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From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Scott Rutherford <srutherford@gso.uri.edu>
Subject: Re: RegEM manuscript
Date: Mon, 06 Jan 2003 09:13:24 -0500
Cc: k.briffa@uea.ac.uk,Phil Jones <p.jones@uea.ac.uk>, Ray Bradley
<rbradley@geo.umass.edu>,mhughes@ltrr.arizona.edu, mann@virginia.edu

Thanks very much Tim,
Your comments are extremely helpful.
I'm open to eliminating the comparison w/ Esper et al --but lets see if there is a
consensus of the group as to what to do here. We're anxiously awaiting comments from the
others...
thanks again,
mike
p.s. Scott can be reached at either U.Va or U.RI email equally well (I believe the former
is forwarded to the latter)..
At 12:16 PM 1/6/2003 +0000, Tim Osborn wrote:

Dear Scott and Mike,

Over the Christmas break I (finally!) had time to read the RegEM manuscript in detail.
Phil had already read and annotated a copy - so I've added my annotations to that and
will mail it to you today.  Mike asked for comments to go to Scott, so please tell me
which address I should use (Rhode Island or Virginia?).
I spoke to Keith and he has partly read it too, and will provide separate comments soon.
Overall, I think the paper is a very nice piece of work and I'm pleased to be involved
with it.  The results regarding robustness with respect to proxy data, method, region
and season are definitely good to publish.
Among the many comments annotated on the manuscript, a few are repeated here so that all
authors may respond if they wish:
(1) Given the overwhelming number of values in the Tables, I suggest halving them by
dropping all the CE values (keeping just RE values).  As the paper points out, getting
the verification period mean right is rewarded by RE but not by CE.  Since we are
interested in changes in the mean, I don't think that's a problem.  CE is fine in
addition, but dropping it would provide benefits of reducing manuscript size - and
especially the size of the tables.
(2) The "mixed-hybrid" approach sounds dubious to me - more justification/explanation of
why it is needed (and hence why it captures more variance than the simpler splitting

into high- and low-frequency components method).
(3) It is not clear to me that the paragraph and figure on the comparison with Esper et al. are either correct or necessary. They also are problematic because it would appear that we (Briffa & Osborn) were contradicting our earlier paper when in fact we aren't.
The paper is already long and to remove these parts would therefore be helpful anyway.
The comparison with Esper et al. is important - but much better dealt with in a separate paper where it could be developed in more detail and with more room to explain the approach and its implications.
(4) I still hope to write up some more detailed comparisons of the reconstructions using just the MXD data but different methods and will let Mike/Scott know my plans on this soon.
Happy new year to you all.
Tim
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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 __________| [1]http://www.cru.uea.ac.uk/~timo/
Norwich NR4 7TJ | sunclock:
UK | [2]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.cru.uea.ac.uk/~timo/
2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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

<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 A1FI 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 variabiliy, 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 metholody) 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.

p.18, line 9: not only are they difficult to forsee, they are simply unforseeable 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
>Email: tim.carter@ymparisto.fi
>Web: http://www.ymparisto.fi/eng/research/projects/finsken/welcome.html
>**********************************************************************

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Ulrich Cubasch <cubasch@zedat.fu-berlin.de>
Subject: Re: multiproxy
Date: Tue, 28 Jan 2003 12:33:35 -0500
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

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
   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
   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 proxi 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 collegues 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

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[4]http://www.evsc.virginia.edu/faculty/people/mann.[5]shtml

References

1. ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf
2. ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf
3. http://www.cru.uea.ac.uk/cru/projects/soap
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: f14@zedat.fu-berlin.de
Subject: Re: program code
Date: Wed, 05 Feb 2003 13:19:29 -0500
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@ltrr.arizona.edu, rbradley@geo.umass.edu

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 identifical 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 (its a short script, which didn't take
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
--
*************************************
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[4]http://www.evsc.virginia.edu/faculty/people/mann.[5]shtml

