we didn't add your name because we weren't sure, but USA Today probably  
won't publish  
it--if not we may try to distribute it.  
But more importantly, as we speak, I am drafting a long description of what they  
done  
wrong. Just over the last 24 hours I've discovered something extremely dishonest  
that it  
appears they did. In their reconstruction based on their 'redo' of the MBH98  
proxy network,  
the one that shows the ridiculous warming in the early centuries, it appears  
that they  
eliminated all of our ITRDB Western North American (and Stahle max latewood  
chronologies)  
from our network. As you guys know, the ITRDB WNA data are fairly important to  
our  
reconstruction. Based on Table 7.5 in their paper, if you read the fine details,  
it looks  
like they've just eradicated the earlier data because they claim they couldn't  
find it on  
the NGDC website--even though we all know the data are there. And more  
importantly, all of  
those data were on our public ftp site on holocene.  
So in one extremely dishonest stroke of data eradication, they removed the most  
important  
indicators from our network from 1400-1600--and I'm pretty sure that's how they  
get their  
spike. Would be interesting to see what cross-validation they get using \*their\*  
network  
available from 1400-present. I bet we're talking REs approaching negative  
infinity...  
So I think that is what they did! Do you guys have the paper--does anyone mind  
double-checking, and assuring that I'm correct about this. If I am, this is  
really  
scandalous, and it should be as broadcast as widely as possible. Note that they  
don't even  
report how many proxy data were available in their network back in time, they  
only show the  
# of reported/found proxies in the Mann et al network (apparently our data site  
was missing  
a few of the series). This is probably intentional as well--they didn't want to  
show how  
many series they had actually eliminated from the set. And of course, if they're  
using a  
completely different set of proxies, then they would have to reapply the  
selection rules,  
they can't just use the basis set that we had determined, based on application  
of the  
selection rules to the data at hand...

cg2003

So its looking increasingly dishonest, deceptive, and intentionally so. I've identified other problems, they used an incorrect version of the the Mann et al proxy dataset that Scott had put into excel format, so the early PC proxy series were overprinted w/ later ones kept in the same column. And they used inconsistent CRU surface temperature datasets and inconsistent normalization conventions to un-normalize the Mann et al EOFs, etc. And all of this could lead to significant differences. But I think its the dropping of the key predictors w/ barely a mention, that gives them the AD 1400-1600 spike. Second opinions--am I imagining this?

Thanks,  
mike

At 10:44 AM 10/30/2003 +0000, Tim Osborn wrote:

At 17:45 29/10/2003, you wrote:

We need to submit within the next hour or so, so its really do-or-die time!

Mike, was away yesterday, so I missed all the fun-and-games! If you went ahead and submitted it with my name on anyway, then that's fine because I would have agreed had I been

here. If you dropped me in my absence, then fine too - you had enough co-signees, I'm sure.

Going back to an earlier email when you were asking whether anyone had reviewed the E&E

piece by M&M (have I got the initials correct? have to avoid confusion with M&M sweets -

do you get them in the US? some are nuts, which seems appropriate!). Anyway, just

wanted to confirm that I did not review it.

Despite the hard and time consuming work that it evidently took you to get to the bottom

of their work's problems, I think it was essential to get this cleared up so soon. It's

important to get this information out as publicly as possible, so that nobody who wants

to push the M&M conclusions can do so while claiming ignorance of the fact that data

problems make their conclusions baseless and wrong. If you want to avoid the climatesceptics list then perhaps one of us (or all of us?) here in CRU could circulate

a note to that list, hence the cc to Keith and Phil. Let us know. Do you ever use the

CLIMLIST mailing list? It's not generally a debating type list, but I'm sure it would

be relevant to post something there that makes clear the M&M conclusions are invalid -

as a public information service?

Cheers

Tim

Dr Timothy J Osborn

Climatic Research Unit

School of Environmental Sciences, University of East Anglia

Norwich NR4 7TJ, UK

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phone: +44 1603 592089

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sunclock: [2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

4964. 2003-10-30

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cc: "raymond s.bradley" <rbradley@geo.umass.edu>, mhughes@lrr.arizona.edu, "Phil Jones" <p.jones@uea.ac.uk>, Scott Rutherford <srutherford@rwu.edu>

date: Thu, 30 Oct 2003 12:11:21 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Can you believe it???

to: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

Thanks Keith,

I really appreciate your help.

I'm happy for us to try to soften the tone, and will look forward to your suggested

changes, etc. in this regard.

I'm about 99% sure, at this point, that my facts are right though--look forward to hearing

what you think I've reading through it--its dense, takes some effort to figure out what

they eliminated. But they appear to have eliminated \*just\* the right series.. It really was

a censoring of data as far as I can tell, key data...

talk to you later,

mike

p.s. as for the target audience/date--I'll defer to you guys. I think, from Tim's comments,

this has to go out quickly. We've got to nip this in the bud before it gets any more play.

So I'm thinking, tomorrow at the latest.

Target audience--i think the idea is the same huge email distribution/listserv that they

sent their disinformation out to in the first place.

At 04:50 PM 10/30/2003 +0000, Keith Briffa wrote:

Mike and others

I am sorry but been in a meeting all day - my first impression of reading the text is to

caution against releasing this statement without more discussion. Do not be bounced into

saying stuff you are not sure of , and using emotive language that smacks of too

emotional a response . I am staying a while to read and comment in detail - and will try

to fax something. Have to go soon because of daughter and need to write 2 PhD proposals

tonight . Please clarify if there is a deadline that you are working too and what target

is this piece aimed at.?

Keith

At 09:35 AM 10/30/03 -0500, Michael E. Mann wrote:

Guys,

I'm right, aren't I????

mike

At 02:13 PM 10/30/2003 +0000, Tim Osborn wrote:

At 14:02 30/10/2003, you wrote:

Guys, can you take a look at this.

I think that everything I say here is true! But we've got to be sure.

There are more technical things they did wrong that I want to add, but this is the critical bit--what do you think. Comments? Thanks...  
mike

The recent paper by McIntyre and McKittrick (Energy and Environment, 14, 751-771) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998) or "MBH98". An audit involves a careful examination, using the same data and following the exact procedures used in the report or study being audited. McIntyre and McKittrick ("MM") have done no such thing, having used neither the data nor the procedures of MBH98. Their analysis is notable only in how deeply they have misrepresented the data, methods, and results of MBH98. Journals that receive critical comments on a previously published papers always provide the authors who are being criticized an opportunity to review the study prior to publication, and offer them the chance to respond. This is standard operating procedure in any legitimate peer-reviewed scientific journal. Mann and colleagues were never given this opportunity, nor were any other leading paleoclimate scientists that we're familiar with. It is unfortunate that the profound errors, and false and misleading statements, and entirely spurious results provided in the McIntyre and McKittrick article were ever allowed to see the light of day by those would have been able to detect them. . We suspect the extremely checkered history of "Energy and Environment" has some role to play in this. The authors should retract their article immediately, and issue a public apology to the climate research community for the injustice they have done in publishing and promoting this deeply deceptive and flawed analysis. Not only were critical errors made in their analysis that render it thoroughly invalid, but there appear to have been several strikingly subjective decisions made to remove key indicators of the original MBH98 network prior to AD 1600, with a dramatic impact on the resulting reconstruction. It is precisely the over which the numerous indicators were removed (pre 1600 period) during which MM reconstruct anomalous warmth that is in sharp opposition to the cold conditions observed in MBH98 and nearly all other independent published estimates that we know of. While the authors dutifully cite the small inconsistency between the number of proxy indicators reported by, and found in the public data archive, of Mann et al back in time (there indeed appear to have been some minor typos in the MBH98 paper), it is odd that they do not cite the number of indicators in their putative version of the Mann et al

network based on the independent collection of data, back time. The reader is literally left to do a huge amount of detective work, based on the tables in their pages 20-23, to determine just what data have been eliminated from the original Mann et al network. It seems odd, indeed, that their "substitutions" of other versions (or in some case, only apparent, and not actual, versions) of proxy data series for those in the original Mann et al (1998) network has the selective effect of deleting key proxy indicators that contribute dramatic cooling during the 16th century, when the MM reconstruction shows an anomalous warming departure from the Mann et al (1998) and all other published Northern Hemisphere temperature reconstructions.

Here are some blatant examples:

- 1) The authors (see their Figure 4) substitute a younger version of one of the Jacoby et al Northern Treeline series for the older version used by MBH98. This substitution has effect of removing a predictor of 15th century cooling [Incidentally, MM make much of the tendency for some tree ring series, such as this one, to show an apparent cooling over the past couple decades. Scientists with expertise in dendroclimatology know that this behavior represents a decrease in the sensitivity to temperature in recent decades that likely is related to conditions other than temperature which are limiting tree growth]
- 2) The authors eliminate, without any justification, the entire dataset of 70 Western North American (WNA) tree-ring series available between 1400 and 1600 (this dataset is represented, by MBH98, in terms of a smaller number of representative Principal Component time series). The leading pattern of variance in this data set exhibits conditions from 1400-1800 that are dramatically colder than the mid and late 20th century, and a very prominent cooling in the 15th century in particular. The authors eliminated this entire dataset because they claimed that the underlying data was not available in the public domain. In point of fact, not only were the individual WNA data all available on the public ftp site provided by Mann and colleagues: [1]<ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/>, but they were also available, despite the claims to the contrary by MM, on NOAA's website as well: [2]<ftp://ftp.ngdc.noaa.gov/paleo/treering/chronologies/northamerica/usa>

The deletion of this critical (see Mann et al, 1999) dataset appears to be one of the more important censorings performed by MM that allows them to achieve their spurious result of apparent 15th-16th century warmth.

We have not, as yet, finished determining just how many important indicators were subtly



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censored from the MBH98 dataset by the various subjective substitutions described on pages 20-23. However, given the relatively small number of indicators available between 1400-1500 in the MBH98 network (22-24) and their elimination of some of the more critical ones, it would appear that this subjective censoring of data, alone, explains the spurious, misleading, and deceptive result achieved by the authors. Incidentally, MBH98 go to great depths to perform careful cross-validation experiments as a function of increasing sparseness of the candidate predictors back in time, to demonstrate statistically significant reconstructive skill even for their earlier (1400-1450) reconstruction interval. MM describe no cross-validation experiments. We wonder what the verification resolved variance is for their reconstruction based on their 1400-1450 available network, during the independent latter 19th century period? There are numerous other serious problems that would render the MM analysis completely invalid, even in the absence of the serious issue raised above, and these are detailed below

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Professor Michael E. Mann  
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Charlottesville, VA 22903

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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832. 2003-10-31

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cc: r.warren@uea.ac.uk, iain Brown  
<Iain.Brown@uea.ac.uk>, anderson\_Kevin, e.tompkins, a.minns@uea.ac.uk  
date: Fri Oct 31 15:52:08 2003  
from: Mike Hulme <m.hulme@uea.ac.uk>  
subject: NERC capital equipment funds  
to: tyn.council

Some of you may have picked up an allocation of £8m by NERC for investment in capital equipment. This scheme has just been announced and the Tyndall Centre, along with 20 universities and the other NERC Centre and Institutes are eligible to submit up to three proposals for funding, typically in the range £50k to £250k each. Some of your universities may be on the shortlist of 20 (UEA is for example), but we have an opportunity to bid separately from our host institutions here. Limited staff costs may be considered. The deadline is 12 January. The announcement is attached. Preference will be given to bids that link to NERC's science strategy, benefit the wider community, and help regional collaboration. Whilst Tyndall Centre might not be a big user of major capital equipment, there may be one or two things that we should think of bidding for, e.g., virtual reality kit, access grid nodes, major compute or data storage facilities. Or maybe people can think laterally about capital equipment that would allow us to do new things. Any ideas to me please over the next few weeks.

Thanks,  
Mike

904. 2003-10-31

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cc: Ravi Sharma <Ravi.Sharma@unep.org>, Mohamed Hassan;;  
date: Fri, 31 Oct 2003 16:02:01 -0500  
from: Neil Leary <nleary@agu.org>  
subject: COP9, Milan  
to: bscholes@csir.co.za, hewitson@egs.uct.ac.za, hcenr@sudanmail.net, goutbi@yahoo.com, esiegfried@tellus.org, atgay@ucad.sn, jadejuwo@oauife.edu.ng, Desanker@psu.edu, manuel@carvalho.uem.mz, DUBEOP@mopipi.ub.bw, ogunlade@energetic.uct.ac.za, p\_batima@yahoo.com, anond@start.or.th, jratna@itmin.com, rlasco@laguna.net, yongyuan.yin@sdri.ubc.ca, wfer@ariel.efis.ucr.ac.cr, crrhcr@sol.racsa.co.cr, barros@at.fcen.uba.ar, agimenez@inia.org.uy, cgay@servidor.unam.mx, conde@servidor.unam.mx, gunab@glaucus.fcien.edu.uy, rawlinsa@carec.paho.org, achen@uwimona.edu.jm, koshy\_k@usp.ac.fj, abouhadid <ruafah@rusys.eg.net>, adepetua@unijos.edu.ng, <nyongao@hisen.org>, <rolph@seychelles.sc>, <knas@iconnect.co.ke>, suppakorn@start.or.th, ian.burton@ec.gc.ca, crrhcr@racsa.co.cr, tom.downing@sei.se, saleemul.huq@iied.org, fuj.jaeger@nexta.at, richard.klein@pik-potsdam.de, isabelle@enda.sn, harasawa@nies.go.jp, PARRYML@aol.com, anand@cc.iitb.ernet.in, bscholes@csir.co.za, Rwatson@worldbank.org, nobre@cptec.inpe.br, lal321@hotmail.com, lindam@atd.ucar.edu, Peter.Whetton@csiro.au, m.hulme@uea.ac.uk, sberesford@agu.org

<x-flowed>  
To: AIACC PIs and Technical Committee  
Dear Friends,  
If you are planning to attend the UNFCCC 9th Conference of the Parties (COP9) in Milan, please inform me of the dates that you will be there. Please also inform me of any events in which you will be presenting -- or if you know of an event in which AIACC participation

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might be welcome.

FYI - UNEP is planning a side event for Tuesday, December 9th, on climate change adaptation. Probably 2 or 3 AIACC participants will be invited to give presentations at the event. Others who are not presenting are encouraged to attend. More information will be sent when we have a more concrete plan.

Cheers,

Neil

--

Neil A. Leary

Science Director

Assessments of Impacts and Adaptations to Climate Change (AIACC)

The International START Secretariat

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Email: nleary@agu.org

Website: www.start.org

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1172. 2003-10-31

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cc: mhughes <mhughes@lrr.arizona.edu>

date: Fri Oct 31 10:47:21 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: RE: CLIMLIST

to: "Michael E. Mann" <mann@virginia.edu>, f055 <T.Osborn@uea.ac.uk>, "p.jones"

<p.jones@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, f055

<T.Osborn@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

Hi all - I too have had some problems as to which specific version is where we are at - BUT

I think the latest draft as sent by Mike really is virtually there (perhaps some typos to

be ironed out (e.g. 'were' instead of 'was' on line 7 of point 2) but I am generally very

happy with the tone and balance. Much of Tim's fears (justifiable points on not providing

them with wiggle out and distraction options) are allayed by the calm provisos about not

being categorical etc. The question now arises as to how to put this out - I believe it

does need to go out early so as to be available when the rest of the press start to pick up

the MM propaganda. Whether it should be just signed by MBH is up to you. I AM NOT averse

to signing, but wonder whether it is a better tactic to put out a separate statement (us,

Tom W. and whoever as suggested by Ray, saying we abhor the way this MM paper has been

published and publicised without proper scrutiny). I fully agree with the statement as now

written however, and willing to go with the majority view. My suggestion about redoing the

"audit" was made in good faith and in no way implied I concurred with MM (in case anyone

got the impression that I was not wholly "on side" here).

So what does everyone else say now?

REGARDING NATURE - spoke to Heike Langenberg, in the London office and she said it

sounded like a potential NEWS item, and asked me to send some details by email and she

would forward to the appropriate office - seemed positive. I will do. The

statement (s)

should anyway go soon on CLIMLIST and then we could quietly contact a few people we know in the media ?

Keith

At 05:38 AM 10/31/03 -0500, Michael E. Mann wrote:

p.s. Keith, any word from Nature. Should I contact them independently? And what about

Science? or "Climatic Change" (I have little doubt that Steve S could find justification to publish this their in an instant)...

thoughts?

mike

At 03:01 AM 10/31/2003 +0000, f055 wrote:

Dear all,

I've just finished preparing a detailed response offline, only to log on to send it to you all and find new versions from Mike plus more comments and information. Well, I don't have time to change my message now, so will paste it below this message. But bear in mind that the new draft may well have allayed many of my concerns - in particular, a quick glance shows the figure to be much more convincing than the one Mike circulated earlier, indeed it seems to be utterly convincing! I'll reply again on Friday

morning once I've had time to read the new draft. In the meantime, here is my message as promised.

\*\*\*\*\*

Dear MBH (cc to CRU),

The number of emails has been rather overwhelming on this issue and I'm struggling to catch up with them! But I will attempt to catch up with a few things here...

(1) The single worst thing about the whole M&M saga is not that they did their study, not that they did things wrong (deliberately or by accident), but that neither they nor the journal took the necessary step of investigating whether the difference between their results and yours could be explained simply by some error or set of errors in their use of the data or in their implementation of your method. If it turns out, as looks likely from Mike's investigation of this, that their results are erroneous, then they and the journal will have wasted countless person-hours of time and caused much damage in the climate policy arena.

(2) Given that this is the single worst thing about the saga, we must not go and do exactly the same in rushing out a response to their paper. If some claims in the response turned out to be wrong, based on assumptions about what M&M did or assumptions about how M&M's assumptions affect the results, then it would end up with a number of iterations of claim and counter claim. Ultimately the issue might be settled, but by then the waters could be so muddied that it didn't matter.

(3) Not only do I advise against an overly rushed response, but I'm also wondering whether it really ought to be only from MBH, for three reasons.

(i) It is your paper/results that are being attacked.

(ii) It is difficult to endorse everything that Mike has put in the draft response because I don't know 100% of the details of MBH and the MBH data. Sure, I can endorse some things, but others I wouldn't know. Sure, I accept Mike's explanation because he's looked at this stuff for 4 days and I believe he'll have got it right - but that's different to an independent check. That must come from Ray or Malcolm if possible.

(iii) If it does come to any independent assessment of who's right and who's wrong, then it would be difficult for us to be involved if we had already signed up to what some might claim to be a knee-jerk reaction to the M&M paper. If that happened, then you would want us to be free to get involved to make sure the process was fair and informed.

This sounds like a cop out, but - like I say - I'm not sure about point (3) so feel free to try to convince me otherwise if you wish. Anyway Keith or Phil may be happy to sign up to a (quick or slow) response, despite my

reservations above.

I really advise a very careful reading of M&M and their supplementary website to ensure that everything in the response is clearly correct - precisely to avoid point (2). I've only just started to do this, but already have some questions about the response that Mike has drafted.

(a) Mike, you say that many of the trees were eliminated in the data they used. Have you concluded this because they entered "NA" for "Not available" in their appendix table? If so, then are you sure that "NA" means they did not use any data, rather than simply that they didn't replace your data with an alternative (and hence in fact continued to use what Scott had supplied to them)? Or perhaps "NA" means they couldn't find the PC time series published (of course!), but in fact could find the raw tree-ring chronologies and did their own PCA of those? How would they know which raw chronologies to use? Or did you come to your conclusion by downloading their "corrected and updated" data matrix and comparing it with yours - I've not had time to do that, but even if I had and I

found some differences, I wouldn't know which was right seeing as I've not done any PCA of western US trees myself? My guess would be that they downloaded raw tree-ring chronologies (possibly the same ones you used) but then applied PCA only to the period when they all had full data - hence the lack of PCs in the early period (which you got round by doing PCA on the subset that had earlier data). But this is only a guess, and this is the type of thing that should be checked with them - surely they would respond if asked? - to avoid my point (2) above. And if my guess were right, then your wording of "eliminated this entire data set" would come in for criticism, even though in practise it might as well have been.

(b) The mention of ftp sites and excel files is contradicted by their email record on their website, which shows no mention of excel files (they say an ASCII file was sent) and also no record that they knew the ftp address. This doesn't matter really, since the reason for them using a corrupted data file is not relevant - the relevant thing is that it was corrupt and had you been involved in reviewing the paper then it could have been found prior to publication. But they will use the email record if the ftp sites and excel files are mentioned.

(c) Not sure if you talk about peer-review in the latest version, but note that

they acknowledge input from reviewers and Fred Singer's email says he refereed it - so any statement implying it wasn't reviewed will be met with an easy response from them.

(d) Your quick-look reconstruction excluding many of the tree-ring data, and the verification RE you obtain, is interesting - but again, don't rush into

using these in any response. The time series of PC1 you sent is certainly different from your standard one - but on the other hand I'd hardly say you "get a similar result" to them, the time series look very different (see their fig 6d). So the dismal RE applies only to your calculation, not to their reconstruction. It may turn out that their verification RE is also very negative, but again we cannot assume this in case we're wrong and they easily counter the criticism.

(e) Claims of their motives for selective censoring or changing of data, or for the study as a whole, may well be true but are hard to prove. They would claim that their's is an honest attempt at reproducing a key scientific result. If they made errors in what they did, then maybe they're just completely out of their depth on this, rather than making deliberate errors for the purposes of achieving preferred results.

(f) The recent tree-ring decline they refer to seems related to tree-ring-width not density. Regardless of width of density, this issue cannot simply be dismissed as a solved problem. Since they don't make much of an issue out of it, best just to ignore it.

(g) [I'm rambling now into an un-ordered list of things, so I'll stop soon!] The various other problems relating to temperature data sets, detrended standard deviations, PCs of tree-ring subsets etc. sound likely errors -

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though I've got no way of providing the independent check that you asked for. But it is again a bit of a leap of faith to say that these \*explain\* the different results that they get. Certainly they throw doubt on the validity of their results, but without actually doing the same as them it's not possible to say if they would have replicated your results if they hadn't made these errors. After all, could the infilling of missing values have made much difference to the results obtained, something that they made a good deal of fuss about?

(h) To say they "used neither the data nor the procedures of MBH98" will also be an easy target for them, since they did use the data that was sent to them and seemed to have used approximately the method too (with some errors that you've identified). This reproduced your results to some extent (certainly not perfectly, but see Fig 6b and 6c). Then they went further to redo it with the "corrected and updated" data - but only after first

doing approximately what they claimed they did (i.e. the audit).

These comments relate to random versions of the draft response, so apologies if they don't all seem relevant to the current draft. I don't have these in front of me, here at home, so I'm doing this from memory of what I've read over the past few days. But nevertheless, the point is that a quick response would ultimately require making a number of assumptions about what they did and assumptions about whether this explains the differences or not - assumptions that might be later shot down (in part only, at most, but still sufficient to muddy the debate for most outsiders). A quick response ought to be limited to something like:

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The recent paper by McIntyre and McKittrick (2003; hereafter MM03) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998; hereafter MBH98). MM03 are unable to reproduce the Northern Hemisphere temperature reconstruction of MBH98 when attempting to use the same proxy data and methods as MBH98, though they obtain something similar with clearly anomalous recent warming (their Figure 6c). They then make many modifications to the proxy data set and repeat their analysis, and obtain a rather different result to MBH98. Unfortunately neither M&M nor the journal in which it was published took the necessary step of investigating whether the difference between their results and MBH98 could be explained simply by some error or set of errors in their use of the data or in their implementation of the MBH98 method. This should have been an essential step to take in a case such as this where the difference in results is so large and important. Simple errors must first be ruled out prior to publication. Even if the authors had not undertaken this by presenting their results to the authors of MBH98, the journal should certainly have included them as referees of the manuscript.

A preliminary investigation into the proxy data and implementation of the method has already identified a number of likely errors, which may turn out to be the cause of the different results. Rather than repeating M&M's failure to follow good scientific practise, we are withholding further comments until we can - by collaboration with M&M if possible - be certain of exactly what changes to data and method were made by M&M, whether these changes can really explain the differences in the results, and eventually which (if any) of these changes can be justified as equally valid (given the various uncertainties that exist) and which are simply errors that invalidate their results.

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Hope you find this all helpful, and despite my seemingly critical approach, take them in the spirit with which they are aimed - which is to obtain a strong and hard hitting rebuttal of bad science, but a rebuttal that cannot be buried by any minor inaccuracies or difficult-to-prove claims.

Best regards

Tim

cg2003  
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1280. 2003-10-31

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date: Fri Oct 31 16:48:59 2003  
from: Keith Briffa <k.briffa@uea.ac.uk>  
subject: Fwd: Attack on Mann et al (IPCC) work  
to: mann@virginia.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu  
Mike et al

Here is what we sent to Heike - I suspect she will forward it to the New York News desk. I am happy to approach others later and you should definitely send to Dick Kerr. We have drafted a posting for CLIMLIST and the skeptics site but Tim is sending these in from home. I have to turn to doing the final PhD proposal here now because the deadline is tonight. At least your response in the in the public domain and I think it is pitched right and was a good decision. Lets see how things go from here and pick up the issue of further supporting statements, work etc when you see how the chips fall. Mike, you need to have a few drinks and step away from this now for a while - take counsel from Ray and Malcolm and take a deserved rest. Have a good weekend all of you - signing off for a few days.

Keith

Date: Fri, 31 Oct 2003 16:33:21 +0000  
To: h.langenberg@nature.com  
From: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Attack on Mann et al (IPCC) work  
Cc: t.osborn@uea.ac.uk, p.jones@uea.ac.uk

Dear Heike

following on from our 'phone conversation this morning, I am attaching a response by Mann and his colleagues, to what seems at this time to be a seriously flawed so-called

"audit" of their well known paper originally published in Nature in 1998. The "audit" was published (with free-access) in Energy and Environment (Vol.14, No6.) see

- [http://www.multi-science.co.uk/ee\\_openaccess.htm](http://www.multi-science.co.uk/ee_openaccess.htm)  
News of this audit, by McInyre and Mckitrick (MM) is spreading rapidly and has

already been reported in USA Today see-

[http://www.usatoday.com/news/opinion/editorials/2003-10-28-schulz\\_x.htm](http://www.usatoday.com/news/opinion/editorials/2003-10-28-schulz_x.htm)  
and is likely to be picked up by the wider press in the near future.

The amazing and depressing aspect of this is that Mann and his colleagues were

never given the opportunity to see or comment on the MM paper before it was published . Nor were they given the chance to comment before the newspaper article . It seems , from a necessarily cursory and indirect examination of MM work by Mann, that MM have made serious errors in their analysis that likely completely negate their results (they reconstruct anomalous warmth in the 15th century AD, in direct contradiction of the Mann et al work) . The reason I feel you may be interested in doing a news item on this issue is that the MM work has also , already been cited in the US Senate , in a blatant attempt to influence the political debate ,when clearly the work has not been subject to any independent scientific scrutiny .

Myself , Tim Osborn and Phil Jones (all at this Unit) are submitting a comment on this to CLIMLIST (and will post a copy of Mann et al reply), where the MM work has also been widely circulated .

If you or your colleagues think this is a suitable subject for a news item , I suggest you contact Mike Mann directly

Michael Mann  
Office: (434) 924-7770  
Cell: (434) 825-3969  
mann@virginia.edu

Our interest in this affair is from the standpoint of preserving the integrity of the scientific process - we are independent observers of the Mann work on Northern Hemisphere temperatures , see eg Our Science perspectives piece on Esper et al.

[1]<http://www.sciencemag.org/cgi/content/summary/295/5563/2227?ijkey=6U4G9GWPALryA&keyty>

pe=ref&siteid=sci

However , in this case we worry that a bad precedent is being set , when a paper (seemingly badly refereed ? ) so much at odds with other work , is so widely and quickly spun, when the authors , or independent researchers have no opportunity to examine, or answer the controversial conclusions.

The issue may also come at you from another angle; Fred Singer , who I believe is actively organising a greenhouse skeptic lobby in Washington , has declared his intention to demand , from Nature, an official retraction of the original Mann et al 1998 paper, justified by the publication of the MM work. Such a request is patent nonsense.

yours sincerely  
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2172. 2003-10-31

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cc: t.osborn@uea

date: Fri, 31 Oct 2003 16:44:49 +0000

from: Tim Osborn <t.osborn@uea.ac.uk>

subject: M&M final preliminary response

to: k.briffa@uea.ac.uk

<x-flowed>

Mike,

I'm glad we have a final version at last. It's getting late now but we will do our best to get this out today.

(1) Mike will you circulate your response around "friends and allies" please.

(2) I've posted it on to a (currently unlinked) webpage. Only had time for quick explanation of why its there. Could have done better with more time, but... oh well. See:

<http://www.cru.uea.ac.uk/~timo/paleo/>

(3) Keith is emailing Heike at Nature with it.

(4) We will jointly (also with Phil) circulate it to CLIMLIST SKEPTICS ETC. here is our proposed note, but little time for editing I'm afraid.

"In response to the postings related to the McIntyre and Mckitrick (Energy and Environment, 14, 751-771, 2003) study of the Northern Hemisphere temperature reconstruction previously published by Mann, Bradley and Hughes (Nature, 392, 779-787, 1998; hereafter MBH98)...

We suggest that those interested in the claim made by McIntyre and Mckitrick (MM) should also read the initial response from Mann and his colleagues.

It is plain that there are serious questions regarding the manner in which MM have attempted to implement the Mann et al. method, and specific problems with the selection of predictors. Amazingly, the journal "Energy and Environment" that published the MM work, made no attempt to provide Mann et al. with the opportunity to review the MM paper or establish the details of the MM work."

MORE HERE KEITH?

Tim

At 16:00 31/10/2003, Michael E. Mann wrote:

>Great Keith, Great!

>

>I've attached, then, the final version with the two additional "censors" I found changed. So lets make sure to use the attached pdf final version.

>

>Can we confirm the procedure now. As I understand it, you guys have an email already to go to all the various list serves to which we'll attach the statement.

>

>As an alternative, can you also post our pdf file on the CRU website, so recipients have two ways to get to our statement, if they can't read the attachment, etc.?

>

>Oh, and I don't want to try to put any constraints on what you three want to write, but can Ray and I see that too before you send out, just to make sure everything seems fine?

>

>Thanks so much,

cg2003

>  
>mike  
>  
>At 03:38 PM 10/31/2003 +0000, Keith Briffa wrote:  
>>Mike Ray Malcolm  
>>I am happy with this now and feel it could go.  
>>  
>>Keith  
>>  
>>At 10:46 AM 10/31/03 -0500, Michael E. Mann wrote:  
>>>oops--there is one "censored" that still has to be changed.  
>>>  
>>>Let me know if there are any additional comments and I'll incorporate  
>>>into one last final version once I hear back.  
>>>  
>>>Thanks,  
>>>  
>>>mike  
>>>  
>>>>Date: Fri, 31 Oct 2003 10:31:48 -0500  
>>>>To: "raymond s. bradley" <rbradley@geo.umass.edu>, Keith Briffa  
>>>><k.briffa@uea.ac.uk>, phil Jones <p.jones@uea.ac.uk>, tim Osborn  
>>>><t.osborn@uea.ac.uk>  
>>>>From: "Michael E. Mann" <mann@virginia.edu>  
>>>>Subject: Re: Malcolm just called READ THIS NOW  
>>>>  
>>>>thanks a bunch ray,  
>>>>  
>>>>Keith, Tim, (Phil?), what do you guys think now?  
>>>>  
>>>>mike  
>>>>  
>>>>At 10:24 AM 10/31/2003 -0500, raymond s. bradley wrote:  
>>>>>looks good to me...i think these final changes alleviate Malcolm's  
>>>>>concerns and adding his name will be ok with him  
>>>>>ray  
>>>>>  
>>>>>At 10:15 AM 10/31/2003 -0500, you wrote:  
>>>>>>All of Malcolm's suggestions are right on target and in keeping w/  
>>>>>>advice from people w/ a legal background I've talked to.  
>>>>>>  
>>>>>>So all of those changes have been made.  
>>>>>>  
>>>>>>Can you guys give this one final read through and just say if you  
>>>>>>think this is now adequate for release. Sorry for the multiple  
>>>>>>emails--its the nature of the beast...  
>>>>>>  
>>>>>>mike  
>>>>>>  
>>>>>>p.s. I'm trying to talk Andy Revkin at the New York Times into doing  
>>>>>>a story on this...  
>>>>>>  
>>>>>>At 09:50 AM 10/31/2003 -0500, raymond s. bradley wrote:  
>>>>>>mike:  
>>>>>>>I just spoke to MKH (he'll call you as soon as he can). I read him  
>>>>>>>the latest version...  
>>>>>>>He requests:  
>>>>>>>1) changing word "censor" to "remove" throughout text;  
>>>>>>>2) use phrase "appear to have" rather than definitive statements  
>>>>>>>like "have" when referring to MM actions.  
>>>>>>>3) Eliminate words "reputable scientific journal" (see my  
>>>>>>>suggestion below that I was writing as he called...)  
>>>>>>>He doesn't want to be included unless these changes are made. If

cg2003

>>>>>>they are, he's happy to sign on. He just wants to make it  
>>>>>>watertight so they can't come back at us...  
>>>>>>  
>>>>>>I agree with these suggested changes.  
>>>>>>  
>>>>>>In the original version that I edited, the text read:  
>>>>>>  
>>>>>>Journals that receive critical comments on a previously published  
>>>>>>papers always provide the authors who are being criticized an  
>>>>>>opportunity to review the study prior to publication, and offer them  
>>>>>>the chance to respond. This is standard operating procedure in all  
>>>>>>peer-reviewed scientific journals. Mann and colleagues [we] were  
>>>>>>never given this opportunity.  
>>>>>>  
>>>>>>I think this is all that is needed on this matter....  
>>>>>>Ray  
>>>>>>  
>>>>>>\_\_\_\_\_  
>>>>>>Professor Michael E. Mann  
>>>>>>Department of Environmental Sciences, Clark Hall  
>>>>>>University of Virginia  
>>>>>>Charlottesville, VA 22903  
>>>>>>\_\_\_\_\_  
>>>>>>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
>>>>>><http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>>>>>>  
>>>>>>Raymond S. Bradley  
>>>>>>Distinguished Professor  
>>>>>>Director, Climate System Research Center\*  
>>>>>>Department of Geosciences  
>>>>>>Morrill Science Center  
>>>>>>611 North Pleasant Street  
>>>>>>AMHERST, MA 01003-9297  
>>>>>>  
>>>>>>Tel: 413-545-2120  
>>>>>>Fax: 413-545-1200  
>>>>>>\*Climate System Research Center: 413-545-0659  
>>>>>><<http://www.paleoclimate.org>>  
>>>>>>Paleoclimatology Book Web Site:  
>>>>>><http://www.geo.umass.edu/climate/paleo/html>  
>>>>>>\_\_\_\_\_  
>>>>>>Professor Michael E. Mann  
>>>>>>Department of Environmental Sciences, Clark Hall  
>>>>>>University of Virginia  
>>>>>>Charlottesville, VA 22903  
>>>>>>\_\_\_\_\_  
>>>>>>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
>>>>>><http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>>>>>>  
>>>>>>\_\_\_\_\_  
>>>>>>Professor Michael E. Mann  
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>>>>>>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
>>>>>><http://www.evsc.virginia.edu/faculty/people/mann.shtml>  
>>>>>>  
>>>>>>--  
>>>>>>Professor Keith Briffa,  
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>>>>>>University of East Anglia  
>>>>>>Norwich, NR4 7TJ, U.K.  
>>>>>>

cg2003

>>Phone: +44-1603-593909

>>Fax: +44-1603-507784

>>

>><http://www.cru.uea.ac.uk/cru/people/briffa/>

>

> \_\_\_\_\_  
> Professor Michael E. Mann  
> Department of Environmental Sciences, Clark Hall  
> University of Virginia  
> Charlottesville, VA 22903  
> \_\_\_\_\_

>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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fax: +44 1603 507784

web: <http://www.cru.uea.ac.uk/~timo/>

sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

2519. 2003-10-31

cc: mhughes <mhughes@lrr.arizona.edu>

date: Fri, 31 Oct 2003 05:37:03 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: RE: CLIMLIST

to: f055 <T.Osborn@uea.ac.uk>, "p.jones" <p.jones@uea.ac.uk>, "raymond s. bradley"

<rbradley@geo.umass.edu>, f055 <T.Osborn@uea.ac.uk>, Keith Briffa

<k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

Thanks very much Tim,

I was hoping that the revisions would allay concerns people had.

I'll look forward to your comments on this latest draft. I agree w/ Malcolm on the need to

be careful w/ the wording in the first paragraph. The first paragraph is a bit of relic of

a much earlier draft, and maybe we need to rethink it a bit. Taking the high road is

probably very important here. If \*others\* want to say that their actions represent

scientific fraud, intellectual dishonesty, etc. (as I think we all suspect they do), lets

let \*them\* make these charges for us!

Lets let our supporters in higher places use our scientific response to push the broader

case against MM. So I look forward to peoples attempts to revise the first par. particular.

I took the liberty of forwarding the previous draft to a handfull of our closet colleagues,

just so they would have a sense of approximately what we'll be releasing later today--i.e.,

a heads up as to

how MM achieved their result...

look forward to us finalizing something a bit later--I still think we need to get this out

ASAP...

mike

SAT 03:01 AM 10/31/2003 +0000, f055 wrote:

Dear all,

I've just finished preparing a detailed response offline, only to log on to send it to you all and find new versions from Mike plus more comments

cg2003

and information. Well, I don't have time to change my message now, so will paste it below this message. But bear in mind that the new draft may well have allayed many of my concerns - in particular, a quick glance shows the figure to be much more convincing than the one Mike circulated earlier, indeed it seems to be utterly convincing! I'll reply again on Friday

morning once I've had time to read the new draft. In the meantime, here is my message as promised.

\*\*\*\*\*

Dear MBH (cc to CRU),

The number of emails has been rather overwhelming on this issue and I'm struggling to catch up with them! But I will attempt to catch up with a few things here...

(1) The single worst thing about the whole M&M saga is not that they did their study, not that they did things wrong (deliberately or by accident), but that neither they nor the journal took the necessary step of investigating whether the difference between their results and yours could be explained simply by some error or set of errors in their use of the data or in their implementation of your method. If it turns out, as looks likely from Mike's investigation of this, that their results are erroneous, then they and the journal will have wasted countless person-hours of time and caused much damage in the climate policy arena.

(2) Given that this is the single worst thing about the saga, we must not go and do exactly the same in rushing out a response to their paper. If some claims in the response turned out to be wrong, based on assumptions about what M&M did or assumptions about how M&M's assumptions affect the results, then it would end up with a number of iterations of claim and counter claim. Ultimately the issue might be settled, but by then the waters could be so muddied that it didn't matter.

(3) Not only do I advise against an overly rushed response, but I'm also wondering whether it really ought to be only from MBH, for three reasons.

(i) It is your paper/results that are being attacked.

(ii) It is difficult to endorse everything that Mike has put in the draft response because I don't know 100% of the details of MBH and the MBH data. Sure, I can endorse some things, but others I wouldn't know. Sure, I accept Mike's explanation because he's looked at this stuff for 4 days and I believe he'll have got it right - but that's different to an independent check. That must come from Ray or Malcolm if possible.

(iii) If it does come to any independent assessment of who's right and who's wrong, then it would be difficult for us to be involved if we had already signed up to what some might claim to be a knee-jerk reaction to the M&M paper. If that happened, then you would want us to be free to get involved to make sure the process was fair and informed.

This sounds like a cop out, but - like I say - I'm not sure about point (3) so feel free to try to convince me otherwise if you wish. Anyway Keith or Phil may be happy to sign up to a (quick or slow) response, despite my reservations above.

I really advise a very careful reading of M&M and their supplementary website to ensure that everything in the response is clearly correct - precisely to avoid point (2). I've only just started to do this, but already have some questions about the response that Mike has drafted.

(a) Mike, you say that many of the trees were eliminated in the data they used. Have you concluded this because they entered "NA" for "Not available" in their appendix table? If so, then are you sure that "NA" means they did not use any data, rather than simply that they didn't replace your data with an alternative (and hence in fact continued to use what Scott had supplied to them)? Or perhaps "NA" means they couldn't find the PC time series published (of course!), but in fact could find the raw tree-ring chronologies and did their own PCA of those? How would they know which raw chronologies to use? Or did you come to your conclusion by downloading their "corrected and updated" data matrix and comparing it with yours - I've not had time to do that, but even if I had and I

found some differences, I wouldn't know which was right seeing as I've not done any PCA of western US trees myself? My guess would be that they downloaded raw tree-ring chronologies (possibly the same ones you used) but then applied PCA only to the period when they all had full data - hence the lack of PCs in the early period (which you got round by doing PCA on the subset that had earlier data). But this is only a guess, and this is the type of thing that should be checked with them - surely they would respond if asked? - to avoid my point (2) above. And if my guess were right, then your wording of "eliminated this entire data set" would come in for criticism, even though in practise it might as well have been.

(b) The mention of ftp sites and excel files is contradicted by their email record on their website, which shows no mention of excel files (they say an ASCII file was sent) and also no record that they knew the ftp address. This doesn't matter really, since the reason for them using a corrupted data file is not relevant - the relevant thing is that it was corrupt and had you been involved in reviewing the paper then it could have been found prior to publication. But they will use the email record if the ftp sites and excel files are mentioned.

(c) Not sure if you talk about peer-review in the latest version, but note that they acknowledge input from reviewers and Fred Singer's email says he refereed it - so any statement implying it wasn't reviewed will be met with an easy response from them.

(d) Your quick-look reconstruction excluding many of the tree-ring data, and the verification RE you obtain, is interesting - but again, don't rush into using these in any response. The time series of PC1 you sent is certainly different from your standard one - but on the other hand I'd hardly say you "get a similar result" to them, the time series look very different (see their fig 6d). So the dismal RE applies only to your calculation, not to their reconstruction. It may turn out that their verification RE is also very negative, but again we cannot assume this in case we're wrong and they easily counter the criticism.

(e) Claims of their motives for selective censoring or changing of data, or for the study as a whole, may well be true but are hard to prove. They would claim that their's is an honest attempt at reproducing a key scientific result. If they made errors in what they did, then maybe they're just completely out of their depth on this, rather than making deliberate errors for the purposes of achieving preferred results.

(f) The recent tree-ring decline they refer to seems related to tree-ring-width not density. Regardless of width of density, this issue cannot simply be dismissed as a solved problem. Since they don't make much of an issue out of it, best just to ignore it.

(g) [I'm rambling now into an un-ordered list of things, so I'll stop soon!] The various other problems relating to temperature data sets, detrended standard deviations, PCs of tree-ring subsets etc. sound likely errors - though I've got no way of providing the independent check that you asked for. But it is again a bit of a leap of faith to say that these \*explain\* the different results that they get. Certainly they throw doubt on the validity of their results, but without actually doing the same as them it's not possible to say if they would have replicated your results if they hadn't made these errors. After all, could the infilling of missing values have made much difference to the results obtained, something that they made a good deal of fuss about?

(h) To say they "used neither the data nor the procedures of MBH98" will also be an easy target for them, since they did use the data that was sent to them and seemed to have used approximately the method too (with some errors that you've identified). This reproduced your results to some extent (certainly not perfectly, but see Fig 6b and 6c). Then they went further to redo it with the "corrected and updated" data - but only after first doing approximately what they claimed they did (i.e. the audit).

cg2003

These comments relate to random versions of the draft response, so apologies if they don't all seem relevant to the current draft. I don't have these in front of me, here at home, so I'm doing this from memory of what I've read over the past few days. But nevertheless, the point is that a quick response would ultimately require making a number of assumptions about what they did and assumptions about whether this explains the differences or not - assumptions that might be later shot down (in part only, at most, but still sufficient to muddy the debate for most outsiders). A quick response ought to be limited to something like:

-----  
The recent paper by McIntyre and McKittrick (2003; hereafter MM03) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998; hereafter MBH98). MM03 are unable to reproduce the Northern Hemisphere temperature reconstruction of MBH98 when attempting to use the same proxy data and methods as MBH98, though they obtain something similar with clearly anomalous recent warming (their Figure 6c). They then make many modifications to the proxy data set and repeat their analysis, and obtain a rather different result to MBH98. Unfortunately neither M&M nor the journal in which it was published took the necessary step of investigating whether the difference between their results and MBH98 could be explained simply by some error or set of errors in their use of the data or in their implementation of the MBH98 method. This should have been an essential step to take in a case such as this where the difference in results is so large and important. Simple errors must first be ruled out prior to publication. Even if the authors had not undertaken this by presenting their results to the authors of MBH98, the journal should certainly have included them as referees of the manuscript.

A preliminary investigation into the proxy data and implementation of the method has already identified a number of likely errors, which may turn out to be the cause of the different results. Rather than repeating M&M's failure to follow good scientific practise, we are withholding further comments until we can - by collaboration with M&M if possible - be certain of exactly what changes to data and method were made by M&M, whether these changes can really explain the differences in the results, and eventually which (if any) of these changes can be justified as equally valid (given the various uncertainties that exist) and which are simply errors that invalidate their results.

-----  
Hope you find this all helpful, and despite my seemingly critical approach, take them in the spirit with which they are aimed - which is to obtain a strong and hard hitting rebuttal of bad science, but a rebuttal that cannot be buried by any minor inaccuracies or difficult-to-prove claims.  
Best regards  
Tim

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2800. 2003-10-31

---

cc: mhughes <mhughes@ltrr.arizona.edu>  
date: Fri, 31 Oct 2003 11:20:03 +0000  
from: Tim Osborn <t.osborn@uea.ac.uk>  
subject: RE: CLIMLIST  
to: "Michael E. Mann" <mann@virginia.edu>, "p.jones" <p.jones@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>  
<x-flowed>

cg2003

Dear all,  
you're up early, Mike. I was hoping to have sent out my thoughts on the latest draft before you got back to your email, but you beat me to it! I think that this is much improved, and (as I said last thing last night) I find the figure extremely convincing, especially the timing and occurrence of the two big peaks in the first 120 years - they match very closely with the MM03 peaks. This has now removed many of the doubts that I still had over whether the real reason for their different results had been identified - it certainly looks like the lack of early tree-ring PCs in their data.

I'm still thinking that this should be an MBH response for reasons I gave in my last email. Once you have made such a response, then we (and others) can certainly join in and strongly support your stance on this in any ensuing wrangling that takes place.

Finally, even though the latest version is much improved, I still urge you to consider the points I made in my email. Some are already dealt with (e.g. the saga of the ftp and excel data is not in your latest draft), but some are still relevant. For example, if my understanding is correct, then they did include the WNA and Tex-Mex trees, but when they did the PCA they only used the period for which they had full data - read carefully the bit about PCA in the presence of missing data to see if I'm interpreting this correctly. This may have the same effect as eliminating the series early on, but is not at all to do with the data not being in the public domain - hence all that stuff can be removed and simply replaced by some sentences explaining that they did not use these early values because they didn't do PCA on the subset that exists earlier - which is a valid thing to do given that the whole calibration is done separately for each period anyway. See also my

I agree with the latest suggestions about more minor wording changes to avoid alienating readers in various places.

Best regards

Tim

P.S. With regard to where to send this, I agree with the various suggestions about mailing lists, and trying to get a news item in Nature, plus all the other media outlets that are interested. But are you planning a formal rebuttal submitted to the journal? Or to EOS? To have a peer-reviewed response that can be cited in the scientific literature seems important.

Dr Timothy J Osborn

Climatic Research Unit

School of Environmental Sciences, University of East Anglia

Norwich NR4 7TJ, UK

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sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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3358. 2003-10-31

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cc: mhughes <[mhughes@ltrr.arizona.edu](mailto:mhughes@ltrr.arizona.edu)>

date: Fri, 31 Oct 2003 06:17:24 -0500

from: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>

subject: RE: CLIMLIST

to: Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, f055 <[T.Osborn@uea.ac.uk](mailto:T.Osborn@uea.ac.uk)>, "p.jones" <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, "raymond s. bradley" <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>, f055 <[T.Osborn@uea.ac.uk](mailto:T.Osborn@uea.ac.uk)>, Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>

Keith,

Thanks--that sounds absolutely great.

I suggest the following

1) I'll fix any remaining typos I find, and incorporate the latest comments received from



cg2003

you guys. I expect that I can finalize this in 15 minutes or so--I agree that this needs to

get out by 8:00 AM eastern standard time, U.S.

I like where Keith is heading in terms of discussion of the strategy.

Why don't we sign this document, "Mann, Bradley, Hughes" that will be ready for distribute

to our closest colleagues and allies. I'll prepare a PDF version for distribution, to make

it difficult for others to alter (you never know w/ these folks)...

Should I go ahead and forward this document to Heike, then, in an email? Also, should I

send this to Dick Kerr at Science separately--Dick has often been helpful. And maybe Jesse

Smith at Science, and a few key journalists (Andy Revkin at New York Times)?

Perhaps, then, Keith, Tim, Phil--you guys, as Keith suggests, can draft a separate email to

go out to the skepticlist (and all of the scientists who were forward it), the CLIMLIST,

etc. stating your opinons on this, and perhaps \*attaching\* at supporting evidence the

document signed by Mann, Bradley, Hughes?

Also, do we ask organizations like Environmental Defense Fund, etc. to post \*both\*

documents (our document, your supporting email) on their websites, etc?

What do you guys think?

Thank-you guys have been wonderful, and I am most personally gracious. This will not soon

be forgotten...

mike

At 10:47 AM 10/31/2003 +0000, Keith Briffa wrote:

Hi all - I too have had some problems as to which specific version is where we are at -

BUT I think the latest draft as sent by Mike really is virtually there (perhaps some

typos to be ironed out (e.g. 'were' instead of 'was' on line 7 of point 2) but I am

generally very happy with the tone and balance . Much of Tim's fears (justifiable points

on not providing them with wiggle out and distraction options) are allayed by the calm

provisos about not being categorical etc. The question now arises as to how to put this

out - I believe it does need to go out early so as to be available when the rest of the

press start to pick up the MM propaganda . Whether it should be just signed by MBH is up

to you . I AM NOT averse to signing , but wonder whether it is a better tactic to put

out a separate statement (us , Tom W. and whoever as suggested by Ray , saying we abhor

the way this MM paper has been published and publicised without proper scrutiny). I

fully agree with the statement as now written however , and willing to go with the

majority view. My suggestion about redoing the "audit" was made in good faith and in no

way implied I concurred with MM ( in case anyone got the impression that I was not

wholly "on side" here).

So what does everyone else say now?

REGARDING NATURE - spoke to Heike Langenberg , in the London office and she said it

sounded like a potential NEWS item , and asked me to send some details by

email and she

would forward to the appropriate office - seemed positive. I will do. The statement (s)

should anyway go soon on CLIMLIST and then we could quietly contact a few people we know

in the media ?

Keith

At 05:38 AM 10/31/03 -0500, Michael E. Mann wrote:

what about p.s. Keith, any word from Nature. Should I contact them independently? And Science? Or "Climatic Change" (I have little doubt that Steve S could find

justification to publish this their in an instant)... thoughts?

thoughts?

mike

At 03:01 AM 10/31/2003 +0000, f055 wrote:  
Dear all,

I've just finished preparing a detailed response offline, only to log on to send it to you all and find new versions from Mike plus more comments and information. Well, I don't have time to change my message now, so will paste it below this message. But bear in mind that the new draft may well have allayed many of my concerns - in particular, a quick glance shows the figure to be much more convincing than the one Mike circulated earlier, indeed it seems to be utterly convincing! I'll reply again on Friday morning once I've had time to read the new draft. In the meantime, here is my message as promised.

\*\*\*\*\*

Dear MBH (cc to CRU),

The number of emails has been rather overwhelming on this issue and I'm struggling to catch up with them! But I will attempt to catch up with a few things here...

(1) The single worst thing about the whole M&M saga is not that they did their study, not that they did things wrong (deliberately or by accident), but that neither they nor the journal took the necessary step of investigating whether the difference between their results and yours could be explained simply by some error or set of errors in their use of the data or in their implementation of your method. If it turns out, as looks likely from Mike's investigation of this, that their results are erroneous, then they and the journal will have wasted countless person-hours of time and caused much damage in the climate policy arena.

(2) Given that this is the single worst thing about the saga, we must not go and do exactly the same in rushing out a response to their paper. If some claims in the response turned out to be wrong, based on assumptions about what M&M did or assumptions about how M&M's assumptions affect the results, then it would end up with a number of iterations of claim and counter claim. Ultimately the issue might be settled, but by then the waters could be so muddied that it didn't matter.

(3) Not only do I advise against an overly rushed response, but I'm also wondering whether it really ought to be only from MBH, for three reasons.

(i) It is your paper/results that are being attacked.

(ii) It is difficult to endorse everything that Mike has put in the draft response because I don't know 100% of the details of MBH and the MBH data. Sure, I can endorse some things, but others I wouldn't know. Sure, I accept Mike's explanation because he's looked at this stuff for 4 days and I believe he'll have got it right - but that's different to an independent check. That must come from Ray or Malcolm if possible.

(iii) If it does come to any independent assessment of who's right and who's wrong, then it would be difficult for us to be involved if we had already signed up to what some might claim to be a knee-jerk reaction to the M&M paper. If that happened, then you would want us to be free to get involved to make sure the process was fair and informed.

This sounds like a cop out, but - like I say - I'm not sure about point (3) so

feel free to try to convince me otherwise if you wish. Anyway Keith or Phil may be happy to sign up to a (quick or slow) response, despite my reservations above.

I really advise a very careful reading of M&M and their supplementary website to ensure that everything in the response is clearly correct - precisely to avoid point (2). I've only just started to do this, but already have some questions about the response that Mike has drafted.

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I found some differences, I wouldn't know which was right seeing as I've not done any PCA of western US trees myself? My guess would be that they downloaded raw tree-ring chronologies (possibly the same ones you used) but then applied PCA only to the period when they all had full data - hence the lack of PCs in the early period (which you got round by doing PCA on the subset that had earlier data). But this is only a guess, and this is the type of thing that should be checked with them - surely they would respond if asked? - to avoid my point (2) above. And if my guess were right, then your wording of "eliminated this entire data set" would come in for criticism, even though in practise it might as well have been.

(b) The mention of ftp sites and excel files is contradicted by their email record on their website, which shows no mention of excel files (they say an ASCII file was sent) and also no record that they knew the ftp address. This doesn't matter really, since the reason for them using a corrupted data file is not relevant - the relevant thing is that it was corrupt and had you been involved in reviewing the paper then it could have been found prior to publication. But they will use the email record if the ftp sites and excel files are mentioned.

(c) Not sure if you talk about peer-review in the latest version, but note that they acknowledge input from reviewers and Fred Singer's email says he refereed it - so any statement implying it wasn't reviewed will be met with an easy response from them.

(d) Your quick-look reconstruction excluding many of the tree-ring data, and the verification RE you obtain, is interesting - but again, don't rush into

using these in any response. The time series of PC1 you sent is certainly different from your standard one - but on the other hand I'd hardly say you "get a similar result" to them, the time series look very different (see their fig 6d). So the dismal RE applies only to your calculation, not to their reconstruction. It may turn out that their verification RE is also very negative, but again we cannot assume this in case we're wrong and they easily counter the criticism.

(e) Claims of their motives for selective censoring or changing of data, or for the study as a whole, may well be true but are hard to prove. They would claim that their's is an honest attempt at reproducing a key scientific result. If they made errors in what they did, then maybe they're just completely out of their depth on this, rather than making deliberate errors for the purposes of achieving preferred results.

(f) The recent tree-ring decline they refer to seems related to tree-ring-width not density. Regardless of width of density, this issue cannot simply be dismissed as a solved problem. Since they don't make much of an issue out of it, best just to ignore it.

(g) [I'm rambling now into an un-ordered list of things, so I'll stop soon!]

cg2003

The various other problems relating to temperature data sets, detrended standard deviations, PCs of tree-ring subsets etc. sound likely errors - though I've got no way of providing the independent check that you asked for. But it is again a bit of a leap of faith to say that these \*explain\* the different results that they get. Certainly they throw doubt on the validity of

their results, but without actually doing the same as them it's not possible to say if they would have replicated your results if they hadn't made these errors. After all, could the infilling of missing values have made much difference to the results obtained, something that they made a good deal of fuss about?

(h) To say they "used neither the data nor the procedures of MBH98" will also be an easy target for them, since they did use the data that was sent to them and seemed to have used approximately the method too (with some errors that you've identified). This reproduced your results to some extent (certainly not perfectly, but see Fig 6b and 6c). Then they went further to redo it with the "corrected and updated" data - but only after first

doing approximately what they claimed they did (i.e. the audit).

These comments relate to random versions of the draft response, so apologies if they don't all seem relevant to the current draft. I don't have these in front of me, here at home, so I'm doing this from memory of what I've read over the past few days. But nevertheless, the point is that a quick response would ultimately require making a number of assumptions about what they did and assumptions about whether this explains the differences or not - assumptions that might be later shot down (in part only, at most, but still sufficient to muddy the debate for most outsiders). A quick response ought to be limited to something like:

-----  
The recent paper by McIntyre and McKittrick (2003; hereafter MM03) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998; hereafter MBH98). MM03 are unable to reproduce the Northern Hemisphere temperature reconstruction of MBH98 when attempting to use the same proxy data and methods as MBH98, though they obtain something similar with clearly anomalous recent warming (their Figure 6c). They then make many modifications to the proxy data set and repeat their analysis, and obtain a rather different result to MBH98. Unfortunately neither M&M nor the journal in which it was published took the necessary step of investigating whether the difference between their results and MBH98 could be explained simply by some error or set of errors in their use of the data or in their implementation of the MBH98 method. This should have been an essential step to take in a case such as this where the difference in results is so large and important. Simple errors must first be ruled out prior to publication. Even if the authors had not undertaken this by presenting their results to the authors of MBH98, the journal should certainly have included them as referees of the manuscript.

A preliminary investigation into the proxy data and implementation of the method has already identified a number of likely errors, which may turn out to be the cause of the different results. Rather than repeating M&M's failure to follow good scientific practise, we are withholding further comments until we can - by collaboration with M&M if possible - be certain of exactly what changes to data and method were made by M&M, whether these changes can really explain the differences in the results, and eventually which (if any) of these changes can be justified as equally valid (given the various uncertainties that exist) and which are simply errors that invalidate their results.

-----  
Hope you find this all helpful, and despite my seemingly critical approach, take them in the spirit with which they are aimed - which is to obtain a strong and hard hitting rebuttal of bad science, but a rebuttal that cannot be buried by any minor inaccuracies or difficult-to-prove claims.  
Best regards

Tim

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Professor Keith Briffa,  
Climatic Research Unit  
University of East Anglia  
Norwich, NR4 7TJ, U.K.  
Phone: +44-1603-593909  
Fax: +44-1603-507784  
[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3702. 2003-10-31

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cc: mhughes <mhughes@ltrr.arizona.edu>  
date: Fri, 31 Oct 2003 03:01:59 +0000  
from: f055 <T.Osborn@uea.ac.uk>  
subject: RE: CLIMLIST  
to: "Michael E. Mann" <mann@virginia.edu>, "p.jones" <p.jones@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, f055 <T.Osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

Dear all,  
I've just finished preparing a detailed response offline, only to log on to send it to you all and find new versions from Mike plus more comments and information. Well, I don't have time to change my message now, so will paste it below this message. But bear in mind that the new draft may well have allayed many of my concerns - in particular, a quick glance shows the figure to be much more convincing than the one Mike circulated earlier, indeed it seems to be utterly convincing! I'll reply again on Friday morning once I've had time to read the new draft. In the meantime, here is my message as promised.

\*\*\*\*\*

Dear MBH (cc to CRU),  
The number of emails has been rather overwhelming on this issue and I'm struggling to catch up with them! But I will attempt to catch up with a few things here...

(1) The single worst thing about the whole M&M saga is not that they did their study, not that they did things wrong (deliberately or by accident), but that neither they nor the journal took the necessary step of investigating whether the difference between their results and yours could be explained simply by some error or set of errors in their use of the data or in their implementation of your method. If it turns out, as looks likely from Mike's investigation of this, that their results are erroneous, then they and the journal will have wasted countless person-hours of time and caused much damage in the climate policy arena.

(2) Given that this is the single worst thing about the saga, we must not go and do exactly the same in rushing out a response to their paper. If some claims in the response turned out to be wrong, based on assumptions

about what M&M did or assumptions about how M&M's assumptions affect the results, then it would end up with a number of iterations of claim and counter claim. Ultimately the issue might be settled, but by then the waters could be so muddled that it didn't matter.

(3) Not only do I advise against an overly rushed response, but I'm also wondering whether it really ought to be only from MBH, for three reasons.

(i) It is your paper/results that are being attacked.

(ii) It is difficult to endorse everything that Mike has put in the draft response because I don't know 100% of the details of MBH and the MBH data. Sure, I can endorse some things, but others I wouldn't know. Sure, I accept Mike's explanation because he's looked at this stuff for 4 days and I believe he'll have got it right - but that's different to an independent check. That must come from Ray or Malcolm if possible.

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I really advise a very careful reading of M&M and their supplementary website to ensure that everything in the response is clearly correct - precisely to avoid point (2). I've only just started to do this, but already have some questions about the response that Mike has drafted.

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using these in any response. The time series of PC1 you sent is certainly different from your standard one - but on the other hand I'd hardly say you "get a similar result" to them, the time series look very different (see their fig 6d). So the dismal RE applies only to your calculation, not to their reconstruction. It may turn out that their verification RE is also very negative, but again we cannot assume this in case we're wrong and they easily counter the criticism.

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Hope you find this all helpful, and despite my seemingly critical approach, take them in the spirit with which they are aimed - which is to obtain a strong and hard hitting rebuttal of bad science, but a rebuttal that cannot be buried by any minor inaccuracies or difficult-to-prove claims.

Best regards  
Tim

4158. 2003-10-31

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date: Fri, 31 Oct 2003 10:23:27 -0500  
from: "Michael E. Mann" <mann@virginia.edu>  
subject: FW: please keep Canadians informed on M&M  
to: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Rbradley@geo.umass.edu

I guess we should add these names too...

Delivered-To: mem6u@virginia.edu

User-Agent: Microsoft-Outlook-Express-Macintosh-Edition/5.02.2022

Date: Fri, 31 Oct 2003 10:17:54 -0500

Subject: FW: please keep Canadians informed on M&M

From: Mike MacCracken <mmaccrac@comcast.net>

To: Michael Mann <mann@virginia.edu>

Mike--My Canadian friends have asked you to keep the following list informed about the

M&M paper. They have to prepare a note on all this for their deputy minister today, so

please do keep them informed. [Similarly, I do hope you send the note to high in US

Admin]

Thanks, Mike

Gordon McBean gmcbear@julian.uwo.ca

Henry Hengeveld henry.hengeveld@ec.gc.ca

Elizabeth Bush elizabeth.bush@ec.gc.ca

Francis Zwiers francis.zwiers@ec.gc.ca

Doug Whelpdale Douglas.whelpdale@ec.gc.ca

John Stone john.stone@ec.gc.ca

Gordon McBean: Ex ADM of the Meteorological Service of Canada, current Professor and

Chair in Policy Institute for Catastrophic Loss Reduction etc.

Henry Hengeveld is Environment Canada's senior science advisor on climate change and our

senior spokesperson. I (Elizabeth Bush) work with Henry. John Stone works as a special

advisor on climate change to our ADM.

Francis Zwiers: head of Canada's modeling group.

Doug Whelpdale is Director of our Climate Research Branch (Francis' director).

Elizabeth Bush is Climate Change Science Advisor in the Science Assessment and

Integration Branch, Meteorological Service of Canada, 4905 Dufferin St. , Downsview, ON,



cg2003

CANADA M3H 5T4, Phone: 416-739-4332, Fax: 416-739-4882, Email:  
Elizabeth.bush@ec.gc.ca

-----Original Message-----

From: Mike MacCracken [[1]mailto:mmaccrac@comcast.net]

Sent: October 30, 2003 8:57 PM

To: Elizabeth Bush; Gordon McBean

Subject: FW: check out who he cc's these to... Fwd: Proposal that Nature  
consider

withdrawing Mann,Bradley, Hughes 1998

Dear Elizabeth and Gordon--This is nominally confidential, but I think you  
should know

about it as things could get pretty ugly pretty fast and I leave tomorrow  
afternoon for

a few days of mtgs in Texas and CA, so want to make sure this gets to you.

Feel free to

contact Mike Mann if you need more info.

Mike

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From: "Michael E. Mann" <mann@virginia.edu>

Date: Thu, 30 Oct 2003 15:56:28 -0500

To: mann@multiproxy.evsc.virginia.edu

Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes"  
<mhughes@ltrr.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn  
<t.osborn@uea.ac.uk>, Mike MacCracken <mmaccrac@comcast.net>, Michael

Oppenheimer

<omichael@Princeton.EDU>, mann@multiproxy.evsc.virginia.edu, Stephen H  
Schneider

<shs@stanford.edu>

Subject: Fwd: check out who he cc's these to... Fwd: Proposal that Nature  
consider

withdrawing Mann,Bradley, Hughes 1998

Keith/Tim/Ray/Malcolm/Phil: Our email response will have to go out ASAP (we're  
preparing

for a mass emailing tomorrow).

To those not yet in the know (please keep it confidential), we can now show  
that M&M

censored most of our early data in their "improved" data set, by replacing  
longer series

we had used (that go back to the 15th century), with "better" shorter versions  
that only

go back to the 17th century. By doing so, they selectively deleted all of our  
proxy

series that indicate significant 15th-16th century cooling. NOT KIDDING!

They justified this by claiming they couldn't find the older data in the  
public domain,

though we can cite two public sources where all these data were available.

Removing the

proxy data that they removed, we reproduce the anomalous warm spike

result--but we can

show that the resulting reconstruction completely fails the standard  
statistical

verification tests, while our original reconstruction of course passed them

fairly well.

Its pretty serious stuff, and we're going to talk to Nature about doing a  
story on this.

And there may be a need for a formal investigation into scientific  
dishonesty--but not

quite the one the authors have in mind...

mike

X-Sender: sepp@mail.his.com

X-Mailer: QUALCOMM Windows Eudora Version 5.2.1

Date: Thu, 30 Oct 2003 08:49:56 -0500

To: smcintyre@cgxenergy.com, rmckitri@uoguelph.ca

cg2003

From: "S. Fred Singer" <singer@sepp.org>

Subject: Proposal that Nature consider withdrawing Mann,Bradley,  
Hughes 1998

Cc: seitz@rockvax.rockefeller.edu, cstarr@epri.com, art@oism.org,  
rlindzen@mit.edu, wsoon@cfa.harvard.edu, sbaliunas@cfa.harvard.edu,  
pabelson@aaaas.org, dek@uclink4.berkeley.edu, fspilhaus@agu.org,  
jmarburg@ostp.eop.gov, James.R.Mahoney@noaa.gov, Vicki.Horton@noaa.gov

Gentlemen

I have now studied yr rejoinder to the rather inadequate reply from Michael  
Mann to yr  
devastating critique (in Energy & Environment) of the underlying data relating  
to the  
"Hockeystick" (the temperature history that has been used by the IPCC and  
others to  
suggest that the 20th century was the warmest in 1000 years). [See

<<http://www.uoguelph.ca/~rmckitri/research/trc.html>>[\[earch/trc.html\]\(http://www.uoguelph.ca/~rmckitri/research/trc.html\) \]](http://www.uoguelph.ca/~rmckitri/res</a></p></div><div data-bbox=)

[I had earlier served as a referee of yr basic paper published in E&E (Oct  
2003), and  
subsequently spent several hours with Steve McIntyre to carefully review its  
main

points. See [www.climate2003.com/index.html](http://www.climate2003.com/index.html) ]

I propose that NATURE be asked to appoint an independent panel of  
statisticians,  
econometricians, (and others NOT connected in any way with climate studies) to  
conduct  
an investigation of the MBH98 paper and its critique by McIntyre and  
McKittrick.

The purpose would be to determine the need to formally withdraw the paper.  
This request to Nature should be signed by a large number of scientists,  
including, if

possible, members of the Royal Society and other academies, editors of  
scientific  
journals, and public figures, such as scientific advisers to presidents and  
prime

ministers.

Pls note that I am not suggesting culpability on the part of Mann or his  
coauthors.

They might not even have been aware of the gross mishandling of the data used  
in their  
publications. Nor can one fault individual scientists connected to the IPCC  
-- since

IPCC accepts publication in a peer-reviewed journal as prima facie endorsement  
of its

correctness. The chief responsibility now lies with the editors of NATURE.

Yr comments on this proposal are most welcome.

Fred Singer

S. Fred Singer, Ph.D.

President, The Science & Environmental Policy Project (SEPP)

1600 S. Eads St., Suite 712-S

Arlington, VA 22202-2907

e-mail: [singer@sepp.org](mailto:singer@sepp.org) Web: [2][www.sepp.org](http://www.sepp.org) <[3]<http://www.sepp.org/>>

Tel: 703-920-2744

E-fax 815-461-7448; notify by e-mail before sending

\*\*\*\*\*

"The improver of natural knowledge absolutely refuses  
to acknowledge authority, as such. For him, scepticism  
is the highest of duties; blind faith the one unpardonable sin."

> Thomas H. Huxley

\*\*\*\*\*

"If the facts change, I'll change my opinion. What do you do, sir? "

cg2003

>J. M. Keynes  
\*\*\*\*\*

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Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4474. 2003-10-31

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date: Fri Oct 31 17:02:03 2003  
from: Keith Briffa <k.briffa@uea.ac.uk>  
subject: Re: M&M final preliminary response  
to: Tim Osborn <t.osborn@uea.ac.uk>

Tim  
added in abit of pointed sentence at the end to emphasise where we stand re MBH  
versus MM -  
though it says nothing directly about who is right/wrong. Do what you like as  
regards edit  
- but do post to the CLIMLIST  
at least if you can.  
Have a good weekend  
Keith  
At 04:44 PM 10/31/03 +0000, you wrote:

Mike,  
I'm glad we have a final version at last. It's getting late now but we will  
do our best  
to get this out today.  
(1) Mike will you circulate your response around "friends and allies" please.  
(2) I've posted it on to a (currently unlinked) webpage. Only had time for  
quick  
explanation of why its there. Could have done better with more time, but...  
oh well.

See:  
[1]<http://www.cru.uea.ac.uk/~timo/paleo/>  
(3) Keith is emailing Heike at Nature with it.  
(4) we will jointly (also with Phil) circulate it to CLIMLIST SKEPTICS ETC.  
here is our  
proposed note, but little time for editing I'm afraid.  
"In response to the postings related to the McIntyre and Mckitrick (Energy and  
Environment, 14, 751-771, 2003) study of the Northern Hemisphere temperature  
reconstruction previously published by Mann, Bradley and Hughes (Nature, 392,  
779-787,  
1998; hereafter MBH98)...  
We suggest that those interested in the claim made by McIntyre and Mckitrick  
(MM) should  
also read the initial response from Mann and his colleagues.  
It is plain that there are serious questions regarding the manner in which MM  
have  
attempted to implement the Mann et al. method, and specific problems with the  
selection  
of predictors. Amazingly, the journal "Energy and Environment" that published

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the MM

work, made no attempt to provide Mann et al. with the opportunity to review

the MM paper

or establish the details of the MM work."

Objective readers, with a desire to get to the "truth" of this issue, would do well

not to jump to premature conclusions and, unlike Energy and Environment, at least allow

these respected, experienced, and invariably careful researchers the courtesy of a

considered response, after they have had time to study the so-called audit in detail.

Tim

At 16:00 31/10/2003, Michael E. Mann wrote:

Great Keith, Great!

I've attached, then, the final version with the two additional "censors" I found

changed. So let's make sure to use the attached pdf final version.

Can we confirm the procedure now. As I understand it, you guys have an email already to

go to all the various list serves to which we'll attach the statement.

As an alternative, can you also post our pdf file on the CRU website, so recipients have

two ways to get to our statement, if they can't read the attachment, etc.?

Oh, and I don't want to try to put any constraints on what you three want to write, but

can Ray and I see that too before you send out, just to make sure everything seems fine?

Thanks so much,

mike

At 03:38 PM 10/31/2003 +0000, Keith Briffa wrote:

Mike Ray Malcolm

I am happy with this now and feel it could go.

Keith

At 10:46 AM 10/31/03 -0500, Michael E. Mann wrote:

oops--there is one "censored" that still has to be changed.

Let me know if there are any additional comments and I'll incorporate into one last

final version once I hear back.

Thanks,

mike

Date: Fri, 31 Oct 2003 10:31:48 -0500

To: "raymond s. bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>,

phil Jones <p.jones@uea.ac.uk>, tim Osborn <t.osborn@uea.ac.uk>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Malcolm just called READ THIS NOW

thanks a bunch ray,

Keith, Tim, (Phil?), what do you guys think now?

mike

At 10:24 AM 10/31/2003 -0500, raymond s. bradley wrote:

looks good to me...i think these final changes alleviate Malcolm's concerns and adding

his name will be ok with him

ray

At 10:15 AM 10/31/2003 -0500, you wrote:

All of Malcolm's suggestions are right on target and in keeping w/ advice from people w/

a legal background I've talked to.

So all of those changes have been made.

Can you guys give this one final read through and just say if you think this is now

adequate for release. Sorry for the multiple emails--its the nature of the

beast...  
mike  
p.s. I'm trying to talk Andy Revkin at the New York Times into doing a story  
on this...  
At 09:50 AM 10/31/2003 -0500, raymond s. bradley wrote:  
mike:  
I just spoke to MKH (he'll call you as soon as he can). I read him the  
latest  
version...  
He requests:  
1) changing word "censor" to "remove" throughout text;  
2) use phrase "appear to have" rather than definitive statements like "have"  
when  
referring to MM actions.  
3) Eliminate words "reputable scientific journal" (see my suggestion below  
that I was  
writing as he called...)  
He doesn't want to be included unless these changes are made. If they are,  
he's happy  
to sign on. He just wants to make it watertight so they can't come back at  
us...  
I agree with these suggested changes.  
In the original version that I edited, the text read:  
Journals that receive critical comments on a previously published papers  
always provide  
the authors who are being criticized an opportunity to review the study prior  
to  
publication, and offer them the chance to respond. This is standard operating  
procedure  
in all peer-reviewed scientific journals. Mann and colleagues [we] were never  
given  
this opportunity.  
I think this is all that is needed on this matter....  
Ray

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall  
University of Virginia  
Charlottesville, VA 22903

---

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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<[3]<http://www.paleoclimate.org>>  
Paleoclimatology Book Web Site: [4]<http://www.geo.umass.edu/climate/paleo/html>

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sunclock: [10]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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[11][http://www.cru.uea.ac.uk/cru/people/briffa\[12\]/](http://www.cru.uea.ac.uk/cru/people/briffa[12]/)

4624. 2003-10-31

---

date: Fri, 31 Oct 2003 08:16:05 -0500  
from: "raymond s. bradley" <rbradley@geo.umass.edu>  
subject: My perspective on the latest draft  
to: "Michael E. Mann" <mann@virginia.edu>, t.osborn@uea.ac.uk, k.briffa@uea.ac.uk,  
p.jones@uea.ac.uk, mhughes@ltrr.arizona.edu  
<x-flowed>

I just reviewed all the back & forth from Tim, Keith etc. and finally found the latest version of what you propose to send out.

1) I think this sentence is unnecessarily inflammatory and needs to be changed: We will refrain from making categorical statements as to the specific motives, but we will state that it seems clear that MM have made critical errors in their analysis that have the effect of grossly distorting the reconstruction of MBH98.

Just state:

It seems clear that MM have made critical errors in their analysis that have the effect of grossly distorting the reconstruction of MBH98.

Since we "refrain from making categorical statements"....why say that?

2) I must say that I very much agree with Tim that we have to be careful not to say ANYTHING--no matter how trivial--that is not absolutely, unimpeachably correct, or it will inevitably lead to a response that will

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only further confuse and alienate even the most willing of observers. Thus, the text we release must not include ANYTHING that could be argued about. It would be better to make only one point that is unarguably correct than to list a bunch of points, if ANY of them could be disagreed with as a matter of opinion. Please go through the text and eliminate anything that meets this criterion. Finally, I really don't understand what the rush is. Why is an 8am EST release so critical? It's Friday--will this really matter if it doesn't go out until Monday-- or even Wednesday as MKH requests. Seriously, M & M have done a lot of damage, but Mike, you are too wrapped up in this to see that a few days at this point won't make a hill of beans difference. The Senate debate is over, Nature and Science etc won't act with such urgency, so better to slow down here. I'd rather have MKH's endorsement of this and I think we should wait until he has time to see it too. Now you are mad as hell at me, I know. So let me say that you have done an amazing job of deciphering what MM did, and I greatly admire your tenacity and insight into all of this. Clearly as "The Man" of Mann et al, it's you who bears the brunt of all criticism, just as you deserve the bulk of the credit for the work in the first place. But Tim's comments are right on target...and a few days of sober reflection won't hurt anything...and might just avoid falling foul of some problem none of us has yet had time to think about. I know I have hardly time over the last few days to do due diligence on this, and obviously Malcolm has not even skimmed the surface of what's gone on....

Ray

At 07:18 AM 10/31/2003 -0500, you wrote:

>Thanks Tim,

>

>Sounds like the best possible plan under the circumstances. Attached is  
>the revised (final?) version, see the note about Malcolm Hughes unable for  
>comment--does this seem ok?

>

>If this looks good to you guys, you don't see any typos, etc. lets  
>consider this the final version. I've attached it in both word and  
>pdf--only the pdf should probably be sent around.

>

>As per your suggestions, I'll await receipt of your CLIMLIST/SCEPTICSLIST  
>email to send this together to other outlets for joint posting. Will you  
>guys will send out to all of the other scientists, etc. who' Timmerata,  
>etc. emailed too?

>

>The appropriate outlets would be: EDF, other NGO groups, Ross Gelbspan's  
>site, David Appell's blog, etc...

>

>So I'll await further word from you,

>

>thanks again,

>

>mike

>

>At 12:05 PM 10/31/2003 +0000, Tim Osborn wrote:

>>Mike et al.,

>>

>>we (Keith and I) are happy with this strategy. Rebuttal will be signed  
>>Mann and Bradley (Hughes to be added later when available to  
>>confirm/modify) and circulated to allies/friends first.

>>

>>As soon as we get a final version from Mike, Keith will forward it with a  
>>message to Heike.

>>

>>We will also draft an email from Keith, me and Phil to send to the email  
>>lists, expressing our views on this and attaching your final version.

>>

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>>As to other people that you mention (Science, EFD), we'll leave that to  
>>you, Mike, to do - though you may well want to use our  
>>CLIMLIST/SCEPTICSLIST email AND your final version together to send to  
>>them, so might want to wait till we've drafted that email.  
>>  
>>Cheers  
>>  
>>Tim  
>>  
>>At 11:17 31/10/2003, Michael E. Mann wrote:  
>>>Keith,  
>>>  
>>>Thanks--that sounds absolutely great.  
>>>  
>>>I suggest the following  
>>>  
>>>1) I'll fix any remaining typos I find, and incorporate the latest  
>>>comments received from you guys. I expect that I can finalize this in 15  
>>>minutes or so--I agree that this needs to get out by 8:00 AM eastern  
>>>standard time, U.S.  
>>>  
>>>I like where Keith is heading in terms of discussion of the strategy.  
>>>  
>>>Why don't we sign this document, "Mann, Bradley, Hughes" that will be  
>>>ready for distribute to our closest colleagues and allies. I'll prepare  
>>>a PDF version for distribution, to make it difficult for others to alter  
>>>(you never know w/ these folks)...  
>>>  
>>>Should I go ahead and forward this document to Heike, then, in an email?  
>>>Also, should I send this to Dick Kerr at Science separately--Dick has  
>>>often been helpful. And maybe Jesse Smith at Science, and a few key  
>>>journalists (Andy Revkin at New York Times)?  
>>>  
>>>Perhaps, then, Keith, Tim, Phil--you guys, as Keith suggests, can draft  
>>>a separate email to go out to the skepticlist (and all of the scientists  
>>>who were forward it), the CLIMLIST, etc. stating your opinions on this,  
>>>and perhaps \*attaching\* at supporting evidence the document signed by  
>>>Mann, Bradley, Hughes?  
>>>  
>>>Also, do we ask organizations like Environmental Defense Fund, etc. to  
>>>post \*both\* documents (our document, your supporting email) on their  
>>>websites, etc?  
>>>  
>>>What do you guys think?  
>>>  
>>>Thank-you guys have been wonderful, and I am most personally gracious.  
>>>This will not soon be forgotten...  
>>>  
>>>mike  
>>>  
>>>At 10:47 AM 10/31/2003 +0000, Keith Briffa wrote:  
>>>>Hi all - I too have had some problems as to which specific version is  
>>>>where we are at - BUT I think the latest draft as sent by Mike really  
>>>>is virtually there (perhaps some typos to be ironed out (e.g. 'were'  
>>>>instead of 'was' on line 7 of point 2) but I am generally very happy  
>>>>with the tone and balance . Much of Tim's fears (justifiable points on  
>>>>not providing them with wiggle out and distraction options) are allayed  
>>>>by the calm provisos about not being categorical etc. The question now  
>>>>arises as to how to put this out - I believe it does need to go out  
>>>>early so as to be available when the rest of the press start to pick up  
>>>>the MM propaganda . Whether it should be just signed by MBH is up to  
>>>>you . I AM NOT averse to signing , but wonder whether it is a better  
>>>>tactic to put out a separate statement (us , Tom W. and whoever as



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>>>>suggested by Ray , saying we abhor the way this MM paper has been  
>>>>published and publicised without proper scrutiny). I fully agree with  
>>>>the statement as now written however , and willing to go with the  
>>>>majority view. My suggestion about redoing the "audit" was made in good  
>>>>faith and in no way implied I concurred with MM ( in case anyone got  
>>>>the impression that I was not wholly "on side" here).

>>>>So what does everyone else say now?

>>>>REGARDING NATURE - spoke to Heike Langenberg , in the London office  
>>>>and she said it sounded like a potential NEWS item , and asked me to  
>>>>send some details by email and she would forward to the appropriate  
>>>>office - seemed positive. I will do. The statement (s) should anyway go  
>>>>soon on CLIMLIST and then we could quietly contact a few people we know  
>>>>in the media ?

>>>>

>>>>

>>>>Keith

>>>>

>>>>At 05:38 AM 10/31/03 -0500, Michael E. Mann wrote:

>>>>p.s. Keith, any word from Nature. Should I contact them independently?

>>>>And what about Science? Or "Climatic Change" (I have little doubt that

>>>>Steve S could find justification to publish this their in an instant)...

>>>>

>>>>thoughts?

>>>>

>>>>mike

>>>>

>>>>At 03:01 AM 10/31/2003 +0000, f055 wrote:

>>>>Dear all,

>>>>

>>>>>I've just finished preparing a detailed response offline, only to log  
>>>>>on to

>>>>>send it to you all and find new versions from Mike plus more comments

>>>>>and information. Well, I don't have time to change my message now, so

>>>>>will paste it below this message. But bear in mind that the new

>>>>>draft may

>>>>>well have allayed many of my concerns - in particular, a quick glance

>>>>>shows the figure to be much more convincing than the one Mike circulated

>>>>>earlier, indeed it seems to be utterly convincing! I'll reply again on

>>>>>Friday

>>>>>morning once I've had time to read the new draft. In the meantime,

>>>>>here is

>>>>>my message as promised.

>>>>>

>>>>>

>>>>>\*\*\*\*\*

>>>>>Dear MBH (cc to CRU),

>>>>>

>>>>>The number of emails has been rather overwhelming on this issue and

>>>>>I'm struggling to catch up with them! But I will attempt to catch up

>>>>>with a

>>>>>few things here...

>>>>>

>>>>>(1) The single worst thing about the whole M&M saga is not that they did

>>>>>their study, not that they did things wrong (deliberately or by

>>>>>accident), but

>>>>>that neither they nor the journal took the necessary step of

>>>>>investigating

>>>>>whether the difference between their results and yours could be explained

>>>>>simply by some error or set of errors in their use of the data or in

>>>>>their

>>>>>implementation of your method. If it turns out, as looks likely from

>>>>>Mike's

>>>>>investigation of this, that their results are erroneous, then they

>>>>>and the  
>>>>>journal will have wasted countless person-hours of time and caused  
>>>>>much damage in the climate policy arena.  
>>>>>  
>>>>>(2) Given that this is the single worst thing about the saga, we must  
>>>>>not go  
>>>>>and do exactly the same in rushing out a response to their paper. If  
>>>>>some  
>>>>>claims in the response turned out to be wrong, based on assumptions  
>>>>>about what M&M did or assumptions about how M&M's assumptions  
>>>>>affect the results, then it would end up with a number of iterations  
>>>>>of claim  
>>>>>and counter claim. Ultimately the issue might be settled, but by  
>>>>>then the  
>>>>>waters could be so muddled that it didn't matter.  
>>>>>  
>>>>>(3) Not only do I advise against an overly rushed response, but I'm also  
>>>>>wondering whether it really ought to be only from MBH, for three reasons.  
>>>>>(i) It is your paper/results that are being attacked.  
>>>>>(ii) It is difficult to endorse everything that Mike has put in the draft  
>>>>>response because I don't know 100% of the details of MBH and the MBH  
>>>>>data. Sure, I can endorse some things, but others I wouldn't  
>>>>>know. Sure,  
>>>>>I accept Mike's explanation because he's looked at this stuff for 4 days  
>>>>>and I believe he'll have got it right - but that's different to an  
>>>>>independent  
>>>>>check. That must come from Ray or Malcolm if possible.  
>>>>>(iii) If it does come to any independent assessment of who's right and  
>>>>>who's wrong, then it would be difficult for us to be involved if we had  
>>>>>already signed up to what some might claim to be a knee-jerk reaction to  
>>>>>the M&M paper. If that happened, then you would want us to be free  
>>>>>to get  
>>>>>involved to make sure the process was fair and informed.  
>>>>>  
>>>>>This sounds like a cop out, but - like I say - I'm not sure about  
>>>>>point (3) so  
>>>>>feel free to try to convince me otherwise if you wish. Anyway Keith  
>>>>>or Phil  
>>>>>may be happy to sign up to a (quick or slow) response, despite my  
>>>>>reservations above.  
>>>>>  
>>>>>I really advise a very careful reading of M&M and their supplementary  
>>>>>website to ensure that everything in the response is clearly correct -  
>>>>>precisely to avoid point (2). I've only just started to do this, but  
>>>>>already  
>>>>>have some questions about the response that Mike has drafted.  
>>>>>  
>>>>>(a) Mike, you say that many of the trees were eliminated in the data they  
>>>>>used. Have you concluded this because they entered "NA" for "Not  
>>>>>available" in their appendix table? If so, then are you sure that "NA"  
>>>>>means they did not use any data, rather than simply that they didn't  
>>>>>replace your data with an alternative (and hence in fact continued to use  
>>>>>what Scott had supplied to them)? Or perhaps "NA" means they couldn't  
>>>>>find the PC time series published (of course!), but in fact could  
>>>>>find the  
>>>>>raw tree-ring chronologies and did their own PCA of those? How would  
>>>>>they know which raw chronologies to use? Or did you come to your  
>>>>>conclusion by downloading their "corrected and updated" data matrix and  
>>>>>comparing it with yours - I've not had time to do that, but even if I  
>>>>>had and  
>>>>>I  
>>>>>found some differences, I wouldn't know which was right seeing as I've  
>>>>>not done any PCA of western US trees myself? My guess would be that

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>>>>>they downloaded raw tree-ring chronologies (possibly the same ones you  
>>>>>used) but then applied PCA only to the period when they all had full  
>>>>>data -  
>>>>>hence the lack of PCs in the early period (which you got round by doing  
>>>>>PCA on the subset that had earlier data). But this is only a guess, and  
>>>>>this is the type of thing that should be checked with them - surely they  
>>>>>would respond if asked? - to avoid my point (2) above. And if my guess  
>>>>>were right, then your wording of "eliminated this entire data set" would  
>>>>>come in for criticism, even though in practise it might as well have  
>>>>>been.  
>>>>>  
>>>>>(b) The mention of ftp sites and excel files is contradicted by their  
>>>>>email  
>>>>>record on their website, which shows no mention of excel files (they say  
>>>>>an ASCII file was sent) and also no record that they knew the ftp  
>>>>>address.  
>>>>>This doesn't matter really, since the reason for them using a corrupted  
>>>>>data file is not relevant - the relevant thing is that it was corrupt  
>>>>>and had  
>>>>>you been involved in reviewing the paper then it could have been found  
>>>>>prior to publication. But they will use the email record if the ftp  
>>>>>sites and  
>>>>>excel files are mentioned.  
>>>>>  
>>>>>(c) Not sure if you talk about peer-review in the latest version, but  
>>>>>note  
>>>>>that  
>>>>>they acknowledge input from reviewers and Fred Singer's email says he  
>>>>>refereed it - so any statement implying it wasn't reviewed will be  
>>>>>met with  
>>>>>an easy response from them.  
>>>>>  
>>>>>(d) Your quick-look reconstruction excluding many of the tree-ring data,  
>>>>>and the verification RE you obtain, is interesting - but again, don't  
>>>>>rush  
>>>>>into  
>>>>>using these in any response. The time series of PC1 you sent is  
>>>>>certainly  
>>>>>different from your standard one - but on the other hand I'd hardly  
>>>>>say you  
>>>>>"get a similar result" to them, the time series look very different  
>>>>>(see their  
>>>>>fig 6d). So the dismal RE applies only to your calculation, not to their  
>>>>>reconstruction. It may turn out that their verification RE is also very  
>>>>>negative, but again we cannot assume this in case we're wrong and they  
>>>>>easily counter the criticism.  
>>>>>  
>>>>>(e) Claims of their motives for selective censoring or changing of  
>>>>>data, or  
>>>>>for the study as a whole, may well be true but are hard to prove. They  
>>>>>would claim that their's is an honest attempt at reproducing a key  
>>>>>scientific result. If they made errors in what they did, then maybe  
>>>>>they're  
>>>>>just completely out of their depth on this, rather than making deliberate  
>>>>>errors for the purposes of achieving preferred results.  
>>>>>  
>>>>>(f) The recent tree-ring decline they refer to seems related to  
>>>>>tree-ring-width not density. Regardless of width of density, this issue  
>>>>>cannot simply be dismissed as a solved problem. Since they don't make  
>>>>>much of an issue out of it, best just to ignore it.  
>>>>>  
>>>>>(g) [I'm rambling now into an un-ordered list of things, so I'll stop  
>>>>>soon!]

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>>>>>The various other problems relating to temperature data sets, detrended  
>>>>>standard deviations, PCs of tree-ring subsets etc. sound likely errors -  
>>>>>though I've got no way of providing the independent check that you asked  
>>>>>for. But it is again a bit of a leap of faith to say that these  
>>>>>\*explain\* the  
>>>>>different results that they get. Certainly they throw doubt on the  
>>>>>validity  
>>>>>of  
>>>>>their results, but without actually doing the same as them it's not  
>>>>>possible  
>>>>>to say if they would have replicated your results if they hadn't made  
>>>>>these  
>>>>>errors. After all, could the infilling of missing values have made much  
>>>>>difference to the results obtained, something that they made a good deal  
>>>>>of fuss about?  
>>>>>  
>>>>>(h) To say they "used neither the data nor the procedures of MBH98" will  
>>>>>also be an easy target for them, since they did use the data that was  
>>>>>sent  
>>>>>to them and seemed to have used approximately the method too (with  
>>>>>some errors that you've identified). This reproduced your results to  
>>>>>some  
>>>>>extent (certainly not perfectly, but see Fig 6b and 6c). Then they went  
>>>>>further to redo it with the "corrected and updated" data - but only after  
>>>>>first  
>>>>>doing approximately what they claimed they did (i.e. the audit).  
>>>>>  
>>>>>These comments relate to random versions of the draft response, so  
>>>>>apologies if they don't all seem relevant to the current draft. I  
>>>>>don't have  
>>>>>these in front of me, here at home, so I'm doing this from memory of what  
>>>>>I've read over the past few days. But nevertheless, the point is  
>>>>>that a quick  
>>>>>response would ultimately require making a number of assumptions  
>>>>>about what they did and assumptions about whether this explains the  
>>>>>differences or not - assumptions that might be later shot down (in part  
>>>>>only, at most, but still sufficient to muddy the debate for most  
>>>>>outsiders).  
>>>>>  
>>>>>A quick response ought to be limited to something like:  
>>>>>  
>>>>>-----  
>>>>>The recent paper by McIntyre and Mckittrick (2003; hereafter MM03) claims  
>>>>>to be an "audit" of the analysis of Mann, Bradley and Hughes (1998;  
>>>>>hereafter MBH98). MM03 are unable to reproduce the Northern  
>>>>>Hemisphere temperature reconstruction of MBH98 when attempting to  
>>>>>use the same proxy data and methods as MBH98, though they obtain  
>>>>>something similar with clearly anomalous recent warming (their Figure  
>>>>>6c). They then make many modifications to the proxy data set and repeat  
>>>>>their analysis, and obtain a rather different result to MBH98.  
>>>>>  
>>>>>Unfortunately neither M&M nor the journal in which it was published took  
>>>>>the necessary step of investigating whether the difference between their  
>>>>>results and MBH98 could be explained simply by some error or set of  
>>>>>errors in their use of the data or in their implementation of the MBH98  
>>>>>method. This should have been an essential step to take in a case such  
>>>>>as this where the difference in results is so large and  
>>>>>important. Simple  
>>>>>errors must first be ruled out prior to publication. Even if the  
>>>>>authors had  
>>>>>not undertaken this by presenting their results to the authors of MBH98,  
>>>>>the journal should certainly have included them as referees of the  
>>>>>manuscript.

>>>>>  
>>>>>A preliminary investigation into the proxy data and implementation of the  
>>>>>method has already identified a number of likely errors, which may turn  
>>>>>out to be the cause of the different results. Rather than repeating  
>>>>>M&M's  
>>>>>failure to follow good scientific practise, we are withholding further  
>>>>>comments until we can - by collaboration with M&M if possible - be  
>>>>>certain  
>>>>>of exactly what changes to data and method were made by M&M, whether  
>>>>>these changes can really explain the differences in the results, and  
>>>>>eventually which (if any) of these changes can be justified as  
>>>>>equally valid  
>>>>>(given the various uncertainties that exist) and which are simply  
>>>>>errors that  
>>>>>invalidate their results.

>>>>>-----

>>>>>  
>>>>>Hope you find this all helpful, and despite my seemingly critical  
>>>>>approach,  
>>>>>take them in the spirit with which they are aimed - which is to obtain a  
>>>>>strong and hard hitting rebuttal of bad science, but a rebuttal that  
>>>>>cannot  
>>>>>be buried by any minor inaccuracies or difficult-to-prove claims.

>>>>>  
>>>>>Best regards

>>>>>  
>>>>>Tim

>>>>>

---

>>>>> Professor Michael E. Mann  
>>>>> Department of Environmental Sciences, Clark Hall  
>>>>> University of Virginia  
>>>>> Charlottesville, VA 22903

---

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Paleoclimatology Book Web Site: <http://www.geo.umass.edu/climate/paleo/html>  
</x-flowed>

4807. 2003-10-31

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cc: mhughes <[mhughes@ltrr.arizona.edu](mailto:mhughes@ltrr.arizona.edu)>  
date: Fri, 31 Oct 2003 07:18:11 -0500  
from: "Michael E. Mann" <[mann@virginia.edu](mailto:mann@virginia.edu)>  
subject: RE: CLIMLIST  
to: Tim Osborn <[t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)>, Keith Briffa <[k.briffa@uea.ac.uk](mailto:k.briffa@uea.ac.uk)>, "p.jones" <[p.jones@uea.ac.uk](mailto:p.jones@uea.ac.uk)>, "raymond s. bradley" <[rbradley@geo.umass.edu](mailto:rbradley@geo.umass.edu)>  
Thanks Tim,  
Sounds like the best possible plan under the circumstances. Attached is the revised (final?) version, see the note about Malcolm Hughes unable for comment--does this seem ok?  
If this looks good to you guys, you don't see any typos, etc. lets consider this the final version. I've attached it in both word and pdf--only the pdf should probably be sent around.  
As per your suggestions, I'll await receipt of your CLIMLIST/SCEPTICSLIST email to send this together to other outlets for joint posting. Will you guys will send out to all of the other scientists, etc. who' Timmerata, etc. emailed too?  
The appropriate outlets would be: EDF, other NGO groups, Ross Gelbspan's site, David Appell's blog, etc...  
So I'll await further word from you,  
thanks again,  
mike  
At 12:05 PM 10/31/2003 +0000, Tim Osborn wrote:  
Mike et al.,  
we (Keith and I) are happy with this strategy. Rebuttal will be signed Mann and Bradley (Hughes to be added later when available to confirm/modify) and circulated to allies/friends first.  
As soon as we get a final version from Mike, Keith will forward it with a message to Heike.

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We will also draft an email from Keith, me and Phil to send to the email lists, expressing our views on this and attaching your final version. As to other people that you mention (Science, EFD), we'll leave that to you, Mike, to do - though you may well want to use our CLIMLIST/SCEPTICSLIST email AND your final version together to send to them, so might want to wait till we've drafted that email. Cheers  
Tim  
At 11:17 31/10/2003, Michael E. Mann wrote:  
Keith,  
Thanks--that sounds absolutely great.  
I suggest the following  
1) I'll fix any remaining typos I find, and incorporate the latest comments received from you guys. I expect that I can finalize this in 15 minutes or so--I agree that this needs to get out by 8:00 AM eastern standard time, U.S.  
I like where Keith is heading in terms of discussion of the strategy. Why don't we sign this document, "Mann, Bradley, Hughes" that will be ready for distribute to our closest colleagues and allies. I'll prepare a PDF version for distribution, to make it difficult for others to alter (you never know w/ these folks)...  
Should I go ahead and forward this document to Heike, then, in an email? Also, should I send this to Dick Kerr at Science separately--Dick has often been helpful. And maybe Jesse Smith at Science, and a few key journalists (Andy Revkin at New York Times)?  
Perhaps, then, Keith, Tim, Phil--you guys, as Keith suggests, can draft a separate email to go out to the skepticlist (and all of the scientists who were forward it), the CLIMLIST, etc. stating your opinons on this, and perhaps \*attaching\* at supporting evidence the document signed by Mann, Bradley, Hughes?  
Also, do we ask organizations like Environmental Defense Fund, etc. to post \*both\* documents (our document, your supporting email) on their websites, etc?  
what do you guys think?  
Thank-you guys have been wonderful, and I am most personally gracious. This will not soon be forgotten...  
mike  
At 10:47 AM 10/31/2003 +0000, Keith Briffa wrote:  
Hi all - I too have had some problems as to which specific version is where we are at - BUT I think the latest draft as sent by Mike really is virtually there (perhaps some typos to be ironed out (e.g. 'were' instead of 'was' on line 7 of point 2) but I am generally very happy with the tone and balance . Much of Tim's fears (justifiable points on not providing them with wiggle out and distraction options) are allayed by the calm provisos about not being categorical etc. The question now arises as to how to put this out - I believe it does need to go out early so as to be available when the rest of the

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press start to pick up the MM propaganda . Whether it should be just signed by MBH is up to you . I AM NOT averse to signing , but wonder whether it is a better tactic to put out a separate statement (us , Tom W. and whoever as suggested by Ray , saying we abhor the way this MM paper has been published and publicised without proper scrutiny). I fully agree with the statement as now written however , and willing to go with the majority view. My suggestion about redoing the "audit" was made in good faith and in no way implied I concurred with MM ( in case anyone got the impression that I was not

wholly "on side" here).

So what does everyone else say now?

REGARDING NATURE - spoke to Heike Langenberg , in the London office and she said it sounded like a potential NEWS item , and asked me to send some details by email and she would forward to the appropriate office - seemed positive. I will do. The statement (s) should anyway go soon on CLIMLIST and then we could quietly contact a few people we know in the media ?

Keith

At 05:38 AM 10/31/03 -0500, Michael E. Mann wrote:

what about p.s. Keith, any word from Nature. Should I contact them independently? And Science? or "Climatic Change" (I have little doubt that Steve S could find justification to publish this their in an instant)... thoughts?

mike

At 03:01 AM 10/31/2003 +0000, f055 wrote:

Dear all,

I've just finished preparing a detailed response offline, only to log on to send it to you all and find new versions from Mike plus more comments and information. Well, I don't have time to change my message now, so will paste it below this message. But bear in mind that the new draft may well have allayed many of my concerns - in particular, a quick glance shows the figure to be much more convincing than the one Mike circulated earlier, indeed it seems to be utterly convincing! I'll reply again on Friday morning once I've had time to read the new draft. In the meantime, here is my message as promised.

\*\*\*\*\*

Dear MBH (cc to CRU),

The number of emails has been rather overwhelming on this issue and I'm struggling to catch up with them! But I will attempt to catch up with a few things here...

(1) The single worst thing about the whole M&M saga is not that they did their study, not that they did things wrong (deliberately or by accident), but that neither they nor the journal took the necessary step of investigating whether the difference between their results and yours could be explained simply by some error or set of errors in their use of the data or in their implementation of your method. If it turns out, as looks likely from Mike's investigation of this, that their results are erroneous, then they and the journal will have wasted countless person-hours of time and caused much damage in the climate policy arena.

(2) Given that this is the single worst thing about the saga, we must not go and do exactly the same in rushing out a response to their paper. If some claims in the response turned out to be wrong, based on assumptions



about what M&M did or assumptions about how M&M's assumptions affect the results, then it would end up with a number of iterations of claim and counter claim. Ultimately the issue might be settled, but by then the waters could be so muddied that it didn't matter.

(3) Not only do I advise against an overly rushed response, but I'm also wondering whether it really ought to be only from MBH, for three reasons.

(i) It is your paper/results that are being attacked.

(ii) It is difficult to endorse everything that Mike has put in the draft response because I don't know 100% of the details of MBH and the MBH data. Sure, I can endorse some things, but others I wouldn't know. Sure, I accept Mike's explanation because he's looked at this stuff for 4 days and I believe he'll have got it right - but that's different to an independent check. That must come from Ray or Malcolm if possible.

(iii) If it does come to any independent assessment of who's right and who's wrong, then it would be difficult for us to be involved if we had already signed up to what some might claim to be a knee-jerk reaction to the M&M paper. If that happened, then you would want us to be free to get involved to make sure the process was fair and informed.

This sounds like a cop out, but - like I say - I'm not sure about point (3) so feel free to try to convince me otherwise if you wish. Anyway Keith or Phil may be happy to sign up to a (quick or slow) response, despite my reservations above.

I really advise a very careful reading of M&M and their supplementary website to ensure that everything in the response is clearly correct - precisely to avoid point (2). I've only just started to do this, but already have some questions about the response that Mike has drafted.

(a) Mike, you say that many of the trees were eliminated in the data they used. Have you concluded this because they entered "NA" for "Not available" in their appendix table? If so, then are you sure that "NA" means they did not use any data, rather than simply that they didn't replace your data with an alternative (and hence in fact continued to use what Scott had supplied to them)? Or perhaps "NA" means they couldn't find the PC time series published (of course!), but in fact could find the raw tree-ring chronologies and did their own PCA of those? How would they know which raw chronologies to use? Or did you come to your conclusion by downloading their "corrected and updated" data matrix and comparing it with yours - I've not had time to do that, but even if I had and I

found some differences, I wouldn't know which was right seeing as I've not done any PCA of western US trees myself? My guess would be that they downloaded raw tree-ring chronologies (possibly the same ones you used) but then applied PCA only to the period when they all had full data - hence the lack of PCs in the early period (which you got round by doing PCA on the subset that had earlier data). But this is only a guess, and this is the type of thing that should be checked with them - surely they would respond if asked? - to avoid my point (2) above. And if my guess were right, then your wording of "eliminated this entire data set" would come in for criticism, even though in practise it might as well have been.