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1. ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf
2. ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf
3. ftp://holocene.evsc.virginia.edu/pub/mann/Rutherfordetal-Jclim03.pdf
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Kabat, dr. P." <P.Kabat@Alterra.wag-ur.nl>, "Schellnhuber (E-mail)" <h.j.schellnhuber@uea.ac.uk>
Subject: Re: Letter of Support
Date: Wed Feb 12 15:45:03 2003
Cc: "Alex Haxeltine (E-mail)" <Alex.Haxeltine@uea.ac.uk>

Pavel
I will certainly make sure a letter reaches you for Friday.  And Good Luck!
Mike
At 14:07 12/02/03 +0100, Kabat, dr. P. wrote:

Dear Mike, John, Alex:
referring to out tel. conversation yesterday with Alex, hereby our request for  a letter of support/recommendation on behalf of Tyndall for our national Global Change Initiative programme proposal called
"Climate changes the spatial planning", ("Climate for Spatial Planning Spatial Planning for Climate); unofficially known  to you I guess as as "Netherlands Tyndall-like  initiative...)
After we have successfully passed  the first round of the selection last year with the Dutch Government, we are know in final stages of submitting the final proposal/business plan (deadline 17/2/03 - next Monday).
The proposed programme has a total budget of 100 million Euro, of which 49 million is requested from the Government, rest contribution of public and private institutions. As a part of this programme we are aiming to set up Netherlands Centre of Excellence (partly virtual) institute, modelled after Tyndall. Leading parties in this effort are all well known to you:
Wageningen (kabat)
VU Amsterdam (vellinga)
RIVM (metz)
KNMI (Komen)
ICIS (Rotmans)
ECN (Bruggink)
plus another almost 50 parties.
Could you pls send us a short letter of support, in which you indicate the importance of this initiative for advancing this type global change science, European dimension, UK - NL collaboration, etc, etc?
We need to receive this by Friday, so send also by fax pls (apologies for the rush). Letter is to be addressed as follows:
Prof. Dr Pavel Kabat
Science Director

Netherlands National Research Initiative "Climate changes the spatial
planning", (ICES KIS 3)
Postal address: PO Box 47, 6700 AA Wageningen
Visiting address: Lawickse Allee 11, IAC building, room 156
Voice +31 317 474314/74713 (office), +31 653489378 (mobile), +31 264463567
(home);
Fax: +31 317495590
I attach 3 documents as background of our proposal
Many thanks for your kind help!
Pavel, Pier en colleagues
<<BPDraft2.3NoFigures.doc>>  <<OrganisatieSchema.doc>>   <<Overview budget
131.xls>>

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From: "Alex Haxeltine" <Alex.Haxeltine@uea.ac.uk>
To: "Armin Haas" <haas@pik-potsdam.de>, "Alexander Wokaun" <wokaun@psi.ch>, "Anco Lankreijer" <lana@geo.vu.nl>, "Andrew Jordan" <a.jordan@uea.ac.uk>, "Antoni Rosell" <antoni.rosell@uab.es>, "Antonio Navarra" <navarra@ingv.it>, Asbjørn Torvanger <asbjorn.torvanger@cicero.uio.no>, <baldur.eliasson@ch.abb.com>, Benito Müller <benito.mueller@philosophy.oxford.ac.uk>, "Bert Metz" <bert.metz@rivm.nl>, <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" <frtca@fy.chalmers.se>, "Christian Flachsland" <christian.flachsland@pik-potsdam.de>, "Christos Giannakopoulos" <cgiannak@meteo.noa.gr>, "Claudia Kemfert" <kemfert@uni-oldenburg.de>, "Daniel Droste" <d.droste@consultants.mvv.de>, "Eberhard Jochem" <eberhard.jochem@isi.fhg.de>, "Eberhard Jochem" <jochem@cepe.mavt.ethz.ch>, "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>, "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 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.engelhard@rwerheinbraun.com>, Jürgen Kurths <jkurths@agnild.uni-potsdam.de>, Jürgen Kurths <juergen@lenne.agnld.uni-potsdam.de>, "Katrin Gerlinger" <Katrin.Gerlinger@pik-potsdam.de>, Klaus Böswald <klaus.boeswald@factorag.ch>, "Klaus Hasselmann" <klaus.hasselmann@dkrz.de>, "Kornelis Block" <K.Blok@chem.uu.nl>, "Leen Hordijk" <hordijk@iiasa.ac.at>, "Lennart Olsson" <lennart.olsson@miclu.lu.se>, "Liudmila Romaniuk" <Romaniuk@mail.lanck.net>, "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 C&E Hanan Abdul-Rida" <h.abdulrida@consultants.mvv.de>, "Nakicenovic" <naki@iiasa.ac.at>, "Neil Adger" <n.adger@uea.ac.uk>, Niklas Höhne <n.hoehne@ecofys.de>, "Ola Johannessen" <ola.johannessen@nersc.no>, "Ottmar Edenhofer" <Ottmar.Edenhofer@pik-potsdam.de>, "Pal Prestrud"

<prestrud@cicero.uio.no>, Pål Prestrud <pal.prestrud@cicero.uio.no>, "Pavel Kabat" <P.Kabat@Alterra.wag-ur.nl>, "Philippe Ambrosi" <ambrosi@centre-cired.fr>, "Pier Vellinga" <pier.vellinga@falw.vu.nl>, "Pier Vellinga" <vell@geo.vu.nl>, "Pim Martens" <P.Martens@icis.unimaas.nl>, "Reinhard G. Budich" <budich@dkrz.de>, "Renaud Crassous" <crassous@centre-cired.fr>, "Richard Klein" <Richard.Klein@pik-potsdam.de>, "Rik Leemans" <rik.leemans@rivm.nl>, "Roger Kasperson" <roger.kasperson@sei.se>, "Rupert Klein" <Rupert.Klein@pik-potsdam.de>, "S.E. van der Leeuw" <vanderle@wanadoo.fr>, "S.E. van der Leeuw" <vanderle@mae.u-paris10.fr>, "Saleemul Huq" <saleemul.huq@iied.org>, "Sebastian Gallehr" <gallehr@e5.org>, "Simone Ullrich" <SU@ier.uni-stuttgart.de>, <SSinger@wwfepo.org>, "Stephane Hallegatte" <hallegatte@centre-cired.fr>, "Sybille van den Hove" <s.vandenhove@terra.es>, "Tim O'Riordan" <t.oriordan@uea.ac.uk>, "Tobias Kampet" <t.kampet@consultants.mvv.de>, "Tom Downing" <tom.downing@sei.se>, "Tom Kram" <Tom.Kram@rivm.nl>, "Tony Patt" <tonypatt@pik-potsdam.de>, "V.K. Dochenko" <donchenkovk@mail.ru>, "Wim Turkenburg" <W.C.Turkenburg@chem.uu.nl>, "Wolfgang Cramer" <Wolfgang.Cramer@pik-potsdam.de>, "Wolfgang Lucht" <Wolfgang.Lucht@pik-potsdam.de>
Subject: Re: AMS proposal
Date: Mon, 10 Mar 2003 17:36:46 -0000

Dear Colleagues,

In the email from Armin Haas (signed by Carlo and Klaus) on 5th March, we were informed that a strategy committee and a research committee had been formed; with the latter being primarily responsible for the preparation of the proposal.

WE NOW HAVE ONLY 20 WORKING DAYS LEFT UNTIL THE PROPOSAL HAS TO BE SUBMITTED!!!

And while I am aware and involved in a number of parallel activities addressing the writing of text for specific work domains and work packages, I have not received any formal communication about what role is expected of me as a member of the research committee (that has primary responsibility for the preparation of the proposal).

Needless to say I find this extremely worrying, and suggest that we URGENTLY need clarification about 1) exactly what the research committee should do; 2) how it should do it; 3) what responsibility for making decisions this committee will have/how it should liaise with the strategy committee.

It seems clear that in order to finalize an overall project structure we will need to meet face-to-face for at least 36 hours, and that this needs to happen with the utmost urgency. I have made a provisional booking of a facility very near Stanstead airport in the UK for next Monday and Tuesday (17th and 18th March), and offer this as a possible time and place to meet; but am of course open to other suggestions. I would imagine that in addition to the research committee assigned so far, we would need to co-opt the writers of several of the work packages and the work domains leaders for the purpose of this meeting.