(b) The mention of ftp sites and excel files is contradicted by their email record on their website, which shows no mention of excel files (they say an ASCII file was sent) and also no record that they knew the ftp address. This doesn't matter really, since the reason for them using a corrupted data file is not relevant - the relevant thing is that it was corrupt and had you been involved in reviewing the paper then it could have been found prior to publication. But they will use the email record if the ftp sites and excel files are mentioned.

(c) Not sure if you talk about peer-review in the latest version, but note that

they acknowledge input from reviewers and Fred Singer's email says he refereed it - so any statement implying it wasn't reviewed will be met with an easy response from them.

(d) Your quick-look reconstruction excluding many of the tree-ring data, and the verification RE you obtain, is interesting - but again, don't rush

into

using these in any response. The time series of PC1 you sent is certainly different from your standard one - but on the other hand I'd hardly say you "get a similar result" to them, the time series look very different (see their fig 6d). So the dismal RE applies only to your calculation, not to their reconstruction. It may turn out that their verification RE is also very negative, but again we cannot assume this in case we're wrong and they easily counter the criticism.

(e) Claims of their motives for selective censoring or changing of data, or for the study as a whole, may well be true but are hard to prove. They would claim that their's is an honest attempt at reproducing a key scientific result. If they made errors in what they did, then maybe they're just completely out of their depth on this, rather than making deliberate errors for the purposes of achieving preferred results.

(f) The recent tree-ring decline they refer to seems related to tree-ring-width not density. Regardless of width of density, this issue cannot simply be dismissed as a solved problem. Since they don't make much of an issue out of it, best just to ignore it.

(g) [I'm rambling now into an un-ordered list of things, so I'll stop soon!] The various other problems relating to temperature data sets, detrended standard deviations, PCs of tree-ring subsets etc. sound likely errors - though I've got no way of providing the independent check that you asked for. But it is again a bit of a leap of faith to say that these \*explain\* the different results that they get. Certainly they throw doubt on the validity of

their results, but without actually doing the same as them it's not possible to say if they would have replicated your results if they hadn't made these errors. After all, could the infilling of missing values have made much difference to the results obtained, something that they made a good deal of fuss about?

(h) To say they "used neither the data nor the procedures of MBH98" will also be an easy target for them, since they did use the data that was sent to them and seemed to have used approximately the method too (with some errors that you've identified). This reproduced your results to some extent (certainly not perfectly, but see Fig 6b and 6c). Then they went further to redo it with the "corrected and updated" data - but only after first

doing approximately what they claimed they did (i.e. the audit).

These comments relate to random versions of the draft response, so apologies if they don't all seem relevant to the current draft. I don't have these in front of me, here at home, so I'm doing this from memory of what I've read over the past few days. But nevertheless, the point is that a quick response would ultimately require making a number of assumptions about what they did and assumptions about whether this explains the differences or not - assumptions that might be later shot down (in part only, at most, but still sufficient to muddy the debate for most outsiders). A quick response ought to be limited to something like:

-----  
The recent paper by McIntyre and Mckittrick (2003; hereafter MM03) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998; hereafter MBH98). MM03 are unable to reproduce the Northern Hemisphere temperature reconstruction of MBH98 when attempting to use the same proxy data and methods as MBH98, though they obtain something similar with clearly anomalous recent warming (their Figure 6c). They then make many modifications to the proxy data set and repeat their analysis, and obtain a rather different result to MBH98. Unfortunately neither M&M nor the journal in which it was published took the necessary step of investigating whether the difference between their results and MBH98 could be explained simply by some error or set of errors in their use of the data or in their implementation of the MBH98 method. This should have been an essential step to take in a case such as this where the difference in results is so large and important. Simple errors must first be ruled out prior to publication. Even if the authors had

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not undertaken this by presenting their results to the authors of MBH98, the journal should certainly have included them as referees of the manuscript.

A preliminary investigation into the proxy data and implementation of the method has already identified a number of likely errors, which may turn out to be the cause of the different results. Rather than repeating M&M's failure to follow good scientific practise, we are withholding further comments until we can - by collaboration with M&M if possible - be certain of exactly what changes to data and method were made by M&M, whether these changes can really explain the differences in the results, and eventually which (if any) of these changes can be justified as equally valid (given the various uncertainties that exist) and which are simply errors that invalidate their results.

-----  
Hope you find this all helpful, and despite my seemingly critical approach, take them in the spirit with which they are aimed - which is to obtain a strong and hard hitting rebuttal of bad science, but a rebuttal that cannot be buried by any minor inaccuracies or difficult-to-prove claims.

Best regards

Tim

---

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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\Rebuttal.pdf" Attachment Converted:  
"c:\eudora\attach\Rebuttal5.doc"

3175. 2003-11-01

date: Sat, 01 Nov 2003 13:50:38 -0500  
 from: "Michael E. Mann" <mann@virginia.edu>  
 subject: E&E paper responses  
 to: cfk@lanl.gov, berger@astr.ucl.ac.be, ammann@ucar.edu,  
 david@atmos.washington.edu, davet@atmos.colostate.edu, wuebbles@atmos.uiuc.edu,  
 dshindell@giss.nasa.gov, gavin@isis.giss.nasa.gov, drdendro@ldeo.columbia.edu,  
 druidrd@ldeo.columbia.edu, mcane@ldeo.columbia.edu, thompson.3@osu.edu,  
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 schlesin@atmos.uiuc.edu, masson@lsce.saclay.cea.fr, mehta@climate.gsfc.nasa.gov,  
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 Barrie.Pittock@csiro.au, m.hulme@uea.ac.uk, n.nicholls@bom.gov.au,  
 mann@virginia.edu

Dear Colleagues,

Tim Osborn, Keith Briffa, and Phil Jones of the University of East Anglia have posted a

commentary on the

recent paper by McIntyre and Mckittrick (Energy and Environment, 14, 751-771, 2003) which

claimed to provide an "audit" of the analysis of Mann, Bradley and Hughes

(Nature, 392,

779-787, 1998; hereafter MBH98), with a link to our response to the paper here:

[1]<http://www.cru.uea.ac.uk/~timo/paleo/>

I imagine that the additional information provided will place a very different perspective

on the matter.

Please feel free to forward this information to anyone who you feel might benefit from it.

Best regards,

Mike Mann

---

Professor Michael E. Mann

cg2003  
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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

328. 2003-11-03

cc: k.briffa@uea.ac.uk, Phil Jones <p.jones@uea.ac.uk>, <T.Osborn@uea.ac.uk>  
date: Mon, 3 Nov 2003 10:54:39 -0500 (EST)  
from: Alan Robock <robock@envsci.rutgers.edu>  
subject: Re: McIntyre and Mckitrick response  
to: "Michael E. Mann" <mann@virginia.edu>

Dear Mike,

OK. But it seems to me that if the climlist message, which would be from a third party (me), contains a link directly to your reply, more people would read it.

Alan

Professor Alan Robock

Editor, JGR - Atmospheres

Director, Center for Environmental Prediction

Department of Environmental Sciences

Phone: +1-732-932-9478

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<http://envsci.rutgers.edu/~robock>

On Mon, 3 Nov 2003, Michael E. Mann wrote:

> Dear Alan,

>

> I strongly support going w/ the CRU version, because this is also the version that has gone out to other mailing lists, my email went to a smaller number of > colleagues. Also, a 3rd party posted the link to M&M. I see know reason why it would not be appropriate therefore that a 3rd party also post the link to the > response. This seems more symmetric in nature to me.

>

> Thanks all for the help,

>

> mike

>

> At 10:44 AM 11/3/2003 -0500, Alan Robock wrote:

> Dear Mike,

>

> Please let us know what you want to do.

>

> Alan

>

> Professor Alan Robock

> Editor, JGR - Atmospheres

> Director, Center for Environmental Prediction

> Department of Environmental Sciences

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> New Brunswick, NJ 08901-8551 USA

<http://envsci.rutgers.edu/~robock>

>

> On Mon, 3 Nov 2003, CLIMLIST wrote:

>

> > Drs. Robock, Osborn, Briffa, and Jones,

>

> > As you are aware, in the interest of fairness, I have agreed to post a > cordial response to the posting of Dr. Bartlett last week, and then I > will declare the issue closed for discussion on CLIMLIST.

>

cg2003

> This morning, I have two messages that are requested to be distributed  
> -- one from Dr. Robock and the other from Drs. Osborn, Briffa, and  
> Jones. I really only want to post one of these. Both messages  
> contained references to the same website, and I appreciate the use of  
> the website as I had requested. I'm more inclined to post Dr. Robock's  
> message, because it contains Dr. Mann's words. While I respect the  
> cordiality and professionalism exhibited by the message of Drs. Osborn,  
> Briffa, and Jones, would you all be in agreement if I posted the message  
> from Dr. Robock instead?

> Below are the two messages for your information.

> Dear Bob,  
> Here is the response from Mike Mann. It provides a link to a web page  
> by Osborn, Briffa, and Jones:  
> <http://www.cru.uea.ac.uk/~timo/paleo/>  
> which provides a link to the response by Mann, Bradley and Hughes:  
> <http://www.cru.uea.ac.uk/~timo/paleo/EandEPaperProblem.pdf>  
> Please post it to CLIMLIST.  
> Alan  
> Professor Alan Robock  
> Editor, JGR - Atmospheres  
> Director, Center for Environmental Prediction  
> Department of Environmental Sciences Phone: +1-732-932-9478  
> Rutgers University Fax: +1-732-932-8644  
> 14 College Farm Road E-mail: [robock@envsci.rutgers.edu](mailto:robock@envsci.rutgers.edu)  
> New Brunswick, NJ 08901-8551 USA <http://envsci.rutgers.edu/~robock>

> ----- Forwarded message -----  
> Date: Sat, 01 Nov 2003 13:50:38 -0500  
> From: Michael E. Mann <[mann@virginia.edu](mailto:mann@virginia.edu)>  
> Subject: E&E paper responses

> Dear Colleagues,  
> Tim Osborn, Keith Briffa, and Phil Jones of the University of East  
> Anglia have posted a commentary on the recent paper by McIntyre and  
> McKittrick (Energy and Environment, 14, 751-771, 2003) which claimed to  
> provide an "audit" of the analysis of Mann, Bradley and Hughes (Nature,  
> 392, 779-787, 1998; hereafter MBH98), with a link to our response to the  
> paper here:

> <http://www.cru.uea.ac.uk/~timo/paleo/>

> I imagine that the additional information provided will place a very

cg2003

> > different perspective on the matter.  
> >  
> > Please feel free to forward this information to anyone who you feel  
> > might benefit from it.  
> >  
> > Best regards,  
> >  
> > Mike Mann  
> >  
> > \_\_\_\_\_  
> > Professor Michael E. Mann  
> > Department of Environmental Sciences, Clark Hall  
> > University of Virginia  
> > Charlottesville, VA 22903  
> > \_\_\_\_\_  
> > e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137  
> > http://www.evsc.virginia.edu/faculty/people/mann.shtml  
> >  
> >  
> >  
> >

~~~~~  
> >
> > f055 wrote:
> > > Dear Dr. Rohli,
> > >
> > > here follows a short response to the posting that was highlighting the
> > > McIntyre and Mckitrick paper, posted (I think) by Prof. Bartlett. I
hope it
> > > is in
> > > a suitable format and style for CLIMLIST and can be posted as it
stands.
> > > If not, please advise me how to change it so that it is suitable.
> > >
> > > -----
> > >
> > > This is a response to the posting related to the McIntyre and Mckitrick
> > > (Energy and Environment, 14, 751-771, 2003) study of the Northern
> > > Hemisphere temperature reconstruction previously published by Mann,
> > > Bradley and Hughes (Nature, 392, 779-787, 1998; hereafter MBH98).
> > >
> > > We suggest that those interested in the claim made by McIntyre and
> > > Mckitrick (MM) should also read the initial response from Mann and his
> > > colleagues.
> > >
> > > We have posted their initial response on our website for those
interested
> > > in this issue:
> > >
> > > http://www.cru.uea.ac.uk/~timo/paleo/
> > >
> > > According to this initial response, it looks likely that there are
serious
> > > questions regarding the manner in which MM have attempted to
> > > implement the Mann et al. method, and specific problems with the
> > > selection of predictors.
> > >
> > > Amazingly, the journal "Energy and Environment" that published the MM
review
> > > work, made no attempt to provide Mann et al. with the opportunity to
> > > the MM paper or establish the details of the MM work.
> > >
> > > Objective readers, with a desire to get to the "truth" of this issue,

cg2003

would do

> > well not to jump to premature conclusions and, unlike Energy and
> > Environment, at least allow these respected, experienced, and
invariably
> > careful researchers the courtesy of a considered response, after they
> > have had time to study the so-called audit in detail.
> >
> > Tim Osborn
> > Keith Briffa
> > Phil Jones
> >
> > -----
> >
> >
> >
> >
> >
> > -----
> > Robert V. Rohli
> > Southern Regional Climate Center, Dept of Geography and Anthropology
> > Louisiana State University, Baton Rouge, LA 70803-4105 U.S.A.
> > +1-225-578-6137 (phone) * +1-225-578-2912 (fax) * climlist@srcc.lsu.edu
> >
>
>

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> http://www.evsc.virginia.edu/faculty/people/mann.shtml
>

358. 2003-11-03

cc: rbradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>,
mhughes@lrr.arizona.edu, phil Jones <p.jones@uea.ac.uk>
date: Mon, 03 Nov 2003 09:00:57 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: RE: posting of message
to: Tim Osborn <t.osborn@uea.ac.uk>

Tim,
OK--try this one. I've given it the same name as the original, and so you can
just
overprint the pdf file in the link...
thanks again,
mike
At 01:51 PM 11/3/2003 +0000, you wrote:
Mike - first sentence (in parentheses) of third paragraph of new version seems
to have a
word or two missing - Tim
At 13:29 03/11/2003, you wrote:
Hey,
Can you guys replace the current pdf w/ the new one using the same name as the
old one?
This is because some websites, blogs, etc. are already pointing to the URL for
the pdf
file, and I want to make sure they point to the updated pdf file.
I promise we won't continue to revise and ask you to repost, etc. I spent the
weekend
looking over this to see if there were any things that needed to be clarified,
and

cg2003

that's how I came up w/ the small number of revisions.

Thanks again for your help,

mike

At 11:08 AM 11/3/2003 +0000, Tim Osborn wrote:

Hi Mike et al,

I added this to the statement on our website, trying to phrase it so that the disagreement between MM and many other (really "*every*"?) reconstructions was seen as

reason to more carefully check MM than as reason to reject MM.

[1]<http://www.cru.uea.ac.uk/~timo/paleo/>

Cheers

Tim

At 16:59 01/11/2003, Michael E. Mann wrote:

The fact that the MM is so at odds with *every* other published estimate, not just MBH98

Dr Timothy J Osborn

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sunclock: [3]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[7]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\EandEPaperProblem8.pdf"

1763. 2003-11-03

cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>, "Raymond s.bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, mann@virginia.edu

date: Mon, 03 Nov 2003 13:00:30 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Fwd: RE: posting of message

to: Tim Osborn <t.osborn@uea.ac.uk>

Hi Tim,

Disregard my previous email, for updates you can refer people to our webpage here:

cg2003

[1]<http://holocene.evsc.virginia.edu/Mann/EEReply.html>

Right now, its just a mirror of your page, but w/ the response updated. I can always

update it at a later time, but why don't you just link here for updates..

Thanks a bunch. Can you get this in before quitting time tonight? I think it's important

people see

the clarified version, lest they be confused about how the PCs were calculated for the sub

networks. Thanks a bunch,

mike

At 03:19 PM 11/3/2003 +0000, Tim Osborn wrote:

Mike,

I will place the revised version on the website, but I would also like to give you the

flexibility to add further information or discussion over the coming weeks.

What I

would like to do is to put the revised version on the site, keep the one posted there on

Saturday as well (with appropriate explanation), and also add in a link to your website

and say that people can go there for later updates and/or further information that you

may add whenever it seems appropriate.

To do this, I just need a web address of where you will put such things. Even if you

have nothing further to add yet, the web address needs to point to a page that exists,

even if almost empty.

Does that sound good to you?

Cheers

Tim

At 14:51 03/11/2003, you wrote:

Hi Tim,

Can you let me know as soon as the revised version has been substituted?

Thanks a bunch,

mike

Date: Mon, 03 Nov 2003 09:00:57 -0500

To: Tim Osborn <t.osborn@uea.ac.uk>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: RE: posting of message

Cc: rbradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>,

Malcolm

Hughes, phil Jones <p.jones@uea.ac.uk>

Tim,

OK--try this one. I've given it the same name as the original, and so you can just

overprint the pdf file in the link...

thanks again,

mike

At 01:51 PM 11/3/2003 +0000, you wrote:

Mike - first sentence (in parentheses) of third paragraph of new version seems to have a

word or two missing - Tim

At 13:29 03/11/2003, you wrote:

Hey,

Can you guys replace the current pdf w/ the new one using the same name as the old one?

This is because some websites, blogs, etc. are already pointing to the URL for the pdf

file, and I want to make sure they point to the updated pdf file.

I promise we won't continue to revise and ask you to repost, etc. I spent the weekend

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Thanks again for your help,

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seen as I added this to the statement on our website, trying to phrase it so that the disagreement between MM and many other (really "*every*"?) reconstructions was

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Cheers

Tim

At 16:59 01/11/2003, Michael E. Mann wrote:

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cg2003

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[12]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2944. 2003-11-03

cc: t.osborn@uea.ac.uk, p.jones@uea.ac.uk
date: Mon, 03 Nov 2003 16:51:31 +0000
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: follow up as per Mike's comments earlier?
to: mann@virginia.edu, rbradley@geo.umass.edu, mann@virginia.edu,
mhughes@ltrr.arizona.edu
<x-flowed>

Mike , Ray, Malcolm

We three have been discussing the weeks shenanigans and thought we should start the wider discussion on the concept and practical aspects of someone (perhaps us - perhaps not us) doing the "independent" audit of your 98 and/or 99 work. It is clear that the debate as regards the M and M results will now likely stall , until one or more people undertake this - but it is unlikely to go away until such time as something is done. The problem , however, is what this audit would set out to do.

If it constitutes taking the exact same data and exact implementation of your method - there seems little value in doing it - the result will be identical to your result(s). The question then , revolves around a fuller experiment in the use of various selection criteria for retaining subsets of the predictors , and presumably working towards gaining some better impression of the stability of the results , and the sensitivity to the inclusion of particular predictors. We could perhaps also compare results with those achieved using other methods (such as our Orthogonal Spatial Regression)? We would not be attempting to do any new reconstructions. The question then , stimulated again by Mike's message , and the message from the Editor of E and E, is whether we should consider going ahead here, with some such work?

We are already motivated to look at the role of the tree-ring data (in collaboration with Malcolm and Ed hopefully, looking at stuff like standardisation issues, the western U.S. recent trend correction etc.) , but we feel some discussion among all of us would help to clarify opinions and prospects for a wider look at the robustness of the Mann98/99 result. We have no particular axe to grind , but it is almost certain that there will be some pressure for some such work, and we suspect that DEFRA here will be quizzed by various bodies for their opinion on this. If so, why not us rather than others ?

It may be that anything we do here would not be seen as "independent" by the skeptics anyway (and we would not consider doing it without some appropriate level of interaction with you lot) - but in the end , what counts, is what is published in the peer-review literature. It was important to get your statement out , but it needs to be followed up now by one or more studies by other groups. We could go with the candidate predictor set you used and do a Monte Carlo approach to selection over different periods , or add in other predictors orwhat? What do you think? We might need to go for a very small amount of money from DEFRA (to pay Harry or someone just to manipulate palaeo data , and then after implementing the method(s) and deciding on the scheme, run the numerous experiments and synthesize

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results), or we may be able to do it by diverting his time from some other stuff anyway.

To get the discussion going, we wish to ask your opinion(s) on the concept, level of interaction between us and you guys (in planning, or also implementation, and synthesis, writing up?). What about this issue of our perceived independence - do we give a damn?

Keith, Tim, Phil

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
<http://www.cru.uea.ac.uk/cru/people/briffa/>
</x-flowed>

4253. 2003-11-03

cc: t.osborn@uea.ac.uk, p.jones@uea.ac.uk, mann@virginia.edu, Scott Rutherford
<srutherford@rwu.edu>

date: Mon, 03 Nov 2003 12:16:00 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: follow up as per Mike's comments earlier?

to: Keith Briffa <k.briffa@uea.ac.uk>, rbradley@geo.umass.edu,
mhughes@lrr.arizona.edu

Hi Keith,

Thanks for your message.

I think as two related issues.

1) Building a bit on the initial response we've already submitted, I believe we can show

beyond a reasonable doubt that the reconstruction MM produced natural results from the

elimination of key predictors from our network--and that the resulting reconstructions,

unlike those we published, do not pass cross-validation.

We're talking to Nature right now about allowing us a formal response, and I believe this

may go forward...This alone will demonstrate the invalidity of the MM03 paper, and I'd like

to think that it will put the matter behind us as far as legitimate scientists are

concerned--the feedback I'm already getting indicates that our colleagues believe that we

have shown already strongly put in doubt the M&M result just with the limited analysis

we've performed...

2) What you suggest, however, would be immensely useful. It is a natural followup to the

paper that we all currently have in review in Journal of Climate w/ Scott as first author,

and I see that as the first step, which is a first stab at the intercomparison issue,

though it doesn't deconstruct the MBH98 predictor network. So I'd like to suggest we

proceed, as you suggest, in that spirit. We can begin to coordinate plans, and it would

seem natural to include Scott in this as he has really been carrying the actual work

forward for us...

3) In parallel, we have been working on a considerable expansion of the original MBH98

network for further reconstructions, and perhaps we should discuss how this

might best mesh
w/ your efforts.
Further comments?
Thanks,
mike

At 04:51 PM 11/3/2003 +0000, Keith Briffa wrote:

Mike , Ray, Malcolm

We three have been discussing the weeks shenanigans and thought we should start the wider discussion on the concept and practical aspects of someone (perhaps us - perhaps not us) doing the "independent" audit of your 98 and/or 99 work. It is clear that the debate as regards the M and M results will now likely stall , until one or more people undertake this - but it is unlikely to go away until such time as something is done. The problem , however, is what this audit would set out to do.

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cg2003

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1749. 2003-11-04

cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>
date: Tue, 04 Nov 2003 07:46:37 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Fwd: RE: posting of message
to: Tim Osborn <t.osborn@uea.ac.uk>

Tim et al,
What do you think of the revised webpage:
[1]<http://holocene.evsc.virginia.edu/Mann/EERReply.html>

Thanks,
mike

At 11:14 AM 11/4/2003 +0000, Tim Osborn wrote:

Hi Mike,

I had meetings etc. yesterday, I so couldn't get the update posted yet.

I will do so today, but am not so happy with your webpage simply being a

mirror of

ours. It just has our names on it, and so the updates plus the correction

that you've

posted at the end, appear to be coming from us or at least endorsed by us.

I'd prefer a webpage that was more clearly authored by you, though of course

linking to

our page to explain our statement/involvement, followed by whatever updates

you wish to

post.

Sorry if this seems ultra-sensitive to you; it's just that we feel that

keeping some

level of independence (and being seen to do so) would be useful for defending

our

subsequent objectivity on this issue - two independent opinions will carry

cg2003

more weight

than two that aren't seen as independent.

Cheers

Tim

At 18:00 03/11/2003, Michael E. Mann wrote:

Hi Tim,

Disregard my previous email, for updates you can refer people to our webpage here:

[2]<http://holocene.evsc.virginia.edu/Mann/EEReply.htm>

Right now, its just a mirror of your page, but w/ the response updated. I can always

update it at a later time, but why don't you just link here for updates..

Thanks a bunch. Can you get this in before quitting time tonight? I think it's important

people see

the clarified version, lest they be confused about how the PCs were calculated for the

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mike

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[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2199. 2003-11-04

date: Tue, 04 Nov 2003 10:12:01 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: status

to: "Raymond s.bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes"

<mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, "Keith Briffa"

<k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>

Dear All,

I've discussed w/ Ray, and the game plan is that we're not going get down into a back-and-forth w/ these folks now. We'll let others, if necessary, comment on their

comments, but we're (i.e., Mann/Bradley/Hughes) going to focus our energy now on a formal

response in the peer-reviewed literature.

I've discussed the matter w/ Nature, who is considering allowing us a response (something

that would be brief, making liberal use of supplementary information for technical details,

and would presumably go out for peer review). If not, they might do a news story/editorial

on this anyway.

The alternative, then, would be "Climatic Change"--Steve Schneider has indicated an

interest in publishing our response (again, peer-reviewed) in the event that Nature feels

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its not appropriate (Heike expressed some reticence about publishing a reply to
a paper
from another journal)...
mike

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Charlottesville, VA 22903

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1651. 2003-11-05

date: Wed, 05 Nov 2003 07:53:15 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: ad additional suggestion...
to: Scott Rutherford <srutherford@rwu.edu>, "Raymond S. Bradley"
<rbradley@geo.umass.edu>, mhughes@lrr.arizona.edu, Tim Osborn
<t.osborn@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>, Phil Jones
<p.jones@uea.ac.uk>, mann@virginia.edu
Dear All,
Hopefully, the JGR paper on which Scott is first author and we're all
co-authors, should be
coming back from review soon (Scott--please contact J. Climate to find out what
the status
is ASAP). As I mentioned before, I see this as a natural first step in the
broader future
collaborative effort that Keith has nicely layed out, in which we can look in
detail at the
sensitivity to selection of candidate predictors, issues of seasonal, spatial
sampling,
etc---all of the things we all know need to be looked at in more detail.
I strongly endorse the idea of making this a collaborative effort of the full
group of us,
perhaps w/ Tim and Scott in the lead of the joint project (do people feel this
is
reasonable?). Between the two groups, I think we're fully funded for this type
of
activity...
Along these lines, I have a suggestion for Scott regarding the J. Climate paper
that should
be coming in from review soon. A few important measures taken here can go a long
way to
combatting the latest E&E criticism of MBH98, since we get essentially the same
results for
the MBH98 network w/ a completely different statistical method, and explicitly
compare
results w/ other networks, etc.
By the time the paper appears, we want to have a supplementary website (to which
we should
plan to refer in the paper!) that will have *all* data, and *all* codes (Scott's
MATLAB
codes--clean these up first though Scott) used, and a *thorough* description of
all
methodological details (no matter how small) so that independent scientists
would have
everything they need to reproduce the results. We're not all in the habit of
doing this,
and its now clear that, in certain cases, we need to...
I also have one other suggestion--Scott, you should go through the MBH98 dataset
(refer to

cg2003

the original description to determine where the termination dates were) and make sure that any extensions beyond the last available data point by persistence that I performed originally are removed--we don't need them, since RegEM can handle the missing data in estimating the required covariances anyway! You should also do an experiment where the MBH98 network is only used in calibration through 1971 (the earliest date for which no series have been extended to the 1980 boundary by persistence), since the "PC" predictor series are already based on some data that have been extended, and its not worth the bother to redo these all. Stopping the calibration in 1971 is another way of avoiding the use of any persistence-extended data... In our reply to MM03, I'll be showing that we get a virtually identical result if we only use a 1902-1971, rather than 1902-1980, training period, taking away from MM03 the argument that the extension of some series by persistence to 1980 makes any difference. If we do all of the above for the in-review Rutherford et al J. Climate paper, and have the website up and running (perhaps working w/ Mark Eakin at NGDC to have the webpage located there to, or at least a link to our webpage), we take away a major source of criticism. Thoughts? mike

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3903. 2003-11-05

date: wed, 05 Nov 2003 09:01:40 +0100
from: Keith Alverson <alverson@pages.unibe.ch>
subject: Re: HOLCLIM
to: Keith Briffa <k.briffa@uea.ac.uk>
Hi Keith,

Thanks for cc'ing me this - I had some discussions with Rick recently about this but have not done anything. In my opinion it would be a great benefit both ways for any successful FW6 paleo program to have the PAGES office as a full partner in the IP. I envision a situation where, PAGES would receive ~100k euro per year from the EU (this is half what the US and Switzerland provide), and provide things like dissemination, workshops, editing, publications and publicity as well as links to the broader international community.

However, clearly we cannot play favorites if there are competing proposals. I am unsure how to best proceed and would appreciate any advice you may have. Good luck with the proposal.

PS. Was the medieval warm period warmer than the 1990s or not?

Keith

on 11/04/2003 01:16 PM, Keith Briffa at k.briffa@uea.ac.uk wrote:

> Hi Jean and Dominique

> I am just sending this brief message to let you know , in case it has not
> filtered back to you , that we (in the form of the ESF HOLIVAR SSC)

cg2003

> http://www.esf.org/esf_article.php?language=0&article=99&domain=3&activity=1
> are still moving towards submitting a proposal for an IP , under Framework
> 6 , in the call that we are hoping will still materialize in a relevant
> form , in the anticipated September 2004 Call for Proposals . At present ,
> our understanding is that there is still no definite Palaeoclimate aspect
> included , but some members of the advisory panel are working hard to
> ensure that one is . Rick Battarbee (Chair of the Holivar SSC) , Eystein
> Jansen , Simon Tett and myself are meeting in UCL , London, on the 17th
> November to discuss the development of what we see as the follow up to our
> initial EoI submitted last year (and on which you were both named).
> The concept marries a study of natural and anthropogenic climate change
> using a combination of various palaeoclimate , observed and simulated
> climate data , using statistical and modeling (incorporating a range of
> complexity from Fully-coupled GCMs to simple EB models) approaches to
> combining, interpreting and predicting evidence of climate change. The time
> period of interest will be the Holocene , in keeping with the HOLIVAR
> mandate and representing a major expansion of my own current and similar
> project that is focused only on the period after AD 1500.
> <http://www.cru.uea.ac.uk/projects/soap/>
> We realize this is early days , and that you may be considering a longer
> time frame project yourselves . However, in a spirit of co-operation and
> hopefully collaboration, we wanted to let you know, and ultimately request
> your involvement. Eystein has now , I believe, decided that an IP (not a
> NoE) and a Holocene (not longer) focus is more likely to be supported than
> the original DOCC EoI.
> The involvement of the ice and ocean communities are vital to our proposed
> project. We would welcome your thoughts.
> Very best wishes
> Keith
> (For Rick, Eystein, Simon etc.)
>
> --
> Professor Keith Briffa,
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> University of East Anglia
> Norwich, NR4 7TJ, U.K.
>
> Phone: +44-1603-593909
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>
>
> --

Keith Alverson
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Sulgeneckstr. 38, 3007 Bern, Switzerland

4163. 2003-11-05

date: wed, 05 Nov 2003 09:55:49 +0000
from: Phil Jones <p.jones@uea.ac.uk>
subject: Fwd: Re: changes in the NH annual cycle
to: t.osborn@uea.ac.uk, k.briffa@uea.ac.uk
Date: wed, 05 Nov 2003 09:29:10 +0000

cg2003

To: Drew Shindell <dshindell@giss.nasa.gov>

From: Phil Jones <p.jones@uea.ac.uk>

Subject: Re: changes in the NH annual cycle

Drew,

Apologies for the reference problem. Got the title off one of the reviewers (Mike

Mann) !

Changed it in another article I'm doing with Mike - a review of paleo for Rev. Geophys.

(submitted a few weeks ago). Mike has been spending too much time with that ridiculous

paper - still I think even he has been worn down by everything ! We may do some more on

the E&E paper but let things settle for a while. We won't be auditing it, but trying to

do

something that will also have some useful science in it. Mike may have made one or two

small mistakes, but nothing major. Skeptics seem to think that if you try and shoot one

paper

down then the rest will fall. Several other groups (including us) have got pretty much

the same

result given the uncertainties. I'll forward an email from the editor of E&E - makes

fun reading.

Gavin and others might like to see it. Doesn't seem to grasp the concept of good

science !

Seems to think we get results just because of who funds us - I just hope that this

doesn't

happen in other areas of science. The editor obviously has no idea how to write a

paper,

nor how hard it is to get proposals supported - we do have failed ones !

The European and Chinese regions are relatively small even in NH terms, but I would

reckon that if I were to regress averages for these two areas against NH temps the

result

wouldn't be too bad. So, I think what you propose is eminently reasonable. It is what I

would

support - we suggest that seasonal cycles in models should be tested and we think that

different forcings should be distinguishable by their seasonal signatures.

Do you want me to send you the series that went into Figs 1 and 2 in the paper? I

would

produce three groups (one for China - the N and E areas are a bit of a misnomer,

one is north of the other) and then one for N. Europe and one for C. Europe.

N Europe

could

combine the Fennoscandian series with the Dutch/CET ones, then the other could use the

Italian stations with the Swiss and Czech.

N.China is 35-45N, 110-120E and E. China 30-35N, 115-120E. European sites/countries

are basically what they say they are. You could get 3 European regions if you split

cg2003

the
Dutch/CET series from Fennoscandia. C. Europe should involve the
Swiss/Czech/Italian
data.

Apart from the Dutch series it is also possible to get the other 2 seasons
as well.

Spring
will be OK, but for autumn there are generally few proxies, so this is the
least good
season
for documentary data.

Just reread your letter. Missed the last couple of sentences first time.

I'll send
the data
and some details. Happy to work with you.
Cheers
Phil

At 14:43 04/11/2003 -0500, you wrote:

Hello Phil,

I hope you had a good trip home from Spain, and haven't had to spend too
much time

dealing with the ridiculous Energy & Environment paper (I'm sure Mike's
outraged, given
his temperament!).

I enjoyed your talk in San Feliu, and downloaded your JGR paper when I got
home. I've

read it through, and wanted to run an idea by you. In your Figure 2, you show
six of the

very long-term records, and in the text you point out that "warmer periods are
associated with lower summer/winter differences". I thought it would be
interesting to

see if the model would reproduce such behavior and perhaps compare with the
data you

show. Now I noticed that you specifically mention that given the limited
amount of data,

it's not appropriate to form a composite. Given that China is quite large, the
northern

and eastern sites could be quite far apart, and there are only two sites in
any case. So

I fully agree that for nearly the entire globe there's too little data to
create any

reasonable averages. What I wonder is about using the four European sites in a
comparison with the GCM simulations specifically for Europe (i.e. the

rectangle
including England and the 3 central European sites). This covers enough of the
model

domain that I believe we could get statistically significant results for the
regional

average temperature response to forcing, though I haven't done the calculation
yet.

Basically I would use the simulations described in our new J. Climate paper
for solar

and volcanic forcings. What I did do so far is to create the summer/winter
differences,

and they are of the opposite sign for the response to solar and volcanic
forcing. I have

the change in the annual cycle per change annual average from the model, which
could be

the most useful quantity. Given that the sign is opposite between the two
forcings, this

seem interesting to me, and conceivably we could make at least a qualitative
comparison

with the 4 proxy records and it would still be valuable. Qualitative would

cg2003

probably be
the most appropriate in any case, given that there are only 4 proxy records
and that the
forcing time series, especially for solar, is not well-calibrated. Since the
proxy data
and the simulations are already published, I would envision simply a short
analysis of
the comparison between them. Do you think it'd be reasonable to use only 4
sites to in
some sense represent regional European temperatures, and if so, would you be
interested
in providing the data from those 4 sites and/or in doing some analysis of this
with me?
Best wishes,
Drew
PS The reference to our J. Climate paper in your JGR has an incorrect title
(Little Ice
Age is too vague I feel). The paper is due out in the Dec 15 issue, and the
full
reference is:
Shindell, D. T., G. A. Schmidt, R. L. Miller, and M. E. Mann, Volcanic and
solar forcing
of climate change during the preindustrial era, J. Climate, 16, 4094-4107,
2003.

~~~~~  
Dr. Drew Shindell  
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2880 Broadway  
New York, NY 10025 USA  
Tel/Fax: (212) 678-5561  
email: dshindell@giss.nasa.gov  
[1]http://www.giss.nasa.gov/~dshindel/  
~~~~~

</blockquote></x-html>

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2349. 2003-11-06

cc: Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, Michael
Oppenheimer <omichael@Princeton.EDU>, Tom Wigley <wigley@meeker.ucar.edu>, tom
crowley <tom@ocean.tamu.edu>, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck
<jto@u.arizona.edu>, Steve Schneider <shs@stanford.edu>
date: Thu, 06 Nov 2003 09:30:48 -0700
from: Tom Wigley <wigley@ucar.edu>
subject: Re: Fwd: McIntyre and McKintrick paper
to: "Michael E. Mann" <mann@virginia.edu>
Mike,

cg2003

Yes -- ignore Sonja. Poor woman, she can't even write or spell properly. Mrs Malaprop would be proud of her. Another sad aspect is the strong hint of paranoia in her letter to you. It is laced with implications that scientists are distorting their science, that we are subservient to political agendas, and so on. Nothing new, of course, but she really seems to believe it. I suspect there is a psychology PhD here. I must commend you on the detective work you did to figure out what M&M did wrong. Perhaps the focus of any 'response' could be on elucidating the details of and justifications for your methods, using M&M as an example of how not to do it? In this way the paper would be a direct contribution to the science, with the rebuttal of M&M coming as a byproduct. I have said this before, but this is how Ben Santer, Karl Taylor and I responded to some junk criticism of our detection work by Legates (in GRL). This puts the science first and relegates the criticism to its proper place as not worth making a direct response to. (Hmmm, is that good grammar?)
Tom.

+++++

Michael E. Mann wrote:

Dear all,

Thought you'd all be interested in this email.

Of course, we have no intention to respond to this, or other further emails from the contrarians.

We're working on a full response that will be formally published. We'll let you know the

venue when its confirmed,

mike m

Delivered-To: [1]mem6u@virginia.edu

From: "Sonja.B-C" [2]<Sonja.B-C@hull.ac.uk>

Date: Thu, 6 Nov 2003 14:30:42 +0000

To: "Michael E. Mann" [3]<mann@virginia.edu>

Subject: McIntyre and McKintrick paper

Cc: [4]L.A.Love@hull.ac.uk, Steve McIntyre [5]<smcintyre@cgxenergy.com>,

Ross McKitrick [6]<rmckitri@uoguelph.ca>,

timo hameranta [7]<timo.hameranta@pp.inet.fi>,

Reto Knutti [8]<knutti@climate.unibe.ch>,

"David R. Legates" [9]<legates@udel.edu>,

George Kukla [10]<kukla@ldeo.columbia.edu>,

Hans von Storch [11]<Hans.von.Storch@gkss.de>,

John Christy [12]<christy@atmos.uah.edu>,

"Keith R. Briffa" [13]<k.briffa@uea.ac.uk, Madhav L. Khandekar"

[14]<mkhandekar@rogers.com>,

"Rajendra K. Pachauri" [15]<chairipcc@teri.res.in>,

Ulrich Cubasch [16]<cubasch@zedat.fu-berlin.de>,

"Spencer R. Weart" [17]<sweart@aip.org>, Aynsley Kellow

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Bjorn Lomborg [19]<bjorn@ps.au.dk>, Bob Foster [20]<fosbob@bigpond.com>,

Chris de Freitas [21]<c.defreitas@auckland.ac.nz>,

Christopher Essex [22]<essex@uwo.ca>, "Craig D. Idso"

[23]<cidso@co2science.org>,

Curt Holder [24]<cholder@uccs.edu>, "David E. Wojick"

[25]<dwojick@shentel.net>,

Henrik Svensmark [26]<hsv@dsri.dk>, Hugh W Ellsaesser

[27]<hughel@comcast.net>,

[28]ian.castles@anu.edu.auKirill.Ya.Kondratyev

Priority: NORMAL

X-Mailer: Execmail for Win32 5.1.1 Build (10)

Dear Professor Mann

I have found a list of scientists which contained your email address, hence I am able to communicate with you directly. As you already know, a paper by McIntyre and McKintrick analysing your famous 'Hockey stick' paper is now available to everybody at [29]www.multi-science.co.uk. The

printed version is due later this month. Your, via the attention it received by the IPCC, is currently widely used by social scientists and many researchers in the energy policy community as 'the' proof for anthropogenic dangerous warming. Humanity should now act, it argued, on the basis of fact rather than the rather suspect 'precautionary principle'.

I would respectfully like to explain to you and other scientistst who may feel offended by the publication from outside 'their' domain, why I have published this and other 'attacks' and why I would appreciate a publishable reply from you and your colleagues. You may yet win the argument! Who knows, but an open debate is overdue.

I do not claim that I or my reviewers can arbitrate on the 'scientific' truth of publications that the IPCC selects as most relevant, but your 1998 certainly was selected as such and as far as I know, there was no protest against its use in global policy advocacy. I may be wrong, for I am more in contact with research that is based on worse case scenarios (from IPCC) than with basic climate science research. ENERGY&ENVIRONMENT has paid attention to the 'science' and 'social science' controversies associated with the IPCC for over a decade and has done so not in order to advance (natural) scientific understanding, but with reference to the profound policy relevance of this understanding and hence of any controversy about the nature of climate and the causes of its variability over time, as well as attempts, in some circles, to stifle associated controversies, presumably to make life easier for policy and policy relevant research.

I am fully aware of the policy significance of the debate between 'you, the IPCC and so-called climate skeptics, and its funding implications for so many. But the implications for humanity are even greater. (In fact, most of the papers I have published in recent years have used the IPCC 'consensus' as baseline.)

I have been an energy policy researcher writing and now editing with an international relations/ political science bias; I have a strong research history in environmental politics, and a basic education in physical geography as well as German literature. (Remember acid rain, the death of Europ'es forests in a few deacdes? Or the death of the global ocean from pollution in the 1970s, the subject of my PhD? Environmental threats have long serves many other agendas, and natural scientists may at least be aware of this.)

I have published 'outsiders' whom I trust because I no longer fully trust many 'research products' - not because of any failings because of individual researchers , but because of the nature of much contemporary research funding, see

[30]<http://www.john-daly.com/sonja-bc.htm>. I do know about research funding from bureaucracies - the importance of the right buzzwords, policy visions, legal commitments and political ambitions.

I simply believe that research controversies related to global warming (science, social science, and technology) should be heard by policy-makers and NGOs in a world where vast amounts of limited finance are about to be spend on 'decarbonisation' on the assumption made by most social scientists and many policy people that IPCC summary pronouncements are undisputed and hence are acceptable as uncontroversial baseline for their work on decarbonisation economics, 'clean' technologoly, carbon finance, Kyoto mechanisms etc). I am encouraging research controversy in the public arena rather than editorial boardrooms. For example and to my considerable regret, even the UK Foreign Office and many of my colleagues in the energy policy research (not in the earth sciences by the way) now believe that they need not pay any attention to scientific issues because all climate skeptics are funded by the oil industry. If this slur is permitted to stand, as it seems to be, then journals like mine are surely permitted to ask and who is funding the 'global warming' modelling community if not governments committed to the UNFCCC, and to explore what agendas

cg2003

have attached themselves to the warming threat.
If I have offended against the ethics of natural science publication,
which I am not sure of given cases that have been reported to me, I
apologise and plead ignorance. I forward to hearing from you not via
a web site, but in the form of a paper or view point that I can
published for libraries and readers.

Best wishes

Sonja Boehmer-Christiansen

Dr.Sonja Boehmer-Christiansen
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1663. 2003-11-10

date: Mon, 10 Nov 2003 10:53:36 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Fwd: the campaign against you
to: rbradley@geo.umass.edu, mhughes@lrr.arizona.edu, Phil Jones
<p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, tim Osborn
<t.osborn@uea.ac.uk>, tom crowley <tom@ocean.tamu.edu>, Jonathan Overpeck
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FYI

Date: Mon, 10 Nov 2003 16:08:36 +0100
From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
User-Agent: Mozilla/5.0 (Windows; U; WinNT4.0; en-US; rv:1.4) Gecko/20030624
X-Accept-Language: en-us, en
To: Mike Mann <mann@multiproxy.evsc.virginia.edu>
Subject: the campaign against you

Dear Mike,

it almost looks like there has been an orchestrated campaign of op-eds coming
out world-wide on the M&M paper within days, in New Zealand, Oz, and so on, all
with a similar structure and content and written by local climate skeptic academics,
and there are at least rumours that this may have been organised by the PR firm
Burson-Marsteller who specialises in covert anti-environmental campaigns for industry clients.
This will be very difficult to prove, of course. Some inside source suggests there is
more of this to come. A similar campaign was focussed on Ben Santer some years ago.
I think it is worth keeping this possibility in mind, even if one can do very
little about it. I hope some good investigative journalists will be on to this topic.
Stefan

--

cg2003

Stefan Rahmstorf
Potsdam Institute for Climate Impact Research (PIK)
For contact details, reprints, movies & general infos see:
[1]<http://www.pik-potsdam.de/~stefan>

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3815. 2003-11-10

date: Mon, 10 Nov 2003 15:23:04 +0000
from: Asher Minns <A.Minns@uea.ac.uk>
subject: Horizon
to: m.hulme@uea.ac.uk

Mike,

I have had this reply from Mark Maslin at UCL - he was one of the consultants on an earlier Horizon series - see below. As an exercise, I have drafted a letter for the Director/Producer, perhaps BBC Wildlife or the Radio Times, which is from me personally - also below. Of course, I will wait to see the programme first. Any comments?

Thanks for effect feedback. It is the publication to date where I have had most responses - which at least means that people are interested enough to respond.

I have some COP9 from Alex, but Neil said that he has none. I'll try MJ at FIELD

Asher

1) Dear Asher

I think (and hope) that the BBC are just recycling the title. As I know that Jochem Marotzke (Southampton), Peter Cox and Adrian Lister (UCL) and others have put alot of time in trying to give the BBC a clear view of the current science and how we got to it. My own meetings, however, suggest they will be going down the Deep Ocean Circulation in the North Atlantic will fail and be bad for Europe/USA. Not sure how much wider the program will be than that despite my efforts to suggest a wide view including the possible shut down of AABW. They also seemed to be very keen on discussing and filming the history of the Palaeoclimates which led from the 1970's to the realisation that the deep ocean could changed and rapidly.

So simple answer is it should not be a repeat of the 1999 Big Chill program ... otherwise I will want my licence fee back! But I do not know how many of the interviews/films they will re-use.

all the best

Mark

2) The science of climate change, and BBC Horizon

BBC2 broadcast The Big Chill on Thursday 13 November as part of its series of Horizon programmes, suggesting that Europe and the US will be plunged into a mini ice-age through global warming. There are fundamental differences between the chilling certainty of Horizon's claims for the next 20 years, and the global warnings of the UK's climate change research experts.

The UK Government and academic community is unique in the world in having a state-of-the-art understanding of climate change predictions for the nation, published first in 1998 and re-researched last year. The UK's climate predictions state that the collapse of the Gulf Stream is unlikely to lead to a cooling of the UK climate within the next 100 years. Apart from a general concern for the misrepresentation of science by mainstream media, inaccurate science communication gives mixed messages to the wider public about what scientists understand about climate change, and the choices that we have in responding and adapting to the impacts of climate change. If Horizon is to advertise itself as a science documentary, then it has to maintain the respect

cg2003

and support of scientists and public alike.
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Tyndall Centre for Climate Change Research HQ
University of East Anglia
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Tel: 07880 547843 / 01603 593906

4580. 2003-11-10

cc: "Michael E. Mann" <mann@virginia.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, <rbradley@geo.umass.edu>, <mhughes@lrr.arizona.edu>, Tom Wigley <wigley@meeke.UCAR.EDU>, tom crowley <tom@ocean.tamu.edu>, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>
date: Mon, 10 Nov 2003 23:24:15 -0800 (PST)
from: Stephen H Schneider <shs@stanford.edu>
subject: Re: Fwd: McIntyre and McKintrick paper
to: Michael Oppenheimer <omichael@Princeton.EDU>
The "postmodernist" Sonja, is anything but naive. I wrote about her earlier--rejects peer review as elitist and anti-democratic etc, and ironically for a left-wing type has linked up with the right wing contrarians--not dull at least, just infuriating and disingenuous. If anyone wants more, let me know. Cheers, Steve
On Thu, 6 Nov 2003, Michael Oppenheimer wrote:
> Mike:
>
> Bizarre, and either incredibly naive or incredibly disingenuous.
>
> Michael
>
> "Michael E. Mann" wrote:
>
> > Dear all,
> >
> > Thought you'd all be interested in this email.
> >
> > Of course, we have no intention to respond to this, or other further
> > emails from the contrarians.
> >
> > We're working on a full response that will be formally published.
> > We'll let you know the venue when its confirmed,
> >
> > mike m
> >
> >
> >> Delivered-To: mem6u@virginia.edu
> >> From: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>
> >> Date: Thu, 6 Nov 2003 14:30:42 +0000
> >> To: "Michael E. Mann" <mann@virginia.edu>
> >> Subject: McIntyre and McKintrick paper
> >> Cc: L.A.Love@hull.ac.uk, Steve McIntyre <smcintyre@cgxenergy.com>,
> >> Ross Mckitrick <rmckitri@uoguelph.ca>,
> >> timo hameranta <timo.hameranta@pp.inet.fi>,
> >> Reto Knutti <knutti@climate.unibe.ch>,
> >> "David R. Legates" <legates@udel.edu>,
> >> George Kukla <kukla@ldeo.columbia.edu>,
> >> Hans von Storch <Hans.von.Storch@gkss.de>,
> >> John Christy <christy@atmos.uah.edu>,
> >> "Keith R. Briffa <k.briffa@uea.ac.uk>, Madhav L. Khandekar"
> >> <mkhandekar@rogers.com>,
> >> "Rajendra K. Pachauri" <chairipcc@teri.res.in>,

cg2003