With warm regards and the utmost sense of urgency,

Alex Haxeltine

Dr Alexander Haxeltine
International Science Co-ordinator
Tyndall Centre for Climate Change Research
School of Environmental Sciences
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Norwich NR4 7TJ, UK

Tel: +44 1603 593902
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Website: [1]http://www.tyndall.ac.uk

References

1. http://www.tyndall.ac.uk/

295. 1047388489.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>,rbradley@geo.umass.edu,
mhughes@ltrr.arizona.edu,srutherford@gso.uri.edu,tcrowley@duke.edu
Subject: Re: Fwd: Soon & Baliunas
Date: Tue, 11 Mar 2003 08:14:49 -0500
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

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 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 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
synchroneity 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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Senior Research Associate | fax: +44 1603 507784
Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
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Norwich NR4 7TJ | sunclock:
UK | [3]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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[4]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.int-res.com/journals/cr/crEditors.html
2. http://www.cru.uea.ac.uk/~timo/
3. http://www.cru.uea.ac.uk/~timo/sunclock.htm
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Phil Jones <p.jones@uea.ac.uk>
To: rbradley@geo.umass.edu,mhughes@ltrr.arizona.edu,srutherford@gso.uri.edu,
"Michael E. Mann" <mann@virginia.edu>,tcrowley@duke.edu
Subject: Fwd: Soon & Baliunas
Date: Tue, 11 Mar 2003 08:49:22 +0000
Cc: k.briffa@uea.ac.uk,jto@u.arizona.edu,drdendro@ldeo.columbia.edu,
keith.alverson@pages.unibe.ch

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Attachment Converted: "c:\eudora\attach\Soon & Baliunas 20031.pdf"

297. 1047474776.txt

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From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tom Crowley <tcrowley@duke.edu>
Subject: Re: Fwd: Soon & Baliunas
Date: Wed, 12 Mar 2003 08:12:56 -0500
Cc: 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

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.
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 accuararely 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,
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> >>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 __________|   [1]http://www.cru.uea.ac.uk/~timo/ Norwich  NR4
> >>7TJ        | sunclock: UK                        |
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298. 1047478548.txt
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To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Soon & Baliunas
Date: Wed, 12 Mar 2003 09:15:48 -0500
Cc: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tom Crowley <tcrowley@duke.edu>, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, mann@virginia.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

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From: Scott Rutherford <srutherford@gso.uri.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Soon & Baliunas
Date: Wed, 12 Mar 2003 10:53:07 -0500
Cc: Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

<x-rich>Dear All,

First, I'd be willing to handle the data and the plotting/mapping. Second, regarding Mike's suggestions, if we use different reference periods for the reconstructions and the models we need to be extremely careful about the differences. Not having seen what this will look like, I suggest that we start with the same instrumental reference period for both (1856-1960). If you are willing to send me your series please send the raw (i.e. unfiltered) series. That way I can treat them all the same. We can then decide how we want to display the results.

Finally, Tom's suggestion of Eos struck me as a great way to get a short, pointed story out to the most people (though I have no feel for the international distribution). My sense (being relatively new to this field compared to everyone else) is that within the neo- and mesoclimate research community there is a (relatively small?) group of people who don't or won't "get it" and there is nothing we can do about them aside from continuing to publish quality work in quality journals (or calling in a Mafia hit). Those (e.g. us) who are engrossed in the issues and are aware of all the literature should be able to distinguish between well done and poor work. Should then the intent of this proposed contribution be to education those who are not directly involved in MWP/LIA issues including those both on the perifery of the issue as well as those outside? If so, then the issue that Phil raised about not letting it get buried is significant and I think Eos is a great way to get people to see it.

Cheers,

Scott

On Wednesday, March 12, 2003, at 10:32 AM, Michael E. Mann wrote:

<excerpt>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:

<smaller>Phil et al,

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<smaller>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.