```

> > Ulrich Cubasch <cubasch@zedat.fu-berlin.de>,
> > "Spencer R. Weart" <sweart@aip.org>, Aynsley Kellow
> > <akellow@utas.edu.au>,
> > Bjorn Lomborg <bjorn@ps.au.dk>, Bob Foster <fosbob@bigpond.com>,
> > Chris de Freitas <c.defreitas@auckland.ac.nz>,
> > Christopher Essex <essex@uwo.ca>, "Craig D. Idso"
> > <cidso@co2science.org>,
> > Curt Holder <cholder@uccs.edu>, "David E. Wojick"
> > <dwojick@shentel.net>,
> > Henrik Svensmark <hsv@dsri.dk>, Hugh W Ellsaesser
> > <hughel@comcast.net>,
> > ian.castles@anu.edu.auKirill.Ya.Kondratyev
> > Priority: NORMAL
> > X-Mailer: Execmail for Win32 5.1.1 Build (10)
> >
> > Dear Professor Mann
> >
> > I have found a list of scientists which contained you email address,
> >
> > hence I am able to communicate with you directly. As you already
> > know, a
> > paper by McIntyre and McKintrick analysing your famous 'Hockey
> > stick'
> > paper is now available to everybody at www.multi-science.co.uk. The
> > printed version is due later this month. Your, via the attention it
> > received by the IPCC, is currently widely used by social scientists
> >
> > and many researchers in the energy policy community as 'the' proof
> > for
> > anthropogenic dangerous warming. Humanity should now act, it argued,
> > on
> > the basis of fact rather than the rather suspect 'precautionary
> > principle'.
> > I would respectfully like to explain to you and other scientistst
> > who
> > may feel offended by the publication from outside 'their' domain,
> > why
> > I have published this and other 'attacks' and why I would appreciate
> > a
> > publishable reply from you and your colleagues. You may yet win the
> > argument! Who knows, but an open debate is overdue.
> >
> > I do not claim that I or my reviewers can arbitrate on the
> > 'scientific'
> > truth of publications that the IPCC selects as most relevant, but
> > your 1998 certainly was selected as such and as far as I know, there
> >
> > was no protest against its use in global policy advocacy. I may be
> > wrong, for I am more in contact with research that is based on worse
> >
> > case scenarios (from IPCC) than with basic climate science research.
> >
> > ENERGY&ENVIRONMENT has paid attention to the 'science' and 'social
> > science' controversies associated with the IPCC for over a decade
> > and
> > has done so not in order to advance (natural) scientific
> > understanding,
> > but with reference to the profound policy relevance of this
> > understanding and hence of any controversy about the nature of
> > climate
> > and the causes of its variability over time, as well as attempts, in
> >
> > some circles, to stifle associated controversies, presumably to make

```

> >> life easier for policy and policy relevant research.

> >> I am fully aware of the policy significance of the debate between

> >> 'you,

> >> the IPCC and so-called climate skeptics, and its funding

> >> implications

> >> for so many. But the implications for humanity are even greater. (

> >> In

> >> fact, most of the papers I have published in recent years have used

> >> the IPCC 'consensus' as baseline.)

> >> I have been an energy policy researcher writing and now editing with

> >> an

> >> international relations/ political science bias; I have a strong

> >> research history in environmental politics, and a basic education in

> >> physical geography as well as German literature. (Remember acid

> >> rain,

> >> the death of Europ'es forests in a few deacdes? Or the death of the

> >> global ocean from pollution in the 1970s, the subject of my PhD?

> >> Environmental threats have long serves many other agendas, and

> >> natural

> >> scientists may at least be aware of this.)

> >> I have published 'outsiders' whom I trust because I no longer fully

> >> trust many 'research products' - not because of any failings

> >> because

> >> of individual researchers , but because of the nature of much

> >> contemporary research funding, see

> >> <http://www.john-daly.com/sonja-bc.htm>. I do know about

> >> research funding from bureaucracies - the importance of the right

> >> buzzwords, policy visions, legal commitments and political

> >> ambitions.

> >> I simply believe that research controversies related to global

> >> warming

> >> (science, social science, and technology) should be heard by

> >> policy-makers and NGOs in a world were vast amounts of limited

> >> finance

> >> are about to be spend on 'decarbonisation' on the assumption made

> >> by

> >> most social scientists and many policy people that IPCC summary

> >> pronouncements are undisputed and hence are acceptable as

> >> uncontroversial baseline for their work on decarbonisation

> >> economics,

> >> 'clean' technologoly, carbon finance, Kyoto mechanisms etc). I am

> >> encouraging research controversy in the public arena rather than

> >> editorial boardrooms. For example and to my considerable regret,

> >> even

> >> the UK Foreign Office and many of my colleaugues in the energy

> >> policy

> >> research (not in the earth sciences by the way) now believe that

> >> they

> >> need not pay any attention to scientific issues because all climate

> >> skeptics are funded by the oil industry. If this slur is permitted

> >> to

> >> stand, as it seems to be, then journals like mine are surely

> >> permitted

> >> to ask and who is funding the 'global warming' modelling community

> >> if

> >> not governments committed to the UNFCCC, and to explore what agendas

> >>

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> > have attached themselves to the warming threat.
> >
> > If I have offended against the ethics of natural science
> > publication,
> > which I am not sure of given cases that have been reported to me, I
> >
> > apologise and plead ignorance. I forward to hearing from you not
> > via
> > a web site, but in the form of a paper or view point that I can
> > published for libraries and readers.

> >
> > Best wishes
> > Sonja Boehmer-Christiansen
> > -----
> > Dr.Sonja Boehmer-Christiansen
> > Reader,Department of Geography,
> > Editor, Energy & Environment
> > (Multi-science,www.multi-science.co.uk)
> > Faculty of Science
> > University of Hull
> > Hull HU6 7RX, UK
> > Tel: (0)1482 465349/6341/5385
> > Fax: (0)1482 466340
> > Sonja.B-C@hull.ac.uk

> >
> > _____
> > Professor Michael E. Mann
> > Department of Environmental Sciences, Clark Hall
> > University of Virginia
> > Charlottesville, VA 22903
> > _____
> >
> > e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434)
> > 982-2137
> > <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
> >

Stephen H. Schneider, Professor
Dept. of Biological Sciences
Stanford University
Stanford, CA 94305-5020 U.S.A.
Tel: (650)725-9978
Fax: (650)725-4387
shs@stanford.edu

5072. 2003-11-10

cc: simon.shackley@umist.ac.uk
date: Mon Nov 10 11:54:59 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: outline of Tyndall North's supergen work
to: Paul Upham <p.upham@umist.ac.uk>

Thanks Paul. That is very helpful as an outline sketch. You may be aware, but there is a major campaign running here in the eastern region over bio-energy (biofuels in fact), which is bringing together partnerships of MPs, producers, the transport industry and scientists. There is a strong lobby of the Treasury to reduce still further the tax differential. The regional newspaper in the eastern region is supporting this campaign aggressively and hence the public are being exposed to some of the issues. Bruce Tofield here at UEA (the CRed project) is following this regional

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debate/lobby quite

closely and at some point you might wish to make contact with him if you feel the eastern

region is suitable as a case study. Elaine Jones, Tyndall's business liaison manager, is

someone else to keep in contact with.

I hope to meet you on friday when John Schellnhuber and I visit.

Mike

At 13:42 06/11/2003 +0000, Paul Upham wrote:

Paul Upham

Research Associate

Tyndall Centre (North)

School of Mechanical Engineering

UMIST, PO Box 88

Manchester

M60 1QD

Phone: +44 (0)161 200 3700/8710

Fax: +44 (0)161 200 3723

[1]<http://www.tyndall.ac.uk/welcome.html>

[2]<http://personalpages.umist.ac.uk/staff/P.Upham/default.htm>

421. 2003-11-12

cc: t.osborn@uea.ac.uk, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, Phil

Jones <p.jones@uea.ac.uk>, keith Briffa <k.briffa@uea.ac.uk>

date: Wed, 12 Nov 2003 00:01:46 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Fwd: MBH98

to: Scott Rutherford <srutherford@rwu.edu>

Scott,

Take a look at this. You need to explain to us (don't email this guy anything!) the

various versions of the data. I'm really confused, and we need to know the precise history

of when the individual MBH98 records were posted, and when the various matlab format files

were posted, and in response to what requests, and these latest changes that were made on

Oct 29, 2003??

Obviously, we don't need to provide these guys with *anything* and we needn't respond to

any of their emails--the raw data are available on the ftp sites, and have been for some

time. But we really now need to know exactly when the data were made available. They claim

that the matrix versions of the data files were posted on the ftp site before their request

for the data. I'm really confused by this.

You need to draft a clear explanation of all of this, so we can provide this to people. Can

you draft an explanation of what was posted when for our internal purposes, and then we can

decide what information to send on...

thanks,

mike

Delivered-To: mem6u@virginia.edu

From: "Steve McIntyre" <smcintyre@cgxenergy.com>

To: "Michael E. Mann" <mann@virginia.edu>

Cc: "Tim Osborn" <t.osborn@uea.ac.uk>, "Ross McKittrick" <rmckittri@uoguelph.ca>

Subject: MBH98

Date: Tue, 11 Nov 2003 23:39:46 -0500

X-Mailer: Microsoft Outlook Express 6.00.2800.1158

X-Authentication-Info: Submitted using SMTP AUTH LOGIN at

cg2003

fep02-mail.bloor.is.net.cable.rogers.com from [65.49.25.138] using ID
<nmcintyre77@rogers.com> at Tue, 11 Nov 2003 23:39:06 -0500
<?xml:namespace prefix = o ns = "urn:schemas-microsoft-com:office:office" />
November 11, 2003

Professor Michael E. Mann
School of Earth Sciences
University of Virginia

Dear Professor Mann,

We apologize for not sending you a copy of our recent paper (MM) in Energy and Environment for comment, as we understood from your email of September 25,

2003 that

time constraints prevented you from considering our material. We notice that you seem to

have subsequently changed your mind and hope that you will both be able to clarify some

points for us and to rectify the public record on other points.

1) You have claimed that we used the wrong data and the wrong computational methodology.

We would like to reconcile our results to actual data and methodology used in MBH98. We

would therefore appreciate copies of the computer programs you actually used to read in

data (the 159 data series referred to in your recent comments) and construct the

temperature index shown in Nature (1998) (MBH98), either through email or, preferably

through public FTP or web posting.

2) In some recent comments, you are reported as stating that we requested an Excel file

and that you instead directed us to an FTP site for the MBH98 data. You are also

reported as saying that despite having pointed us to the FTP site, you and your

colleague took trouble to prepare an Excel spreadsheet, but inadvertently introduced

some collation errors at that time. In fact, as you no doubt recall, we did not request

an Excel spreadsheet, but specifically asked for an FTP location, which you were unable

or unwilling to provide. Nor was an Excel spreadsheet ever supplied to us; instead we

were given a text file, pcproxy.txt. Nor was this file created in April 2003. After we

learned on October 29, 2003 that the pertinent data was reported to be located on your

FTP site [1]ftp://holocene.evsc.virginia.edu/pub (and that we were being faulted for not

getting it from there), we examined this site and found it contains the exact same file

(pcproxy.txt) as the one we received, bearing a date of creation of August 8, 2002. On

October 29, 2003, your FTP site also contained the file pcproxy.mat, a Matlab file, the

header to which read: MATLAB 5.0 MAT-file, Platform: SOL2, Created on: Thu Aug 8

10:18:19 2002. Both files contain identical data to the file pcproxy.txt emailed to one

of us (McIntyre) in April 2003, including all collation errors, fills and other problems

identified in MM. It is therefore clear that the file pcproxy.txt as sent to us was not

prepared in April 2003 in response to our requests, nor was it prepared as an Excel

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spreadsheet, but in fact it was prepared many months earlier with Matlab. It is also clear that, had we gone to your FTP site earlier, we would simply have found the same data collation as we received from Scott Rutherford. Would you please

forthwith issue a statement withdrawing and correcting your earlier comments.

3) In reported comments, you also claimed that we overlooked the collation errors in

pcproxy.txt and slid the incorrect data into our calculations, a statement which is

untrue and made without a reasonable basis. In MM, we described numerous errors

including, but not limited to, the collation errors, indicating quite obviously that we

noticed the data problems. We then describe how we firewalled our data from the errors

contained in the data you provided us, by re-collating tree ring proxy data from

original sources and carrying out fresh principal component calculations. We request

that you forthwith withdraw the claim that we deliberately used data we knew to be in

error.

4) On November 8, 2003, when we re-visited your FTP site, we noticed the following

changes since October 29, 2003: (1) the file pcproxy.mat had been deleted from your FTP

site; (2) the file pcproxy.txt no longer was displayed under the /sdr directory, where

it had previously been located, although it could still be retrieved through an exact

call if one previously knew the exact file name; (3) without any notice, a new file

named mbhfilled.mat prepared on November 4, 2003 had been inserted into the directory.

Obviously, the files pcproxy.mat and pcproxy.txt are pertinent to the comments referred

to above and we view the deletion of pcproxy.mat from the archival record under the

current circumstances as unjustifiable. Would you please restore these files to your FTP

site, together with an annotated text file documenting the dates of their deletion and

restoration.

5) We note that the new file mbhfilled.mat is an array of dimension 381x2016. Could you

state whether this file has any connection to MBH98, and, if so, please explain the

purpose of this file, why it has been posted now and why it was not previously available

at the FTP site.

6) Can you advise us whether the directory MBH98 has been a subdirectory within the

folder pub since July 30, 2002 or whether it was transferred from another (possibly

private) directory at a date after July 30, 2002? If the latter, could you advise on the

date of such transfer.

We have prepared a 3-part response to your reply to MM. The first, which we have

released publicly, goes over some of the matters raised in points #2-#5 above. The

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second is undergoing review. It deals with additional issues of data quality and disclosure, resulting from inspection of your FTP site since October 29, 2003. The third part will consider the points made in your response, both in terms of data and methodology, and will attempt a careful reconciliation of our calculation methods, hence the necessity of our request in point #1. Thank you for your attention.
Yours truly,
Stephen McIntyre
cc: Timothy Osborn

Ross Mckitrick

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1070. 2003-11-12

cc: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>, "Ross Mckitrick" <rmckitri@uoguelph.ca>, "Keith Briffa" <k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>
date: Wed, 12 Nov 2003 10:39:50 +0000
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: MM Reply to MBH Response
to: "Steve McIntyre" <smcintyre@cgxenergy.com>
<x-flowed>

Dear Stephen McIntyre,
thank you for your email, copied below for the benefit of my colleagues. Your perception of our involvement in this aspect of climate science is indeed correct and I am pleased that you seem to feel our contribution can be seen as independent and useful in moving the debate and science forwards. I will consult with Keith Briffa and Phil Jones before responding to your request. Phil is away in Germany until Friday, and then I am away on Friday. If Phil has access to email, then we may be able to respond this week. If not, then we will respond next week. In that case, even if we decide to examine part 2 of your response, it would be unlikely that we would do so by 19th November.
In the meantime, for your information, I copy below a reply made to Bob Ferguson in relation to a similar (though not identical request). It will give you an idea about my views on this process.
Regards
Tim

>Date: Wed, 05 Nov 2003 09:27:45 +0000
>To: Bob Ferguson <bferguson@ff.org>
>From: Tim Osborn <t.osborn@uea.ac.uk>
>Subject: Re: M&M response
>Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>
>
>At 15:38 03/11/2003, you wrote:
>>Dear Tim,
>>
>> McIntyre and Mckitrick are preparing a response to Mann et al.
>>
>>Can we expect you to post it on your web site also? In the spirit of
>>fairness you asked of us?
>>
>>Cordially,
>>Bob Ferguson

cg2003

>
>Dear Bob,
>
>The answer to your question is "possibly".
>
>I have urged Mann et al. to develop a more definitive response to the
>McIntyre and Mckitrick paper, one that is published in the peer-reviewed
>literature (thus representing a more long-lasting contribution that can be
>cited when necessary). In doing this, I have also suggested that they
>communicate with McIntyre and Mckitrick to ensure, as much as possible,
>that their response is based on what McIntyre and Mckitrick actually did,
>rather than based on what Mann et al. surmise they did based on a reading
>of their paper and supplementary information.
>
>My preference is to await the outcome of this process rather than posting
>any more interim documents.
>
>You might ask, then, why we posted the Mann et al. interim response. Had
>Mann et al. been involved in the reviewing stage of the McIntyre and
>Mckitrick paper then we probably wouldn't have got involved in this issue
>at all. But Mann et al. told us they had not been given the opportunity
>to review the paper and they demonstrated that it was possible (even
>likely?) that some errors (as opposed to equally valid alternative
>choices) might explain the different results. It seemed appropriate to
>quickly get this possibility into the public domain, to avoid wrong
>conclusions being drawn in what is a policy-relevant area.
>
>The exact content of the McIntyre and Mckitrick response could, of course,
>influence our decision on posting interim documents, which is why I
>answered "possibly" rather than "no" to your original question.
>
>Regards
>
>Tim

At 04:52 12/11/2003, Steve McIntyre wrote:
>November 11, 2003

>
> <?xml:namespace prefix = o ns = "urn:schemas-microsoft-com:office:office" />
>
>Dear Dr. Osborn
>
>
>
>Based on your correspondence with Sonja Boehmer-Christiansen, we perceive
>an interest on your part to pursue the issues raised in our paper in a
>professional way, and a willingness on the part of you and your associates
>to try to enhance the quality and tone of the public discussion of these
>issues.
>
>
>
>We have developed a 3-part reply to public comments made by Professor Mann
>to reporters and journalists and to the response document from Professors
>Mann, Bradley and Hughes posted on your web site (which we will call
>MBH-r). The first part concerns the identity and use of some key files and
>was released for public information today. This part does not involve
>climate issues, but file usage, and, accordingly, we did not feel that we
>should impose on you in this part of our reply.
>
>
>
>The second part is a detailed examination of the contents of the

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>newly-identified FTP site, which Professor Mann says was the data archive
>for MBH98. The third will present a reconciliation of key indicators and
>computational methods between MM and MBH-r and, once again, carry out a
>re-calculation of the temperature index.

>

>

>

>We write with two requests, which can be severed if you wish.

>

>

>

>First, would you please forward the accompanying letter from us to
>Professor Mann, keeping a copy for yourself. This letter asks him, among
>other things, to release the computer programs used in construction of the
>temperature index in MBH98, to identify the "159 series" now identified as
>being used in MBH98 and correct some public statements made recently on
>our use of files. In its own right, CRU/UEA might well have an interest in
>the disclosure of these 159 series, as this number now introduced in MBH-r
>is a different number than used in MBH98 and we are unaware of any
>previous public information on this topic. The quality of the proposed
>re-calculation and related debate would obviously be much enhanced by
>disclosure by Professor Mann of his exact methodology, which, in our view,
>is long overdue in any event.

>

>

>

>Second, we would like you, Keith Briffa and/or Phil Jones to examine Part
>2 prior to its release. If you are willing to do so, we propose the
>following terms

>

>

> * The document is only concerned with published data and there is no
> need to obtain private information from Professor Mann in order to check
> the claims we make. Consequently we would require you to treat the
> document as confidential.

> * Since we have been asked many times when our response will be
> available, if you are willing to examine it, while it is in your
> possession, we will post the statement that this part of our reply is
> being critically examined at the Climate Research Unit, University of
> East Anglia.

> * If you identify any flaws in our document, we will rectify them, and
> you are at liberty to hold us to public account if we fail to do so.

> * If you find our document raises valid and meritorious concerns, you
> will give us a short statement to that effect which we are entitled to
> publish.

> * We will have your comments by November 19th.

>

>

>

>In principle, we would be prepared to make a similar arrangement on Part
>3, following completion of Part 2.

>

>

>

>If you wish to amend these terms please revert to us as quickly as
>possible. If they are acceptable, please advise and we will send you the
>document immediately. We appreciate your consideration of this arrangement
>and hope that it will contribute to avoiding unnecessary conflict and
>highlighting important issues.

>

>

>

>Sincerely

cg2003

>
>
>
>
>
>Stephen McIntyre
>
>
>
>Ross McKittrick
Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK
e-mail: t.osborn@uea.ac.uk
phone: +44 1603 592089
fax: +44 1603 507784
web: http://www.cru.uea.ac.uk/~timo/
sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
</x-flowed>

1235. 2003-11-12

date: wed, 12 Nov 2003 08:21:45 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: FW: Invite to Roundtable "IPCC, 'Hockey Stick' Curve, & Illusion
to: Michael Oppenheimer <omichael@Princeton.EDU>, "Michael E. Mann"
<mann@virginia.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones
<p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, <rbradley@geo.umass.edu>,
<mhughes@ltrr.arizona.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, tom crowley
<tom@ocean.tamu.edu>, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck
<jto@u.arizona.edu>

FYI,

For those who haven't seen this. This is the kind of thing these folks are up
to...

mike

Delivered-To: mem6u@virginia.edu
Subject: FW: Invite to Roundtable "IPCC, 'Hockey Stick' Curve, & Illusion of
Experience"
Date: Mon, 10 Nov 2003 11:33:57 -0500
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: Invite to Roundtable "IPCC, 'Hockey Stick' Curve, & Illusion of
Experience"
Thread-Index: AconmUvtd9mn3CPDRq+3z16kZPYw4QAAJ+kwAAnnV6A=
From: "Loschnigg, Johannes (Govt Affairs)"
<Johannes_Loschnigg@govt-aff.senate.gov>
To: "Michael E. Mann" <mann@virginia.edu>
X-OriginalArrivalTime: 10 Nov 2003 16:34:09.0178 (UTC)
FILETIME=[77253FA0:01C3A7A8]
"urn:schemas-microsoft-com:office:smanttgs">

Mike -

Looks like I'll have the chance to grill these guys on the hill (see below). I
was going

to use your 3-page overview (03nov03.pdf) as ammunition. Anything else I
should be armed
with?

Johannes

-----Original Message-----

From: George C. Marshall Institute [[1]mailto:info@marshall.org]

Sent: Monday, November 10, 2003 9:45 AM

To: George C. Marshall Institute

Subject: Invite to Roundtable "IPCC, 'Hockey Stick' Curve, & Illusion of

cg2003

Experience"

George C. Marshall Institute
Join us for a discussion
The IPCC, the Hockey StickCurve, and the Illusion of Experience:
Reevaluation of Data Raises Significant Questions
With

Stephen McIntyre and Ross Mckitrick
Tuesday, November 18, 2003 12:00 Noon
Longworth House Office Building - Room 1324
Independence Avenue and South Capitol Street, SE
Lunch provided.

The Intergovernmental Panel on Climate Changes (IPCC) Third Assessment Report concluded that it is likely that the rate and duration of the warming of the 20th century is larger than any other time during the last 1,000 years. The 1990s are likely to have been the warmest decade of the millennium in the Northern Hemisphere, and 1998 is likely to have been the warmest year. The primary basis for this assertion was a climate reconstruction that produced the so-called hockey stickshaped graph, which shows that the 20th century was unusually warm compared to preceding centuries. A new evaluation of the underlying data used to create that graph by Canadian businessman Stephen McIntyre and economist Ross Mckitrick raises serious questions as to its validity. McIntyre and Mckitrick examined the construction and use of the data set of proxies for past climate, which were used to estimate the temperature record from 1400 to 1980. Their review found four categories of error: collation errors, unjustified truncation and extrapolation, use of obsolete data, and calculation mistakes. Correcting for these errors, they found that temperature for the early 15th century was actually higher than the 20th century. The McIntyre-Mckitrick findings challenge one of the most influential aspects of the climate change debate. The hockey stickgraph has been accepted as fact by the international community and many domestic interests pushing the Kyoto Protocol and McCain-Lieberman.

Reservations Required
Please RSVP to [2]info@marshall.org
George C. Marshall Institute
1625 K Street, NW, Suite 1050
Washington, DC 20006

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1741. 2003-11-12

date: wed, 12 Nov 2003 08:06:54 -0500

Page 934

cg2003

from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: Fwd: Re: clarification re Mann / Mckitrick andMcIntyre <fwd>
to: Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@Princeton.EDU>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, <rbradley@geo.umass.edu>, <mhughes@ltrr.arizona.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, tom crowley <tom@ocean.tamu.edu>, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>, mann@virginia.edu

Dear All,

We have an official response to be submitted shortly for peer-review. We will send the

response to all of you for your comments, whether or not you get it for review. We hope to

have it finalized within a week or so, depending on Ray's ability to read and comment while

travelling. This will provide more of the details behind our "initial" response...

It is best to let things play out this way. These folks appear to have some very large

industry groups behind them running the show, setting up forums for them on capitol hill

(the latest sponsored by the infamous "Marshall Institute") and its best for scientists not

to exchange any emails with them--they will only quote you out of context and misrepresent

your comments.

Please feel free to contact me to discuss further. So I strongly advise against any

scientists communicating with these people. Understand that anything you send to them, you

are giving to a highly organized industry PR firm that is behind this effort. An investigative reporter in the media may be revealing the dubious details behind

this in an article in the near future.

Please feel free to contact me to discuss further,

mike

At 12:53 PM 11/12/2003 +0000, Tim Osborn wrote:

Dear all,

I'm forwarding this because I'm not sure which of you received it.

I'm also not sure which of you would want to have received it - apologies if you've had

enough of this sort of thing, you can probably predict most of the contents and it is

rather long!

One thing I will add which may be of more interest...

McIntyre has emailed me asking whether (under certain terms and conditions!) we (Keith,

Phil and I) would "examine" (review?) part of their response to the Mann et al.

preliminary response.

I haven't yet discussed this with Keith and Phil, who are away, but there a some clear

reasons to decline their request, so I think it unlikely that we will say yes.

Regards

Tim

From: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>

Date: Tue, 11 Nov 2003 14:18:11 +0000

To: gavin_watson@hotmail.com, Aynsley Kellow <akellow@utas.edu.au>

Subject: Fwd: Re: clarification re Mann / Mckitrick andMcIntyre <fwd>

Cc: gsmith@socsci.soton.ac.uk, climatesceptics@yahooogroups.com,

Tim Osborn <t.osborn@uea.ac.uk>, Bob Ferguson <bferguson@ff.org>

Priority: NORMAL

X-Mailer: Execmail for Win32 5.1.1 Build (10)

Dear All,

cg2003

The letter below makes good reading and its author (Aynsley) is thanked! It is a welcome 'defence' of E&E in the current furore over another paper (by McIntyre and McKittrick 'Corrections to Mann et al..' about to appear in print but already on the multi-science web page) the IPCC community does not like because it comes from outsiders and challenges the consensus of the 'climate science community'. (Also see 'The Economist' this week, which takes up the Castles and Henderson paper from earlier this year [14 2/3] and suggests that Treasuries may at least be taking an interests in the IPCC). I am sending this beyond the original people involved because Prof. Mann has allowed much of this particular 'hocky stick' debate to appear on a web site run by a journalist - see below - and also because my UK colleagues in political science and International Relations have, as far as I know, completely ignored the book by Aynsley and me. One UK political scientist a few years and who should have known better because he had studied the attempts of the coal industry to discredit the IPCC (this failed), dismissed my work as conspiracy theory. One Australian/ WMO meteorologist did the same more recently when reviewing the book by Aynsley and me (International Environmental Policy: Interests and the Failure of the Kyoto Process, Edward Elgar Publishing, November 2002). He may be forgiven for he had an interest to defend.

We do not put forward conspiracy theories, but talk about a flexible coalition of advocacy based on interests, ideology and some science still plagued by much uncertainty; lots of baptists are forming 'partnerships' with boot-leggers, the research enterprise being just one of (often reluctant) many partners in the 'decarbonisation' by subsidisation (and vice versa) game.

Sonja

--- Begin Forwarded Message ---

Date: Tue, 11 Nov 2003 16:15:57 +1100

From: Aynsley Kellow <akellow@utas.edu.au>

Subject: Fwd: Re: clarification

Sender: Aynsley Kellow <akellow@utas.edu.au>

To: rbradley@geo.umass.edu

Cc: Sonja.B-C@hull.ac.uk, "Michael E. Mann" <mann@virginia.edu>,

, "David R. Legates" <legates@udel.edu>, Eigil

Friis-Christensen <efc@dsri.dk>, rmckittri@uoguelph.ca

Reply-To: Aynsley Kellow <akellow@utas.edu.au>

Message-ID: <v04210104bbd5e77b94d5@[131.217.125.10]>

Dear Professor Bradley,

I have been meaning to respond to your message to Sonja Boehmer-Christiansen, but many other duties have conspired to deny me the time to do so. I think it is important that I do so, particularly because of the nature of the extraordinary attack on her for daring to publish the M&M paper in E&E.

I should declare that I recently co-authored a book with Sonja, and recently accepted an invitation to join the Editorial Board of E&E, having previously published two papers with it. I speak, therefore, with some experience of both Sonja and the journal. The journal can stand by its own reputation - by the quality of its multidisciplinary content (which is always likely to provoke occasional controversy), but I am disturbed by the attacks on Sonja, which have been personal and included derogatory comments.

Sonja has an excellent track record of publication in science politics and policy, including both research monographs and articles in leading journals, including Nature, Energy Policy, Environmental Politics, and Global Environmental Change. She is perhaps unequalled in her understanding of the issues involved and is widely cited by those on all sides of the climate change issue. The attack on her character is regrettable, all the more so because it has been conducted under protection of anonymity, thanks largely to the manner in which Dr Mann first engaged the M&M paper.

cg2003

For reasons best known to him, Dr Mann responded to this paper first on David Appell's blog 'Quark Soup' - an unfortunate choice, I must say. (Dr Appell reported Dr Mann's initial response at 8.02 am on 29 October - two days before the first draft of your collective initial response was posted on the East Anglia site). I was not previously familiar with this blog - there is an awful lot of junk in cyberspace and it is hard to track it all. Dr Appell professes to be a journalist, but his blog lies squarely in the realm of commentary, and provides a forum for anonymous gratuitous comment of the kind that no quality newspaper allows. It is a practice permitted by the tabloid press, perhaps fittingly, because that is the quality of journal which might reprint Sonja's e-mail to Dr Mann - deliberately circulated widely - and trumpet that it had obtained a copy of a 'leaked e-mail'.

To further illustrate my point about quality, Dr Appell also slurs Theodor Landscheidt under the heading 'E&E publishes an astrologer!', when Landscheidt's book is quite clearly an evidenced-based critique of astrology. He also describes the critique of SRES published by Ian Castles and David Henderson as 'a third specious paper published recently by Energy and Environment'. For the record, Castles is a former Government Statistician and Head of the Finance Department in the Australian Government, and (until recently) was Vice-President of the Academy of Social Sciences in Australia. David Henderson was formerly Head of the Economics Department at the OECD. Prior to drawing attention to problems with SRES, Castles did much the same with the misuse of statistics in the UNDP World Development Report, a matter which was referred to the UN Statistical Commission, which upheld his critique.

Dr Appell seems to have his loyal retinue of readers, though I see that few other than a couple of characters called 'Uncle E' and 'Dano' bother to contribute their anonymous patter. All the more surprising, then, that Dr Mann would select a medium such as this as his outlet. (Indeed, he gave his permission for e-mails between himself and M&M to be posted).

Ironically, Dr Appell's website incorporates a quotation by Heinrich Heine about book-burning. The irony lies in the calls by Dr Appell and his acolytes for the non-publication of M&M and other pieces which do not accord with his position on the issue, and the celebration of the resignation of members of editorial boards from journals for publishing (or, most recently, intending to publish) work they disagreed with (or, most recently, by people they disagree with, since there is no suggestion that Professor Hulme has even seen the piece over which he is resigning). I suppose if we can suppress publication of books (and articles) we can save ourselves the trouble of burning them!

It is entirely appropriate that Sonja should invite Mann et al to respond to the M&M paper, but I think you are wrong in expecting that you should have been given access to the paper before any decision was made to publish.

Had the M&M paper simply been a comment on Mann et al, then it probably should have been written as a letter to Nature, and referred to Mann, yourself and Hughes for a rejoinder. But it was much more than that, and they have stated quite explicitly why they wished a longer piece to be considered for publication. As a paper in its own right, the authors had every reason to have it subjected to review as a paper in any journal they chose. As such, it would have been wholly inappropriate for it to be sent to any of the Mann et al authors for review, as to do so would have placed you in a conflict of interest: reviewing a paper which reflected critically on your work. Moreover, Dr Mann (as I understand it) quite explicitly cut off communication with M&M before the paper was completed and submitted. M&M cannot then be held responsible for your lack of involvement in the final version.

cg2003

As you rightly note, in peer review there should be an independence between the authors and the reviewers. This cuts both ways: there should be no positive or negative relationship between them. But peer review is not the only determinant of science, important as it is. The US Supreme Court (in *Daubert v Merrill Dow*) has provided a good statement of what constitutes scientific evidence. Publication after anonymous peer review is an important part of that, but so too is the requirement that it should have withstood several attempts at verification or falsification.

I guess many of us have had concerns over the treatment of Mann et al in IPCC TAR on these very grounds: Mann being a lead author, TAR being drafted before exactly the kind of paper M&M have written could have appeared, the political use of the implications of the paper (especially given the combination of proxy and instrumental data, when science without political purpose would have been satisfied with merely the proxy reconstruction). The production of a consensus (especially by an Intergovernmental Panel) is an inherently political process, and that is where Sonja and I have our interest and expertise. But we both know that science is controversial, and attempts to create and enforce consensus are not typical of the usual way in which science is progressed.

For the record, while we think TAR erred in allowing new storylines rather than new science (as Tom Wigley has pointed out) to drive a new upper limit to the temperature range which is improbable in the extreme, Sonja and I are on the record as stating we consider we are probably in for 1-3 deg C of warming and that something less than this is probably anthropogenic. We see much unresolved uncertainty in the science. We are critical of the Kyoto Protocol as a policy instrument and of the Kyoto process as a means of developing policy instruments - but that is our expertise.

Regardless of the outcome of Mann et al vs M&M, it is quite clear that science will have been advanced as a result of the attempt of both teams to further our understanding of complex and important issues.

I would suggest, however, that science is best advanced by conducting the terms of the debate on civil terms, and in media where participants are prepared to stand by their views and opinions. I get very worried when I see ad hominem attacks, along with commission of the genetic fallacy, use of argumentum ad populum, etc. My first reaction is to think that those using them do so in desperation in the absence of an argument. So please let's conduct the debate according to accepted rules, and submit your response to E&E. If it holds water, it stands the test of time - that's the deal with science. If M&M are wrong, show how and why.

Incidentally, I agree with Sonja on your depiction of the politics of science. You would fail introductory political science with such a caricatured account of the manner in which politics might influence science. There are straw men everywhere! If you want a better appreciation removed from the cut and thrust of climate science, try Robert Proctor's *The Nazi War on Cancer* (Princeton UP). Study question: why was German science and policy on tobacco at least 20 years ahead of Sir Richard Doll in the UK and the US Surgeon-General?

Best,

Aynsley Kellow

>From: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>
>Date: Thu, 6 Nov 2003 19:43:32 +0000
>To: "Raymond S. Bradley" <rbradley@geo.umass.edu>
>Subject: Re: clarification
>Cc: "Michael E. Mann" <mann@virginia.edu>, L.A.Love@hull.ac.uk,
> "David R. Legates" <legates@udel.edu>, Aynsley Kellow
><akellow@utas.edu.au>,
> Eigil Friis-Christensen <efc@dsri.dk>
>Priority: NORMAL

>Status: RO

>

>Dear Raymond, or should I say Dear Distinguished Professor Bradley?

>

>You clearly are not a political scientist, not that this matters, but
>ignorance tends to lead to simplification by all of us, and I do
>include myself as far as your work is concerned. Where I disagree with
>you is summarised below. I hope you forgive the wider participating
>audience, for you are making a few points about peer review and
>publication issues were a wider debate is essential for me and my
>position as editor.

>

>>From my perspective your argument about US policy is wrong: there is no
>such thing as 'a' government and the politics I talk about is rarely
>confined to political parties, except for very few decisions. Ratifying
>a treaty is one of them.

>I know enough about the USA to be sure that many of its parts (DOE, EA,
>sections of the State Department and by now all departments with
>significant research budgets) are in favour of Kyoto..and hence see
>global warming as a serious threat, a threat that 'enables' them
>without asking for much now. Bureaucracies like such issues. Met one of
>your chaps only yesterday, at Chatham House, Royal Institute for
>International Affairs, a Richard Bradley for US DOE International
>Affairs who poked a lot of fun at Bush and friends...and made it quite
>clear where he stood, and that was with Kyoto. The resistance in US
>(and Australia and Russia) does not come primarily from the middle and
>lower sections of the administrative machinery, but from top
>politicians and the Senate, that is from people accountable to
>electors. Support for Kyoto does come from the ENRONs and all those
>who want subsidies in one form or another, less from those that have to
>raise the money for decarbonisation and emission buying.

>(I am in favour of subsidies and hence taxes if they solve real
>problems, but not when they go to fund visions and model predictions.)

> I know quite a lot about how governments work; one friend negotiated
>UNFCCC for one country I am familiar with. In another country I know
>well, I know top scientists who will say one thing in public and
>another in private....but gets loads of money to study carbon, and
>doing useful science. Even the geologists are now persuaded that
>carbon is a threat, look at the sequestration issue in geological
>formations...and why not...until international mandatory law tries to
>impose rules and regulations on others that are likely to be harmed by
>them.

>Could write much more, but perhaps you have time to read a bit about
>global warming policy and politics.(Attached..) By the way, I am not
>that distinguished, but would be pleased if a science journal did look
>at my work. On the other hand, the link is the other way round; in this
>case the policy relevance of science is meat for me. On the other
>hand, it would do science defined as research no harm to worry a bit
>more about who funds them and why, and above all who simplifies their
>findings for what purpose.

>

>By the way, no need to lecture me on peer review, have been in this game
>long enough too, on both sides. There is a growing trend for peers to
>belong to a mutual support group, and the reasons for that is the
>emphasis now put, by funders, on peer review, as if this were the best
>way to assess 'quality'...this again serves bureaucracies rather than
>science, and works sometimes, but not always.

>I do stand corrected however on your point about returning peer
>reviewed papers to the author of a paper that has been criticised. I
>have never come across this in the social sciences, I did not learn it
>from my shushanmd who was a space physicist, and I myself have never
>had this experience. I (and A Kellow) have been accused by
>non-political scientists of conspiracy theory..a top WMO person did

cg2003

>this last, and apologised in private...People who have had different
>experiences of the peer review process might like to contact you
>directly.

>Best wishes

>Sonja

> On Thu, 06 Nov

>2003 12:38:57 -0500 "Raymond S. Bradley" <rbradley@geo.umass.edu> wrote:

>

> > In a recent email to Mike Mann you ask:

> > "journals like mine are surely permitted to ask and who is funding the
> > 'global warming' modelling community".

> > It is public knowledge that almost all of the modeling research in the US
> > is funded by a government firmly committed to NOT adopting the Kyoto

> > protocols. If your argument is that we are all somehow brainlessly

> > following the dictates of our funding, would it not follow that we would

> > all be publishing results that support this government

> > position? Apparently we are not. Could it be that the entire research

> > community is perversely seeking to have their funding terminated, or

> > perhaps that 100% of the community are Democrats? This seems inherently

> > unlikely.....

> > Scientists seek to publish what they discover, wherever the chips may

fall,

> > and other scientists (NOT selected by the authors) review their procedures

> > and data, then recommend whether the research should see the light of

> > day. This is known as peer review. If other scientists then find fault

> > with the published research, they are free to write a critique which is

> > always --ALWAYS-- sent to the original authors to assess and respond to

> > BEFORE it is published. You apparently do not follow such procedures,

> > which clearly demonstrates that you are not interested in an open

dialogue,

> > but only concerned with pushing your own political agenda--the very

> > criticism that you seem to level at climate scientists who worked on the

> > IPCC research assessment.

> > As for the McIntyre and McKintrick paper that you published as a

> > "correction" to our work, following an "audit" of our data and procedures,

> > you have done the research community a great disservice by giving voice to

> > a flawed and erroneous study which neither correctly "audited" our work,

> > nor "corrected" it. Furthermore, you did not give us the common courtesy

of

> > seeing the paper before it was rushed into print. Had you done so, we

> > would have pointed out the errors and misunderstandings that pervade their

> > study. Let me emphasise that I believe anybody has the right to carry out

> > a climate reconstruction and submit their results for publication, but

> > nobody has the right to claim they have undertaken an audit when they have

> > manifestly not done so. I'd have thought that a company CEO like McIntyre

> > would understand what the word audit meant even if you do not.

> > Since you clearly "do not claim that I or my reviewers can arbitrate on

the

> > 'scientific' truth of publications that the IPCC selects" I really think

it

> > would be best if you don't stray into that arena and stick to what you

feel

> > you can best evaluate. I suspect you would not appreciate an evaluation

of

> > your work published in the Journal of Geophysical Research.

> > Sincerely

> >

> > Raymond S. Bradley

> > University Distinguished Professor

> > Director, Climate System Research Center*

> > Department of Geosciences

> > Morrill Science Center

> > 611 North Pleasant Street

cg2003

> > AMHERST, MA 01003-9297
> >
> > Tel: 413-545-2120
> > Fax: 413-545-1200
> > *Climate System Research Center: 413-545-0659
> > <[1]http://www.paleoclimate.org>
> > Paleoclimatology Book Web Site:
[2]http://www.geo.umass.edu/climate/paleo/html

> >
> >
> >
>
>-----
>Dr.Sonja Boehmer-Christiansen
>Reader,Department of Geography,
>Editor, Energy & Environment
>(Multi-science,www.multi-science.co.uk)
>Faculty of Science
>University of Hull
>Hull HU6 7RX, UK
>Tel: (0)1482 465349/6341/5385
>Fax: (0)1482 466340
>Sonja.B-C@hull.ac.uk
>
>
>
>
--- End Forwarded Message ---

Dr.Sonja Boehmer-Christiansen
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(Multi-science,www.multi-science.co.uk)
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Tel: (0)1482 465349/6341/5385
Fax: (0)1482 466340
Sonja.B-C@hull.ac.uk

Dear Professor Bradley,
I have been meaning to respond to your message to Sonja Boehmer-Christiansen,
but many other duties have conspired to deny me the time to do so. I think it is
important that I do so, particularly because of the nature of the extraordinary attack on her
for daring to publish the M&M paper in E&E.
I should declare that I recently co-authored a book with Sonja, and recently
accepted an invitation to join the Editorial Board of E&E, having previously published two
papers with it. I speak, therefore, with some experience of both Sonja and the
journal. The journal can stand by its own reputation - by the quality of its
multidisciplinary content (which is always likely to provoke occasional controversy), but I am
disturbed by the attacks on Sonja, which have been personal and included derogatory
comments.
Sonja has an excellent track record of publication in science politics and
policy, including both research monographs and articles in leading journals, including
Nature,

Energy Policy, Environmental Politics, and Global Environmental Change. She is perhaps unequalled in her understanding of the issues involved and is widely cited by those on all sides of the climate change issue. The attack on her character is regrettable, all the more so because it has been conducted under protection of anonymity, thanks largely to the manner in which Dr Mann first engaged the M&M paper. For reasons best known to him, Dr Mann responded to this paper first on David Appell's blog 'Quark Soup' - an unfortunate choice, I must say. (Dr Appell reported Dr Mann's initial response at 8.02 am on 29 October - two days before the first draft of your collective initial response was posted on the East Anglia site). I was not previously familiar with this blog - there is an awful lot of junk in cyberspace and it is hard to track it all. Dr Appell professes to be a journalist, but his blog lies squarely in the realm of commentary, and provides a forum for anonymous gratuitous comment of the kind that no quality newspaper allows. It is a practice permitted by the tabloid press, perhaps fittingly, because that is the quality of journal which might reprint Sonja's e-mail to Dr Mann - deliberately circulated widely - and trumpet that it had obtained a copy of a 'leaked e-mail'. To further illustrate my point about quality, Dr Appell also slurs Theodor Landscheidt under the heading 'E&E publishes an astrologer!', when Landscheidt's book is quite clearly an evidenced-based critique of astrology. He also describes the critique of SRES published by Ian Castles and David Henderson as 'a third specious paper published recently by Energy and Environment'. For the record, Castles is a former Government Statistician and Head of the Finance Department in the Australian Government, and (until recently) was Vice-President of the Academy of Social Sciences in Australia. David Henderson was formerly Head of the Economics Department at the OECD. Prior to drawing attention to problems with SRES, Castles did much the same with the misuse of statistics in the UNDP World Development Report, a matter which was referred to the UN Statistical Commission, which upheld his critique. Dr Appell seems to have his loyal retinue of readers, though I see that few other than a couple of characters called 'Uncle E' and 'Dano' bother to contribute their anonymous patter. All the more surprising, then, that Dr Mann would select a medium such as this as his outlet. (Indeed, he gave his permission for e-mails between himself and M&M to be posted). Ironically, Dr Appell's website incorporates a quotation by Heinrich Heine about book-burning. The irony lies in the calls by Dr Appell and his acolytes for

the non-publication of M&M and other pieces which do not accord with his position on the issue, and the celebration of the resignation of members of editorial boards from journals for publishing (or, most recently, intending to publish) work they disagreed with (or, most recently, by people they disagree with, since there is no suggestion that Professor Hulme has even seen the piece over which he is resigning). I suppose if we can suppress publication of books (and articles) we can save ourselves the trouble of burning them!

It is entirely appropriate that Sonja should invite Mann et al to respond to the M&M paper, but I think you are wrong in expecting that you should have been given access to the paper before any decision was made to publish.

Had the M&M paper simply been a comment on Mann et al, then it probably should have been written as a letter to Nature, and referred to Mann, yourself and Hughes for a rejoinder. But it was much more than that, and they have stated quite explicitly why they wished a longer piece to be considered for publication. As a paper in its own right, the authors had every reason to have it subjected to review as a paper in any journal they chose. As such, it would have been wholly inappropriate for it to be sent to any of the Mann et al authors for review, as to do so would have placed you in a conflict of interest: reviewing a paper which reflected critically on your work.

Moreover, Dr Mann (as I understand it) quite explicitly cut off communication with M&M before the paper was completed and submitted. M&M cannot then be held responsible for your lack of involvement in the final version.

As you rightly note, in peer review there should be an independence between the authors and the reviewers. This cuts both ways: there should be no positive or negative relationship between them. But peer review is not the only determinant of science, important as it is. The US Supreme Court (in Daubert v Merrill Dow) has provided a good statement of what constitutes scientific evidence. Publication after anonymous peer review is an important part of that, but so too is the requirement that it should have withstood several attempts at verification or falsification.

I guess many of us have had concerns over the treatment of Mann et al in IPCC TAR on these very grounds: Mann being a lead author, TAR being drafted before exactly the kind of paper M&M have written could have appeared, the political use of the implications of the paper (especially given the combination of proxy and instrumental data, when science without political purpose would have been satisfied with merely the proxy reconstruction). The production of a consensus (especially by an Intergovernmental

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Panel) is an inherently political process, and that is where Sonja and I have our interest and expertise. But we both know that science is controversial, and attempts to create and enforce consensus are not typical of the usual way in which science is progressed.

For the record, while we think TAR erred in allowing new storylines rather than new science (as Tom Wigley has pointed out) to drive a new upper limit to the temperature range which is improbable in the extreme, Sonja and I are on the record as stating we consider we are probably in for 1-3 deg C of warming and that something less than this is probably anthropogenic. We see much unresolved uncertainty in the science. We are

critical of the Kyoto Protocol as a policy instrument and of the Kyoto process as a means of developing policy instruments - but that is our expertise.

Regardless of the outcome of Mann et al vs M&M, it is quite clear that science will have been advanced as a result of the attempt of both teams to further our understanding of complex and important issues.

I would suggest, however, that science is best advanced by conducting the terms of the debate on civil terms, and in media where participants are prepared to stand by their views and opinions. I get very worried when I see ad hominem attacks, along with

commission of the genetic fallacy, use of argumentum ad populum., etc. My first reaction is to think that those using them do so in desperation in the absence of an argument. So

please let's conduct the debate according to accepted rules, and submit your response to

E&E. If it holds water, it stands the test of time - that's the deal with science. If

M&M are wrong, show how and why.

Incidentally, I agree with Sonja on your depiction of the politics of science. You would

fail introductory political science with such a caricatured account of the manner in which politics might influence science. There are straw men everywhere! If you want a

better appreciation removed from the cut and thrust of climate science, try Robert

Proctor's The Nazi War on Cancer (Princeton UP). Study question: why was German science

and policy on tobacco at least 20 years ahead of Sir Richard Doll in the UK and the US

Surgeon-General?

Best,

Aynsley Kellow

From: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>

Date: Thu, 6 Nov 2003 19:43:32 +0000

To: "Raymond S. Bradley" <rbradley@geo.umass.edu>

Subject: Re: clarification

Cc: "Michael E. Mann" <mann@virginia.edu>, L.A.Love@hull.ac.uk,

<akellow@utas.edu.au>, "David R. Legates" <legates@udel.edu>, Aynsley Kellow

Eigil Friis-Christensen <efc@dsri.dk>

Priority: NORMAL

Status: RO

Dear Raymond, or should I say Dear Distinguished Professor Bradley? You clearly are not a political scientist, not that this matters, but ignorance tends to lead to simplification by all of us, and I do include myself as far as your work is concerned. Where I disagree with you is summarised below. I hope you forgive the wider participating audience, for you are making a few points about peer review and publication issues where a wider debate is essential for me and my position as editor.

>From my perspective your argument about US policy is wrong: there is no such thing as 'a' government and the politics I talk about is rarely confined to political parties, except for very few decisions. Ratifying a treaty is one of them.

I know enough about the USA to be sure that many of its parts (DOE, EA, sections of the State Department and by now all departments with significant research budgets) are in favour of Kyoto..and hence see global warming as a serious threat, a threat that 'enables' them without asking for much now. Bureaucracies like such issues. Met one of your chaps only yesterday, at Chatham House, Royal Institute for International Affairs, a Richard Bradley for US DOE International Affairs who poked a lot of fun at Bush and friends...and made it quite clear where he stood, and that was with Kyoto. The resistance in US (and Australia and Russia) does not come primarily from the middle and lower sections of the administrative machinery, but from top politicians and the Senate, that is from people accountable to electors. Support for Kyoto does come from the ENRONs and all those who want subsidies in one form or another, less from those that have to raise the money for decarbonisation and emission buying.

(I am in favour of subsidies and hence taxes if they solve real problems, but not when they go to fund visions and model predictions.) I know quite a lot about how governments work; one friend negotiated UNFCCC for one country I am familiar with. In another country I know well, I know top scientists who will say one thing in public and another in private....but gets loads of money to study carbon, and doing useful science. Even the geologists are now persuaded that carbon is a threat, look at the sequestration issue in geological formations...and why not...until international mandatory law tries to impose rules and regulations on others that are likely to be harmed by them.

Could write much more, but perhaps you have time to read a bit about global warming policy and politics.(Attached..) By the way, I am not that distinguished, but would be pleased if a science journal did look at my work. On the other hand, the link is the other way round; in this case the policy relevance of science is meat for me. On the other hand, it would do science defined as research no harm to worry a bit more about who funds them and why, and above all who simplifies their findings for what purpose.

By the way, no need to lecture me on peer review, have been in this game long enough too, on both sides. There is a growing trend for peers to belong to a mutual support group, and the reasons for that is the emphasis now put, by funders, on peer review, as if this were the best way to assess 'quality'..this again serves bureaucracies rather than science, and works sometimes, but not always.

I do stand corrected however on your point about returning peer reviewed papers to the author of a paper that has been criticised. I have never come across this in the social sciences, I did not learn it from my shushanmd who was a space physicist, and I myself have never had this experience. I (and A Kellow) have been accused by non-political scientists of conspiracy theory..a top WMO person did this last, and apologised in private...People who have had different experiences of the peer review process might like to contact you directly.

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Best wishes

Sonja

On Thu, 06 Nov

2003 12:38:57 -0500 "Raymond S. Bradley" <rbradley@geo.umass.edu> wrote:

> In a recent email to Mike Mann you ask:
> "journals like mine are surely permitted to ask and who is funding the
> 'global warming' modelling community".
> It is public knowledge that almost all of the modeling research in the US
> is funded by a government firmly committed to NOT adopting the Kyoto
> protocols. If your argument is that we are all somehow brainlessly
> following the dictates of our funding, would it not follow that we would
> all be publishing results that support this government
> position? Apparently we are not. Could it be that the entire research
> community is perversely seeking to have their funding terminated, or
> perhaps that 100% of the community are Democrats? This seems inherently
> unlikely.....
> Scientists seek to publish what they discover, wherever the chips may fall,
> and other scientists (NOT selected by the authors) review their procedures
> and data, then recommend whether the research should see the light of
> day. This is known as peer review. If other scientists then find fault
> with the published research, they are free to write a critique which is
> always --ALWAYS-- sent to the original authors to assess and respond to
> BEFORE it is published. You apparently do not follow such procedures,
> which clearly demonstrates that you are not interested in an open dialogue,
> but only concerned with pushing your own political agenda--the very
> criticism that you seem to level at climate scientists who worked on the
> IPCC research assessment.
> As for the McIntyre and McKintrick paper that you published as a
> "correction" to our work, following an "audit" of our data and procedures,
> you have done the research community a great disservice by giving voice to
> a flawed and erroneous study which neither correctly "audited" our work,
> nor "corrected" it. Furthermore, you did not give us the common courtesy of
> seeing the paper before it was rushed into print. Had you done so, we
> would have pointed out the errors and misunderstandings that pervade their
> study. Let me emphasise that I believe anybody has the right to carry out
> a climate reconstruction and submit their results for publication, but
> nobody has the right to claim they have undertaken an audit when they have
> manifestly not done so. I'd have thought that a company CEO like McIntyre
> would understand what the word audit meant even if you do not.
> Since you clearly "do not claim that I or my reviewers can arbitrate on the
> 'scientific' truth of publications that the IPCC selects" I really think it
> would be best if you don't stray into that arena and stick to what you feel
> you can best evaluate. I suspect you would not appreciate an evaluation of
> your work published in the Journal of Geophysical Research.
> Sincerely

>
> Raymond S. Bradley
> University Distinguished Professor
> Director, Climate System Research Center*
> Department of Geosciences
> Morrill Science Center
> 611 North Pleasant Street
> AMHERST, MA 01003-9297

>
> Tel: 413-545-2120
> Fax: 413-545-1200
> *Climate System Research Center: 413-545-0659
> <<[3]<http://www.paleoclimate.org>>[4]<http://www.paleoclimate.org>>
> Paleoclimatology Book Web Site:

<[5]<http://www.geo.umass.edu/climate/paleo/html>>[6]<http://www.geo.umass.edu/climate/paleo/html>

cg2003

>
>
>
>

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[9]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1819. 2003-11-12

cc: mhughes@lrr.arizona.edu, rbradley@geo.umass.edu, Scott Rutherford
<srutherford@rwu.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>
date: Wed, 12 Nov 2003 14:16:42 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Fwd: Re: [Fwd: VL: McIntyre-Mckitrick Reply to Mann - Part 1]
to: Tom Wigley <wigley@cgd.ucar.edu>

Dear All,

Needless to say, the information I provided below is very sensitive. Please keep
this

completely confidential. We do not want to in any way do something that might
influence

these pieces seeing the light of day. So please do not pass along to anyone!!

Thanks,

mike

Date: Wed, 12 Nov 2003 13:43:57 -0500

To: Tom Wigley <wigley@cgd.ucar.edu>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: [Fwd: VL: McIntyre-Mckitrick Reply to Mann - Part 1]

Cc: mhughes@lrr.arizona.edu, rbradley@geo.umass.edu, Scott Rutherford

cg2003

<srutherford@rwu.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>
Bcc: Annie_Petsonk@environmentaldefense.org, Michael Oppenheimer
<omichael@Princeton.EDU>

Hi Tom,

There is a myth being perpetuated by these people, and your falling into the trap of letting them set the rules. We cannot allow that. The data has all been available back through July 2002 here on our public ftp site:
[1]ftp://holocene.evsc.virginia.edu/pub/MBH98/
All of the data used by MBH98 have been there, plain and simple.
USA Today is going to be publishing a retraction tomorrow or Friday of the claim made last week in their op-ed pages (by an industry shill) that we hadn't made our data publicly available. That should clear this all up in a hurry!
Meanwhile, we're going ahead w/ a peer-reviewed submission expanding on our initial response, and we believe that should settle the issue. I don't see any problem if others want to download the data (which have been there all along) and try the analyses themselves, but I can't allow myself to be distracted with all of that right now. It would set me back years in my own research plans, which is part of the motive of this effort...

mike

At 11:01 AM 11/12/2003 -0700, Tom Wigley wrote:

Mike,

I presume you have seen this. One of their buzz phrases is 'the basic standards of data disclosure'. Personally, I see no reason why one should disclose all data and all methodological details -- unless required to by the funding authority. I had a long exchange with Timo on this issue, which I will forward to you.
These guys are primarily accusing you of either making errors or being incompetent.
(I have not seen this directly, but they may also be implying that you deliberately distorted your analysis -- but it is best not to get into this possibility.)
There are three possible responses. The first is to prove to *them* that your results are correct. The second is to tell them to go to hell. The third is to use an independent arbiter (a statistician) to repeat your analysis.
The first is difficult. You could give them all the data in an easily used form and tell them exactly what you did -- and then see if they can repeat it and get your results. This is tricky because I doubt that one can trust them to do this honestly -- indeed, one could say to them (and the world) that you neither trusted their motives nor their competence, as a lead in to option three. Competence can be challenged since they have no track record in the field, nor are they qualified as bona fide statisticians.
The second rests on whether you are bound by disclosure conditions. Using this option could be justified, but it sure would piss them off. A possible holding action

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would be

to

just say that a full paper describing the methods used was in preparation, and they have to wait. (In other words, go to hell for now, and I'll tell you when to come back out.)

The third option seems the best. The three statisticians who could help are Richard

Smith, Francis Zwiers and Dan Wilks.

The approach I would use here is to say that, since both sides are either directly or

indirectly accusing the other of at least some level of incompetance, and since you

(MBH)

see no reason why your data should be made available at this stage (i.e., you can agree

with 'full disclosure' in principle, but only in a 'timely manner' where the data

producer

is the one who decides on the time frame), the only way to reconcile the differences

between

you and the two Ms is through independent 'arbitration'. Since, once the data are given,

this

is purely a statistical issue, then the arbiter must be a bona fide, highly-respected

statistician and one with some experience in climate science -- OF YOUR CHOICE.

One of the problems is that options one and three may create dangerous precedents

under the data quality act. Actually, the way I have set up option three creates a

possibly

good precedent, especially with you choosing the arbiter. M&M may not agree with this,

but you could add that your choice has to be agreed to by the appropriate panel of the

NAS (who would definitely support the above three names).

Wotcha think? (Share with others if you wish.)

Tom.

+++++

----- Original Message -----

Subject: VL: McIntyre-McKittrick Reply to Mann - Part 1

Date: Wed, 12 Nov 2003 18:34:48 +0200

From: Timo Hämeranta [2]<timo.hameranta@pp.inet.fi>

To: Tom M. L. Wigley [3] <wigley@ucar.edu>

Dear Tom,

FYI attached.

All the best

Timo

[illegible]

Timo Hämeranta, M.LL.

Moderator, Climatesceptics

Martinlaaksontie 42 B 9

01620 Vantaa

Finland, Member State of the European Union

Moderator: [4]timohame@yahoo.co.uk

```
Private: [5]timo.hameranta@app.inet.fi
```

Home page: [6]<http://personal.inet.fi/koti/hameranta/climate.htm>

Moderator of the discussion group "Sceptical Climate Science"

[7] <http://groups.yahoo.com/group/climatesceptics>

"To dwell only on horror scenarios of the future

cg2003
shows only a lack of imagination". (Kari Enqvist)
"If the facts change, I'll change my opinion.
What do you do, Sir" (John Maynard Keynes)
xx

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[9]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2593. 2003-11-12

date: wed, 12 Nov 2003 18:49:59 -0000
from: "Elaine Jones" <E.L.Jones@uea.ac.uk>
subject: Re: new DG-Research at OST
to: "Mike Hulme" <m.hulme@uea.ac.uk>
Thanks Mike,
Stable isotope guru (out of Geochronology). Interestingly knighted at the same time as his (former) fellow Cambridge geologist Ron Oxburgh became a life peer. Note the nuclear connection (last para) - a D.King/TB recruit ? A 2002 nature paper was the 'Science of Nuclear Warheads' ! a ? follow-up headline was "Work on weapons adds to public distrust of science" but that was amongst other things !! (perhaps he is to spearhead the European research agenda) e.g.
Evidence for Stronger Thermohaline Circulation Prior to Northern Hemisphere Glaciation from Nd and Pb Isotopes in Ferromanganese Crusts
Martin Frank (frank@erdw.ethz.ch)¹, Nicholas Whiteley (nickw@earth.ox.ac.uk)², Sabine Kasten (skasten@geochemie.uni-bremen.de)³, James R. Hein (jimh@octopus.wr.usgs.gov)⁴ & R. Keith O'Nions (keitho@earth.ox.ac.uk)²
<http://www.the-conference.com/JConfAbs/5/409.pdf>
Chief Scientific Adviser launches new series of leaflets on cutting edge technology
Published Tuesday 27th March 2001
Sir Keith O'Nions, the MOD's Chief Scientific Adviser, launched on 27 March a new series of leaflets outlining emerging technologies which might impact on defence issues.
The first leaflet covers Nanotechnology - a field of science concerned with microscopically small components, around a millionth of a millimetre across. The leaflet: explains the basic principles of the technology; examines the various areas, civil and military, where it might have an effect; and outlines the work being undertaken by the MOD to track its development.

6 July 1999
NEW CHIEF SCIENTIFIC ADVISER, MINISTRY OF DEFENCE
George Robertson, Defence Secretary, announced today that, with the agreement of the Prime Minister, Professor Sir Keith O'Nions FRS, of Oxford University, has been appointed Chief Scientific Adviser at the Ministry of Defence. Professor O'Nions, replaces Professor Sir David Davies, and will take up his new appointment in January 2000. His appointment will be for three years.

- ends -

NOTES TO EDITORS

1. Biography:

Professor Sir Keith O'Nions has been Professor of the Physics and Chemistry of Minerals, and Head of Department of Earth Sciences, University of Oxford since 1995. He was born on 26 September 1944 and educated at the University of Nottingham where he graduated in Geology, later gaining a Ph.D at the University of Alberta and becoming a Postdoctoral Fellow at the University of Oslo. From 1971 to 1975 he was Demonstrator and then Lecturer in Geochemistry at the University of Oxford. He became Professor of Geology at Columbia University in 1975, moved to Cambridge in 1979 as Royal Society Research Professor, before taking up his current appointment in Oxford in 1995.

In addition to his role at Oxford, Professor Sir Keith O'Nions has enjoyed extensive participation in a broad range of academic and technological committees. He became a Fellow of the American Geophysical Union in 1979, and a Member of the Norwegian Academy of Science and Letters in 1980. In 1983, he was elected a Fellow of the Royal Society (FRS). He became a Member of Academia Europaea in 1990, Geochemistry Fellow (Joint Geochemical Society/European Association of Geochemistry) in 1997, and Honorary Fellow of the Indian Academy of Sciences in 1998. He has been the chairman, or a member, of a number of committees of the Natural Environment Research Council since 1981, and a member of the Council of Science and Technology since 1998. He received a Knighthood for services to Earth Sciences in the recent Queen's Birthday Honours.

Professor Sir Keith O'Nions is married with three daughters.

2. George Robertson made the announcement in response to a written Parliamentary Question from Lorna Fitzsimons, MP, (Rochdale).

He said:

"I am announcing today that Professor Sir Keith O'Nions will become the Chief Scientific Adviser to the MoD on 4 January 2000. His appointment will be for three years."

Professor Sir Keith O'Nions FRS

Sir Keith was born on 26 September 1944 and educated at the Universities of Nottingham and Alberta. He held academic positions in Universities of Oxford (1971 to 1975), Columbia (1975 - 1979), Cambridge (1979 - 1995 as Royal Society Research Professor) and Oxford (1995 onwards as Professor of the Physics and Chemistry of Minerals and the Head of Department of Earth Sciences). He took up his current post as Chief Scientific Adviser at the Ministry of Defence in January 2000.

He became a Fellow of the American Geophysical Union in 1979, and a Member of the Norwegian Academy of Science and Letters in 1980, a Fellow of the Royal Society (1983), a Member of Academia Europaea in 1990, Geochemistry Fellow (Joint Geochemical Society/European Association of Geochemistry) in 1997, Honorary Fellow of Indian Academy of Sciences in 1998 and National Indian Science Academy in 2001. He has been the chairman, or a member, of a number of committees of the Natural Environment Research Council, and was a member of the Council of Science and Technology until 2000. He has been a Trustee of the Natural History Museum since 1995 (and took over as Chairman in 2003). He received a Knighthood for services to Earth Sciences in the 1999 Queen's Birthday Honours.

Professor Sir Keith O'Nions has been Chief Scientific Adviser at the Ministry of Defence since 4 January 2000. In addition to being a member of the Defence Council and Defence Management Board his specific responsibilities include managing the MOD's £450M annual research programme and chairing the Investment Approvals Board. He is also the UK Principal for the 1958 UK/US Mutual Defence Agreement on nuclear matters and for the 1985 UK/US Memorandum of Understanding on Ballistic Missile Defence technologies.

----- Original Message -----

From: "Mike Hulme" <m.hulme@uea.ac.uk>

To: <h.j.schellnhuber@uea.ac.uk>; <e.l.jones@uea.ac.uk>

cg2003

Sent: Wednesday, November 12, 2003 5:37 PM
Subject: new DG-Research at OST

>
> Keith O'Nions, the Ministry of Defence's chief scientific advisor, is to
be
> the next director general of the research councils. According to sources
> close to the government, he will take over from John Taylor at the end of
> the year. O'Nions will be the first academic to be appointed to the post.
> Before coming to MoD in 2000 he was head of the department of earth
> sciences at the University of Oxford. In contrast, Taylor came to the post
> from Hewlett Packard, and his predecessor, John Cadogan, came from BP.
>
>

2858. 2003-11-12

cc: "Tim Osborn" <t.osborn@uea.ac.uk>, "Ross Mckitrick" <rmckitri@uoguelph.ca>
date: Wed, 12 Nov 2003 00:36:23 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: MBH98
to: "Steve McIntyre" <smcintyre@cgxenergy.com>

Dear Mr. McIntyre,

There seems to be some confusion on your part regarding the public posting of
the MBH98
data.

All of the data used by MBH98 have been available in plain ASCII format on this
public ftp
site

[1]ftp://holocene.evsc.virginia.edu/pub/MBH98/

They have been available in the various clearly indicated sub-directories, back
through at

least summer 2002 according to the dates on those directories. This includes all
159

predictors used by MBH98 back to AD 1400 *and* all of the proxy data that go
into those
indicators.

When I sent you the below email message on 4/9/2003, it was my expectation that
you would

go to that ftp site to get the individual data series in question. I was not
party to the

various emails you and Scott Rutherford exchanged regarding alternative versions
of the

dataset that he prepared, though I am told he offered you all of the proxy
data, and you

instead preferred a dataset of 112 proxy indicators (that is the number of
indicators

available back to 1820).

With regard to the latest changes made by Scott on the ftp site, I believe this
was to

replace the incorrect spreadsheet version of the data that had been posted
previously with

a corrected version, so that people do not continue to download an incorrect
version of the

data set.

To reiterate once last time, the original data that you requested before and now
request

again are all on the indicated ftp site, in the indicated directories, and have
been there

since at least 2002. I therefore trust you should have no problem acquiring the
data you

now seek.

Mike Mann

>Dear Mr. McIntyre,

cg2003

>
>These data are available on an anonymous ftp site we have set up. I've
forgotten the exact
>location, but I've asked my Colleague Dr. Scott Rutherford if he can provide
you with that
>information.

>
>best regards,

>
>Mike Mann

At 01:47 PM 4/8/2003 -0400, Steve McIntyre wrote:

Dear Dr. Mann,

I have been studying MBH98 and 99. I located datasets for the 13 series used in
99 at

[2]ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/Millennium/DATA/PROXIES/
(the

convenience of the ftp: location being excellent) and was intereseted in
locating similar

information on the 112 proxies referred to in MBH98, as well as listing (the
listing at

[3]http://www.ngdc.noaa.gov/paleo/ei/data_supp.html is for 390 datasets, and I
gather/presume that many of these listed datasets have been condensed into PCs,
as

mentioned in the paper itself. Thank you for your attention.

Yours truly,
Stephen McIntyre,
Toronto, Canada

At 11:39 PM 11/11/2003 -0500, Steve McIntyre wrote:

<?xml:namespace prefix = o ns = "urn:schemas-microsoft-com:office:office" />
November 11, 2003

Professor Michael E. Mann
School of Earth Sciences
University of Virginia

Dear Professor Mann,

We apologize for not sending you a copy of our recent paper (MM) in Energy and
Environment for comment, as we understood from your email of September 25,
2003 that

time constraints prevented you from considering our material. We notice that
you seem to

have subsequently changed your mind and hope that you will both be able to
clarify some

points for us and to rectify the public record on other points.

1) You have claimed that we used the wrong data and the wrong computational
methodology.

We would like to reconcile our results to actual data and methodology used in
MBH98. We

would therefore appreciate copies of the computer programs you actually used
to read in

data (the 159 data series referred to in your recent comments) and construct
the

temperature index shown in Nature (1998) (MBH98), either through email or,
preferably

through public FTP or web posting.

2) In some recent comments, you are reported as stating that we requested an
Excel file

and that you instead directed us to an FTP site for the MBH98 data. You are
also

reported as saying that despite having pointed us to the FTP site, you and
your

colleague took trouble to prepare an Excel spreadsheet, but inadvertently
introduced

some collation errors at that time. In fact, as you no doubt recall, we did

not request an Excel spreadsheet, but specifically asked for an FTP location, which you were unable or unwilling to provide. Nor was an Excel spreadsheet ever supplied to us; instead we were given a text file, pcproxy.txt. Nor was this file created in April 2003. After we learned on October 29, 2003 that the pertinent data was reported to be located on your FTP site [4]ftp://holocene.evsc.virginia.edu/pub (and that we were being faulted for not getting it from there), we examined this site and found it contains the exact same file (pcproxy.txt) as the one we received, bearing a date of creation of August 8, 2002. On October 29, 2003, your FTP site also contained the file pcproxy.mat, a Matlab file, the header to which read: MATLAB 5.0 MAT-file, Platform: SOL2, Created on: Thu Aug 8 10:18:19 2002. Both files contain identical data to the file pcproxy.txt emailed to one of us (McIntyre) in April 2003, including all collation errors, fills and other problems identified in MM. It is therefore clear that the file pcproxy.txt as sent to us was not prepared in April 2003 in response to our requests, nor was it prepared as an Excel spreadsheet, but in fact it was prepared many months earlier with Matlab. It is also clear that, had we gone to your FTP site earlier, we would simply have found the same data collation as we received from Scott Rutherford. Would you please forthwith issue a statement withdrawing and correcting your earlier comments.

3) In reported comments, you also claimed that we overlooked the collation errors in pcproxy.txt and slid the incorrect data into our calculations, a statement which is untrue and made without a reasonable basis. In MM, we described numerous errors including, but not limited to, the collation errors, indicating quite obviously that we noticed the data problems. We then describe how we firewalled our data from the errors contained in the data you provided us, by re-collating tree ring proxy data from original sources and carrying out fresh principal component calculations. We request that you forthwith withdraw the claim that we deliberately used data we knew to be in error.

4) On November 8, 2003, when we re-visited your FTP site, we noticed the following changes since October 29, 2003: (1) the file pcproxy.mat had been deleted from your FTP site; (2) the file pcproxy.txt no longer was displayed under the /sdr directory, where it had previously been located, although it could still be retrieved through an exact call if one previously knew the exact file name; (3) without any notice, a new file named mbhfilled.mat prepared on November 4, 2003 had been inserted into the directory.

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Obviously, the files pcproxy.mat and pcproxy.txt are pertinent to the comments referred

to above and we view the deletion of pcproxy.mat from the archival record under the

current circumstances as unjustifiable. Would you please restore these files to your FTP

site, together with an annotated text file documenting the dates of their deletion and restoration.

5) We note that the new file mbhfilled.mat is an array of dimension 381x2016. Could you

state whether this file has any connection to MBH98, and, if so, please explain the

purpose of this file, why it has been posted now and why it was not previously available

at the FTP site.

6) Can you advise us whether the directory MBH98 has been a subdirectory within the

folder pub since July 30, 2002 or whether it was transferred from another (possibly

private) directory at a date after July 30, 2002? If the latter, could you advise on the

date of such transfer.

We have prepared a 3-part response to your reply to MM. The first, which we have

released publicly, goes over some of the matters raised in points #2-#5 above.

The second is undergoing review. It deals with additional issues of data quality

and disclosure, resulting from inspection of your FTP site since October 29, 2003.

The third part will consider the points made in your response, both in terms of data and

methodology, and will attempt a careful reconciliation of our calculation methods, hence

the necessity of our request in point #1. Thank you for your attention.

Yours truly,

Stephen McIntyre

Ross McKittrick

cc: Timothy Osborn

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

3794. 2003-11-12

cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford <srutherford@rwu.edu>

date: Wed, 12 Nov 2003 13:19:12 -0700

from: Tom Wigley <wigley@cgd.ucar.edu>

subject: Re: fine

to: "Michael E. Mann" <mann@virginia.edu>

I will check out possibilities here. My thinking is that the only way to truly squash M&M

is to have an independent third party come along and say ... I used exactly the same data

and method as MBH and got exactly the same results, and, furthermore, I endorse the

cg2003

method. I will read your paper with interest -- this will be a good putdown, but
M&M

may still say that you are a mutual admiration society.

Tom.

+++++++=

Michael E. Mann wrote:

Thanks Tom,

Fair enough on all counts. You know how this works--hard to get every single
nitty

gritty detail in the short Nature space, then someone comes along, obviously
w/ hostile

intent, and your inclination to help them is limited--then they turn around
and say you

didn't disclose the data, methods, etc. (which is at least partly an outright
lie,

though there is a minor kernal to the claim that they can try to grab on to,
because the

methodological description was terse).

Actually, Tim, Keith, Phil, Ray, Malcolm, Scott and I are all planning to
pursue a much

more careful intercomparison of results, methods, etc. We have a paper, the
draft of

which I'm forwarding separately as it is pretty big (in review J. Climate)
which should

go a long way in this regard. It controls for different datasets and
methodologies, and

shows that the results are basically robust, with the conclusion that spatial
and

seasonal sampling seems to matter (as we would expect) the most, but results
seem pretty

robust with respect to statistical methodology (if you've done it right!).
would have

been nice if this were in the press right now, but alas its still in review...

Nonetheless, wouldn't be a bad idea to have some graduate students, or some

NCAR

postdocs(?) try this--I'd be happy to help out where I can, but be hands off
too to keep

the effort independent.

Let me know what you think...

Thanks again,

mike

At 12:36 PM 11/12/2003 -0700, Tom Wigley wrote:

OK, Mike. So you are choosing my option 2 (rightly so).

But there are broader issues, and it may still come down to option 3. Perhaps
a middle ground would be to try to get one of the people I named to get

the data and do an honest and informed version of what M&M tried to do?

It would be a nice student's warm up exercise, at the beginning grad student
level in a stats dept.

>From the flurry of emails, there may still be some things about the method
that

you would have to pass on. I must admit that, having read the papers, I don't
think there is enough information for *me* to reproduce what you have done.

I could certainly do something similar, and I might discover the nuances as I
proceeded. But it would still be tough.

I still don't think that hard-earned data needs to be made freely available.

Tom.

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: [1]mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

date: wed, 12 Nov 2003 11:01:22 +0000
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Fwd: MBH98
to: "Keith Briffa" <k.briffa@uea.ac.uk>,"Phil Jones" <p.jones@uea.ac.uk>
<x-flowed>

In the meantime, here is an email (copied below) to Mike Mann from McIntyre, requesting data and programs (and making other criticisms). I do wish Mike had not rushed around sending out preliminary and incorrect early responses - the waters are really muddled now. He would have done better to have taken things slowly and worked out a final response before publicising this stuff. Excel files, other files being created early or now deleted is really confusing things!

 \succ

>
 >1) You have claimed that we used the wrong data and the wrong
 >computational methodology. We would like to reconcile our results to
 >actual data and methodology used in MBH98. We would therefore appreciate
 >copies of the computer programs you actually used to read in data (the 159
 >data series referred to in your recent comments) and construct the
 >temperature index shown in Nature (1998) ("MBH98"), either through email
 >or, preferably through public FTP or web posting.

>
 >
 >
 >2) In some recent comments, you are reported as stating that we requested
 >an Excel file and that you instead directed us to an FTP site for the
 >MBH98 data. You are also reported as saying that despite having pointed us
 >to the FTP site, you and your colleague took trouble to prepare an Excel
 >spreadsheet, but inadvertently introduced some collation errors at that
 >time. In fact, as you no doubt recall, we did not request an Excel
 >spreadsheet, but specifically asked for an FTP location, which you were
 >unable or unwilling to provide. Nor was an Excel spreadsheet ever supplied
 >to us; instead we were given a text file, pcproxy.txt. Nor was this file
 >created in April 2003. After we learned on October 29, 2003 that the
 >pertinent data was reported to be located on your FTP site
 ><ftp://holocene.evsc.virginia.edu/pub>ftp://holocene.evsc.virginia.edu/pub
 >(and that we were being faulted for not getting it from there), we
 >examined this site and found it contains the exact same file (pcproxy.txt)
 >as the one we received, bearing a date of creation of August 8, 2002. On
 >October 29, 2003, your FTP site also contained the file pcproxy.mat, a
 >Matlab file, the header to which read: "MATLAB 5.0 MAT-file, Platform:
 >SOL2, Created on: Thu Aug 8 10:18:19 2002." Both files contain identical
 >data to the file pcproxy.txt emailed to one of us (McIntyre) in April
 >2003, including all collation errors, fills and other problems identified
 >in MM. It is therefore clear that the file pcproxy.txt as sent to us was
 >not prepared in April 2003 in response to our requests, nor was it
 >prepared as an Excel spreadsheet, but in fact it was prepared many months
 >earlier with Matlab. It is also clear that, had we gone to your FTP site
 >earlier, we would simply have found the same data collation as we received
 >from Scott Rutherford. Would you please forthwith issue a statement
 >withdrawing and correcting your earlier comments.

>
 >
 >
 >3) In reported comments, you also claimed that we overlooked the collation
 >errors in pcproxy.txt and "slid" the incorrect data into our calculations,
 >a statement which is untrue and made without a reasonable basis. In MM, we
 >described numerous errors including, but not limited to, the collation
 >errors, indicating quite obviously that we noticed the data problems. We
 >then describe how we "firewalled" our data from the errors contained in
 >the data you provided us, by re-collating tree ring proxy data from
 >original sources and carrying out fresh principal component calculations.
 >We request that you forthwith withdraw the claim that we deliberately used
 >data we knew to be in error.

>
 >
 >
 >4) On November 8, 2003, when we re-visited your FTP site, we noticed the
 >following changes since October 29, 2003: (1) the file pcproxy.mat had
 >been deleted from your FTP site; (2) the file pcproxy.txt no longer was
 >displayed under the /sdr directory, where it had previously been located,
 >although it could still be retrieved through an exact call if one
 >previously knew the exact file name; (3) without any notice, a new file
 >named "mbhfilled.mat" prepared on November 4, 2003 had been inserted into
 >the directory. Obviously, the files pcproxy.mat and pcproxy.txt are
 >pertinent to the comments referred to above and we view the deletion of

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>pcproxy.mat from the archival record under the current circumstances as
>unjustifiable. Would you please restore these files to your FTP site,
>together with an annotated text file documenting the dates of their
>deletion and restoration.

>

>

>

>5) We note that the new file mbhfilled.mat is an array of dimension
>381x2016. Could you state whether this file has any connection to MBH98,
>and, if so, please explain the purpose of this file, why it has been
>posted now and why it was not previously available at the FTP site.

>

>

>6) Can you advise us whether the directory MBH98 has been a subdirectory
>within the folder "pub" since July 30, 2002 or whether it was transferred
>from another (possibly private) directory at a date after July 30, 2002?
>If the latter, could you advise on the date of such transfer.

>

>

>

>

>

>we have prepared a 3-part response to your reply to MM. The first, which
>we have released publicly, goes over some of the matters raised in points
>#2-#5 above. The second is undergoing review. It deals with additional
>issues of data quality and disclosure, resulting from inspection of your
>FTP site since October 29, 2003. The third part will consider the points
>made in your response, both in terms of data and methodology, and will
>attempt a careful reconciliation of our calculation methods, hence the
>necessity of our request in point #1. Thank you for your attention.

>

>

>

>

>

>Yours truly,

>

>

>

>Stephen McIntyre

Ross McKittrick

>

>

>

>

>cc: Timothy Osborn
>Dr Timothy J Osborn
>Climatic Research Unit
>School of Environmental Sciences, University of East Anglia
>Norwich NR4 7TJ, UK
>e-mail: t.osborn@uea.ac.uk
>phone: +44 1603 592089
>fax: +44 1603 507784
>web: <http://www.cru.uea.ac.uk/~timo/>
>sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
</x-flowed>

5349. 2003-11-13

cc: a.minns@uea.ac.uk, v.mcgregor@uea.ac.uk
date: Thu, 13 Nov 2003 15:04:55 -0000
from: "Prof B.E. Launder" <brian.launder@umist.ac.uk>
reply-to: brian.launder@umist.ac.uk

cg2003

subject: Re: Fwd: Tyndall-CMI Symposium
to: Harry Elderfield <he101@esc.cam.ac.uk>, m.hulme@uea.ac.uk,
h.j.schellnhuber@uea.ac.uk, "B.E. Launder" <mcjtsb1@fs4.umist.ac.uk>,
<simon.shackley@umist.ac.uk>, eaboyle@po12.mit.edu, John Shepherd
<j.g.shepherd@soc.soton.ac.uk>

John:

I would have thought Sir Alec Broers, President of the Royal Academy of Engineering and VC of Cambridge was the obvious person to open the conference. He could also enlist the considerable resources of the RAEng to provide contacts. (I'd be happy to interact with him if you wished but you and Harry have all this at your fingertips so I'd assume that a joint approach by you both might be best.)

Brian

> Harry et al.

>

> No problem with these suggestions, (but we shall need to identify
> someone specific to work on the communication to industry end) and I would
> be happy to approach Bob May or Ron Oxburgh, which would (all) you prefer ??

>

> I have just sent another message which bears on the publicity
> issue, as it's not completely straightforward....

>

> John

>

> At 12:18 10/11/2003 +0000, Mike Hulme wrote:

> >John and Asher,

> >

> >I didn't see you copied in on Harry's email, so I am forwarding this to
> >you both since there are issues here about organisation and publicity.

> >

> >Mike

> >

> >>Date: Thu, 6 Nov 2003 11:08:44 +0000

> >>Subject: Tyndall-CMI Symposium

> >>From: Harry Elderfield <he101@esc.cam.ac.uk>

> >>To: m.hulme@uea.ac.uk,

> >> H.J.Schellnhuber@uea.ac.uk

> >>X-Mailer: Apple Mail (2.552)

> >>

> >>Dear John and Mike

> >>

> >>I spoke to the head of CMI (Michael Kelly) a week or so ago and had a
> >>videoconference with Ed Boyle and the CMI programme manager at MIT
> >>yesterday. Here are some summary notes:

> >>

> >>(1) Kelly wants me to talk to the new CMI PR person who starts work next
> >>week to discuss publicity issues.

> >>

> >>(2) CMI is extremely keen to see industry involvement (the outreach
> >>component of CMI is being pushed strongly)

> >>

> >>(3) CMI wants to make sure it gets its due recognition!

> >>

> >>(4) They ask who will "open" the symposium. Suggestions by Kelly- Bob
> >>May; Ron Oxburgh.

> >>

> >>(5) Kelly and CMI emphasise they do not want to tell us what to do but do
> >>want to ensure that the "product" is communicated very efficiently to
> >>industry, government etc etc

> >>

> >>Clearly some coordination will be needed. I will attend to item (1) and
> >>report back.

cg2003

> >>
> >>Best wishes
> >>Harry
>

1558. 2003-11-14

date: Fri, 14 Nov 2003 10:30:39 +0000
from: Nick Brooks <nick.brooks@uea.ac.uk>
subject: Re: Science Article
to: dust-health <m.hulme@uea.ac.uk>, Tim Mitchell <t.mitchell@uea.ac.uk>
Mike

This seems OK as far as content is concerned - no obvious howlers, although a few comments on specific elements below. And it's good to see something going from the UK government to a US journal that tackles the Bush administration's deliberate sabotage of mitigation efforts. From a UK perspective the closing paragraphs seem rather tame, but considering the audience the pitch is probably appropriate. I know you requested comments just on the science, but I can't resist commenting on the role of this kind of material in the climate change debate, so I'll start with some general comments before moving onto specifics.

The article is very much along the lines of trying to persuade on the basis of scientific evidence, with implied political pressure (if we can imagine any pressure from the UK on the US being at all relevant). While this might strengthen the hand of those who are seeking action by the US administration, it to a certain extent is missing the point - the debate about climate change is only partly one about science and evidence; when dealing with those wielding power and influence, particularly in the US, we must recognise that this is an argument about ideology and vested interests, not science. A failure to recognise this is the reason that progress on the issue is so slow. We will not change the minds of those interests that are determined to ignore climate change by presenting more evidence - their collective will is set and will not be broken by scientific facts or moral arguments. Evidence is not sufficient persuasion and the question remains as to where leverage can be exerted. The US government demands proof of substantial future risk before it even considers acting on climate change. A threat to its national security has to be proved beyond all doubt, unlike in the spheres of terrorism and military security, where hundreds of billions are spent to deal with poorly defined, or undefined, threats that may just appear in the future. This fact alone should illustrate that the facts are more or less irrelevant. The US will only act to serve its immediate interests as perceived by a narrow governing elite, and it is only the discomfort of this elite that will change policy. Economic sanctions and political isolation might have some influence, gentle persuasion and presentation of evidence will not. David King will be pilloried in the right-wing media in the US (if they are aware of what is published in Science), and ignored by the administration. He is preaching only to the converted. I know this is bleak, but I believe this to be the nature of the situation. I'm not against the publication of such articles, indeed I applaud them, but we must not overestimate their impact.

A few specific comments follow.

I'm always wary of claims (p3) that we are entering a period of unprecedented warmth. I do not know what the mean global temperature was in the Holocene climatic optimum, but research suggests tropical sea-surface temperatures some 5-6 degrees higher than present. Even a smaller change would of course be catastrophic for many societies today, but unless there have been serious comparisons between today and the mid-Holocene and we can say with confidence that anthropogenic warming scenarios exceed such palaeoclimatic conditions such claims may come back to haunt us.

Later in p3 the role of rotting vegetation exposed by melting permafrost could also be included as a potential positive feedback.

There is a very brief mention of vulnerability on pp3-4, very much from the top-down climate impacts perspective, assuming no adaptation (reminds me of

cg2003

the work that demonstrates that the world will be bankrupt by 2065, when economic damage from climate-related disasters will exceed world GDP, based on current trends). There is a lot more that could be said about vulnerability, but perhaps the precipitation of conflict by water scarcity could be emphasised. This is likely to occur in all the regions that Americans are scared of - the Middle East, North Africa, the Sahel etc. This type of conflict tends to be internal, between different social groups within a country (eg nomads and settled farmers in Niger and Chad), and can lead to a political vacuum where all sorts of unsavoury characters can flourish - I believe the fashionable term is "failed state". Good conditions for al Qaeda and its ilk. That might catch some attention.

P4 - there is a statement quantifying reductions in flooding associated with carbon stabilisation - this would make a lot more sense if the associated timescale was specified.

The flood projections on p5 assume no coastal realignment, and is thus a bit like the bankrupt world example above - it illustrates a point but we cannot assume no adaptation. Also, are not some flood plain areas already, or about to become, uninsurable in the UK?

p6 - surely all coasts have the potential to experience erosion? Insertion of the word "significant" or "serious" might be good here.

pp7-8 The concept of emissions intensity might be more widely understood in the US than in the UK, but it is not transparent - indeed it was invented by the Bush administration deliberately to mislead people. I think total emissions should be referred to here, or the rate of increase of emissions.

p8 - The US government is not "unaccountably" failing to tackle global warming - the reasons it is failing to do so are obvious, and are to do with ideology and the self interest of those in and close to the administration.

p9 - Technology transfer and capacity building are not the holy grail for developing countries that many think. While they can help, responses to climate change are likely to be most successful if they are based on local conditions and indigenous traditions, for example of land management. Often the state and international institutions simply prevent people from adapting in a way appropriate to their circumstances - the solutions do not necessarily come from the developed world.

Finally, I would reiterate that this paper does not address the really important political obstacles to change. Furthermore, the evidence presented relates overwhelmingly to the UK - the fact that the UK will suffer will not convince those who need to be convinced in the US that action is needed, as they are not concerned with the impacts on other nations. It is hubris to think that the UK is sufficiently influential to have a significant impact on US policy, particularly acting in its tradition role as an "honest friend" of the US. It is better to work with the many groups within the US that share our concerns than to appeal to those at the top of this administration, although of course it is not an either/or situation.

I realise some of this sounds disheartening, but this is still a positive step in a very long journey, and I wish David King well in his efforts here. In the meantime I'll support mitigation, but pragmatically devote all my research efforts to adaptation!

Nick

--

Dr Nick Brooks
Tyndall Centre for Climate Change Research
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
Tel: +44 1603 593904
Fax: +44 1603 593901
Email: nick.brooks@uea.ac.uk
<http://www.cru.uea.ac.uk/~e118/welcome.htm> (personal site)
<http://www.tyndall.ac.uk> (Tyndall Centre site)
<http://www.uea.ac.uk/sahara> (Saharan Studies Programme)

--

On 13/11/03 9:33 am, "Mike Hulme" <m.hulme@uea.ac.uk> wrote:

cg2003

> Tim and Nick,
>
> Sir David King - government chief scientist - has asked the Tyndall Centre (me
> et al.) to check and comment on this draft manuscript which he is planning to
> publish in Science ahead of the high-level climate change seminar next
> February in Washington he is chairing and speaking at (to try to knock a few
> American heads together about climate change).
>
> As a prelude to our work together for DEFRA on stabilisation projects, could
> both of you have a read through his text and let me have your comments by the
> end of Friday (i.e., tomorrow). Obviously the tone and message are his - what
> he wants us to make sure is that he has made no factual errors and that the
> referencing is as strong as it can be.
>
> I am going to put together my response on Saturday, so would appreciate any
> i/p from you before then.
>
> Many thanks,
>
> Mike
>
>> From: King MPST <MPST.KING@dti.gsi.gov.uk>
>> To: "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>
>> Subject: Science Article
>> Date: Wed, 12 Nov 2003 09:21:32 -0000
>> X-Mailer: Internet Mail Service (5.5.2656.59)
>>
>> Dear Professor Hulme
>>
>> Please find attached the draft science article by Sir David. Many thanks for
>> agreeing to comment and helping us to identify the references.
>>
>> <<Science policy forum.2.doc>> <<sciencefigures.ppt>>
>>
>> Regards
>>
>> Michael Evans
>>
>> _____
>> Michael Evans
>> Private Secretary to Sir David King
>> Chief Scientific Adviser to H. M. Government
>> Room 472
>> Office of Science and Technology
>> 1 Victoria Street
>> London
>> SW1H 0ET
>>
>> Tel: ++ 44 (0) 20 7215 3824
>> Fax: ++ 44 (0) 20 7215 0314
>>
>>
>>
>>
>

1864. 2003-11-14

cc: Mike Hulme <m.hulme@uea.ac.uk>
date: Fri, 14 Nov 2003 16:25:48 +0000
from: Tim Mitchell <t.mitchell@uea.ac.uk>
subject: Droughts paper
to: Sari Kovats <Sari.Kovats@lshtm.ac.uk>

cg2003

Sari,
Regional information on regions at risk

Sahel:

This is critically dependent on the extent to which the West African Monsoonal rains penetrate inland. Changes in patterns of rainfall distribution (inland v coastal) may be critical and are hard to predict. The models do not show a consistent sign in the region of interest (TAR WGI Fig 10.6, Box 10.1 Fig 2).

Southern Africa:

The source of rain depends on the area concerned, since the land lies at the meeting point of oceans. Most rain is during summer (DJF), for which the models do not show a consistent sign in the region of interest (TAR WGI Fig 10.6, Box 10.1 Fig 2).

India:

The Southern Asian Monsoon dominates of course. Monsoonal changes are difficult to predict without a firmer grasp of (a) how the monsoon is connected to El Nino and (b) how El Nino will change. However, the models consistently suggest a small increase in precipitation (TAR WGI Fig 10.6, Box 10.1 Fig 2). The distribution of any change will, of course, be critical.

This may not be relevant, but on the other hand, you might be interested in:
<http://www.cru.uea.ac.uk/~timm/movies/index.html>

Regards

Tim

Dr. Tim Mitchell
Tyndall Centre for Climate Change Research
email: t.mitchell@uea.ac.uk
web: <http://www.cru.uea.ac.uk/~timm/>
phone: +44 (0)1603 59 1378 = CHANGED JULY
fax: +44 (0)1603 59 3901
post: Tyndall, ENV, UEA, Norwich, NR4 7TJ, UK

4095. 2003-11-14

cc: Mike Hulme <m.hulme@uea.ac.uk>
date: Fri, 14 Nov 2003 16:35:28 +0000
from: Tim Mitchell <t.mitchell@uea.ac.uk>
subject: Flooding paper
to: Sari Kovats <Sari.Kovats@lshtm.ac.uk>

Sari,

Quantification of uncertainty in precip projections:

This must be regionally specific to be useful, and lies beyond the scope of this background document. I advise researchers to quantify for themselves using:

http://www.cru.uea.ac.uk/~timm/grid/TYN_SC_2_0.html

Which countries/regions most affected:

This is not a relevant question unless it is contextually embedded. A country with currently low rainfall but a small increase in the future may be 'more affected' than a country with high rainfall and the same small increase. Again, individual regions must be examined.

Probabilistic approaches:

Yes - I agree, but there is no global data available. Or even continental data. For the UK, see:

Osborn TJ and Hulme M (2002) Evidence for trends in heavy rainfall events over the United Kingdom. Philosophical Transactions of the Royal Society London series A 360, 1313-1325

Asian monsoon:

Summary from IPCC WG1 TAR p568

" One of the most significant aspects of regional interannual variability is the Asian Monsoon. Several recent studies (Kitoh et al., 1997; Hu et al., 2000a; Lal et al., 2000) have corroborated earlier results (Mitchell et al.,

cg2003

1990; Kattenberg et al., 1996) of an increase in the interannual variability of daily precipitation in the Asian summer monsoon with increased greenhouse gases. Lal et al. (2000) find that there is also an increase in intra-seasonal precipitation variability and that both intra-seasonal and inter-annual increases are associated with increased intra-seasonal convective activity during the summer. Less well studied is the Asian winter monsoon, although Hu et al. (2000b) find reductions in its intensity with a systematic weakening of the north-easterlies along the Pacific coast of the Eurasian continent. However, they find no change in the interannual or inter-decadal variability.

"The effect of sulphate aerosols on Indian summer monsoon precipitation is to dampen the strength of the monsoon compared to that seen with greenhouse gases only (Lal et al., 1995; Cubasch et al., 1996; Meehl et al., 1996; Mitchell and Johns 1997; Roeckner et al., 1999), reinforcing preliminary findings in the SAR. The pattern of response to the combined forcing is at least partly dependent on the land-sea distribution of the aerosol forcing, which in turn may depend upon the relative size of the direct and indirect effects (e.g., Meehl et al., 1996; Roeckner et al., 1999). There is still considerable uncertainty in these forcings (Chapter 6). To date, the effect of aerosol forcing (direct and indirect) on the variability of the monsoon has not been investigated.

"In summary, an intensification of the Asian summer monsoon and an enhancement of summer monsoon precipitation variability with increased greenhouse gases that was reported in the SAR has been corroborated by new studies. The effect of sulphate aerosols is to weaken the intensification of the mean precipitation found with increases in greenhouse gases, but the magnitude of the change depends on the size and distribution of the forcing."

regards

Tim

Dr. Tim Mitchell
Tyndall Centre for Climate Change Research
email: t.mitchell@uea.ac.uk
web: <http://www.cru.uea.ac.uk/~timm/>
phone: +44 (0)1603 59 1378 = CHANGED JULY
fax: +44 (0)1603 59 3901
post: Tyndall, ENV, UEA, Norwich, NR4 7TJ, UK

4151. 2003-11-14

date: Fri, 14 Nov 2003 09:17:03 -0700
from: Tom Wigley <wigley@cgd.ucar.edu>
subject: brief question
to: Ben Santer <santer1@llnl.gov>, "Karl E. Taylor" <taylor13@llnl.gov>, Keith Briffa <k.briffa@uea.ac.uk>, Jerry Meehl <meehl@ucar.edu>, Jerry Mahlman <jmahlman@ucar.edu>, Steve Smith <:ssmith@pnl.gov>, rrichels <rrichels@epri.com>, Richard Moss <richard.moss@pnl.gov>, Steve Schneider <shs@stanford.edu>, Bob Harriss <harriss@ucar.edu>, Bob Watson <Rwatson@worldbank.org>, Bill Gutowski <gutowski@iastate.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Alan Robock <robock@envsci.rutgers.edu>, Caspar M Ammann <ammann@ucar.edu>, Chick Keller <cfk@vega4.llnl.gov>, covey1@llnl.gov, DANNY HARVEY <Harvey@geog.utoronto.ca>, Dave Schimel <schimel@ucar.edu>, Haroon Khehsgi <haroon.s.kheshgi@exxonmobil.com>, "James A. (Jae) Edmonds" <jae@pnl.gov>, Jane Leggett <Leggett.Jane@epamail.epa.gov>, Joel Smith <JSmith@stratusconsulting.com>, Linda Mearns <lindam@ucar.edu>, Martin Manning <mmanning@aol.noaa.gov>, Martin Parry <parrym1@aol.com>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Marty Hoffert <mih1@bellatlantic.net>, "Michael E. Mann" <mem6u@multiproxy.evsc.virginia.edu>, Michael Oppenheimer <omichael@princeton.edu>, Michael Schlesinger <scheslin@uiatma.atmos.uiuc.edu>, Mike Hulme <m.hulme@uea.ac.uk>, Mike MacCracken <maccrac@comcast.net>, Peter Backlund <backlund@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Raymond Bradley <rbradley@geo.umass.edu>, Tim Osborn

cg2003

<t.osborn@uea.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, Tim Carter
<Tim.Carter@ymparisto.fi>, Naki Nakicenovic <naki@iiasa.ac.at>, Hugh M Pitcher
<hugh.pitcher@pnl.gov>, Warren Washington <wmw@ucar.edu>, Ron Stouffer
<rjs@gfdl.gov>, Steve Fetter <sfetter@wam.umd.edu>, "simon.shackley"
<simon.shackley@umist.ac.uk>, Tom Wigley <wigley@ucar.edu>
</x-flowed>

Dear all,

I have had a disagreement with someone about a statement they made in
which

I was mentioned. When I read this, I thought it implied that I was
endorsing their

view. The statement is given below together with two questions. For each
question

all I want is a YES, NO or MAYBE answer

>For the record, while we think TAR erred in allowing new storylines

>rather than new science (as Tom Wigley has pointed out) to drive a

>new upper limit to the temperature range ...

Q1: Do you think this implies that I endorse the claim that the TAR

(i.e., IPCC)

erred?

Q2: Do you think this amounts to an accusation that the TAR (IPCC) used the
SRES scenarios because they produced a higher upper-bound warming

than

previously?

In question 2, I am not asking about the truth of the 'accusation', but
whether or

not the statement could be construed as an accusation. The key word in the
statement is 'allowing'.

Thanks for your response,

Tom.

</x-flowed>

3185. 2003-11-16

cc: Ben Santer <santer1@llnl.gov>, "Karl E.Taylor" <taylor13@llnl.gov>, Keith
Briffa <k.briffa@uea.ac.uk>, Jerry Meehl <meehl@ucar.edu>, Jerry Mahlman
<jmahlman@ucar.edu>, Steve Smith <ssmith@pnl.gov>, rrichels <rrichels@epri.com>,
Richard Moss <richard.moss@pnl.gov>, Steve Schneider <shs@stanford.edu>, Bob
Harriss <harriss@ucar.edu>, Bob Watson <Rwatson@worldbank.org>, Bill Gutowski
<gutowski@iastate.edu>, Malcolm Hughes <mhughes@lrr.arizona.edu>, Alan Robock
<robock@envsci.rutgers.edu>, Caspar M Ammann <ammann@ucar.edu>, Chick Keller
<cckf@vega4.lanl.gov>, covey1@llnl.gov, DANNY HARVEY <Harvey@geog.utoronto.ca>, Dave
Schimel <schimel@ucar.edu>, Haroon Khehsgi <haroon.s.kheshgi@exxonmobil.com>,
"James A. (Jae) Edmonds" <jae@pnl.gov>, Jane Leggett
<Leggett.Jane@epamail.epa.gov>, Joel Smith <JSmith@stratusconsulting.com>, Linda
Mearns <lindam@ucar.edu>, Martin Manning <mmanning@al.noaa.gov>, Martin Parry
<parrym1@aol.com>, Kevin Trenberth <trenbert@cgd.ucar.edu>, "Michael E. Mann"
<mem6u@multiproxy.evsc.virginia.edu>, Michael Oppenheimer <omichael@princeton.edu>,
Michael Schlesinger <schlesin@uiatma.atmos.uiuc.edu>, Mike Hulme
<m.hulme@uea.ac.uk>, Mike MacCracken <mmaccrac@comcast.net>, Peter Backlund
<backlund@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Raymond Bradley
<rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk>, "tim.carter"
<tim.carter@vyh.fi>, Tim Carter <Tim.Carter@ymparisto.fi>, Naki Nakicenovic
<naki@iiasa.ac.at>, Hugh M Pitcher <hugh.pitcher@pnl.gov>, Warren Washington
<wmw@ucar.edu>, Ron Stouffer <rjs@gfdl.gov>, Steve Fetter <sfetter@wam.umd.edu>,
"simon.shackley" <simon.shackley@umist.ac.uk>, Tom Wigley <wigley@ucar.edu>,
grubler@yale.edu, riahi@amazon.iiasa.ac.at
date: Sun, 16 Nov 2003 11:16:04 -0500
from: Marty Hoffert <mih1@bellatlantic.net>
subject: Re: brief question

to: Nebojsa Nakicenovic <naki@eeg.tuwien.ac.at>, Tom Wigley <wigley@cgd.ucar.edu>

Naki & Tom et al:

It may not be useful to express uncertainties of future climate change in terms

of combined uncertainties of atmospheric physics and SRES emissions projections based on forecasts of social-economic-technology evolution in the 21st century. The SRES authors were right in my opinion not to assign probabilities to their 40 scenarios. The mere existence of a possible emission path shouldn't effect climate change uncertainty estimates if it's probability can't be estimated by a rational and tested methodology. Sorry, Steve Schneider, I usually agree with you, but not on this.

Issac Asimov, in his classic SF Foundation novels -- space operas about a future human galactic civilization roughly modeled on "The Decline and Fall of the Roman Empire" -- invented the hypothetical science of "psychohistory" with which statistical probabilities of different futures were calculable. We'd probably call these these probability distribution functions (pdfs) now. Asimov's idea was plausible in the 50s and 60s, before we knew about nonlinear dynamics and chaos theory. Paleontologist Steven Jay Gould, in *Wonderful Life: The Burgess Shale and the Nature of History*, reminds us that history is contingent on apparently small random events with large future consequences. Rewind the tape of life and replay it, and evolution might not lead to *Homo sapiens* sapiens, and we wouldn't be here asking these questions. A counter-argument is that weather is unpredictable beyond a short time horizon; but climate (perhaps an "attractor" for weather states) is predictable. That's why we're in this business. So might the probability of our future carbon emissions be predictable, in principle. Proponents of the "Many-worlds" interpretation of quantum mechanics (see, e.g., J. Richard Gott's *Time Travel in Einstein's Universe*) would argue that all 40 SRES storylines, along with countless others, actually exist in a spacetime multiverse of parallel universes, into which reality is constantly splitting. (In the classical quantum mechanics experiment, and according to "Many worlds," a single photon goes through two separate slits by splitting into parallel universes and reemerging in ours). The wavefunctions of the SRES parallel universes are roughly analogous to their pdfs. I think you will all agree that we're a long way from being able to compute these, though one can't rule it out in the fullness of cosmic time. It does pose some interesting problems relating to free will, the role of humans in the cosmos, etc.

It seems more productive to focus on atmospheric science uncertainties -- like climate sensitivity -- which we can estimate not only from climate models but from paleoclimate records (see, e.g., Hoffert & Covey attached). These haven't changed for decades from the 1.5 to 4.5 degrees Celsius steady state warming for a CO2 doubling (this uncertainty range resulting mainly from cloud radiative feedback uncertainties). These can be

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linked to allowable carbon emissions and CO₂-emitting energy production and energy demand implied by projected growth of GDP and declining energy intensity (E/GDP). The shortfall between the allowable energy from fossil fuels necessary to keep global warming below some specified level and total energy demand has to be made up by new emission-free-energy technologies, including the effect of climate sensitivity uncertainties. An analysis of this problem by Ken Caldeira, Atul Jain, and me was published last March in Science (attached). Comments most welcome.

Cheers,

Marty Hoffert

Professor of Physics

Andre and Bella Meyer Hall of Physics

Room 525, Mail Code 1026

4 Washington Place

New York University

New York, NY 10003-6621

NYU Phone: 212-998-3747

NYU Fax: 212-995-4016

Home Phone: 516-466-9418

Home Fax: 516-487-0734

Cellphone: 516-972-4779

Email: marty.hoffert@nyu.edu

Web page: <http://www.physics.nyu.edu/people/hoffert.martin.html>

At 9:34 PM +0100 11/14/03, Nebojsa Nakicenovic wrote:

Dear Tom and Colleagues,

It is not easy to respond to your request. At face value, my reaction to Q1 is YES and

to Q2 and undecided MAYBE.

However, the original statement at which the questions are directed is at best misleading. In my view it is simply wrong. First, storylines have little to do with the

actual SRES emissions. The primary determinant of the ranges of emissions (the full

range and not only the upper values) was primarily the body of emissions scenarios

literature. Storylines were used as a tool for framing the driving forces and their

relationships for six integrated assessment models that developed the scenarios. In TAR,

nine different integrated models were used. There are probably two dozen or so multi-regional integrated models in the world. The six SRES (and nine TAR) models were

representative of different modeling approaches including bottom-up and top-down. Thus,

jointly they do not have any obvious upward or downward bias with respects to future

emissions. It is quite curious to see this research effort reduced to "storylines"

versus "science" which appears to imply that all the demographic, economic, engineering,

etc. approaches that converge in integrated models, emissions scenarios and the

underlying literature are labelled as something else than "science". I challenge

anyone to produce a literature assessment that results in reduced range of future

emissions compared to SRES. In fact, the opposite would be the case, the full range in

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the literature is broader than SRES. The 40 SRES scenarios jointly cover about 5th to 95th percentile of the frequency distributions of driving forces and emissions in the literature. This unequivocally means that the highest emissions scenarios are substantially higher than SRES and that there are some mitigation scenarios that are substantially lower than the SRES range.

The question (Q3) that I would like to ask is what is the "new science" that is so much different from SRES and other scenarios in the literature? As we all know, it was not the SRES scenarios that determined the range of temperature change, but rather indeed GCMs and simple climate models. My understanding is that only about half of this uncertainty is due to scenarios and the other half is due to the climate uncertainties.

Curiously, the difference is that the emissions uncertainties can be reduced through mitigation. In other words, about half of the TAR temperature range could be reduced through mitigation measures and policies.

Best regards, Naki

At 09:17 AM 11/14/2003 -0700, Tom Wigley wrote:

Dear all,

I have had a disagreement with someone about a statement they made in which I was mentioned. When I read this, I thought it implied that I was endorsing their view. The statement is given below together with two questions. For each question

all I want is a YES, NO or MAYBE answer

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>new upper limit to the temperature range ...

Q1: Do you think this implies that I endorse the claim that the TAR (i.e., IPCC) erred?

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In question 2, I am not asking about the truth of the 'accusation', but whether or

not the statement could be construed as an accusation. The key word in the statement is 'allowing'.

Thanks for your response,
Tom.

Attachment Converted: "c:\eudora\attach\Caldeira_et alScience .pdf" Attachment Converted:

"c:\eudora\attach\Hoffert&Covey_Nature-92.pdf"

4224. 2003-11-18

date: Tue, 18 Nov 2003 10:59:03 -0000
from: malcolm eames <malcolm.eames@NTLWORLD.COM>
subject: SDR-Network Mailing: 18 November 2003
to: SDRN@JISCMAIL.AC.UK

Contents:

1. RURAL DELIVERY REPORT
2. DFT HORIZONS RESEARCH CALL
3. ESRC RESEARCH SEMINARS COMPETITION
4. GREENING GOVERNMENT 2003 NEW EAC REPORT
5. FUTURE OF UK ENVIRONMENTAL SCIENCES

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6. LSE CENTRE FOR ENVIRONMENTAL POLICY AND GOVERNANCE
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SOCIETY UCL
9. SUSTAINABLE INNOVATION 04 - C A L L F O R P A P E R S
10. SOCIAL MARKET FOUNDATION SEMINAR: 'SHARING RESPONSIBILITY FOR OUR PUBLIC
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11. Policy Studies Institute job opportunities
12. CORPORATE PROGRAMMES OFFICER AT EARTHWATCH EUROPE
13. HEAD, TECHNICAL SUPPORT FOR IPCC
14. INSTITUTE OF ENERGY AND SUSTAINABLE DEVELOPMENT
15. MARIE CURIE OPPORTUNITIES FOR DOCTORAL STUDENTS
16. RESEARCHER - THE CLIMATE GROUP/CONFERENCE OF THE REDUCERS
The SDR-Network Mailing is an information resource and dissemination service for
SDR-Network members. You can make use of this service by posting details of
forthcoming
events, funding opportunities, job vacancies, research outputs, policy
developments,
consultations, etc to SDR-Network Coordinator, Malcolm Eames
([1]m.eames@psi.org.uk). If
you send an item for inclusion in the Mailing and would like list members to be
able to
contact you directly you must include your email address in the body of the
item. The SDNRN
list is a moderated list. For SD web resources see - [2]www.sd-research.org.uk
or
[3]www.sustainable-development.gov.uk

1. RURAL DELIVERY REPORT
In November 2002, Margaret Beckett, Secretary of State for Environment, Food and
Rural
Affairs, invited Lord Haskins to carry out an independent review of the
arrangements for
delivering government rural policies in England. Lord Haskins report, which
includes 33
separate recommendations for improving delivery of rural policies, has now been
completed
and was published on 11 November 2003. For details see
[4]<http://www.defra.gov.uk/rural/ruraldelivery/default.htm>
DEFRA has begun work on a detailed response, which will be published in the
Spring of next
year. Comments or suggestions can be sent by e-mail to the Defra implementation
team at:
[5]ruraldeliveryprogramme@defra.gsi.gov.uk. It may not be possible to answer
every e-mail
individually, but the Q&A will be updated regularly with answers to the
questions we
receive. Please check online for these regular updates, which can be found in
the Rural
Delivery Review section of our website.
2. DFT HORIZONS RESEARCH CALL
The DfT is looking to support innovative research about future challenges and
opportunities
with a horizon of between 10 and 30 years. Proposals are sought that either: a)
research
issues that may affect meeting, in the longer term, DfT's objective, or b) look
at the
specific impacts on transport of new technologies.
The first Call for the new programme is now on the science and research pages of
the DfT

website. This gives fuller details of the type of research proposal DfT are interested in and how to express interest in the programme. News about the progress of the programme and supported projects will be published on the website regularly. See:

[6]http://www.dft.gov.uk/stellent/groups/dft_science/documents/page/dft_science_025627.hcsp

3. ESRC RESEARCH SEMINARS COMPETITION

Information regarding the 2003/4 call for the ESRCs annual seminar competitions is now available on the ESRC website at:

[7]<http://www.esrc.ac.uk/ESRCContent/researchfunding/seminars.asp>

4. GREENING GOVERNMENT 2003 NEW EAC REPORT

Greening Government is the process of incorporating environmental objectives in both operational aspects of departmental performance and policy appraisal and development. As

part of its audit role in holding departments to account, the Environmental Audit Committee

has published its latest report on this topic, Greening Government 2003, HC 961 of Session

2002-03. The Committee's report, Greening Government 2003, is available on the Committee's website at

[8]www.parliament.uk/parliamentary_committees/environmental_audit_committee.cfm.

It offers a critical analysis of the Governments Sustainable Development in Government: First

Annual Report, published in November 2002, which is at

[9]<http://www.sustainable-development.gov.uk/sdig/reports/index.htm>

5. FUTURE OF UK ENVIRONMENTAL SCIENCES

The final report of the review of the future of the environmental science in the UK, commissioned by the Environmental Research Funders Forum is now available together with the

Forums initial responses to the recommendations made. See

[10]www.erff.org.uk/whatsnew.asp

6. LSE CENTRE FOR ENVIRONMENTAL POLICY AND GOVERNANCE

The LSE have established a new Research Centre for Environmental Policy and Governance. The

Director is Professor Yvonne Rydin and enquiries can be made to

Y.Rydin@lse.ac.uk or [11]environ.policy@lse.ac.uk Further details can be found at

[12]<http://www.lse.ac.uk/collections/geographyAndEnvironment/CEPG/>

Among two of the first public events organised by the Centre are:

- a talk by the Canadian Ambassador for the Environment on Canada's approach to sustainable

development (at 1.00pm on Thursday 20th November in D302 at LSE); and

- a debate on GMOs and agricultural policy with a panel comprising Michael

Meacher, Ben

Gill, Chris Pollack, George Gaskell and Tim Dyson (at 6.30pm on Thursday 27th November in

the Old Theatre at LSE).

All are welcome

7. EARTHSCAN HAS A NEW HOME

Earthscan was originally founded by the International Institute for Environment and

Development, and for the last 12 years we have been a subsidiary of Kogan Page, a publisher

of business and management books. Earthscan has now joined forces with James & James, an

environmental technology publisher in order to create a more focused, dynamic

and effective

publisher on environmental and development issues. Earthscan staff and operations will all shortly be moving to the J&J offices in Camden, London. Earthscan plan to expand their publishing to meet the demands of the constituencies they serve through the range of media that J&J already publish in, and which include directories and reference works, journals and magazines, as well as books.

If you would like to receive future announcements from Earthscan, including new titles and

special offers, please ensure that you are on our e-newsletter mailing list by subscribing

at [13]www.earthscan.co.uk

8. 'HAPPINESS AND PUBLIC POLICY' - INTERNATIONAL CENTRE FOR HEALTH & SOCIETY UCL 2003 Seminar Series

Monday 1 December 5.00pm

RSVP attendance essential

Richard Layard, Director of the Programme on Well-Being, London School of Economics

Abstract: Despite economic growth, longer holidays and better health, happiness has

stagnated in Britain over the last 50 years. The main reason has been excessive focus on

incentives for individual wealth creation and inadequate focus on the practical ways in

which misery can be reduced and happiness increased. Policy implications include the

following.

I. Since much income generation is aimed at improving relative income (a zero-sum game),

taxation is less inefficient than is usually supposed.

II. Far more resources should go on treating mental illness, and on related research.

III. There is no need to increase mobility, which increases crime and damages families.

IV. Excessive individualism generates anxiety and should be replaced by a new commitment to

the common good (ie the greatest happiness of all).

V. Social science research should be refocused towards explaining happiness and what we can

do to affect it.

RSVP seminar attendance by 28/11/03, E-mail: [14]ichs@public-health.ucl.ac.uk (indicating any special needs and for directions to the seminar room)

See also [15]www.ucl.ac.uk/epidemiology

9. SUSTAINABLE INNOVATION 04 - C A L L F O R P A P E R S

Creating and developing sustainable and responsible new business models

Towards Sustainable Product Design 9

9th International Conference

25th - 26th October 2004

Bush Hotel, Farnham, Surrey, UK

Conference topics

Sustainable Innovation 04 welcomes conceptual and research-based papers covering sustainable and responsible business innovation in the context of a range of issues:

- * Market development
- * Technology development
- * New business models
- * Entrepreneurship
- * Stimulating innovation
- * Products and service development
- * Product Service Systems (PSS)

- * Marketing and communications
- * Business development
- * Co-development
- * Network management

Living laboratory

Sustainable Innovation 04 welcomes blue-sky, thought-provoking, radical concepts and ideas

with an emphasis on new business models rather than solutions focused on 'business as

usual'. Submission details: please email, fax or post 500 words describing your proposed

paper by 31st January 2004. The paper will then be sent to the Advisory Board for

evaluation and authors will be given feedback by the end of February.

Living laboratory: please email, fax or post 2 pages that a) visualise, b) describe your

proposed sustainable business, product, service or PSS concept and c) outline the

financial, social and environmental impacts (both positive and negative) of your idea.

Please send your outline by 31st January 2004. Proposals will then be sent to the Advisory

Board for evaluation and feedback will be returned by the end of February with successful

entrants asked to present their concepts at the event. A template will be downloadable from

[16]www.cfsd.org.uk/events/tspd9

For full details of this call for papers see: [17]www.cfsd.org.uk

10. SOCIAL MARKET FOUNDATION SEMINAR: 'SHARING RESPONSIBILITY FOR OUR PUBLIC SPACES

Speaker: Rt. Hon Alun Michael, MP

Tuesday 11th December 2003, 1-2.30 pm

Social Market Foundation, 11 Tufton Street, London SW1

On launching the Defra consultation document earlier this year, Alun Michael remarked that

'successful, thriving and prosperous communities are characterized by streets, parks and

open spaces that are clean safe and attractive.tackling failure such as litter graffiti,

etc is for many the top public service priority'. In this seminar, he will be looking at

how government, business, the voluntary sector and other interested parties can collaborate in making our public spaces 'fit for habitation'.

To attend this seminar contact Annette Bullen, at the Social Market Foundation on 020 7227

4401 or email [18]abullen@smf.co.uk

11. Policy Studies Institute job opportunities

A wholly-owned subsidiary of the University of Westminster

Environmental Policy Research

Research Fellow: £21,379-£32,424 pa incl LWA

Research Officer: £19,139-£24,804 pa incl LWA

The Policy Studies Institute Institute (PSI), one of Britains leading social and economic

research institutes, is expanding its Environment Group. The Group is led by Professor Paul

Ekins and includes Professor Jim Skea, Director of the Institute. The Group takes a

problem-focused, interdisciplinary approach. The approach to research is team-based,

problem-focused and interdisciplinary. Successful candidates will be appointed to the

Institutes permanent staff and may work on a number of different projects.

We are making two types of appointment. We are looking for a Research Fellow

with

quantitative analytical skills relating to the environment and sustainable development, especially those linking energy, the environment and the economy, such as econometrics,

inputoutput analysis and energy-environment modelling. This person would be expected to

work initially on a European project addressing the implications for industrial competitiveness of environmental tax reform (COMETR).

The other appointment will be for a Research Officer to work on a project which is being

taken forward as part of the Sustainable Hydrogen Economy Consortium (SHEC), one of four

such consortia funded over four years by the Engineering and Physical Sciences Research

Council (EPSRC). The work will entail keeping up to date with developments in all aspects

of development in hydrogen technology, contributing to the characterisation of that

technology and helping to develop scenarios that envisage it becoming a major fuel. The

work will also entail some basic project administration. Other work packages in the project

will be developed as the Consortium timetable unfolds.

We are looking for people with broad research experience and quantitative skills relating

to environmental economics and/or policies. The Research Fellow will have a post-graduate

degree in a relevant subject, or a good first degree and several years research experience.

The Research Officer will have a good first degree in a relevant discipline and may also

have a post-graduate qualification.

For an application form and further details, visit [19]www.psi.org.uk or contact

Hilary

Salter, Policy Studies Institute, 100 Park Village East, London NW1 3SR, Tel:

020-7468-2219, e-mail: [20]salterh@psi.org.uk. Informal enquiries should

initially be made

via Hilary Salter. Closing date: 5pm Friday 5 December 2003

12. CORPORATE PROGRAMMES OFFICER AT EARTHWATCH EUROPE

Earthwatch has an active programme working with the corporate sector, aiming to promote

awareness of, and action on, environmental and sustainability issues throughout companies

and their employees. We engage with senior corporate managers, hold a series of events,

produce publications and playing the role of an informed 'stakeholder' in our member

companies. We are working on a portfolio of projects and partnerships to support and

promote corporate action for biodiversity on an international scale.

We are looking for an bright and highly motivated team player with an interest in corporate

social responsibility and environmental sustainability to work in our Corporate Programmes

team. Applicants should be able to demonstrate an ability to work with partner and donor

organisations at a range of levels, and have an understanding of the interface between the

private and NGO sector. The ability to juggle multiple projects and meet deadlines is

important, as are IT and admin skills. The role will require hands-on management of ongoing

activities, as well as taking part in developing Earthwatch's overall role within the corporate sector. Candidates must have excellent written and spoken English and a second European language would be an advantage.

This is a permanent full time position with opportunities for the right person to develop significantly within the role.

Location: Oxford, UK

Salary: £19,000 (tbc)

Application deadline: Thursday 27th November

Send CV to: David Davies, Personnel Officer ddavies@earthwatch.org.uk

Tel: +44 (0)1865 318874

13. HEAD, TECHNICAL SUPPORT FOR IPCC

£45,257 -£67,885

Permanent post (or possibly a 4-year secondment) at the Met Office, Exeter

The Met Office is the UK's national meteorological service, providing weather forecasts, observations, climate and environmental data for a variety of

customers.

The Met Office's Hadley Centre for Climate Prediction and Research is seeking a senior

scientist with excellent management and organisational abilities to lead the Technical

Support Unit for Working Group II of the Intergovernmental Panel on Climate Change

(IPCC). This unit has the responsibility for co-ordinating international teams of experts

in the preparation of IPCC assessment reports of knowledge on impacts, adaptation and

vulnerability to climate change. IPCC assessment reports are accepted worldwide as one of

the most authoritative sources of information on climate change for policy makers and

experts in academia, government and industry. Fields of particular relevance include:

water, health, agriculture and biodiversity; experience in climate impacts/adaptation in

one or more of these and other areas would be essential. You must have: a good degree in a

relevant scientific subject and a PhD, or equivalent post-graduate research experience, in

at least two or more aspects of climate change impacts assessment or adaptation to climate

change;

at least 10 years experience in both management and research, including at least 2 years at

the international level. The Met Office offers a choice of final salary and stakeholder

pensions, giving you the flexibility to choose the pension that suits you best. Career

prospects for non-UK nationals may be limited. Staff have a mobility commitment. The Met

Office is

an Equal Opportunities employer. Visit our web site [21] www.metoffice.com

The closing date for applications is 21 November 2003.

Interviews will be held during December 2003.

Application forms and further information:

Capita RAS Innovation Court New Street

Basingstoke Hampshire RG21 7JB

Tel: 01325 745500 (24 hours) [22] www.capitaras.co.uk

Please quote reference B7407

14. INSTITUTE OF ENERGY AND SUSTAINABLE DEVELOPMENT

Junior Research Fellow: Community Energy Initiatives

cg2003

£11,962 - £21,125 pa (appointment likely to be made from £17,624 - £20,311 pa)

Pay award pending

Fixed Term for two years, commencing January 2004 or as soon as feasible thereafter

The Institute is a founder member of the prestigious Faraday Partnership for Integration of

New and Renewable Energy in Buildings and seeks to make a worthwhile and significant

contribution to sustainable development through multidisciplinary research, consultancy and

learning provision.

A highly motivated social scientist is required to play a leading role in the ESRC

Sustainable Technologies Programme project Community Energy Initiatives: Embedding

Sustainable Technology at the Local Level. This project involves collaboration between De

Montfort University, Staffordshire University and the University of Northumbria and is

supported by a range of public, private and voluntary sector organisations.

Your role will be to evaluate the role of community initiatives in the embedding of

sustainable energy technologies in the UK. You will examine the emergence of community-orientated programmes within national policy, the conditions under which

different forms of community energy project have been developed, the interpretation of

'community' within these initiatives (including more dispersed 'communities of interest'),

the extent to which aims and outcomes are being achieved and the factors promoting and

obstructing their success.

Candidates will have good undergraduate and preferably post-graduate degrees in a relevant

social science discipline (preferably psychology, geography, politics, sociology or

environmental studies). Expertise and experience in the use of both qualitative and

quantitative methods are essential. Knowledge or experience of social and behavioural

aspects of energy, community processes and the uptake of new technologies, are also

desirable.

Application forms and further details are available from:

The Human Resources Team

De Montfort University

The Gateway

Leicester LE1 9BH

Tel: 0116 250 6433 (24 hours answerphone)

To apply on line visit our website: [23]<http://www.dmu.ac.uk>

Please Quote Ref: 2978

Closing Date: 1 December 2003 and Interviews will be held on 9 December 2003

15. MARIE CURIE OPPORTUNITIES FOR DOCTORAL STUDENTS

Fondazione Eni Enrico Mattei (FEEM) is welcoming applications from doctoral students

wishing to develop their studies in the analysis of environmental policies within the

framework of the Marie Curie Training Site bursaries. FEEM is a non-profit, non-partisan

research institution established to carry out research in the field of sustainable

development. Deadline for sending applications: 08 December 2003

[24]www.feem.it/Feem/Pub/Conferences/Programmes/mariecurie.htm

16. RESEARCHER - THE CLIMATE GROUP/CONFERENCE OF THE REDUCERS

Researcher Weybridge, Surrey, UK

The Climate Group (working title) is a new non-profit organisation currently in the start-up phase. Founded by the Rockefeller Brothers Fund following a successful Conference of the Reducers meeting in the Hague earlier this year, the organisation will seek to catalyse action among governments and companies to address the challenge of global climate change. The organisation will achieve this through the promotion of peer networking among the leading entities (government and corporate) on reducing greenhouse gas emissions, the identification and documenting of best practice in an accessible form, advocacy and communications initiatives. The organisation, which has received considerable interest from prospective member organisations, partners and funders, will hold a London launch event in early 2004 and a second major Conference of the Reducers in Toronto in May 2004 (in partnership with the City of Toronto). We are now looking for a highly motivated researcher to support this exciting new initiative. It is envisaged that the role will involve the following tasks.

- Research into current leaders in the field of emissions reductions at corporate and national/state/local/city government level;
- Tracking climate change initiatives and developments globally;
- Assistance in maintaining a contact database of member organisations and key stakeholders;
- Compilation of a series of case studies covering examples of best practice in reducing greenhouse gas emissions, to form the basis of a How to resource for policymakers and corporate officers globally;
- Compilation of a series of guides to technological, energy efficiency and policy solutions to climate change;
- Responding to information requests from member organisations;
- Producing material for the organisations website and reports; and
- Contribute to the demands of running a small organisation in a dynamic, fast-moving environment.

The ideal candidate will be team-oriented, personable, organised and numerate with excellent written and verbal communication skills. Education to Masters level in a relevant environmental field, 1 to 3 years relevant work experience and a sound understanding of climate change issues are also desirable. Knowledge of MS Office Applications, particularly Word, Excel and Powerpoint, and Lotus Notes is an asset.

Language skills are also an advantage for this role.

Based on experience, the position will offer competitive remuneration including travel loan and pension benefits. Interested candidates should in the first instance send or e-mail a

CV and covering note outlining details of current remuneration to: Jim Walker, Operations

cg2003

Manager, The Climate Group, Abbey House, Wellington Way, Weybridge KT13 0TT.
E-mail
[25]jim@reducers.org.
Deadline for applications: 25th November 2003.
- ENDS -

1619. 2003-11-20

cc: "Cotter, Rosalind" <R.Cotter@nature.com>, "Campbell, Philip"
<P.Campbell@nature.com>, rbradley@geo.umass.edu, mhughes@lrrr.arizona.edu, Tim
Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>, Scott Rutherford <srutherford@rwu.edu>
date: Thu, 20 Nov 2003 07:16:20 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: RE: Energy and Environment Paper
to: "Langenberg, Heike" <H.Langenberg@nature.com>

Dear Heike,

Thanks for your message. We're happy to help Nature out in any way we can
here...

First a little more background. Mckitrick and McIntyre have been deliberately
trying to

create a controversy where there is none. They know that their own published
"correction"

has been shown to be total nonsense as demonstrated by a paper in submission (a
preliminary

version of which was made for distribution after their study came out), and also
this very

nice article published in "USA Today" by their staff science writer Dan Vergano
the other

day:

[1]http://www.usatoday.com/weather/climate/2003-11-18-warming-debate_x.htm

So instead they've been trying to manufacture a controversy about data
availability where

there is none (incidentally, they have been making similar false threats against
NSF

program directors--I won't go into the politics behind this, but its pretty
transparent

what they're up to).

The have been intentionally misleading about the availability of our proxy
data. The data

have all been available on our public ftp site since July 2002 here:

[2]<ftp://holocene.evsc.virginia.edu/pub/MBH98/>

and other scientists have successfully acquired that data. This forced USA to
publish a

retraction of the claim made by Mckitrick and McIntyre that we hadn't made our
data

publicly available last week:

c) USA TODAY - THURSDAY - November 13, 2003 - 14A

Corrections & Clarifications

In an Oct. 29 Forum article about new research that challenges the
findings of an

earlier study on global warming, the writer said the data on the original
study by

University of Virginia assistant professor Michael Mann aren't available
online. The

data can be accessed at [3]<ftp://holocene.evsc.virginia.edu/pub/MBH98/>

Note that the full data set could not be made available until a few years after
the '98

study, because we had to give various researchers who provided us unpublished
data on a

proprietary basis the opportunity to publish those data first.

The description of the methodology used in our analysis in the MBH98 paper is
complete

enough that other researchers have independently reproduced it without any additional information from us:
 Zorita, E., F. Gonzalez-Rouco, and S. Legutke, Testing the Mann et al. (1998) approach to paleoclimate reconstructions in the context of a 1000-yr control simulation with the ECHO-G Coupled Climate Model, J. Climate, 16, 1378-1390, 2003.
 so we see no need to expand on it.
 The only potential exception is the description of how some of the proxy indicator sub-groups were represented in the data set, and that is actually a "data set" issue which we will clarify (see below).
 The data is available in a particular directory tree structure (see sub-directories) of the above ftp directory. This is related to the fact that different groups of data were used over different time intervals owing to the stepwise nature of the reconstruction which was described in our article.
 We agree that some additional descriptive files in each directory and/or a reorganization of the directory structure might have helped to clarify precisely which data were used over precisely which time intervals, and had we known that a concerted effort was going to be made to mispresent our study and our dataset, we would have put more effort into this.
 Conveniently enough, we had planned to create a simpler reorganized directory structure of the data anyway, to address these sorts of scurrilous accusations, especially since the same dataset (and other dataset) are used in a paper co-authored by Scott Rutherford, Ray Bradley, Malcolm Hughes, Phil Jones, Keith Briffa, and Tim Osborn which we expect to be published sometime in the near future.
 So we will create ASAP a new version of the dataset organized in a simpler manner--it will simply contain all of the series (and only the series) that were used for each sub-interval in our reconstruction separately. As indicated in our original Nature supplementary information (we have kept a mirror here:

<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/MultiProxy/stats-supp.html>) this

involves the following number of distinct indicators over the various sub-intervals:

Back to 1820: 112
 Back to 1800: 102
 Back to 1780: 97
 Back to 1760: 93
 Back to 1750: 89
 Back to 1730: 79
 Back to 1700: 74
 Back to 1600: 57
 Back to 1500: 28
 Back to 1450: 24
 Back to 1400: 22

So the easiest way to provide the full data set used is in terms of 11 matrices of data

cg2003

containing the precise set of indicators used, and a "README" file describing the data format in detail, to make sure there can be *no* uncertainty as to precisely how these data were used in the MBH98 study. This was also include a short description of the procedure (used to represent subgroups of certain proxy data networks by a smaller number of "PCs" (and the objective criterion used to determine how many PCs were kep) which we agree was terse in the original paper and supplementary information. I will work with our associate Scott Rutherford who has handled the data for over the past few years to create the above version of the dataset and README file ASAP and will be in contact with Nature as soon as soon as this is available, which should be shortly.

Is there a particular individual on the technical staff at Nature that we should be communicating with directly?

Thanks for your help,

Mike

At 11:23 AM 11/20/2003 +0000, Langenberg, Heike wrote:

Dear Mike,

In the wake of the debate started by the publication of the Energy and Environment

paper, we have had a request from McKittrick and McIntyre for a full list of the data

sets and the computational procedures used in your 1998 Nature paper.

In line with our policy that data and methods of a paper published in Nature must be

available to academic researchers for their own use

(<http://www.nature.com/nature/submit/policies/index.html#6>) and in order to put an end

to any discussion about the data sets and methods used, we decided that it would be best

for us to publish an addendum to the paper (just saying that interested readers can find

the data on our website), with a link to the full set of data and methods as Supplementary Information.

Could you therefore please supply the full set of data series and a description of the procedures used to us?

Best regards,

Heike

-----Original Message-----

From: Michael E. Mann [[4]mailto:mann@virginia.edu]

Sent: 06 November 2003 02:49

To: Langenberg, Heike

Subject: RE: Energy and Environment Paper

;Hi Heike,

Just a followup to my terse email earlier (sent it from a plane).

As I mentioned before, I understand the decision--I think its probably a wise decision.

If Nature does decide to do a story on this, please let me know if I can be of any help.

Thanks again for your consideration of the issue. We'll let you know when our formal

response to the paper is published (probably in "Climatic Change").

best regards,

mike

cg2003

At 05:26 PM 11/5/2003 +0000, Langenberg, Heike wrote:

Dear Mike,

Thanks again for the information you provided to us on the debate.

As mentioned on the phone, we have discussed the issue at length, but have now

decided not to publish your rebuttal of the E&E paper. Obviously, this decision is

editorial and does not reflect in any way on its scientific quality.

We might still take up the issue elsewhere in the journal, but nothing definitive is

planned at this stage.

I just wanted to let you know about our decision regarding your rebuttal as soon as

possible, so that you can pursue publication elsewhere.

Best wishes,

Heike

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Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4461. 2003-11-20

date: Thu Nov 20 17:09:54 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: Email for Editorial Board Climate Policy

to: Climate Policy <climatepolicy@imperial.ac.uk>

Mike,

I hope to be able to join the editorial team meeting in Milan and also am happy to continue

as a member of the editorial board if you so wish me to do.

See you in Milan,

Mike

cg2003

At 11:36 19/11/2003 +0000, you wrote:

Dear Editorial Board

This years Board meeting will again be held at the COP and the main item on the agenda

will concern fundamental decisions about the future of the journal. It appears that

more people are coming to Milan for the second week and we propose a Board meeting for

Tuesday 9 December.

You will be aware from the papers to last years Board meeting, and from the Annual

Report which brought some of these elements and was circulated in January, that the

relationship with Elsevier has not been an easy one, and there has not been positive

progress. The initial contracts expire at the end of this year. I append a note on the

process that has been gone through since I first, in April, formally raised with

Elsevier my concerns and questions about the journals future in the context of contract

renewal.

During this period I was approached by another publisher asking if I might contemplate

working with them to launch a journal / magazine targeted at the international climate

change policy community. They seem much more 'tuned in' to the kind of things the wider

community might need, and far more committed to the idea of a journal of this nature.

This other publisher has expressed interest in the possibility of acquiring Climate

Policy from Elsevier should this become an option.

I am still awaiting final decisions from Elsevier about whether another division wishes

to take on the journal and if so what terms they may offer.

I aim to secure a firm clear offer or statement from the other publisher in advance of

the Editorial Board Meeting.

At the Board meeting, I thus expect to be seeking guidance of the Board either about the

choice between two options, or about the detailed terms of relaunching the journal with

a new publisher.

In addition, at the Board meeting we need to undertake a comprehensive review of Board

membership, taking account of the extent to which members have been able to make

contributions since the journals launch, and to approach a number of new persons.

PLEASE LET US KNOW IF IN PRINCIPLE YOU DO WISH TO CONTINUE ON THE BOARD.

I also intend to review the Associate Editors and their roles. So far, Axel Michaelowa

and Yoshiki Yamagata have responded to a recent enquiry about this with strong commitment to continue and enhance their engagement.

I do hope that you can attend the Board meeting, which will unquestionably be the most

important since the journals establishment, and I would also appreciate any written

comments from those unable to attend.

With best wishes,

Michael Grubb

871. 2003-11-21

date: Fri, 21 Nov 2003 16:00:30 +0000
 from: Tim Osborn <t.osborn@uea.ac.uk>
 subject: Re: Tyndall Phase 2
 to: <tyn.rtl@uea.ac.uk>

<x-flowed>

At 13:01 21/11/2003, John Turnpenny wrote:

>Dear Tim et al,

>

>useful points. this raises the issue of research needs vs. political
 >considerations. If we felt that NERC was under-represented in Theme 1 (or
 >even throughout Tyndall) i would not be happy if we were to 'fix' the
 >research to ensure a 'balance' between research council funders. quite
 >apart from anything else it assumes the amounts of money each council are
 >prepared to put in correlates with the type of research needed. and also,
 >as you point out tim, the scale over which we would average is unclear (do
 >all themes have to reflect the funding balance, or all projects, or Tyndall
 >as a whole?).

>

>i believe that if we genuinely identify research needs which do not balance
 >the funding input then we should be honest and make that clear to the
 >research councils.

>

>cheers, John

John,

the range and depth of research needs are clearly beyond what the Tyndall
 Centre alone can achieve and we could, therefore, identify a vast range of
 phase 2 work plans. So it should be possible to develop a research plan
 that does balance the funding input - if we want to - and Rachel's response
 clearly demonstrates that the range of NERC-domain science that was
 (unfortunately only implicitly) already underpinning the key questions is
 probably enough to balance NERC funding input.

My answer to the question of whether we want to balance the funding input
 is that we ideally would want to ignore it and identify the "best" (in our
 view) work plan. But pragmatically we don't want to disaffect any of the
 main funders -- even if we could put up a good argument as to why we had
 chosen that work plan.

I believe a balance to the funding input would increase the chances of
 funding for phase 2. But I think it only needs to be an approximate
 balance and only over the whole centre. Certainly individual projects do
 not need it, and probably not individual themes. My concern was that RT1
 is clearly a theme where there could and should be a balance, yet the
 document so far did not show such a balance. The type of material that
 Rachel just circulated could be included and would then provide this (for
 NERC, at least) and I encourage that type of information to be included.

Cheers

Tim

Dr Timothy J Osborn

Climatic Research Unit

School of Environmental Sciences, University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

fax: +44 1603 507784

web: <http://www.cru.uea.ac.uk/~timo/>

sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

3333. 2003-11-21

date: Fri, 21 Nov 2003 12:00:27 +0000
 from: Ian Harris <i.harris@uea.ac.uk>

cg2003

subject: Mann's Proxies
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>

Hi Keith,
Phil asked me to access Mann's MBH98 predictors from the ftp server in Virginia.

Apparently there are 159. However, with no guidance I'm unable to identify a set of 159 data series from the thousands of files in the 'MBH98' directory structure.

Furthermore, attempts to download the entire directory hierarchy for examination (it's only about 20Mb in total) have been fraught with failures - 3 hours on Wednesday afternoon and all this morning, and still incomplete.

If Steve McIntyre's having the same difficulties, no wonder he's pissed off!

Anyway I should have a complete file listing this afternoon; we can look over it then and hopefully identify the predictors.

By the way - what does this have to do with HOLSMEER? *grin*

Cheers

Harry

</x-flowed>

662. 2003-11-25

cc: <M.Hulme@uea.ac.uk>
date: Tue, 25 Nov 2003 10:09:01 +0000
from: "Merylyn Hedger" <merylyn.hedger@environment-agency.gov.uk>
subject: Re: Help for small islands
to: "Merylyn Hedger" <merylyn.hedger@environment-agency.gov.uk>,
<a.watkinson@uea.ac.uk>
(I gather from Neil the attachment didn't get through)

Dear Merylyn

Thanks for getting back to me. The issue that we are looking into is doing computer simulations for what sea level rise will mean for many of the lower lying SIDS. We did one for Male' in Maldives, but they had good charts etc and some gifted computer boffins. All that it requires is a topographical map of the selected islands, and then juxtaposing this to IPCC findings. For Male' it was a significant loss of land by 2030 and disappearance in 2100.

The question is - is this something that you or colleagues could assist us with? For some islands like Tuvalu the charts are all with the Admiralty or FCO and we do not have an easy "in" with them.

Looking forward to hearing your views on this.

Regards

Espen

>>> Merylyn Hedger 11/24/03 08:01pm >>>

Hi Andrew et al.

Can you in anyway help do a quick and dirty assessment for Espen.

I have checked and he still wants the assistance- the email got stuck and lost in my in box whilst I thought what to do.

Dear Merylyn

(No its not too late. Please let me know what you hear from Tyndall.

Regards

Espen)

Espen used to raise hell for the Marshall Islands in the UNFCCC processes. he is now part of the New York UN corps. I know anything you can get together he would value and you'd actually be producing a very useful output!

Plse let me know what you decide to do and what happens.

Merylyn

This message is confidential as it contains information about the person we are sending it to. If you have received this message by mistake, please delete it and do not copy it to anyone else.

cg2003

If this message contains information that you have requested from us please see our standard notice for details of how you may use that information. If the notice is not attached and you require a copy please telephone 0845 9333111 and ask for the customer contact.

For further information about the Environment Agency call the number above or look at our web site at <http://www.environment-agency.gov.uk>

5324. 2003-11-25

date: Tue, 25 Nov 2003 11:28:42 -0500

from: tom crowley <tcrowley@duke.edu>

subject: Re: reminder re new NH data

to: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Keith, I am so sorry I have been so sorry about sending you this data (termed CLH1.2 in our paper*)- here it is - we have scaled it against the decadal smoothed mean annual temp. record from 1880-1960 for 30-90N. the reference level has been set to the temperatures predicted from an ebm for the interval of 1000-1850 with no external forcing.

hope this helps, tom

fyi, CLH refers to Crowley-Lowery-Hegerl

| | |
|------|--------|
| 1000 | -0.063 |
| 1001 | -0.075 |
| 1002 | -0.109 |
| 1003 | -0.122 |
| 1004 | -0.125 |
| 1005 | -0.122 |
| 1006 | -0.119 |
| 1007 | -0.120 |
| 1008 | -0.126 |
| 1009 | -0.091 |
| 1010 | -0.097 |
| 1011 | -0.089 |
| 1012 | -0.096 |
| 1013 | -0.084 |
| 1014 | -0.069 |
| 1015 | -0.039 |
| 1016 | -0.038 |
| 1017 | -0.040 |
| 1018 | -0.050 |
| 1019 | -0.057 |
| 1020 | -0.058 |
| 1021 | -0.063 |
| 1022 | -0.059 |
| 1023 | -0.035 |
| 1024 | -0.067 |
| 1025 | -0.052 |
| 1026 | -0.070 |
| 1027 | -0.089 |
| 1028 | -0.075 |
| 1029 | -0.056 |
| 1030 | -0.065 |
| 1031 | -0.075 |
| 1032 | -0.048 |
| 1033 | -0.053 |
| 1034 | -0.075 |
| 1035 | -0.061 |
| 1036 | -0.097 |
| 1037 | -0.124 |
| 1038 | -0.108 |
| 1039 | -0.125 |
| 1040 | -0.126 |

| | |
|------|--------|
| 1041 | -0.130 |
| 1042 | -0.146 |
| 1043 | -0.139 |
| 1044 | -0.152 |
| 1045 | -0.156 |
| 1046 | -0.155 |
| 1047 | -0.143 |
| 1048 | -0.122 |
| 1049 | -0.119 |
| 1050 | -0.132 |
| 1051 | -0.136 |
| 1052 | -0.110 |
| 1053 | -0.085 |
| 1054 | -0.116 |
| 1055 | -0.104 |
| 1056 | -0.077 |
| 1057 | -0.055 |
| 1058 | -0.048 |
| 1059 | -0.030 |
| 1060 | -0.013 |
| 1061 | 0.032 |
| 1062 | 0.036 |
| 1063 | 0.005 |
| 1064 | -0.009 |
| 1065 | -0.008 |
| 1066 | -0.002 |
| 1067 | -0.024 |
| 1068 | -0.032 |
| 1069 | -0.009 |
| 1070 | -0.008 |
| 1071 | -0.042 |
| 1072 | -0.070 |
| 1073 | -0.073 |
| 1074 | -0.039 |
| 1075 | -0.005 |
| 1076 | 0.035 |
| 1077 | 0.053 |
| 1078 | 0.122 |
| 1079 | 0.141 |
| 1080 | 0.125 |
| 1081 | 0.150 |
| 1082 | 0.191 |
| 1083 | 0.217 |
| 1084 | 0.258 |
| 1085 | 0.272 |
| 1086 | 0.293 |
| 1087 | 0.295 |
| 1088 | 0.277 |
| 1089 | 0.280 |
| 1090 | 0.252 |
| 1091 | 0.231 |
| 1092 | 0.222 |
| 1093 | 0.220 |
| 1094 | 0.211 |
| 1095 | 0.201 |
| 1096 | 0.191 |
| 1097 | 0.188 |
| 1098 | 0.204 |
| 1099 | 0.200 |
| 1100 | 0.198 |
| 1101 | 0.222 |
| 1102 | 0.234 |
| 1103 | 0.211 |

| | |
|------|--------|
| 1104 | 0.170 |
| 1105 | 0.137 |
| 1106 | 0.111 |
| 1107 | 0.089 |
| 1108 | 0.075 |
| 1109 | 0.039 |
| 1110 | 0.023 |
| 1111 | 0.011 |
| 1112 | -0.034 |
| 1113 | -0.050 |
| 1114 | -0.045 |
| 1115 | -0.028 |
| 1116 | -0.009 |
| 1117 | -0.002 |
| 1118 | 0.003 |
| 1119 | -0.043 |
| 1120 | -0.078 |
| 1121 | -0.084 |
| 1122 | -0.102 |
| 1123 | -0.113 |
| 1124 | -0.135 |
| 1125 | -0.150 |
| 1126 | -0.158 |
| 1127 | -0.163 |
| 1128 | -0.164 |
| 1129 | -0.173 |
| 1130 | -0.134 |
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| 1133 | -0.088 |
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| 1149 | -0.003 |
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| 1153 | 0.043 |
| 1154 | 0.043 |
| 1155 | 0.053 |
| 1156 | 0.046 |
| 1157 | 0.086 |
| 1158 | 0.099 |
| 1159 | 0.102 |
| 1160 | 0.104 |
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| 1162 | 0.087 |
| 1163 | 0.121 |
| 1164 | 0.116 |
| 1165 | 0.107 |
| 1166 | 0.091 |

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| 1167 | 0.099 |
| 1168 | 0.083 |
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| 1170 | 0.108 |
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| 1172 | 0.133 |
| 1173 | 0.134 |
| 1174 | 0.117 |
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| 1179 | 0.135 |
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| 1182 | 0.093 |
| 1183 | 0.081 |
| 1184 | 0.046 |
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| 1469 | -0.183 |
| 1470 | -0.194 |
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| 1474 | -0.190 |
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| 1492 | -0.099 |
| 1493 | -0.099 |
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| 1823 | -0.287 |
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| 1831 | -0.262 |
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| 1845 | -0.171 |
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| 1861 | -0.166 |
| 1862 | -0.172 |
| 1863 | -0.162 |
| 1864 | -0.203 |
| 1865 | -0.211 |
| 1866 | -0.238 |
| 1867 | -0.257 |
| 1868 | -0.247 |
| 1869 | -0.261 |
| 1870 | -0.247 |
| 1871 | -0.250 |
| 1872 | -0.237 |
| 1873 | -0.199 |
| 1874 | -0.193 |
| 1875 | -0.178 |
| 1876 | -0.186 |
| 1877 | -0.178 |
| 1878 | -0.153 |
| 1879 | -0.162 |
| 1880 | -0.177 |
| 1881 | -0.200 |
| 1882 | -0.192 |
| 1883 | -0.188 |
| 1884 | -0.193 |
| 1885 | -0.190 |
| 1886 | -0.180 |
| 1887 | -0.180 |
| 1888 | -0.191 |
| 1889 | -0.191 |
| 1890 | -0.171 |
| 1891 | -0.135 |
| 1892 | -0.108 |
| 1893 | -0.096 |
| 1894 | -0.137 |
| 1895 | -0.153 |
| 1896 | -0.139 |
| 1897 | -0.152 |
| 1898 | -0.147 |
| 1899 | -0.143 |
| 1900 | -0.168 |
| 1901 | -0.166 |
| 1902 | -0.187 |
| 1903 | -0.186 |
| 1904 | -0.195 |
| 1905 | -0.140 |
| 1906 | -0.132 |
| 1907 | -0.163 |
| 1908 | -0.147 |
| 1909 | -0.130 |
| 1910 | -0.109 |
| 1911 | -0.091 |
| 1912 | -0.085 |
| 1913 | -0.057 |
| 1914 | -0.064 |
| 1915 | -0.041 |
| 1916 | -0.041 |
| 1917 | -0.013 |
| 1918 | 0.002 |
| 1919 | 0.030 |
| 1920 | 0.037 |
| 1921 | 0.055 |
| 1922 | 0.063 |

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| | |
|------|-------|
| 1923 | 0.074 |
| 1924 | 0.093 |
| 1925 | 0.117 |
| 1926 | 0.111 |
| 1927 | 0.144 |
| 1928 | 0.160 |
| 1929 | 0.191 |
| 1930 | 0.189 |
| 1931 | 0.221 |
| 1932 | 0.236 |
| 1933 | 0.257 |
| 1934 | 0.280 |
| 1935 | 0.301 |
| 1936 | 0.309 |
| 1937 | 0.344 |
| 1938 | 0.349 |
| 1939 | 0.345 |
| 1940 | 0.365 |
| 1941 | 0.389 |
| 1942 | 0.378 |
| 1943 | 0.375 |
| 1944 | 0.357 |
| 1945 | 0.363 |
| 1946 | 0.343 |
| 1947 | 0.334 |
| 1948 | 0.324 |
| 1949 | 0.300 |
| 1950 | 0.299 |
| 1951 | 0.268 |
| 1952 | 0.245 |
| 1953 | 0.232 |
| 1954 | 0.222 |
| 1955 | 0.245 |
| 1956 | 0.219 |
| 1957 | 0.218 |
| 1958 | 0.206 |
| 1959 | 0.138 |
| 1960 | 0.116 |

>

</x-flowed>

2146. 2003-11-28

date: Fri Nov 28 13:21:13 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Fwd: Progress towards a global climate community based on equity
to: v.mcgregor

Please print for me.

Mike

Reply-To: <titus@cewc.org>
From: "Titus Alexander" <titus.alexander@mcr1.poptel.org.uk>
To: <antti.pentikainen@ehtisaari.fi>
Cc: <ennals@kingston.ac.uk>
Subject: Progress towards a global climate community based on equity (WBGU

report &

Chanctonbury Initiative)

Date: Fri, 28 Nov 2003 13:05:34 -0000

X-Mailer: Microsoft Outlook, Build 10.0.4510

Importance: High

Dear Colleague,

I am delighted to inform you that our conference on climate change at Wilton
Park (15-17

November) proposed a bold Initiative for a global climate community based on

contraction

and convergence. I attach a statement which emerged through working groups.

It does not

represent the views of every participant, but a consensus of the large majority. A

report from the event will follow. You are warmly invited to discuss, endorse

and

disseminate this proposal, post it on your website and pass it on to anyone who may be

interested.

As you may know, the recent report of the German Advisory Council on Global Change

(WBGU) also recommends the creation of a "global climate community" based on contraction

and convergence to equal per capita emissions rights (

[1]http://www.wbgu.de/wbgu_sn2003_engl.pdf). This is a detailed and important

document, which offers a real opportunity to create an equitable global solution to

climate change.

It may be seen as a positive response to the call by Prime Minister Atal Bihari Vajpayee

at the end of CoP 9: "We don't believe that the ethical principles of democracy could

support any norm other than that all citizens in the world should have equal rights to

use ecological resources."

Other governments could make an historic breakthrough by approaching the German

government to discuss these ideas.

Key passages the WBGU report are as follows:

The Council's recommendation:

Aim towards equal per-capita emission rights and linear harmonization of emissions

shares

The WBGU recommends that emission rights for the greenhouse gases covered by the Kyoto

Protocol be allocated according to the contraction and convergence approach, taking 2050

as convergence year. This means that global emissions would need to be reduced substantially over the long term (contraction). In a further step, it would be

agreed

that the per-capita emissions of all states must reach equal levels in a continuous

process extending until 2050 (convergence). In particular, this means that the percapita

emissions of industrialized countries, which are still comparatively high at present,

must be reduced, while some developing countries can initially increase their per-capita

emissions. The principle of constancy requires that there be no sudden switch to equal

per-capita emissions, because of the resulting stresses on the global economy. The

approach further presupposes a functioning global emissions trading scheme, in order to

reduce the costs of the transformation process.

On p 58 the report says:

The Council is aware of the danger that individual states could entirely refuse to adopt

emission limits and could thus assume a free rider position. To cope with this, the

London based Global Commons Institute, which originally developed the C&C

model, has proposed a Global Climate Community': A group of core states (EU, some Umbrella Group states, developing countries) adopts emissions reductions according to the C&C principle. The Council similarly recommends to the coalition of voluntary participants that it retains the basic idea of the C&C allocation approach despite the absence of important countries.

On the other hand, the Council expressly warns that such a situation the climate change mitigation goal would most probably not be attained this will be all the more so the more large-scale emitters refuse to join the regime. The goal of coalition members must therefore be to expand the group of participants as swiftly and comprehensively as possible. Positive incentives alone will probably not have sufficient effect. The resources required to buy the participation of all free riders could not be mustered.

Therefore the coalition members should agree that they will impose political and economic sanctions against free rider states when the need arises.

6.2

Shaping commitments equitably

Aiming towards equal emission rights

The WBGU bases its arguments additionally on the egalitarian principle, which can be derived from the human right to equal treatment. In terms of relations among the parties to the Convention or Protocol, this corresponds to the principle of equity (Art. 3(1) UNFCCC).

It follows that ultimately, only an allocation of emission rights according to equal per-capita shares can be considered just.

Implementing contraction and convergence

At the long-term global emissions must be reduced significantly (contraction). In addition, the WBGU postulates the principle of constancy, according to which abrupt measures leading to drastic effects should be avoided in socio-economic systems. A sudden switch to a per-capita allocation of tradable emission certificates is therefore not recommended: The resulting high transfer payments from industrialized to developing countries could have severe effects that would impact on the economies of all regions. For these reasons, the Council argues in favour of moving in a continuous process from the present allocation of shares, which entails very great imbalance in per-capita emissions, towards allocation according to equal per-capita shares (convergence). Building upon the review of scenario computations set out in Chapter 3, the WBGU recommends this contraction and convergence (C&C) approach with a linear convergence of emissions shares towards equal per-capita emission

rights by the year 2050. This should embrace the emissions of CO₂, CH₄, N₂O, HFCs, PFCs and SF₆ (the Kyoto basket of greenhouse gases) from energy, industry, agriculture and waste management. The emissions of other greenhouse gases would be accounted for as CO₂-equivalent values according to their global warming potential, as already provided for in the Kyoto Protocol.

If developing countries are unable or unwilling accept national-level emission caps in accordance with the C&C approach from the outset, the WBGU recommends an opt-out clause for countries with relatively low economic capacities, i.e. relatively low per-capita emissions and per-capita income. This means that states would need to agree on a threshold allowing to make use of the opt-out clause. For instance, per-capita income and per-capita emissions could be combined in one indicator. When states exceed this threshold, they would be obliged to participate in the global C&C regime. The reduction burden of developing countries which make use of the opt-out clause would be spread across the participating countries. This would ensure attainment of the stabilization target and thus compliance with the climate window. In this context, CDM projects in nonparticipating countries could have the function reducing burdens and integrating non-participants into the system. It needs to be noted that such a gradual transition from the present structure of the Kyoto Protocol (with its distinction between Annex-I and non-Annex-I states) towards a global C&C regime can only succeed if opt-out criteria are tight enough for the participating countries to be able to cope with the additional emissions reduction burdens.

6.5

Linking climate protection consistently with global governance
 Supporting convergence between industrialised and developing countries

To do justice to the vision of sustainable development, social and economic exigencies must be taken into account besides the climate protection goal. So that the climate protection goal can be attained over the long term at low costs, climate policy needs to be linked consistently with global governance and development policy. The aim must be to promote social and economic convergence between industrialized and developing countries, and to facilitate technology transfer. In addition to development cooperation activities focussed more firmly upon sustainability, a first step towards convergence can be to open markets to the products of developing countries.

So that in the course of the globalization process, worldwide economic and social

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convergence can occur under circumstances characterized by declining rates of population growth over the long term (from 2050 onwards), development cooperation needs to be further intensified. In order to avoid an increase of the global population beyond the year 2050, education and health programmes for women in developing countries need to be promoted, as does the introduction of systems of social security.

[2]http://www.wbgu.de/wbgu_sn2003_eng1.pdf

Yours sincerely

Titus Alexander

Development Consultant

Council for Education in World Citizenship

32 Carisbrooke Road

London

E17 7EF

Tel: 020 8521 6977

Mobile: 07720394740

Email: titus@cewc.org

[3]www.cewc.org

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mob +44 (0) 7789 483 221

owt@parliament.uk

[4]<http://www.oneworldtrust.org>

Charity No. 210180

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Mobile: 07720394740

Email: titus@cewc.org

[5]www.cewc.org

4628. 2003-11-28

date: Fri, 28 Nov 2003 09:49:42 -0000

from: "Mark Lynas" <marklynas@zetnet.co.uk>

subject: Climate change analysis

to: "Climate Change Info Mailing List" <climate-l@lists.iisd.ca>

Dear Climate-L readers,

This week's New Statesman cover story in the UK is an analysis of why the climate change policy process seems to have proceeded so slowly and with so little result - we contend that this is the result of a mass societal 'denial' about the climate change problem. It's on the web free for one week:

<http://www.newstatesman.co.uk/nscoverstory.htm> or after that on my website www.marklynas.org

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Cheers,

Mark Lynas

You are currently subscribed to climate-l as: m.hulme@uea.ac.uk
To unsubscribe send a blank email to leave-climate-l-15281Y@lists.iisd.ca
- Subscribe to Linkages Update to receive our fortnightly, html-newsletter on what's new in the international environment and sustainable development area:
<http://www.iisd.ca/email/subscribe.htm>
- Archives of Climate-L and Climate-L News are available online at:
<http://www.iisd.ca/email/climate-L.htm>
- Archives of Water-L and Water-L News are available online at:
<http://www.iisd.ca/email/water-L.htm>

1021. 2003-12-01

date: Mon, 1 Dec 2003 15:27:16 UT
from: eos@agu.org
subject: 2003ES000529 Request to Review from Eos
to: K.Briffa@uea.ac.uk

Dear Keith:

Would you be willing and available to review "Low Frequency Ambition and High Frequency Ratification" by Jan Esper, David Frank, Robert Wilsonn, submitted for possible publication in Eos?

To record your decision whether or not to review the article, please click the link below. If you accept this review, you may begin the review immediately or return at a later time. You will shortly receive an e-mail with review and access instructions. If you must decline to review this article, you will receive an on-screen confirmation message on the web page.

<<http://eos-submit.agu.org/cgi-bin/main.plex?el=A3U5nXe7A7mTr4F5A9qLedvo8dsvTuqPeGZNQnWZ>>

Thank you for your consideration and support of Eos.

Sincerely,
Keith Alverson
Editor, Eos

1748. 2003-12-01

date: Mon, 1 Dec 2003 17:23:30 -0000
from: "Jefferiss, Paul" <Paul.Jefferiss@rspb.org.uk>
subject: some possible consultees
to: "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>

Mike,

Here's a few. I'll send more if I think of them.

Paul

I'd have thought all the major environmental (and development) NGOs:

Transport 2000

Green Alliance

National Trust

RSPB

WWF

FOE

Greenpeace

CPRE

wildlife Trusts

wildfowl and wetlands trust

Bat Conservation

The British Trust for Ornithology

The Butterfly Conservancy

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Plantlife
Pesticides Action Network
National Society for Clean Air
IIED
Oxfam
Actionaid
CAFOD
UNED/Stakeholder Forum
Think Tanks/consultancies/legal policy organisations:
IPPR
IEEP (Institute for European Environmental Policy)
FIELD (Foundation for International environmental law and development)
ELF (Environmental Law Foundation)
RIIA (Royal Institute for International Affairs)
ERM
Ecotech
Oxera
Cambridge Econometrics
AEA
Policy Studies Institute
New Economics Foundation
Agencies/quangos/regulators:
EST
Carbon Trust
Environment Agency
English nature
JNCC
Countryside Agency
Sustainable Development Commission
OFGEM
OFWAT
Trade associations/ngos:
Association for the Conservation of Energy
The environmental industries commission

4104. 2003-12-01

date: Mon Dec 1 15:31:38 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Low Frequency Ambition and High Frequency Ratification
to: eos@agu.org

MESSAGE FOR KEITH ALVERSON re above request for me to review

Keith

I think it best if I decline the offer to review this . I am aware of the piece , and in fact declined to be an author. I think there are several things the authors need to explore and I have discussed these with them . In the circumstances , it is not appropriate for me to repeat the same points . I would suggest that perhaps Ed Cook might be considered , but perhaps he too is a little close to the authors. Malcolm Hughes would be an appropriate referee. Sorry , but I am sure this is the right course of action as regards my reviewing this.

Keith

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909

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Fax: +44-1603-507784

[1][http://www.cru.uea.ac.uk/cru/people/brieffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/brieffa[2]/)

1391. 2003-12-02

cc: David Cromwell <ddc@soc.soton.ac.uk>, Peter Challenor
<P.Challenor@soc.soton.ac.uk>, gschmidt@giss.nasa.gov, h.j.schellnhuber@uea.ac.uk,
"B.E. Launder" <mcjtsbl@fs4.umist.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Katy Hill
<klh@soc.soton.ac.uk>, "Quinn, Rachel" <Rachel.Quinn@royalsoc.ac.uk>,
dave.griggs@metoffice.com, a.minns@uea.ac.uk

date: Tue, 02 Dec 2003 17:04:01 +0100

from: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>

subject: Re: FW: Anti Global Warming Petition Project

to: John Shepherd <j.g.shepherd@soc.soton.ac.uk>

<x-flowed>

Dear John,

I'm happy to give scientific advice and support in formulating such a web site.

If you want to do something more proactive, then such a web site could be launched with press release etc. once it is finished.

Stefan

--

Stefan Rahmstorf

Potsdam Institute for Climate Impact Research (PIK)

For contact details, reprints, movies & general infos see:

<http://www.pik-potsdam.de/~stefan>

</x-flowed>

1584. 2003-12-02

date: Tue Dec 2 10:55:01 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: Workshop Invitation - Redcliffe

to: "John Turnpenny" <j.turnpenny@uea.ac.uk>

John,

You could claim costs against your Round 2,3 project I guess; there is some relevance.

Alternatively you could apply for Mobility Grant funding through the usual route.

Mike

At 09:52 02/12/2003 +0000, you wrote:

mike,

thanks for that link - it looks fascinating. do you think tyndall could pay for me to go?!

cheers, john

----- Original Message -----

From: "Mike Hulme" <m.hulme@uea.ac.uk>

To: <j.turnpenny@uea.ac.uk>

Sent: Friday, November 28, 2003 4:11 PM

Subject: Fwd: Workshop Invitation - Redcliffe

John,

Asher tells me you have agreed to speak to the Church of Scotland project on ethics and environment re. climate change. Many thanks for this. I would have liked to have joined in but know I will just be too busy in January.

You may be interested to know about the meeting mentioned below from the John Ray Initiative.

Mike

>To: m.hulme@uea.ac.uk

>Subject: Workshop Invitation - Redcliffe

>From: john.mckeown@jri.org.uk

>Date: Fri, 28 Nov 2003 15:35:21 +0000 (GMT)

>

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>To: Mike Hulme
>
>The John Ray Initiative is collaborating with Redcliffe
>College to offer a one-day workshop: 'Christian perspectives
>on issues in the global environment' on Saturday 17th January
>at Redcliffe College, Gloucester. [1]<http://www.redcliffe.org/>
>
>The speakers are: Dr Peter Carruthers, Revd Margot Hodson,
>Dr Martin Hodson, Sir John Houghton, Professor Colin Russell
>and Professor Gordon Wenham.
>
>There will be plenty of time for questions and discussion,
>and the day will provide an opportunity to explore issues
>from a Christian viewpoint in some depth, in a relaxed
>and informal atmosphere. The cost is £25 for the day
>plus £3 for a hot lunch (or bring a packed lunch).
>
>Book by emailing Diane Carter at: admin@redcliffe.org
>Enquiries by telephone to: 01452 308097
>
>Further details on printable leaflet available from:
>[2]<http://www.jri.org.uk/whatson.htm#RedcliffeJan2004>
>or contact the JRI office for a paper leaflet.
>
>Dr Peter Carruthers
>Executive Director, John Ray Initiative
>University of Gloucestershire, Francis Close Hall,
>Swindon Road, Cheltenham GL50 4AZ
>pcarruthers@glos.ac.uk; [3]www.jri.org.uk
>01242 543580 (office); 077 915 912 52 (Mobile)

3840. 2003-12-02

date: Tue, 2 Dec 2003 15:03:50 -0000
from: "Neil Adger" <N.Adger@uea.ac.uk>
subject: Fw: GEC
to: "Katrina Brown" <k.brown@uea.ac.uk>, <m.hulme@uea.ac.uk>

Kate and Mike
FYI. see below and attached
Martin has not told Mary at Elsevier that he has sent me this material. I spoke
to him this morning.
His purpose is partly to encourage us so that we will take over when he steps
down at end of January.
Many climate papers submitted - need to change that.
Neil

----- Original Message -----

From: [1]PARRYML@aol.com
To: [2]n.adger@uea.ac.uk
Sent: Tuesday, December 02, 2003 2:40 PM
Subject: re: GEC

Dear Neil:

Here is status list.

Cynthia is paid £6,000. Actual input if about 9 hours a week.

I am paid £2,000. Actual input is about 3 hours a week.

Regards,

Martin

Dr Martin Parry,
Co-Chair Working Group II (Impacts and Adaptation),
Intergovernmental Panel on Climate Change,
Hadley Centre,
UK Met Office,

cg2003

Fitzroy Road
Exeter EX1 3PB, UK
Direct tel: +44 (0) 1986 781437
TSU Tel: + 44 (0) 1392 88 4665
Fax: +44 (0) 1986 781437
direct e-mail: parrym1@aol.com
e-mail for WGII Technical Support Unit: ipccwg2@metoffice.com Attachment

Converted:

"c:\eudora\attach\1December, 2003.doc"

647. 2003-12-03

date: Wed, 03 Dec 2003 16:56:40 +0000
from: Climate Policy <climatepolicy@imperial.ac.uk>
subject: Milan meeting
to: naki@iiasa.ac.at, hadi@sdri.ubc.ca, inoble@worldbank.org,
Jorgen.Wettstad@fni.no, schellnhuber@pik-potsdam.de,
h.j.schellnhuber@uea.ac.uk, cdegouvello@worldbank.org,
shs@leland.stanford.edu, Jonathan.PERSHING@iea.org, RKinley@unfccc.int,
Sylvie.Faucheux@c3ed.uvsq.fr, m.hulme@uea.ac.uk,
ZhangZ@EastWestCenter.org, pretel@chmi.cz, zkundze@man.poznan.pl,
jae@pnl.gov, ogunlade@energetic.uct.ac.za, Eberhard.Jochem@isi.fhg.de,
hoesung@unitel.co.kr, naki@iiasa.ac.at, kchomitz@worldbank.org,
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EHaites@netcom.ca, michael.grubb@imperial.ac.uk, t.jackson@surrey.ac.uk,
sujatag@teri.res.in, a-michaelowa@hwwa.de, Emilio@ppe.ufrj.br,
yamagata@nies.go.jp, nkete@wri.org
<x-flowed>

Dear Editorial Board

Sorry if you have already informed us you are not attending COP 9 but for those of you that will be there: we will meet at 9.30am. (Tuesday 9th December) at the coffee bar next to the main computing room in the conference centre. Michael will seek to book a small meeting room and if you are late, check with Lee in the Conference Secretariat about location, or call my mobile 07949 565127 (the default will be that there is space to meet just behind the coffee bar!)

For those that cannot attend, we will let you know the outcome as soon as possible, and we expect to send a full email concerning the situation on 17th December.

I look forward to seeing you there

Michael

</x-flowed>

2974. 2003-12-03

cc: David Cromwell <ddc@soc.soton.ac.uk>, Peter Challenor
<P.Challenor@soc.soton.ac.uk>, gschmidt@giss.nasa.gov, h.j.schellnhuber@uea.ac.uk,
"B.E. Launder" <mcjtsbl@fs4.umist.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Katy Hill
<klh@soc.soton.ac.uk>, "Quinn, Rachel" <Rachel.Quinn@royalsoc.ac.uk>, Laura
Middleton <Laura.Middleton@uea.ac.uk>

date: Wed, 03 Dec 2003 11:22:10 +0000

from: "Griggs, Dave" <dave.griggs@metoffice.com>

subject: RE: to engage or not

to: Asher Minns <A.Minns@uea.ac.uk>, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>,
John Shepherd <j.g.shepherd@soc.soton.ac.uk>

Dear All

Thank you for copying me in on the discussion. To respond on one point:
Asher mentions the recent "Big Chill" programme, and the unfortunate "scare" line
the programme took. As you know Hadley Centre scientists (Richard Wood) were

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involved, and the programme showed his "what if" scenarios with artificially-forced THC collapse. What Richard Wood did say many times to camera (but cut from the programme) and producer was that this scenario, whilst not impossible, was unlikely in the next century or so - he published this in Nature about 3 years ago and of course other modellers find the same sort of result. The programme chose to play up the view of other scientists, especially those from Woods Hole, who had very different views which we think they presented in a rather extreme way - but that's up to them of course - and they just may be right!

"sceptics ask, scientists answer" may sound like a good idea, but there are a whole range of sceptics, a wide range of things to be sceptical about, and good scientists who are also sceptics too. All the people filmed on the Big Chill programme were scientists, but we are sceptical about some of their views. It works both ways!

Dave

>PLEASE NOTE NEW ADDRESS, TELEPHONE AND FAX

>

Dr Dave Griggs, Director Climate Research
>Met Office Hadley Centre for Climate Prediction and Research
>Fitzroy Road Exeter Devon EX1 3PB United Kingdom
>Tel: +44 (0)1392 886615 Fax: +44 (0)1392 885681
>E-mail: dave.griggs@metoffice.com <http://www.metoffice.com>

>

-----Original Message-----

From: Asher Minns [mailto:A.Minns@uea.ac.uk]

Sent: 02 December 2003 18:40

To: Stefan Rahmstorf; John Shepherd

Cc: David Cromwell; Peter Challenor; gschmidt@giss.nasa.gov;

h.j.schellnhuber@uea.ac.uk; B.E. Launder; Mike Hulme; Katy Hill; Quinn, Rachel;

Griggs, Dave; Laura Middleton

Subject: to engage or not

Dear John, Stefan et al,

You have clearly been having an interesting exchange of views about whether scientists should engage with environmental skeptics. From my experience of trying different approaches I pretty-much am in agreement with Stefan about public rebutting of skeptics.

- 1) we bestow skeptics with scientific credibility and legitimacy
 - 2) we give skeptics a free ride on our reputation and kindly provide them with the oxygen of our publicity
 - 3) we are looking at considerable resources if we are to refute skeptics, do it properly, and do a good job. For example, last week's contribution to my inbox is an 8 page analysis of 55 years of temperature data for eleven small US cities, which, the author claims, shows that there is no evidence of global warming because there is no heat-island effect, and demonstrates regional cooling. Who will volunteer to re-analyse and rebutt this analysis, by tomorrow or earlier? I'd rather I used our resources elsewhere
 - 4) 4.1 million UK viewers watched BBC2 Horizon's 'Big Chill' a couple of weeks ago which included Hadley scientists presenting scenarios of a catastrophic mini ice-age in 20 and 50 years time for the UK. It will soon be syndicated to Discovery and shown worldwide. How do we effectively refute the 'Big Chill' to 4 million people and then worldwide? Broadcast media is the really important audience.
 - 5) we would do better to concentrate our resources on world class research and effectively communicating it to target and influential audiences, including journalists and the media
 - 6) replacing objectivity with ideology (which is how arguments can appear to the public) can damage an organisation's or a scientists credibility
- However, I should point-out that I am not entirely negative about engaging with skeptics, but I do think Tyndall should be very selective about when it chooses to engage, with whom, and how. I am also very open to ideas for developing web-initiatives - and will give this web page idea some proper consideration.

Some recent links related to skeptic topics:

<http://www.newstatesman.co.uk/nscoverstory.htm>

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All of the BBC climate change message boards
<http://www.bbc.co.uk/cgi-perl/h2/h2.cgi?state=threads&board=weather.environment&>
Regards to all,
Asher

Mr Asher Minns
Communication Manager
Tyndall Centre for Climate Change Research
www.tyndall.ac.uk
Mob: 07880 547 843
Tel: +44 0 1603 593906

----- Original Message -----

From: "John Shepherd" <j.g.shepherd@soc.soton.ac.uk>
To: "Stefan Rahmstorf" <rahmstorf@pik-potsdam.de>
Cc: "David Cromwell" <ddc@soc.soton.ac.uk>; "Peter Challenor"
<P.Challenor@soc.soton.ac.uk>; <gschmidt@giss.nasa.gov>;
<h.j.schellnhuber@uea.ac.uk>; "B.E. Launder" <mcjtsbl@fs4.umist.ac.uk>; "Mike
Hulme" <m.hulme@uea.ac.uk>; "Katy Hill" <klh@soc.soton.ac.uk>; "Quinn, Rachel"
<Rachel.Quinn@royalsoc.ac.uk>; <dave.griggs@metoffice.com>; <a.minns@uea.ac.uk>
Sent: Tuesday, December 02, 2003 1:11 PM
Subject: Re: FW: Anti Global Warming Petition Project
Hi Stefan

Many thanks for your very helpful comments. Essentially I agree on all counts, and indeed the "sceptics ask, scientists answer" web-page that you have set up is exactly the sort of thing I had in mind as a possible minimal response that we (Tyndall et al, and even maybe the Royal Society if it wants to get involved) might undertake. Wherever possible this could/should refer to other reputable sites (incl IPCC, Hadley Centre, the ones you mention, etc etc) rather than duplicating the material. I would envisage that such a site could be maintained by a consortium of the willing, in this case involving (say) Tyndall, Hadley & PIK. We could then asked the RS (et al) to mention it and link to it on some sort of "sound science" page on their own web-site(s) (Rachel, do you think that this might fly?).

We had an interesting debate on this at the Tyndall Advisory Board last week, and the consensus was very much in line with your views, except for the journalist present (Roger Horobin), who wanted something more pro-active. I am more sympathetic to his view than most of you, I think, but the question is what more would be useful, effective, and not too burdensome? So far I don't think I have identified anything, but I do think that the sort of web-page mentioned above would be a start, and so I am copying this to Asher Minns, for him to consider and discuss with John & Mike at Tyndall Central.

John

PS for Dave Griggs, I've added you to the circulation of this, as you weren't part of the previous exchanges. Your views would be very welcome.

At 12:56 24/11/2003 +0100, Stefan Rahmstorf wrote:

>Dear John,

>

>my feeling is that it is not the role of scientific institutions to do
>this kind of thing - this is likely going to backfire, in that it
>damages our reputation if we get involved in "dirty" battles with
>climate sceptics on the internet. There is a great danger in being seen
>as one party in a fight. We need to emphasise that we are neutral
>scientists who look at all the evidence and give reasoned and balanced
>information to the public, rather than being advocates for a particular
>cause. The sceptics are exactly trying to push us into that corner:
>they claim we are "believers" and advocates of some ideology. One has
>to be very careful to avoid this impression, by being very careful
>about when and how to respond to sceptics. It is more important to
>present our own work and conclusions than to be responsive (and easily
>perceived as defensive) with respect to sceptics. We should not let
>them set the agenda and the terms of debate. This is not to say we
>should never respond to sceptics - sometimes this is indeed necessary,
>but with caution, and I am still learning how best to do this. To give

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>some examples. I have learned (the hard way) that it is completely
>futile and counterproductive to respond to the mass e-mailing by people
>like Timo Hämeranta etc. - this is a no-win situation. It only gives
>their e-mail fora an importance that they don't deserve, if reputable
>scientists get involved there. Journalists who are on these lists start
>to believe that this is where the important discourse on global warming
>science takes place - and it's the sceptics who set the agenda there.
>Hence my advice
>is: only write to these people to ask to be taken off their
>distribution
list.
>A positive example: a group of us has compiled a web site "sceptics
>ask, scientists answer" (in German), this site is hosted bei the German
>government's environment agency (Umweltbundesamt) at
><http://www.umweltbundesamt.de/klimaschutz/faq.htm>
>It has responses to all the favorite sceptics arguments, and whenever
>some journalist or member of public asks about any sceptics arguments,
>we can simply refer them to this site. Thus, at least the scientific
>community cannot be accused of not having answers to the sceptics
>stuff. If Tyndall wants to do something about the sceptics, perhaps
>hosting a similar site in english would be a good idea. Finally a mixed
>example: a group of 14 scientists issued a media release questioning
>the Shaviv&Veizer paper some weeks ago, since this was taking on a life
>of its own in the media, being heralded as disproving global warming.
>You can find this (also in english) at
>http://www.pik-potsdam.de/news/discussion/web_uebersicht.html
>Even though this was quite a reaonable response by reputable
>scientists, pointing out the scientific flaws in the paper, the media
>response (even in left-wing papers) was partly rather negative (like:
>the scientific establishment is sulking because they had their favorite
>toy, CO2, taken away from them....) Hence my words of caution - if you
>get too involved with sceptics, you start to be seen as partisan.
>Lastly: what is important, I think, is writing scientific rebuttals for
>journals to the sceptics papers that have recently appeared in
>journals.
>
>Stefan
>
>--
>Stefan Rahmstorf
>Potsdam Institute for Climate Impact Research (PIK)
>For contact details, reprints, movies & general infos see:
><http://www.pik-potsdam.de/~stefan>

4161. 2003-12-03

date: Wed Dec 3 10:13:20 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: GEC
to: <k.brown@uea.ac.uk>, "'Neil Adger'" <N.Adger@uea.ac.uk>
Kate,
Sounds an interesting option to consider.
Let's see what Elsevier come back with. Martin was getting £8,000 p.a. in
total.
Mike
At 09:24 03/12/2003 +0000, Katrina Brown wrote:
Mike and Neil
IF our joint editorship of GEC goes ahead, I have a suggestion of who we could
employ as
Editorial Assistant. I have a new PhD student called Mike Robbins. He did his
MA here
last year and I supervised his dissertation he shared the dissertation prize
with a top

cg2003

mark of 80% and wrote it with little support from me. He used to be a science writer for

one of the CGIAR centres ICRISAT or ICARDA I think he is in mid-late 40s. He writes

really well and would be more than capable of doing a first sifting and evaluation of

papers. He is researching soils and carbon sequestration but has good background on

environment, climate change, agriculture etc. He is funding himself and will be looking

for consultancy work so it may be attractive to him as it would provide a little bit of

steady income, have flexible hours and be UEA based.

Let's keep him in mind let me know if you would like me to sound him out, but otherwise

I won't mention it yet

Kate

Dr Katrina Brown

School of Development Studies

University of East Anglia

Norwich UK NR4 7TJ

Tel: +44 (0)1603 593 529

Fax: +44 (0)1603 451 999

5227. 2003-12-03

cc: Asher Minns <A.Minns@uea.ac.uk>, John Shepherd <j.g.shepherd@soc.soton.ac.uk>, David Cromwell <ddc@soc.soton.ac.uk>, Peter Challenor <P.Challenor@soc.soton.ac.uk>, gschmidt@giss.nasa.gov, h.j.schellnhuber@uea.ac.uk, "B.E. Launder" <mcjtsbl@fs4.umist.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Katy Hill <klh@soc.soton.ac.uk>, "Quinn, Rachel" <Rachel.Quinn@royalsoc.ac.uk>, Laura Middleton <Laura.Middleton@uea.ac.uk>

date: Wed, 03 Dec 2003 15:57:10 +0100

from: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>

subject: Re: to engage or not

to: "Griggs, Dave" <dave.griggs@metoffice.com>

Dear Dave,

your comment about "skeptics" is of course correct - being skeptical is a scientific

virtue, maybe even the essence of science.

What we are really talking about is not skeptics, but deniers - people who are deliberately

trying to mislead the public about global warming with pseudo-scientific arguments.

Unfortunately the term "climate skeptics" is already established for these people.

Attached is a nice cartoon from the Washington Post this week.

Stefan

--

Stefan Rahmstorf

Potsdam Institute for Climate Impact Research (PIK)

For contact details, reprints, movies & general infos see:

[1]<http://www.pik-potsdam.de/~stefan>

Embedded Content: WashPostCartoon.gif: 00000001,02ef452f,00000000,00000000

65. 2003-12-05

date: Fri, 5 Dec 2003 19:41:04 +0700

from: "Pak Sum Low" <low@un.org>

subject: Re: Various

to: <m.kelly@uea.ac.uk>

-----boundary-LibPST-iamunique-2118148202_--

Content-Type: text/plain; charset="us-ascii"

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Dr Tin Ponlok can possibly get the Minister of the Environment of Cambodia to sign the letter.

----- Forwarded by Pak Sum Low/BKK/UNO on 05/12/03 19:39 -----

Pak Sum Low

To: <m.kelly@uea.ac.uk>

05/12/03 19:36

cc:

Subject: Re: Various(Document link:

Pak Sum Low)

I can approach Ms Martha Perdomo, Manager of Non-Annex I Programme of UNFCCC secretariat or Dr George Manful, UNFCCC GEF Coordinator on your behalf if you think that it is useful.

Other suggestrion are:

Mr Chow Kok Kee, D-G of Malaysian Meterological Services, former SBSTA Chair (1998-2000). He is a good friend. I'm sure that he will write something for you.

Mr Mahboob Elahi, D-G of SACEP (South Asia Cooperative Enviroment Programme), and former D-G of Department of Environment of Pakistan. Also a good friend.

I can also getr the UNFCCC Focal Point of Uzbekistan, Dr Tatyana Osokova, to write something. But she is now in Milan.

Also special adviser to the Minister of Environment of Cambodia and UNFCCC project Focal Point of Cambodia , Dr Tin Ponlok.

Pak Sum

"Mick kelly"

<low@un.org>

<m.kelly@uea.ac.u

To: "'Pak Sum Low'"

k>

cc:

Subject: Various

05/12/03 19:19

Please respond to

m.kelly

It's ok - I got his autoreply and relayed the invoice request to his colleagues as suggested.

Incidentally, do you know of any high level types that might write a line or two (no more) of support for Tiempo. We need this kind of evidence (again!) from southern politicians or govt officials or maybe UNFCCC/IPCC people that the project is needed... Asap!

Mick

Mick Kelly Climatic Research Unit
School of Environmental Sciences
University of East Anglia Norwich NR4 7TJ
United Kingdom
Tel: 44-1603-592091 Fax: 44-1603-507784

cg2003

Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

> -----Original Message-----

> From: Pak Sum Low [mailto:low@un.org]
> Sent: 05 December 2003 12:04
> To: m.kelly@uea.ac.uk
> Subject: RE: Tiempo sponsorship
> Importance: High

> I forgot to tell you that Matt is away until 16 Dec.

> "Mick Kelly"

> <m.kelly@uea.ac.u To:
> "'Pak Sum Low'" <low@un.org> cc:
> k>

Subject: RE:

> Tiempo sponsorship

> 05/12/03 19:00

> Please respond to

> m.kelly

> OK - if I can track down a better copy I'll do so.
> Mick

> Mick Kelly Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia Norwich NR4 7TJ
> United Kingdom
> Tel: 44-1603-592091 Fax: 44-1603-507784
> Email: m.kelly@uea.ac.uk
> Web: <http://www.cru.uea.ac.uk/tiempo/>

> > -----Original Message-----

> > From: Pak Sum Low [mailto:low@un.org]
> > Sent: 05 December 2003 11:53
> > To: m.kelly@uea.ac.uk
> > Subject: RE: Tiempo sponsorship
> > Importance: High

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[illegible]

cg2003

> > United Kingdom
> > Tel: 44-1603-592091 Fax: 44-1603-507784
> > Email: m.kelly@uea.ac.uk
> > Web: <http://www.cru.uea.ac.uk/tiempo/>

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2501. 2003-12-05

cc: <m.hulme@uea.ac.uk>
date: Fri, 5 Dec 2003 17:02:46 -0000
from: "Neil Adger" <N.Adger@uea.ac.uk>
subject: Fw: Invitation to Jan. 22 and 23 Workshop in Japan
to: <j.koehler@econ.cam.ac.uk>

Jonathon

Obviously don't want diversity of models per se. Sorry about that. Lets keep our eye on the process.

Neil

----- Original Message -----

From: "John Weyant" <weyant@stanford.edu>
To: "Neil Adger" <N.Adger@uea.ac.uk>
Sent: Friday, December 05, 2003 4:19 PM
Subject: Re: Invitation to Jan. 22 and 23 Workshop in Japan

> Neil:

> Sorry you can't make it! Gary Yohe thought you would be one of the of best

> people in the world

> to represent the climate change impacts community in this process. Also, > thanks for the

> recommendation of Jonathan Kohler, but I can't invite him to this meeting > as we are

> trying to keep it quite small and are long on people who have built major > integrated

> assessment models. In fact, I have not been able to invite about a half > dozen of the teams who

> have been active in our studies over the years. once are process starts > rolling we will

> be able to invite more people like

> Jonathan.

> Best,

> John W.

>

>

> At 10:08 AM 12/4/2003, you wrote:

> >Dear John

> >

> >Many many thanks for the invitation. Unfortunately I cannot make the dates

> >due to prior commitments here in the UK. Can I suggest an alternative

> >attendee? Jonathon Kohler from the Tyndall Centre here and from Dept Applied

> >Economics in Cambridge would be an excellent person to have at the meeting.

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> >As you may well know Jonathon is leading the Integrated Assessment
modelling
> >efforts here in the Tyndall Centre. He can be contacted at the email
> >addresses above.
> >
> >Best wishes
> >
> >Neil
> >
> >----- Original Message -----
> >From: "John Weyant" <weyant@stanford.edu>
> >To: <n.adger@uea.ac.uk>; <U7642DC@VM1.HQADMIN.DOE.GOV>;
> ><cubasch@zedat.fu-berlin.de>; <delachesnayes@stanford.edu>;
> ><jae@pnl.gov>;
> ><bfisher@abare.gov.au>; <hjacoby@MIT.EDU>; <gjjenkins@metoffice.com>;
> ><jtkon@cgd.ucar.edu>; <hskhesh@erenj.com>; <naki@iiasa.ac.at>;
> ><RRICHELS@epri.com>; <schlesin@uiatma.atmos.uiuc.edu>; "Richard Tol"
> ><tol@dkrz.de>; "vanvuuren" <Dettef.van.Vuuren@rivm.nl>; "Tom Wigley"
> ><Tom.Wigley@qgate.ucar.edu>; "gary yohe" <gyohe@wesleyan.edu>
> >Sent: Wednesday, December 03, 2003 6:43 AM
> >Subject: Invitation to Jan. 22 and 23 Workshop in Japan
> >
> >
> > > All:
> > > Please see attached invitation.
> > >
> > >
> > > Professor John P. Weyant
> > > Department of Management Science and Engineering
> > > Room 446 Terman Building
> > > Stanford University
> > > Stanford, CA 94305-4026
> > > Phone: (650)-723-3506
> > > Fax: (650)-725-5362
> >
> > Professor John P. Weyant
> > > Department of Management Science and Engineering
> > > Room 446 Terman Building
> > > Stanford University
> > > Stanford, CA 94305-4026
> > > Phone: (650)-723-3506
> > > Fax: (650)-725-5362
> >
> >

4869. 2003-12-07

date: Sun, 7 Dec 2003 07:49:37 -0800 (PST)
from: Stephen H Schneider <shs@stanford.edu>
subject: Dessai-Hume review (fwd)
to: Mike Hulme <m.hulme@uea.ac.uk>
Hi Mike, hope all is well. Haven't heard back about the April review and
my very close schedule--any decisions?
I forward to you--spoke to Suraje already about it--my review of your
excellent paper on uncertainties, but of course a few mostly
narcissistic nit-picks. hope it is useful. Cheers, Steve
PS pls forward to Suraje, I've misplaced my address book

Stephen H. Schneider, Professor
Dept. of Biological Sciences
Stanford University
Stanford, CA 94305-5020 U.S.A.
Tel: (650)725-9978

Fax: (650)725-4387
shs@stanford.edu

----- Forwarded message -----

Date: Sun, 7 Dec 2003 07:40:24 -0800 (PST)

From: Stephen H Schneider <shs@stanford.edu>

To: Climate Policy <climatepolicy@imperial.ac.uk>

Subject: Dessai-Hume review

HI Ray, sorry to take so long with this, but I finally read it on the plane to COP9--just discussed my minor complaints with Suraje, so he knows who I am--I nearly always self-confess, as I encourage most Climatic Change reviewers to do, but of course I do not insist.

In short, this is an excellent review, brings lots of literature in--some of which even I who am at the center of this uncertainties battle--didn't know, so it will be a clearly valuable entry for the Climate Policy readership and beyond. It lays out the paradigmatic differences among groups fairly, and tries to be neutral in laying out pros and cons. Some in certain schools will think that wimpy, but it is the best summary I've seen of the state of the art, so my hat off to Suraje and Mike for a fair and balanced piece. It could be shorter and still make it's main points, but then some of the excellent scholarship would be lost so I vote to publish it about how it is now. Of course, I have a few nit-picks, mostly narcissistic, which I'll list below. Other than that I think it should be provisionally accepted right now subject to a final version that deals with my minor comments and other reviewers comment--presuming you get some of those too.

P10--analogs discussion. While literature is cited about analogs to past adaptation, the authors need to warn the readers that global change forcing may be unique and no-analog impacts seem likely, so analogs, either to paleoclimatic states or adaptations are just the backdrop against which we calibrate our understanding of how the system works, but not necessarily analogs to the unique and transient changes now evolving. Also on this page, in the middle, the Pielke and Sarewitz little polemical sentence is quoted suggesting irrelevancy of probabilities for "climate adaptation policy". This is a speciesist prejudice--only humans count. For plants and animals, for which adaptation is much less likely, but systems would be damaged, for humans to decide how much they worry about this possibility, relative to other calls on our scarce resources, probabilities are essential, not irrelevant. The likelihood of 2 versus 5 degrees is the difference between some species lost and a mass extinction event. Also, in a sentence below the meaningless word "accurate" is given. As Moss and I complained two thousand times in the TAR, words like accurate, definitive, certain etc are meaningless rhetoric if not defined versus a quantitative scale of subjective probabilities, since one analyst's "accuracy" might be a 95% chance of something being true, where another's is a 5% chance because they adhere to precaution rather than proof. Just unpack this a bit with caveats along the lines I call for here.

P14; The worry that uncertainty may increase with more research is a certainty, in fact lots of literature--including later in this paper--show how climate sensitivity has grown with research. Of course it will narrow as nature continues to perform the warming experiment, but no need to be tentative--some things will grow less certain, others more as research progresses, depending on the maturity of the field at the point of the research increase and to some extent on luck. More complex systems more likely to have uncertainty grow at first with more research than simple well-constrained systems.

P15

The point that neither I nor Naki/Arnulf explicitly mentioned reflexivity" is a bit unfair for two reasons. (1) We were debating in a narrow column-SRES scenarios/storylines which were self-constructed to be "policy independent". Now they can criticize rightly SRES for thinking such a thing is meaningful, but we kept our debate in those citations to those issues mainly for the one-point-at-a-time principle. (2) The second reason

is in my rebuttal to Naki/Arnulf a year later in Climatic Change (2002) that Suraje/Mike do cite, I explicitly address this as in the quoted section below (see especially the caps), though I don't use the word "reflexive" but rather feedback, but it means the same (quote on page 445 of my Editorial):

Moreover, Grübler and Nakicenovic (2001) also argue that probabilities in natural science are different from those in social science, since we can perform frequency experiments in the former, whereas in the latter we must make judgments.

Grübler and Nakicenovic say that in an interdisciplinary scientific assessment, the concept of probabilities as used in natural sciences should not be imposed on the social sciences. Probability in the natural sciences is a statistical approach relying on repeated experiments and frequencies of measured outcomes, in which the system to be analysed can be viewed as a 'black box'. Scenarios describing possible future developments in society, economy, technology, policy and so on, are radically different. First, there are no independent observations and no repeated experiments:

the future is unknown, and each future is 'path-dependent': that is, it results from a large series of conditionalities ('what if. . . then' assumptions) that need to be followed through in constructing internally consistent scenarios. Socio-economic variables and their alternative future development paths cannot be combined at will and are not freely interchangeable because of their inter-dependencies.

However, natural scientific projections for the future still require judgments,

as no frequency experiments can be made before the fact. We must still assume

that our assumptions which govern the structural design of our systems models

will hold in the future, often for values of dependent variables that are outside

of the range of past experience. Moreover, there are conditionalities in natural science as well, and the solutions are, like Grübler and Nakicenovic

rightly assert for social systems, 'path dependent' for natural systems as well as social

systems.

Therefore, I believe there is no in principle difference between natural and social

sciences in this regard, since both require feedback mechanisms and contain path

dependent systems. However, I agree there is one aspect in which social systems

are harder to predict than natural systems. Although in both social and natural

systems interactions among subsystems can cause alterations over time, IN THE CASE

OF SOCIAL SYSTEMS, CHANGING BELIEFS AND ATTITUDES, THEMSELVES PARTIALLY DRIVEN BY INFORMATION ABOUT HOW THE SYSTEM IS EVOLVING, CAN LEAD TO MODIFICATIONS OF POLICY CHOICES.

While the latter property of social systems is different in kind from natural

system predictions, to me both natural and social systems models involve the

necessity to model feedback processes, and thus are very similar. In essence, we

need a systems model that explicitly deals with the many subcomponents that we

believe will influence the evolving emergent properties of a complex

socio-natural system, and that when social sciences are included, the system becomes more complex in detail, but not necessarily in principle. For us simply to redefine the classical definition of risk to consequences alone, because subjective probabilistic analysis is fraught with deep uncertainties, is in essence to offer no advice to the policy community as to how it should order its investments in alternative actions, for without probabilities it is very difficult to engage in risk management. And if we in the scientific assessment business do not offer some explicit notions of the likelihood of projected events, then the users of our products - policy analysts and policy makers - must guess what we think these likelihood estimates are. That is hardly preferable in my view to a carefully worded set of subjective probabilistic estimates in which our (often low) confidence in such estimates accompanies any likelihood statements.

p16-be careful about nobody does reflexive modeling assertions. What about the whole integrated assessment cabal with agent-based decision making responding to evolving climate and mitigation costs. Nordhaus' DICE is the most famous example. I have been personally critical of the assumptions he and other neo-classical economists use in their current models, but in principle they are modeling human reactions to evolving climate and imposing policy changes that feed back on the climate and society. In fact I've said one gets emergent properties of coupled socio-natural systems in the pages of Climate Policy-particularly when abrupt changes are included. See:

Mastrandrea, M. and S.H. Schneider, 2001: Integrated Assessment of Abrupt Climatic Changes. *Climate Policy*, 1, 433-449.

So, to be sure feedback-reflexivity-is a major obstacle as asserted, but because it is hard doesn't mean there haven't been some heroic-even if weak-attempts and that many more will and should be forthcoming. Just tell the story straight.

P18-Myles Allen has already started, not about to as said at bottom. Might also note that part of the model-data inter-comparison test will reveal model errors, part will reveal errors in the forcings used to drive the model simulations and some error will be in the instrumental data themselves. Thus independent tests-like looking for climate signals in plants and animals--also needed. See, e.g.:

Root, T.L., J.T. Price, K.R. Hall, S.H. Schneider, C. Rosenzweig, and A. Pounds, 2003: Fingerprints of Global Warming on Wild Animals and Plants. *Nature*, 421, 57-60.

P24. I think the Clark /Pulwarty quote is itself misleading, since it is missing an essential requirement (in the Moss/Schneider guidance paper to IPCC on uncertainties), which is all probabilistic info-via pdfs, presumably, should also contain a measure of subjective confidence in the pdf itself. So I fully agree we should not wait for perfect information via a single pdf, but we can offer pdfs AND confidence assessments of them in the meanwhile, as better than offering no pdfs at all. Just because some who do not understand probabilities will also not understand probabilistic formulations for problems other than that of climate policy-how about medical or military policy. We cannot refuse to do probabilistic information because of ignorance outside of us, when that is the most honest assessment of the state of the art. What is called for in my view is expert popularization using gambling, health and insurance

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metaphors to make probabilistic formulations clearer to non-specialists, not abandonment of the most honest descriptors of the state of the art. Most scientists are obscure and lousy popularizers I admit, but correct the problem right, not by suppressing pdfs and subjective confidence estimation-that is my view and I don't expect the authors to necessarily agree with it but I do expect they will raise these issues explicitly in their text and give their views.

P25, 1st paragraph-anthropocentrism again.

P 26

Statement "Human reflexive uncertainty is unquantifiable in probabilistic terms" is certainly wrong-it has been done in the economics/integrated assessment literature for a dozen years already. Now, is it very credible?--that is another thing. Some predictions-like production will respond to price signals--probably pretty robust, whereas others-how will future generations see the intrinsic value of a songbird-much tougher to have even medium confidence in. But ALL are quantifiable via various techniques: modeling, CV or decisional analytic elicitations. That is where the confidence assessment part comes in, for some such predictions will carry very low confidence and that must be said explicitly-but not all will and thus don't over generalize or miss the distinction between the possibility of quantification per se and its relative credibility-two different things that should be explicitly separated in the text.

OK That's my nit-pick list. I look forward to seeing this in Climate Policy soon.

OK Ray, LET ME KNOW IT CAME OK. i'll ATTACH A WORD VERSION OF THE LETTER IF THAT IS USEFUL TO YOU. Cheers, Steve

Stephen H. Schneider, Professor

Dept. of Biological Sciences

Stanford University

Stanford, CA 94305-5020 U.S.A.

Tel: (650)725-9978

Fax: (650)725-4387

shs@stanford.edu

Attachment Converted: "c:\eudora\attach\Dessai-HumeReview.doc"

1285. 2003-12-08

date: Mon, 8 Dec 2003 16:57:51 -0000
from: "Richard Starkey" <r.starkey@umist.ac.uk>
subject: RE: Will Hutton's A-level essay
to: "Mike Hulme" <m.hulme@uea.ac.uk>

Hi Mike

Thanks a lot for the info - extremely interesting.

I know that SBC is editor of EE and I know she is a CC sceptic. Is his the "hidden agenda"

of EE too? Do you think WH was briefed by SBC? If I get round to replying to WH, would it

be legitimate to mention the contentious (non-neutral?!?!) nature of MM03 or would this do

more harm than good?

Any draft reply, I would of course be happy to run by you and Asher.

Richard

PS Copying to Kevin for his interest. (He might also help me draft a reply!)

-----Original Message-----

From: Mike Hulme [mailto:m.hulme@uea.ac.uk]

Sent: 08 December 2003 14:10

To: Richard Starkey

Cc: simon.shackley@umist.ac.uk; a.minns@uea.ac.uk

Subject: Re: Will Hutton's A-level essay

Richard,

The McIntyre and McKittrick paper (MM03) has got a hidden agenda behind it. Check out this

cg2003

web site for some commentary on it. As with the contentious Soon and Baliunas paper, MM03

has been published by Energy & Environment and is part of Sonja Christriansen-Boehmer's on-going campaign.

[1]<http://holocene.evsc.virginia.edu/Mann/EEReply.html>

So while not endorsing this attempt at undermining our basis for current exceptional global

warming, I must say I find myself in sympathy with much of what Will Hutton writes. In

particular his conclusion that the debate around climate change is fundamentally about

power and politics rather than the environment seems undeniable. There are not that many

"facts" about (the meaning of) climate change which science can unequivocally reveal.

I am copying this to Asher Minns, since Asher has been giving the issue of "sound science"

and Tyndall's reaction to it some thought recently.

Mike

At 11:37 08/12/2003 +0000, you wrote:

Dear Mike

Did you see Will Hutton's article in the Observer yesterday. See

[2]<http://observer.guardian.co.uk/columnists/story/0,12877,1101658,00.html>

An appalling article in my view. One of the key paras is

An important and neutral paper by Canadians Steven McIntyre and Ross McKittrick suggests

that the best guess is that, while temperatures are currently rising, they probably lie

within the range for the past 600 years. Environmentalists, just as in a battle over a new

runway, are being as partisan in their use of science as their opponents.

Do you know of these (neutral!!!!!!!!!!!!) guys and their paper. Do they have credibility?

Is Hutton's interpretation correct?

I'd like to do a reply but could do with some insight into the science.

Richard

Richard Starkey

Researcher

Tyndall Centre for Climate Research

UMIST

PO Box 88

Manchester M60 1QD

Tel: +44 (0) 161 200 3763 (direct)

1849. 2003-12-08

cc: mann@virginia.edu

date: Mon, 08 Dec 2003 08:37:17 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: Fwd: Mann, Bradley and Hughes

to: Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@Princeton.EDU>,

Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>,

<rbradley@geo.umass.edu>, <mhughes@ltrr.arizona.edu>, Tom Wigley

<wigley@meeker.UCAR.EDU>, tom crowley <tom@ocean.tamu.edu>, Gabi Hegerl

<hegerl@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>, rbradley@geo.umass.edu,

mhughes@ltrr.arizona.edu

Dear Tim,

Thanks for the message These guys, as Tom W has noted previously, seem to simply to simply

want to try to make as big a stink as possible here. They didn't get the media

cg2003

attention

they wanted (and got blasted in the one mainstream news article on this that appeared in

USA Today a couple weeks back), and they haven't been taken seriously by the scientific

community so I guess they're trying to generate any controversies they can. I would

STRONGLY encourage you not to bother responding to any of their emails under any circumstances. History has proven consistently (talk to Phil!) that they'll

simply try to

take anything you say out of context, and turn your own words on you. This is what they did

w/ the attempts on our part to help them in response to their initial inquiries, which they

twist and distort in their comments below (we I only told them I wouldn't respond to

further inquiries after the tone of their emails had become unacceptable, and their hostile

intent clear--something this guy, as just about everything else, conveniently distorts...

They've been making threats against NSF about supposed data policies and even against Ray,

Tom Crowley, and others too, claiming that they have a right to all of our data and

computer programs (the hubris!). Confidentially, NSF lawyers have found their threats

baseless as well as obnoxious, and will be telling them formally that NSF policy in no way

legally requires funded scientists to provide their data (let alone computer codes!) for

public access, but scientists are *encouraged* to provide their data. NSF will be telling

them to stop pestering them. I'm forwarding a formal email (based on numerous informal

discussion w/ Dave Verardo) to NSF, which is confidential (!), that provides some more

information....

As we all know, we had made all of our data available previously, so the accusations by

these bozos are baseless, though we agree that we would have given more care to the

completeness of documentation had we known a stunt like this was to be pulled by the

contrarians..

Confidentially, we will be releasing a revised, more user-friendly version of the dataset

(all of the data, including the CRU temperature dataset we used, which isn't available any

longer) in concert w/ our published reply to their paper, submitted to "Climatic

Change"---will keep you posted on status to their paper. We can make a copy of the

manuscript available to anyone who wants to see it, but we don't want to corrupt the

potential reviewer pool prior to selection of reviewers, so we've resisted sending this

out to colleagues yet. The data will also be available on Nature's supplementary information website (we're working w/ Nature on this right now).

mike

At 02:34 PM 12/8/2003 +0000, Tim Osborn wrote:

Dear all,

see the forwarded message. McIntyre is attempting to rope CRU into the ongoing fall-out

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from their paper in E&E, apparently because we "published" MBH's preliminary response by posting it on our website. Anyone got any comments, before I reply to say that I don't consider appearance on a web page as publication, and hence we aren't in a position to ask MBH for any data or

programs.

Cheers

Tim

From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>

To: "Tim Osborn" <t.osborn@uea.ac.uk>

Cc: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>, "Ross Mckitrick" <rmckitri@uoguelph.ca>

Subject: Mann, Bradley and Hughes

Date: Mon, 1 Dec 2003 15:57:06 -0500

X-Mailer: Microsoft Outlook Express 6.00.2800.1158

Dear Dr. Osborn,

We regret that you declined our offer to submit our forthcoming paper to CRU/UEA for

review, especially since you had been critical about Energy & Environment review

public policies. Our offer reflected our desire for the highest possible standard of debate on these matters.

UEA/CRU recently published an article by Mann, Bradley and Hughes ("MBH-r") responding

to our paper in Energy & Environment, together with your own editorial comments. We are

seeking the following supplementary information in connection with this article and

commentary:

1) an identification of the 159 series, referred to in MBH-r;

2) a copy of the computer programs used to collate input data and

generate the output data plotted in the Figure in MBH-r;

3) verification that these programs are the same as the corresponding programs used

in MBH98 and, if not, a copy of the programs used to collate input data and generate

output data for MBH98.

We have requested this information from Professor Mann, but he has refused and has cut

off further communication. In your capacity as publishers of his response article, we

accordingly request the information from you directly.

We have some other concerns with your own commentary on our article in Energy &

Environment. We do not claim to show that 15th century temperatures were higher than

20th century temperatures. We only claim that application of MBH methods to corrected

and updated data do not entitle MBH to claim 20th century uniqueness. We do not endorse

the MBH98 methods and consequently did not put forward a reconstruction of our own.

You also stated that we did not attempt to investigate the differences of results with

MBH. This is untrue and indeed unfair. The email record shows clearly that we sought

clarifications from Mann, first on our inability to replicate his temperature principal

components calculations and secondly on both verification of the integrity of

cg2003

the dataset sent to us and on further particulars of his reconstruction methodology, noting problems in the early period. Mann refused to answer and stated that he would not respond to further inquiries on the subject. It is unfair of you to blame us for the fact that the correspondence ended there without satisfactory resolution. Full disclosure of the data and methods used in MBH-r (and MBH98), as requested above, will allow all interested observers to quickly get to the core points of disagreement in our analyses. Thank you for your consideration.
Stephen McIntyre/Ross Mckitrick
Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK
e-mail: t.osborn@uea.ac.uk
phone: +44 1603 592089
fax: +44 1603 507784
web: [1]<http://www.cru.uea.ac.uk/~timo/>
sunclock: [2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

2037. 2003-12-08

cc: shackley_simon,a.minns@uea.ac.uk
date: Mon Dec 8 14:10:21 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: Will Hutton's A-level essay
to: "Richard Starkey" <r.starkey@umist.ac.uk>
Richard,
The McIntyre and Mckitrick paper (MM03) has got a hidden agenda behind it. Check out this web site for some commentary on it. As with the contentious Soon and Baliunas paper, MM03 has been published by Energy & Environment and is part of Sonja Christiansen-Boehmer's on-going campaign.
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I am copying this to Asher Minns, since Asher has been giving the issue of "sound science" and Tyndall's reaction to it some thought recently.
Mike

cg2003

At 11:37 08/12/2003 +0000, you wrote:

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An important and neutral paper by Canadians Steven McIntyre and Ross McKittrick suggests

that the best guess is that, while temperatures are currently rising, they probably lie

within the range for the past 600 years. Environmentalists, just as in a battle over a

new runway, are being as partisan in their use of science as their opponents.

Do you know of these (neutral!!!!!!!) guys and their paper. Do they have credibility? Is Hutton's interpretation correct?

I'd like to do a reply but could do with some insight into the science.

Richard

Richard Starkey

Researcher

Tyndall Centre for Climate Research

UMIST

PO Box 88

Manchester M60 1QD

Tel: +44 (0) 161 200 3763 (direct)

4080. 2003-12-08

cc: mann@virginia.edu

date: Mon, 08 Dec 2003 08:37:25 -0500

from: "Michael E. Mann" <mann@virginia.edu>

subject: Fwd: Re: data access

to: Tim Osborn <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@Princeton.EDU>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, <rbradley@geo.umass.edu>, <mhughes@ltrr.arizona.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Tom Crowley <tom@ocean.tamu.edu>, Gabi Hegerl <hegerl@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu

Date: Fri, 05 Dec 2003 13:39:00 -0500

To: "Verardo, David J." <dverardo@nsf.gov>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: data access

Cc: mann@virginia.edu

Dear Dave,

Thanks for your inquiry.

As we encourage any good-faith attempts by other scientists to repeat our analysis, we

have indeed already made the data associated with our NSF-funded research which includes

the Mann et al, 1998 Nature article ('MBH98'); Mann et al, 1999 GRL article ('MBH99'),

and Mann et al, 2000 Earth Interactions article, available publicly.

All of the time series data shown in MBH98 (the hemispheric temperature reconstruction

and uncertainties, the reconstructed principal components "RPC" series, etc) were made

available both on this website:

[1]<http://www.people.virginia.edu/~mem6u/mbh98.html>

and through the NOAA paleo data site:

[2]<http://www.ngdc.noaa.gov/paleo/pubs/mann1998/frames.htm>

at the time of publication.

All data (proxy indicators used and the reconstructions and uncertainties) associated

cg2003

with MBH99 were made available at the time of publication, here:

[3]<http://www.people.virginia.edu/~mem6u/mbh99.html>

as well as here:

[4]ftp://ftp.ngdc.noaa.gov/paleo/contributions_by_author/mann1999/

We then made the detailed yearly spatial reconstructions available in 2000 at the NOAA

paleo website:

[5]http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html

From the time of publication of MBH98, a listing of all of the proxy data (with some minor typos) was provided here:

[6]<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/MultiProxy/data-suppl.html>

while details of the number of proxy indicators used in the stepwise reconstruction approach were provided here:

[7]<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/MultiProxy/stats-suppl.html>

and the instrumental temperature data, including eigenvectors and eigenvalues, and

instrumental series shown in the various figures, were provided here:

[8]<ftp://eclogite.geo.umass.edu/pub/mann/MANNETAL98/>

All of the proxy data used in MBH98 were made available on our public ftp site once the

various researchers that contributed data to our network were able to publish their own

data (July 2002). The data (all individual proxy indicators used as well as the various

PC representations of proxy sub networks for different time intervals) were provided in

the various clearly labeled directories here:

[9]<ftp://holocene.evsc.virginia.edu/pub/MBH98/>

We provided extensive documentation of the data sets used in the supplementary information lodged at Nature's web site in association with the publication of MBH98, so

that those wishing to repeat our analyses could either go to the same public domain

sources as us, or approach the colleagues who had kindly made data available to us. We

made considerable efforts to make the various data and numerical results readily

available online as soon as we were free to do so (in 2002), by setting up the public

ftp site referred to above, although we were under no known obligation to provide the

data in that particular medium. We gave as detailed a description of our methods as was

possible in the confines of a short paper, and in all these respects must have satisfied

the stringent standards set by the editor and reviewers of the journal in which we

published.

In order to facilitate any attempt to reproduce our results we are now taking a further

step beyond those normally required in the publication of such research. We are working

with Nature to provide the MBH98 proxy data set in a more transparent, user-friendly

format than that set up in 2002, including additional documentation, fixing of minor

typos in the descriptions of different datasets, and providing some additional minor

cg2003

methodological details of the MBH98 analysis. We are also providing the full
raw instrumental University of East Anglia/Climatic Research Unit surface
temperature dataset 1854-1993 (Briffa and Jones, 1992), because CRU has since updated
their surface temperature dataset, and no longer archives the version that we used when we
began our study in the mid 1990s.
Please let me know if there is any additional information I can provide you
that would be of help in this matter. I will of course update you once we/Nature have
released the revised data archive.

Best regards,

mike

At 11:31 AM 12/5/2003 -0500, you wrote:

Dear Mike,

With regards to the recent request made by Stephen McIntyre and Ross
Mckittrick for access to data that you and your colleagues used in a series
of peer-reviewed publications, please let me know how and when you are planning
to release the data relevant to their request.

Thanks in advance for your help.

Dave

David J. Verardo
Director, Paleoclimate Program
Division of Atmospheric Sciences (Room 775)
National Science Foundation
4201 Wilson Blvd.
Arlington, VA 22203
phone: 703-292-8527
fax: 703-292-9023
email: dverardo@nsf.gov
[10]<http://www.nsf.gov>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[11]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[12]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

4599. 2003-12-08

date: Mon, 08 Dec 2003 14:59:45 +0000

from: Phil Jones <p.jones@uea.ac.uk>

subject: Grape Harvest Dates

to: k.briffa@uea.ac.uk

<x-flowed>

>Date: Mon, 8 Dec 2003 14:01:54 +0000

>Subject: Re: Scottish Mainland (SMT), Island (SIT) and N. Irish temps

>From: Ian Harris <i.harris@uea.ac.uk>

>To: Phil Jones <p.jones@uea.ac.uk>

>X-Mailer: Apple Mail (2.552)

cg2003

>
>On Monday, December 8, 2003, at 01:35 PM, Phil Jones wrote:
>> Harry,
>> There is a paper on this submitted to IJC. David Lister has a copy
>> which I'm happy for
>> you to pass on. It has all the locations.
>
>Cheers, Phil - he's looking for it now.
>
>Here are the Grape Dates. I've put the folder in your pigeonhole, adding
>my own printout. I've check it all once (finding a single error).
>
>Cheers
>
>Harry
>
>(begins)
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>Ian "Harry" Harris

cg2003

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>United Kingdom
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NR4 7TJ
UK

</x-flowed>

377. 2003-12-09

date: Tue, 9 Dec 2003 12:21:15 +0000
from: Ian Harris <i.harris@uea.ac.uk>
subject: Re: data access
to: Keith Briffa <k.briffa@uea.ac.uk>
<x-flowed>
Hi Keith,
On Tuesday, December 9, 2003, at 10:10 AM, Keith Briffa wrote:
>> Date: Mon, 08 Dec 2003 08:37:25 -0500
>> To: Tim Osborn <t.osborn@uea.ac.uk>,
>> Michael Oppenheimer <omichael@Princeton.EDU>,
>> Phil Jones <p.jones@uea.ac.uk>, Keith Briffa
>> <k.briffa@uea.ac.uk>,
>> <rbradley@geo.umass.edu>, <mhughes@ltrr.arizona.edu>,
>> Tom Wigley <wigley@meeker.UCAR.EDU>,
>> tom crowley <tom@ocean.tamu.edu>, Gabi Hegerl
>> <hegerl@duke.edu>,
>> Jonathan Overpeck <jto@u.arizona.edu>, rbradley@geo.umass.edu,
>> mhughes@ltrr.arizona.edu
>> From: "Michael E. Mann" <mann@virginia.edu>
>> Subject: Fwd: Re: data access
<snip>
>>> In order to facilitate any attempt to reproduce our results we are
>>> now taking a further step beyond those normally required in the
>>> publication of such research. We are working with Nature to provide
>>> the MBH98 proxy data set in a more transparent, user-friendly
>>> format than that set up in 2002,
Well at least he implicitly acknowledges the labyrinthine nature of the
existing site!
Aaaaaand it's obviously not just me having trouble with it :-)
>>> including additional documentation, fixing of minor typos in the
>>> descriptions of different datasets, and providing some additional
>>> minor methodological details of the MBH98 analysis. We are also
>>> providing the full raw instrumental University of East
>>> Anglia/Climatic Research Unit surface temperature dataset 1854-1993
>>> (Briffa and Jones, 1992), because CRU has since updated their
>>> surface temperature dataset, and no longer archives the version that
>>> we used when we began our study in the mid 1990s.
Ooh! Are we being scolded?
Needless to say, I have this work on a medium priority. There's plenty
of comparatively urgent work for HOLSMEER!
By the way Keith - any more news or thoughts regarding possible PhD
directions?
Cheers
Harry
Ian "Harry" Harris
Climatic Research Unit

University of East Anglia
 Norwich NR2 4HG
 United Kingdom
 </x-flowed>

531. 2003-12-09

cc: <bane@cs.man.ac.uk>
 date: Tue, 9 Dec 2003 20:02:19 -0000
 from: "Rachel Warren" <R.Warren@uea.ac.uk>
 subject: new versions of MAGICC/SCENGEN
 to: "christopher barton" <c.barton@uea.ac.uk>, <m.hulme@uea.ac.uk>
 Chris, Mike,
 FYI : the new versions of MAGICC and SCENGEN have been provided to me as
 executables.
 So far, Tom Wigley has declined to give me the source code! Apparently he is
 not giving
 the
 source code of the new SCENGEN to anyone at all. Tim Mitchell thinks it's no
 great loss
 and that
 he could easily create an alternative, which he plans to do anyway as a part of
 Nigel
 Arnell's Tyndall round 3 project.
 However, the more important question is the new version of MAGICC itself.
 Tom has offered to help convert my existing MAGICC code to a version which will
 be
 consistent with the new code.
 The reason for this is that the new version of SCENGEN, which he is not allowing
 anyone to
 have, requires different inputs from MAGICC to the original version of SCENGEN
 (so the new
 version of MAGICC won't go with the old version of SCENGEN).
 However Tom has taken on board my point that our results should be consistent,
 and offered
 to take my magicc.tar
 code and modify it so that it is scientifically the same as the new version.
 There were
 some significant changes (scientifically) between the version I was given and
 the new
 version, connected with ice melt and tropospheric ozone forcing to name a
 couple. He and I
 are having an ongoing e-mail conversation about the conversion ... I will let
 you know when
 I have a new version of code consistent with the latest version.
 I don't anticipate this creating any work for the softIAM team since inputs and
 outputs
 remain the same.
 I will simply provide revised code for MAGICC (SCM) when the time comes.
 Then we will just need to do a test case within and without the softIAM
 framework.
 When Tim Mitchell creates a new SCENGEN version next summer, the softIAM team
 (possibly
 just myself by that time)
 will need to talk to him about how this affects the interface within softIAM if
 we want to
 incorporate the new version.
 Rachel

725. 2003-12-10

cc: mann@virginia.edu
 date: Wed, 10 Dec 2003 08:44:20 -0500

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from: "Michael E. Mann" <mann@virginia.edu>

subject: Re: abstract for Clivar. Due soon!

to: Keith Briffa <k.briffa@uea.ac.uk>

Hi Keith,

Thanks, changes and suggestions all very helpful.

I've made two additional small changes--I didn't like "pseudo", so I've rephrased to

express what I think (?) you meant there. I've eliminated a redundant statement about only

anthro forcing

can explain 20th century warming (was mentioned in two places), to bring under the 400 word limit.

I think I like your idea of including others (Ray, Malcolm, Phil, Tim, Scott) as

"collaborating authors" rather than adding to the primary author list (which is just you and me).

Please let me know if you have any remaining comments, and I'll submit a final version once

I hear back from you...

thanks for getting back to me so quickly on this,

mike

At 12:44 PM 12/10/2003 +0000, Keith Briffa wrote:

Mike

I have edited and reformatted the abstract quite a bit , so you had better check it

thoroughly again. Please note that I have no real objection to Phil being a co author -

other than a possible lack of balance with two CRU people. This would be helped

considerably if we considered one more author (Ray Bradley) perhaps. What do you

think? Anyway, I am happy with the abstract now. We could include a whole group of

"collaborating authors " (as in IPCC reports) to acknowledge the input "the usual

subjects" will undoubtedly make. Cheers

Keith

At 12:36 PM 12/9/03 -0500, you wrote:

Hi Keith,

I hope all is well.

Deadline for submitting our CLIVAR abstract vastly approaches, so I've taken the liberty

of drafting an abstract, which I've attached in word format. Truth be told, I've

borrowed liberally from some other recent abstracts, including ones that Phil and I

wrote jointly--I vote we make Phil a co-author on the abstract for this reason, among

others--is that ok w/ you?

Please let me know if you have any comments, and feel free to edit and send me a revised

version. Its presently exactly at the 400 word limit, so we can't lengthen, but we can

remove. I need to submit this electronically by Friday, because I'll be travelling and

away from my email through after the deadline after then. If I don't hear from you by

Friday, I'll submit as is, w/ our three names on it...

thanks in advance for any feedback you can provide,

mike

cg2003
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Fax: +44-1603-507784
[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

1010. 2003-12-10

date: Wed Dec 10 12:54:17 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: tree rings and Rapid Climate Change
to: andreas.kirchhefer@ib.uit.no

Andreas
first congratulations on parenthood. As for the RAPID bid , I can only wish you well . As far as I interpret the British call - there is NO restriction on using ocean data only. I am not sure how the Norwegians will interpret their proposal call - but I suspect your suspicion is correct as they are strongly influenced by the oceanic community. I have supported Danny's previous proposals strongly , and was very disappointed that he did not get support under the first RAPID call . I did my best to get his proposal funded and it was on the border line , literally, when it went down . Of all the people working with isotopes in wood , he has the clearest , and reasonably honest approach and I would back him above others to produce valuable results. It remains to be seen whether these will eventually yield significantly better / different results to warrant the effort in combining the isotope and ring width / density input - but I remain supportive of him getting the chance to prove it. I have no holy insight into whether what you suggest will succeed - but from what you say , it is not likely to fly without a close link to model studies (and perhaps some support evidence of the influence of cyclone track variability from ocean circulation ?) . The is also the need to specify the nature of the "Rapid" (i.e. in time event or whatever) focus. Just saying we will produce long (even high-resolution)

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data will not likely mark your project out strongly enough. I am saying this in an attempt to be constructive - even if I sound somewhat negative.

For my part, I congratulate you on the progress you are making. I would certainly happily support any future proposals or applications for extended support you might make to the Norwegian funders.

As of now, I have no specific EC plans - though some of us are continuing to fight a long battle in Brussels to get a palaeoclimate "New instrument" included under FP6 . As I speak, this is very far from certain and even if we manage to get some palaeo work included in the call, expected in summer or autumn 2004, this may be for a vague or extended time scale (including inter glacials) and competing proposals will then likely be submitted reflecting this lack of focus. I have been pushing for a Holocene focus - linking data and models - but this is looking much less likely, if not dead already. We will not know more for some time.

Finally, one point and request. I have started to put together multiple data to construct a picture of late-Holocene temperature change in Northern Fennoscandia and hopefully (as part of an existing EC project, ALP-IMP), compare the aggregated (and hopefully more robust) series with similar tree-ring data in the Greater Alpine Region. I would be very grateful for permission to use your data in this exercise. I am also committed to give talks on tree-ring variability in early January in Bangor, and later in Bergen next year, and similarly, any chronology data you could provide me with for these talks (to produce comparison plots) would be similarly appreciated. I would also like to move towards putting a review paper together (perhaps for Quaternary Science Reviews) of long European chronology-based climate inference, and would like to do this as a joint paper with Yourself, Hakan Grudd, the Finns and other European colleagues. Any interest?

very best wishes
Keith

At 01:01 PM 12/8/03 +0100, you wrote:

Dear Keith,

together with Danny McCarroll and Neil Loader, I intend to submit a proposal to the Rapid Climate Change call (15.12.). We thought about extending our 13C-record from tree rings in Forfjorddalen back to 1500, produce another one in Scotland, and then interpret all that in terms of storm tracks and other parameters related to the North Atlantic.

Now, the Norwegian Research Council says that the program is strictly oceanographic, whereas the British program description seems to be slightly wider. I see that you are in the steering committee of RAPID, so do you have any information/advice concerning our proposal?

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Else, my postdoc-project is approaching its end (late April incl. 4 weeks of parental leave). The Forfjord-chronology is prolonged back to ca. AD 1200 (EPS 85%), and the Stongslandseidet-chronology back to ca. 1450 (final revision awaiting). Last week I probably filled the gap in the Dividalen chronology, so now it's continuous back to AD 320. The period 1000-1500 is still poorly replicated, but I have another 50 samples to be measured. I hope I can start working on the subfossil samples in spring. Fortunately, a student of Dieter Eckstein will join me in January. He'll do his master on climate response of my lakeshore pines.

Best regards,

Andreas

PS: Please, let me know if there are any EU-projects under preparation which could be relevant for me to join :)

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[1]<http://www.ib.uit.no/~andreas>

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[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

3373. 2003-12-10

date: Wed Dec 10 15:04:07 2003
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Fwd: Re: abstract for Clivar
to: "raymond s. bradley" <rbradley@geo.umass.edu>
Your comments reinforce just what I feel about the selective (appropriately tuned) use of EBMs . I want to inject some results from our SOAP project (from the HadCM3 and ECHO-G) runs (see <http://www.cru.uea.ac.uk/projects/soap/>). I think you know what I think about the 2000 year series (as does Phil) . of course it does not matter (in the greater context of the infinite universe or the shrinking context of my own remaining span) who is on the author list - it might just help in the struggle to present a balanced view of the evidence if you sit on my end of the seesaw . My back is really greatly improved and I am suffering less from the occasional seize ups that do occur. I am 95 per cent certain to go to the Fritts meeting and will be happy to let you refund some of your Stellenbosch (never could spell it) wine bill that I am still

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paying off. As for what you say about Baltimore , I am concerned to say the least.. and

might consider my own attendance. Best wishes to you

and

kisses to Jane

Keith

p.s.with regard to your comment about Cambridge,

My older daughter is in her first year at Christ's , , and loving it.

At 09:56 AM 12/10/03 -0500, you wrote:

I don't think it matters about the authorship of this...although it is true that you

need 2 CRU boys to balance a Mann...

If I could add any suggestion it would be to make it clear what you mean by: "several

modelling centres have run climate simulations based on models with varying levels of complexity .."

I don't think Mike is thinking of coupled AOGCMs here, which would be ideal, but mostly

energy balance models and MICs, and it's hard to use these to look at anything but the

very largest scales. Furthermore, the model output is very much determined by the time

series of forcing that is selected, and the model sensitivity which essentially scales

the range. Mike only likes these because they seem to match his idea of what went on in

the last millennium, whereas he would savage them if they did not.

Also--& I'm sure you agree--the Mann/Jones GRL paper was truly pathetic and should never

have been published. I don't want to be associated with that 2000 year "reconstruction".

I don't plan on going to the Clivar mtg (I assume this is the Baltimore one?).

Being

close to DC, no doubt it will attract a lot of nuts.

I hope you are well & over your back problem. Will you be coming over for the "Fritts

mtg" in Tucson in early April...I hope you will. We need to have few beers and try to

get back onto the sunny side of what we do. It's been all aggravation and gloom around

here, compounded by living in a country that has been taken over by fascists. Cambridge

never looked so good...

Ray

At 08:10 AM 12/10/2003, you wrote:

FOR YOUR INFO - IF MIKE AGREES I HOPE YOU WILL ALSO ,

AS SOME

FURTHER ELEMENT

OF BALANCE RE SCIENTIFIC JUDGEMENT AND INDEPENDENT OPINION WOULD BE

FORTHCOMING

Date: Wed, 10 Dec 2003 12:44:47 +0000

To: "Michael E. Mann" <mann@virginia.edu>

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: abstract for Clivar. Due soon!

Mike

I have edited and reformatted the abstract quite a bit , so you had better check it

thoroughly again. Please note that have no real objection to Phil being a co author -

other than a possible lack of balance with two CRU people. This would be helped

considerably if we considered one more author (Ray Bradley) perhaps. What do you

cg2003

think? Anyway, I am happy with the abstract now. We could include a whole group of "collaborating authors " (as in IPCC reports) to acknowledge the input "the usual subjects" will undoubtedly make. Cheers
Keith
At 12:36 PM 12/9/03 -0500, you wrote:
Hi Keith,
I hope all is well.
Deadline for submitting our CLIVAR abstract vastly approaches, so I've taken the liberty of drafting an abstract, which I've attached in word format. Truth be told, I've borrowed liberally from some other recent abstracts, including ones that Phil and I wrote jointly--I vote we make Phil a co-author on the abstract for this reason, among others--is that ok w/ you?
Please let me know if you have any comments, and feel free to edit and send me a revised version. Its presently exactly at the 400 word limit, so we can't lengthen, but we can remove. I need to submit this electronically by Friday, because I'll be travelling and away from my email through after the deadline after then. If I don't hear from you by Friday, I'll submit as is, w/ our three names on it...
thanks in advance for any feedback you can provide,
mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--
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Raymond S. Bradley
University Distinguished Professor
Director, Climate System Research Center*
Department of Geosciences, University of Massachusetts
Morrill Science Center
611 North Pleasant Street
AMHERST, MA 01003-9297
Tel: 413-545-2120
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*Climate System Research Center: 413-545-0659

cg2003

<[4]<http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [5]<http://www.geo.umass.edu/climate/paleo/html>

--

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[6][http://www.cru.uea.ac.uk/cru/people/briffa\[7\]/](http://www.cru.uea.ac.uk/cru/people/briffa[7]/)

4073. 2003-12-10

date: Wed Dec 10 12:44:47 2003

from: Keith Briffa <k.briffa@uea.ac.uk>

subject: Re: abstract for Clivar. Due soon!

to: "Michael E. Mann" <mann@virginia.edu>

Mike

I have edited and reformatted the abstract quite a bit , so you had better check it

thoroughly again. Please note that I have no real objection to Phil being a co author -

other than a possible lack of balance with two CRU people. This would be helped considerably if we considered one more author (Ray Bradley) perhaps. What do you think?

Anyway, I am happy with the abstract now. We could include a whole group of "collaborating

authors " (as in IPCC reports) to acknowledge the input "the usual subjects" will

undoubtedly make. Cheers

Keith

At 12:36 PM 12/9/03 -0500, you wrote:

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Deadline for submitting our CLIVAR abstract vastly approaches, so I've taken the liberty

of drafting an abstract, which I've attached in word format. Truth be told, I've

borrowed liberally from some other recent abstracts, including ones that Phil and I

wrote jointly--I vote we make Phil a co-author on the abstract for this reason, among

others--is that ok w/ you?

Please let me know if you have any comments, and feel free to edit and send me a revised

version. Its presently exactly at the 400 word limit, so we can't lengthen, but we can

remove. I need to submit this electronically by Friday, because I'll be travelling and

away from my email through after the deadline after then. If I don't hear from you by

Friday, I'll submit as is, w/ our three names on it...

thanks in advance for any feedback you can provide,

mike

Professor Michael E. Mann
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--

cg2003

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[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

3159. 2003-12-11

date: Thu, 11 Dec 2003 09:13:34 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: abstract for Clivar. Due soon!
to: Keith Briffa <k.briffa@uea.ac.uk>

Thanks Keith,

Lets adds Tom C, Simon, Hans then, and also Gavin Schmidt and Drew Shindell
since they've

been doing quite a bit of relevant work in this area, and keep it there?

Happy holidays to you too!

Look forward to being in touch again soon,

mike

At 09:08 AM 12/11/2003 +0000, Keith Briffa wrote:

All fine - I think the list is fine (but perhaps add Tom C , and Simon Tett
and Hans v

S.) . Just in case you were wondering about the model simulations I intend to
describe -

these are primarily the Hadcm3 and ECH0-G runs that we have used in our SOAP
project ,

forced from 1500 with natural only and from 1750 with natural and anthro. I
resume we

will also include similar runs from Caspar and others . All the best and a
good

christmas

Keith

At 08:44 AM 12/10/03 -0500, you wrote:

HI Keith,

Thanks, changes and suggestions all very helpful.

I've made two additional small changes--I didn't like "pseudo", so I've
rephrased to

express what I think (?) you meant there. I've eliminated a redundant
statement about

only anthro forcing

can explain 20th century warming (was mentioned in two places), to bring under
the 400

word limit.

I think I like your idea of including others (Ray, Malcolm, Phil, Tim, Scott)
as

"collaborating authors" rather than adding to the primary author list (which
is just you

and me).

Please let me know if you have any remaining comments, and I'll submit a final
version

once I hear back from you...

thanks for getting back to me so quickly on this,

mike

At 12:44 PM 12/10/2003 +0000, Keith Briffa wrote:

Mike

I have edited and reformatted the abstract quite a bit , so you had better
check it

thoroughly again. Please note that have no real objection to Phil being a co
author -

other than a possible lack of balance with two CRU people. This would be
helped

cg2003

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Keith
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Hi Keith,
I hope all is well.
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thanks in advance for any feedback you can provide,
mike

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

522. 2003-12-14

date: Sun, 14 Dec 2003 10:19:49 -0000
from: "Alan Strange" <big1a1@ntlworld.com>
subject: Re: mission
to: "f037" <M.Hulme@uea.ac.uk>
Thank you - I shall give this further thought, but I warm to it on first glance.
You may like to note the Anglican 5 marks of mission, agreed at some
terribly-high-up gathering. Interesting overlap.

To Proclaim, the good news of the kingdom
To Teach, baptise and nurture new believers
To Respond, to human need by loving service
To Seek, to transform the unjust structures of society
To Strive, to safeguard the integrity of creation and sustain and renew the
life of the earth

Ever,
Alan

----- Original Message -----

From: "f037" <M.Hulme@uea.ac.uk>
To: <big1a1@ntlworld.com>
Cc: <m.hulme@uea.ac.uk>
Sent: Saturday, December 13, 2003 9:12 PM
Subject: mission

> Alan,

>

> A few thoughts following our conversation from a few weeks back.

>

> "Towards full-member mission: a new framework for Trinity"

>

> Principles

- > 1. Mission is what the church exists for - so each member has a mission role
- > 2. A functional rather than a geographical approach to mission
- > 3. Equipping the whole church for holistic mission
- > 4. Financial portfolio should be spread broadly across the five functions
- > 5. The mission display board should reflect the five functions
- > 6. Each function should have a "champion"

>

> Five functions (in no order)

> A. Social justice - e.g. Matthew Project, the Sextons, Traidcraft, Fergus
> Drake

> B. Personal evangelism - e.g. Friends International, the Jesus video project,
> Christianity explored

> C. Community service - e.g. the Mustard Tree, the Jenny Lind sports
> initiative

> D. Training, educating, resourcing - e.g. the Smiths, Rob Crofton, the
> Bearups, Emily Singleton

> E. Professional service - e.g. Liz Bennett, David Thomson, the London
> Institute for Contemporary Christianity, Alison Vinall, Jill Leggett, Alison
> Talbert, etc., etc.]

>

> [Notes: The examples are of course exactly that, mere examples. Every member
> of the church should fit into one of the five functions. The functions can be
> fulfilled anywhere in the world, i.e. no geographic bias. "Mission partner"
> would take on a different meaning. It would be interesting to map our current

cg2003

> "mission" expenditure against these five functions].
>
> Mike
>

4525. 2003-12-16

date: Tue, 16 Dec 2003 13:36:41 +0100
from: "Andreas J. Kirchhefer" <Andreas.Kirchhefer@ib.uit.no>
subject: SV: tree rings and Rapid Climate Change
to: "Keith Briffa" <k.briffa@uea.ac.uk>

Hei Keith,

Thanks a lot for your comments. Because our odds were not good anyway, Danny just recycled the old RAPID application. We'll see what happens. I go to Swansea the week after the Bangor meeting, and we will discuss how to continue our search for funding then.

Of course, you are welcome to use my chronology data. I attach a zipped xls-file with my Dividalen chronology, as submitted to a special edition of 'Arbeiten aus dem Institut für Landschaftsökologie', Münster, in honor of my former supervisor Holtmeier, who retires next year. It still has the gap around 1200, which will be closed (well replicated?) in the next version. Do you need the original measurement data?

The updated coastal series are not really mature for distribution and presentation yet, I'm afraid. I assume that you have the old versions (ITRDB). For Stonglandseidet, I haven't even a nice figure, but I attach the preliminary series from Forfjorddalen. Be aware that before ca. AD 1100 the chronology is just rubbish. The trees included here show slow growth - that's all I can say so far. The individual tree/radius series just don't match, and cofecha-ing against Torneträsk doesn't help much. Some material still is not included, among those the former (and still) oldest snag (AD 877, date probably to be adjusted). And finally a warning regarding "Norway's oldest pine" from Forfjorddalen which I believed was nearly 800 years old. Together with Danny and Neil I took a new core to get the innermost rings. Back in the lab, the tree proved to be ca. 675 years old, only (AD 1338). I realized, that I mixed up cores from that old living and a snag, partly because the bark was lacking on both samples. I hope I learn from this and don't take cores in a rush during an excursion anymore... (I could try to blame the midges, of course - they were terrible that day).

The chronologies will be finally revised, and hopefully manuscripts submitted, until April. Then we could work on a Nordic review article. Hans Linderholm asked me for comparing his Swedish series with mine, and a review article could be useful when applying funding for a more detailed synthesis of the Nordic chronologies.

Cheers,

Andreas

Andreas J. Kirchhefer
Institutt for biologi
Universitetet i Tromsø
9037 Tromsø
tlf 776 46 061
fax 776 46 333
andreas.kirchhefer@ib.uit.no
<http://www.ib.uit.no/~andreas>

-----Opprinnelig melding-----

Fra: Keith Briffa [mailto:k.briffa@uea.ac.uk]

Sendt: 10. desember 2003 13:54

Til: Andreas J. Kirchhefer

Emne: Re: tree rings and Rapid Climate Change

Andreas

...
Finally, one point and request. I have started to put together multiple data to construct a picture of late-Holocene temperature change in Northern Fennoscandia and hopefully (as part of an existing EC project, ALP-IMP), compare the aggregated (and hopefully more robust) series with similar

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tree-ring data in the Greater Alpine Region. I would be very grateful for permission to use your data in this exercise. I am also committed to give talks on tree-ring variability in early January in Bangor , and later in Bergen next year , and similarly, any chronology data you could provide me with for these talks (to produce comparison plots) would be similarly appreciated. I would also like to move towards putting a review paper together (perhaps for Quaternary Science Reviews) of long European chronology-based climate inference , and would like to do this as a joint paper with Yourself , Hakan Grudd , the Finns and other European colleagues. Any interest?

very best wishes

Keith

Attachment Converted: "c:\eudora\attach\Norwegian_chronologies.zip"

4376. 2003-12-17

date: Wed, 17 Dec 2003 13:45:18 +0000

from: Climate Policy <climatepolicy@imperial.ac.uk>

subject: 2 messages from Michael Grubb and Ray Purdy

to: jpershing@wri.org,naki@iiasa.ac.at,hadi@sdri.ubc.ca,inoble@worldbank.org,

Jorgen.Wettestad@fni.no,schellnhuber@pik-potsdam.de,

h.j.schellnhuber@uea.ac.uk,cdegouvello@worldbank.org,

shs@leland.stanford.edu,RKinley@unfccc.int,Sylvie.Faucheux@c3ed.uvsq.fr,

m.hulme@uea.ac.uk,ZhangZ@EastWestCenter.org,pretel@chmi.cz,

zkundze@man.poznan.pl,jae@pnl.gov,ogunlade@energetic.uct.ac.za,

Eberhard.Jochem@isi.fhg.de,hoesung@unitel.co.kr,naki@iiasa.ac.at,

kchomitz@worldbank.org,dlashof@nrdc.org,Tom.Jacob@USA.dupont.com,

snishiok@nies.go.jp,pachauri@teri.res.in,mack.mcfarland@usa.dupont.com,

jake.werksman@undp.org,ArroyoV@pewclimate.org,tom.downing@sei.se,

enikitina@mtu-net.ru,EHaites@netcom.ca,michael.grubb@imperial.ac.uk,

t.jackson@surrey.ac.uk,sujatag@teri.res.in,a-michaelowa@hwwa.de,

Emilio@ppe.ufrj.br,yamagata@nies.go.jp,nkete@wri.org

Message from Michael Grubb

Dear Climate Policy Board member

Further to my last email to the Board, I can now let you all know the following.

At the Editorial Board meeting last week I announced my resignation as

Editor-in-Chief of

Climate Policy. If you have been reading previous emails, and the Annual Report from the

beginning of this year, you will know the main reasons. I eventually came to the conclusion

that with Elsevier it would be fundamentally impossible to fulfill the stated Aims and

Objectives upon which the journal had been founded, specifically that: 'a primary aim of

the journal is make complex, policy-related analysis of climate change issues accessible to

a wide policy audience, and to facilitate debate between the diverse constituencies now

involved in the development of climate policy.'

I have been approached by the newly merged Earthscan/James & James company who intend to

launch a new international research journal, as their flagship venture, which would address

these aims (and which as part of this would be priced at a level intended to secure large

subscription and bearing some relationship to cost, ie. A small fraction of the Elsevier

price and strategy which focused on the academic library

and ScienceDirect markets). This new venture would also carry various sections additional

to the core of academic research papers. After my

contract with Elsevier terminates (at the end of this calendar year) I will

cg2003

engage in more

detailed discussions with Earthscan/James and James.

At present, I cannot tell you anything about the future of Climate Policy. The current

Publishing Editor at Elsevier has indicated over the past few months that in response to my concerns they have been looking to move CP to a

new division - Materials Science was their initial suggestion, more recently and appropriately, social sciences and geography.

However, I was told that the relevant Publishing editor did not wish to discuss anything with me. The day after I confirmed my resignation to the Board

meeting, I at last received an email from the Publisher editor of Social Science and Geography asking if she could meet me in January to discuss the possible future of Climate Policy.

Apart from that I have no idea whether Elsevier intend to continue the journal. Those present at the Board meeting in Milan expressed understanding and interest in leaving

Climate Policy to join the new venture proposed by Earthscan/James and James. I consulted

with a number of other Board members during the week in Milan, and for those whom I did not

see there, I would be interested in your initial views. For the present, we are contacting authors and informing them of the situation. I will contact you again early in

January when things are clearer.

With best wishes and Seasons Greetings

Michael

Message from Ray Purdy

I am also leaving Climate Policy and this will be my last day working for the journal. I

will continue as a senior fellow in environmental law at University College London and I

will not be involved in any new climate journal published by Earthscan/James and James. I

would like to take this opportunity to thank the board for all their support over the last

three and a half years and particularly for all the refereeing you have done. I hope you

have a good Christmas/Holiday break and would like to wish you all the very best for

2004.

Cheers and good wishes - Ray

3060. 2003-12-19

cc: "'alison.crompton@dti.gsi.gov.uk'" <alison.crompton@dti.gsi.gov.uk>,
"'graham.pendlebury@dft.gsi.gov.uk'" <graham.pendlebury@dft.gsi.gov.uk>,
"'j-troni@dfid.gov.uk'" <j-troni@dfid.gov.uk>, "'j-wheatley@dfid.gov.uk'"
<j-wheatley@dfid.gov.uk>, "'l-brown@dfid.gov.uk'" <l-brown@dfid.gov.uk>,
"'rob.mason@fco.gsi.gov.uk'" <rob.mason@fco.gsi.gov.uk>,
"'meg.patel@fco.gsi.gov.uk'" <meg.patel@fco.gsi.gov.uk>,
"'terry.carrington@dti.gsi.gov.uk'" <terry.carrington@dti.gsi.gov.uk>,
"'vbakshi@no10.x.gsi.gov.uk'" <vbakshi@no10.x.gsi.gov.uk>,
"'rupert.furness@dft.gsi.gov.uk'" <rupert.furness@dft.gsi.gov.uk>,
"'abigail.howells@wales.gsi.gov.uk'" <abigail.howells@wales.gsi.gov.uk>,
"'william.lochhead@odpm.gsi.gov.uk'" <william.lochhead@odpm.gsi.gov.uk>,
"'rebecca.pankhurst@dti.gsi.gov.uk'" <rebecca.pankhurst@dti.gsi.gov.uk>, "Coyne,

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Matthew (GA)" <Matthew.Coyne@defra.gsi.gov.uk>, "Crabbe, Simon (GA)"
<simon.crabbe@DEFRA.GSI.GOV.UK>, "Gorman, Pete (SEP)"
<Pete.Gorman@defra.gsi.gov.uk>, "Hathaway, Roy A (EPI)"
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<Teressa.Hrastic@defra.gsi.gov.uk>, "Jones, Jackie (GA)"
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<Nicola.Lettington@DEFRA.GSI.GOV.UK>, "Nelson, David (EED)"
<david.nelson@DEFRA.GSI.GOV.UK>, "Pearson, Elizabeth (GA)"
<elizabeth.pearson@DEFRA.GSI.GOV.UK>, "Penman, Jim (GA)"
<Jim.Penman@defra.gsi.gov.uk>, "WarriLOW, David (GA)"
<David.WarriLOW@defra.gsi.gov.uk>, "Wilkins, Diana (GA)"
<Diana.Wilkins@defra.gsi.gov.uk>, "Stow, Bill (EP)" <bill.stow@DEFRA.GSI.GOV.UK>,
"Dalton, Howard (SD)" <howard.dalton@DEFRA.GSI.GOV.UK>, "Derwent, Henry (CEER)"
<Henry.Derwent@defra.gsi.gov.uk>, "Davies, Bob (EPE)" <Bob.Davies@defra.gsi.gov.uk>
date: Fri, 19 Dec 2003 12:37:28 -0000
from: "Jones, Ross (GA)" <Ross.Jones@DEFRA.GSI.GOV.UK>
subject: Nominations for IPCC Fourth Assessment Report
to: "'dave.griggs@metoffice.com'" <dave.griggs@metoffice.com>,
"'rmilne@ceh.ac.uk'" <rmilne@ceh.ac.uk>, "'dcmo@ceh.ac.uk'" <dcmo@ceh.ac.uk>,
"'jeff.lampert@aeat.co.uk'" <jeff.lampert@aeat.co.uk>,
"'john.d.watterson@aeat.co.uk'" <john.d.watterson@aeat.co.uk>,
"'justin.goodwin@aeat.co.uk'" <justin.goodwin@aeat.co.uk>, "'d.viner@uea.ac.uk'"
<d.viner@uea.ac.uk>, "'geoff.jenkins@metoffice.com'"
<geoff.jenkins@metoffice.com>, "'HEDGN@entecuk.co.uk'" <HEDGN@entecuk.co.uk>,
"'jon.turton@metoffice.com'" <jon.turton@metoffice.com>, "'petergsimmonds@cs.com'"
<petergsimmonds@cs.com>, "'peter.g.taylor@aeat.co.uk'"
<peter.g.taylor@aeat.co.uk>, "'dljl@leicester.ac.uk'" <dljl@leicester.ac.uk>,
"'parryml@aol.com'" <parryml@aol.com>, "'sari.kovats@lshtm.ac.uk'"
<sari.kovats@lshtm.ac.uk>, "'n.w.arnell@soton.ac.uk'" <n.w.arnell@soton.ac.uk>,
"'r.nicholls@mdx.ac.uk'" <r.nicholls@mdx.ac.uk>, "'plevy@ceh.ac.uk'"
<plevy@ceh.ac.uk>, "'simon.aumonier@erm.com'" <simon.aumonier@erm.com>,
"'philippa.harris@aeat.co.uk'" <philippa.harris@aeat.co.uk>,
"'richard.tipper@eccm.uk.com'" <richard.tipper@eccm.uk.com>,
"'chris.west@ukcip.org.uk'" <chris.west@ukcip.org.uk>,
"'debbie.danaher@dti.gsi.gov.uk'" <debbie.danaher@dti.gsi.gov.uk>,
"'poutc@bre.co.uk'" <poutc@bre.co.uk>, "'humphrin@imcgroup.co.uk'"
<humphrin@imcgroup.co.uk>, "'anne.webb@umist.ac.uk'" <anne.webb@umist.ac.uk>,
"'D.S.Lee@mmu.ac.uk'" <D.S.Lee@mmu.ac.uk>, "'foxleyd@raeng.co.uk'"
<foxleyd@raeng.co.uk>, "'dwl@nerc.ac.uk'" <dwl@nerc.ac.uk>,
"'h.j.schellenhuber@uea.ac.uk'" <h.j.schellenhuber@uea.ac.uk>,
"'r.derwent@btopenworld.com'" <r.derwent@btopenworld.com>, "Dickson, Bob (CEFAS)"
<r.dickson@cefass.co.uk>, "Holmes, John (SERAD)"
<John.Holmes@scotland.gsi.gov.uk>, "Dare, Barry (NAWAD)"
<Barry.Dare@wales.GSI.Gov.UK>, "'rodger.lightbody@doeni.gov.uk'"
<rodger.lightbody@doeni.gov.uk>, "'alistair.manning@metoffice.com'"
<alistair.manning@metoffice.com>, "'bo.lim@undp.org'" <bo.lim@undp.org>,
"'w.r.keatinge@qmul.ac.uk'" <w.r.keatinge@qmul.ac.uk>,
"'john.firth@severntrent.co.uk'" <john.firth@severntrent.co.uk>,
"'andlug@hotmail.com'" <andlug@hotmail.com>,
"'merylyn.hedger@environment-agency.gov.uk'"
<merylyn.hedger@environment-agency.gov.uk>, "'m.hulme@uea.ac.uk'"
<m.hulme@uea.ac.uk>, "'csu@ceh.ac.uk'" <csu@ceh.ac.uk>, "'jrm@ceh.ac.uk'"
<jrm@ceh.ac.uk>, "'pam.berry@eci.ox.ac.uk'" <pam.berry@eci.ox.ac.uk>,
"'terry.dawson@eci.ox.ac.uk'" <terry.dawson@eci.ox.ac.uk>,
"'mike.harley@english-nature.org.uk'" <mike.harley@english-nature.org.uk>,
"'oliver.watts@rspsb.org.uk'" <oliver.watts@rspsb.org.uk>, "'brett.orlando@iucn.org'"
<brett.orlando@iucn.org>, "'richardsmithers@woodland-trust.org.uk'"
<richardsmithers@woodland-trust.org.uk>, "'a.j.thorpe@reading.ac.uk'"
<a.j.thorpe@reading.ac.uk>

Dear All,

The IPCC is requesting governments to make nominations for Coordinating Lead

cg2003

Authors, Lead Authors, Contributing Authors, Expert Reviewers or Review Editors for the different chapters of the Fourth Assessment Report (AR4) of the IPCC.

Please see letter attached outlining the tasks and responsibilities of the above roles and the department's policy for supporting contributors to the IPCC.

If you wish to be nominated for one of the above roles please see guidelines on how to do so in the letter attached. All nominations need to be sent to me electronically or via the post by 16th January 2004 at the very latest. Please forward this email on to anyone else who you think may be interested in being involved in the preparation of the AR4.

Kind regards

Ross Jones

Global Atmosphere Division

Defra Area 3/A1

Ashdown House

123 Victoria St.

London SW1E 6DE

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Fax: 0207 082 8151

Email: ross.jones@defra.gsi.gov.uk

<<IPCC LET.doc.dot>>

"Department for Environment, Food and Rural Affairs (Defra)

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1954. 2003-12-23

cc: alex.haxeltine@uea.ac.uk

date: Tue Dec 23 15:06:34 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: Re: Fwd: Nominations for IPCC Fourth Assessment Report

to: "Sari Kovats" <Sari.Kovats@lshtm.ac.uk>

Sari,

My understanding is that DEFRA can legitimately nominate people to the IPCC - but also the

Tyndall Centre (as an accredited organisation) can also nominate directly.

However, if we

nominate directly then DEFRA won't pay expenses (and then Tyndall won't either - no

money!). So it is best to submit your form directly to DEFRA and see if David Warrilow

will put you forward to the IPCC. We had earlier circulated a list of names, including you

I think, to DEFRA, so they are aware of the people who are interested.

Best wishes,

Mike

At 13:44 22/12/2003 +0000, you wrote:

Hi Mike

I hope you have a great christmas and a restful holiday.

I have just a quick question about this IPCC AR4 nomination process - do we need to fill

cg2003

the in the nominator part as well - if so, would you nominate me? or does DEFRA do

nominating to the IPCC.
Thanks very much for any advice
Sari

Sari Kovats

Lecturer

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fax: +44 20 7580 4524

[1]sari.kovats@lshtm.ac.uk

Return-path: <ross.jones@defra.gsi.gov.uk>

Received: from postbox.lshtm.ac.uk

(mailgw.lshtm.ac.uk [193.63.251.36])

by s-nst5.lshtm.ac.uk; Fri, 19 Dec 2003 12:40:53 +0000

Received: from gsi-swi-mail2.gsi.gov.uk (gateway202.gsi.gov.uk
[212.137.34.41])

by postbox.lshtm.ac.uk (Postfix) with ESMTP id 772241560E3

for <sari.kovats@lshtm.ac.uk>; Fri, 19 Dec 2003 12:38:39 +0000 (GMT)

Received: from mailgw2.defra.gov.uk ([51.64.35.210]

helo=gsi01vc.defra.gsi.gov.uk)

by gsi-swi-mail2.gsi.gov.uk with smtp

id 1AXJtt-0002TU-JJ

for sari.kovats@lshtm.ac.uk; Fri, 19 Dec 2003 12:38:37 +0000

Received: from SMTP agent by mail gateway

Fri, 19 Dec 2003 12:08:41 -0000

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(Content Technologies SMTPRS 4.2.10) with ESMTP id

<T669ae59a3f94fc10fb0ee@gsi01vc.defra.gsi.gov.uk>;

Fri, 19 Dec 2003 12:36:42 +0000

Received: by gb099xi.maff.gov.uk with Internet Mail Service (5.5.2655.55)

id <ZCL8VLWW>; Fri, 19 Dec 2003 12:37:35 -0000

Message-ID: <CBB39E27CF14D611B4500002A5931FDF03065EF8@gb140xs.maff.gov.uk>

From: "Jones, Ross (GA)" <Ross.Jones@DEFRA.GSI.GOV.UK>

To: "'dave.griggs@metoffice.com'" <dave.griggs@metoffice.com>,

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"'dcmo@ceh.ac.uk'" <dcmo@ceh.ac.uk>,

"'jeff.lampert@aeat.co.uk'" <jeff.lampert@aeat.co.uk>,

"'john.d.watterson@aeat.co.uk'" <john.d.watterson@aeat.co.uk>,

"'justin.goodwin@aeat.co.uk'" <justin.goodwin@aeat.co.uk>,

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cg2003

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"r.derwent@btopenworld.com" <r.derwent@btopenworld.com>,
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"pam.berry@eci.ox.ac.uk" <pam.berry@eci.ox.ac.uk>,
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"brett.orlando@iucn.org" <brett.orlando@iucn.org>,
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<richardsmithers@woodland-trust.org.uk>,
"a.j.thorpe@reading.ac.uk" <a.j.thorpe@reading.ac.uk>
Cc: "alison.crompton@dti.gsi.gov.uk" <alison.crompton@dti.gsi.gov.uk>,
"graham.pendlebury@dft.gsi.gov.uk"
<graham.pendlebury@dft.gsi.gov.uk>,
"j-troni@dfid.gov.uk" <j-troni@dfid.gov.uk>,
"j-wheatley@dfid.gov.uk" <j-wheatley@dfid.gov.uk>,
"l-brown@dfid.gov.uk" <l-brown@dfid.gov.uk>,
"rob.mason@fco.gsi.gov.uk" <rob.mason@fco.gsi.gov.uk>,
"meg.patel@fco.gsi.gov.uk" <meg.patel@fco.gsi.gov.uk>,
"terry.carrington@dti.gsi.gov.uk" <terry.carrington@dti.gsi.gov.uk>,
"vbakshi@no10.x.gsi.gov.uk" <vbakshi@no10.x.gsi.gov.uk>,
"rupert.furness@dft.gsi.gov.uk" <rupert.furness@dft.gsi.gov.uk>,
"abigail.howells@wales.gsi.gov.uk"
<abigail.howells@wales.gsi.gov.uk>,
"william.lochhead@odpm.gsi.gov.uk"
<william.lochhead@odpm.gsi.gov.uk>,
"rebecca.pankhurst@dti.gsi.gov.uk"
<rebecca.pankhurst@dti.gsi.gov.uk>,
"Coyne, Matthew (GA)" <Matthew.Coyne@defra.gsi.gov.uk>,
"Crabbe, Simon (GA)" <simon.crabbe@DEFRA.GSI.GOV.UK>,
"Gorman, Pete (SEP)" <Pete.Gorman@defra.gsi.gov.uk>,
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"Wilkins, Diana (GA)" <Diana.Wilkins@defra.gsi.gov.uk>,
"Stow, Bill (EP)" <bill.stow@DEFRA.GSI.GOV.UK>,
"Dalton, Howard (SD)" <howard.dalton@DEFRA.GSI.GOV.UK>,
"Derwent, Henry (CEER)" <Henry.Derwent@defra.gsi.gov.uk>,
"Davies, Bob (EPE)" <Bob.Davies@defra.gsi.gov.uk>

Subject: Nominations for IPCC Fourth Assessment Report

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Date: Fri, 19 Dec 2003 12:37:28 -0000

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RCVD_IN_RFCI 0.10)

Dear All,

The IPCC is requesting governments to make nominations for Coordinating Lead Authors, Lead Authors, Contributing Authors, Expert Reviewers or Review Editors for the different chapters of the Fourth Assessment Report (AR4) of the IPCC.

Please see letter attached outlining the tasks and responsibilities of the above roles and the department's policy for supporting contributors to the IPCC.

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Kind regards

Ross Jones

Global Atmosphere Division

Defra Area 3/A1

Ashdown House

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Fax: 0207 082 8151

Email: ross.jones@defra.gsi.gov.uk

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4181. 2003-12-23

date: Tue Dec 23 16:20:13 2003

from: Mike Hulme <m.hulme@uea.ac.uk>

subject: more research

to: laura.middleton@uea

Laura,

And one more bit of research for January for the same article. Margaret Thatcher made a major speech to the Royal Society in either Sept. 1988 or Sept. 1989 in which she made a big play of global warming. Can you try and track down a copy of this speech. The Royal Society itself (web site or helpline) would be the obvious place to start.

cg2003

Thanks,
Mike

5275. 2003-12-23

date: Tue Dec 23 09:57:26 2003
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: RE: Request for referees comments (2003 Eureka Prize nomination o
to: <Simon.Torok@csiro.au>
Thanks Simon. The report looks great - a cross between an IPCC assessment, national scenarios, and a impacts review group. Even (!) the UK have not managed something quite as integrated as this. Pass on my congratulations to Barrie when you see him next. And also congratulate Penny when you see her. I hope you can all have a great party for \$10,000! Or maybe take the group out to the Boxing Day Test at the MCG. The Aussies have got a battle on now to beat India. Yes, the Tyndall Centre continues to mature - we've got our main evaluation coming up in spring 2004, so much preparation to be done for this to make sure we're on track for the 2005-2010 renewal. Do keep in touch; I'd hoped to visit Queensland in November for an invited conference, but it clashed with our annual Advisory Board meeting. So maybe next year
Happy Christmas,
Mike

At 15:00 23/12/2003 +1100, you wrote:

Hi Mike,
I have just realised that nobody sent you a note again thanking you for the reference for Barrie and his group, and that you may not have heard that they won the award.
Your reference would have added to the other positive comments about -- and impressive achievements of -- the group that were included in the successful nomination.
So, thank you very much for contributing, and my apologies for not letting you know earlier. A note about the award is at [1]<http://www.dar.csiro.au/news/2003/mr10.html>
Meanwhile, a book edited by Barrie caused quite a splash here after being presented in Milan by our Environment Minister. The book is online at [2]<http://www.greenhouse.gov.au/science/guide/index.html> and will be printed next year. It's an excellent summary for this part of the world, but you might find it a handy reference.
2003 seems to have been an exciting time for Tyndall and I have to admit missing a few things about Norwich. It has been good staying in touch with your activities, particularly through Asher and Ali, and I hope to find other ways to work together next year.
Have a very merry Christmas and an exciting 2004.
Regards, Simon.

===== Dr Simon Torok =====
--_|\ Communication and Marketing Manager
<_--.*> CSIRO Atmospheric Research
v PMB 1, Aspendale, Victoria 3195, Australia
~~~~~ Tel: +61 (0)3 9239 4645; Fax: +61 (0)3 9239 4444  
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===== Mobile: +61 (0)409 844 302 =====

-----Original Message-----

From: Mike Hulme [[4]<mailto:m.hulme@uea.ac.uk>]
Sent: Friday, 16 May 2003 7:12 PM
To: Torok, Simon (AR, Aspendale)
Subject: RE: Request for referees comments (2003 Eureka Prize nomination

cg2003

o f Peter Whetton, Barrie Pittock and the Climate Impacts Group)
OK Simon - here it is.

Mike

At 11:32 16/05/2003 +1000, you wrote:

>Hi Mike,

>

>Thank you very much for putting time towards writing such a positive
>reference. I have it here on our green fax paper. Would it be possible
>for you to also email the reference to me as a word document so Willem
>Bouma, who is collecting the information, can print it in colour? He
>feels this will look more impressive.

>

>The prize is only \$10,000 and a trophy -- but the series of awards is
>well recognised in Australia. I have a nomination in for my series of
>children's books in the 'science promotion' category, and we have
>another group nominated for the 'interdisciplinary science' category --
>confidentially, I found it difficult to take the latter nomination
>seriously after being in the Tyndall Centre.

>

>I'm grateful for your effort even if we don't end up featured in the
>ceremony.

>

>Regards, Simon.

>

>-----Original Message-----

>From: Mike Hulme [[5]mailto:m.hulme@uea.ac.uk]

>Sent: Wednesday, 14 May 2003 10:16 PM

>To: Simon.Torok@csiro.au

>Subject: RE: Request for referees comments (2003 Eureka Prize
>nomination o f Peter Whetton, Barrie Pittock and the Climate Impacts
>Group)

>

>

>Simon,

>

>A faxed version of this letter - on Tyndall paper - should be with you.

>Hope it comes off. What's the Prize? Shame it's too far for me to

>come for the ceremony.

>

>Mike