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> >Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF

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From: "Michael E. Mann" <mann@virginia.edu>
To: Scott Rutherford <srutherford@gso.uri.edu>
Subject: Re: Soon & Baliunas
Date: Wed, 12 Mar 2003 11:07:43 -0500
Cc: Tom Crowley <tcrowley@duke.edu>,Phil Jones <p.jones@uea.ac.uk>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>,rbradley@geo.umass.edu,
k.briffa@uea.ac.uk,t.osborn@uea.ac.uk,mann@virginia.edu

   Thanks Scott,
   I concur. We may want to  try a few different alignment/scaling choices in the
end, and
   then just vote on which we like the best,
   Anxious to here others' thoughts on all of this,

mike
At 10:53 AM 3/12/2003 -0500, Scott Rutherford wrote:

Dear All,
First, I'd be willing to handle the data and the plotting/mapping. Second, regarding
Mike's suggestions, if we use different reference periods for the reconstructions and
the models we need to be extremely careful about the differences. Not having seen what
this will look like, I suggest that we start with the same instrumental reference period
for both (1856-1960). If you are willing to send me your series please send the raw
(i.e. unfiltered) series. That way I can treat them all the same. We can then decide how
we want to display the results.
Finally, Tom's suggestion of Eos struck me as a great way to get a short, pointed story
out to the most people (though I have no feel for the international distribution). My
sense (being relatively new to this field compared to everyone else) is that within the
neo- and mesoclimate research community there is a (relatively small?) group of people
who don't or won't "get it" and there is nothing we can do about them aside from
continuing to publish quality work in quality journals (or calling in a Mafia hit).
Those (e.g. us) who are engrossed in the issues and are aware of all the literature
should be able to distinguish between well done and poor work. Should then the intent
of this proposed contribution be to education those who are not directly involved in
MWP/LIA issues including those both on the perifery of the issue as well as those
outside? If so, then the issue that Phil raised about not letting it get buried is
significant and I think Eos is a great way to get people to see it.
Cheers,
Scott
On Wednesday, March 12, 2003, at 10:32 AM, 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.
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 accuarately 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
> >  synchroneity 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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References

1. http://www.cru.uea.ac.uk/~timo/
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5. http://www.evsc.virginia.edu/faculty/people/mann.shtml
6. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>,Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Soon & Baliunas
Date: Wed, 12 Mar 2003 12:12:02 -0500
Cc: rbradley@geo.umass.edu,mhughes@ltrr.arizona.edu,srutherford@gso.uri.edu, k.briffa@uea.ac.uk,mann@virginia.edu

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 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

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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml
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References

1. http://www.cru.uea.ac.uk/~timo/
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3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>,Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Soon & Baliunas
Date: Wed, 12 Mar 2003 16:16:16 +0000
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

<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 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.
>>>>
>>>>  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 accuaratelv 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
>>>>>> > >  synchroneity 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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Daer Armin,
I would like to confirm that RIVM is strongly committed to make a substantial contribution to the AMS proposal, as was clear from our active involvement in the discussions so far (except the Paris meeting where we unfortunately could not send a representative). We have been in touch with several other partners in developing ideas for the workpackage, but in view of the high pressure under which the proposal is being put together, communication is not always easy. I therefore include a list of elements we would like to contribute to the respective parts of the proposal:.

WP1. Scenarios: involved with proposal Brian O'Neill (contact: Detlef van Vuuren). Important issues: delineation with scenarios in other workpackages - no response so far.

WP 3.1. Possible contribution, depends on connection with WP1
3.3. Primarily through cooperation with Un.Utrecht - proposal sent to Wokaun but no response. Possible to add global context with IMAGE/TIMER and add non-energy emssion reductions not covered in original proposal by Wokaun
3. 4. and 3.5: as for 3.3

WP 4.1. Suggested role for multi-gas stabilization profiles, burden sharing regimes and EU action with IMAGE-FAIR combination (building on work we have done with other partners for the European Commission). Current proposal by Haxeltine, Leemans and Adger has 100% focus on impacts and adaptation and should be broadened. We are ready to contribute
4.2. Now contains the regimes that should go under 4.1
4.6. Involved actively: see proposal Olsson&Metz that went to John Schellnhuber

WP 5.4. Strong interest, but no response from coordinator (C. Jaeger) and WP coordinator Hasselmann refers back to CJ (!). We will put together proposal with Tyndall towards development of CIAS model.

Best regards,

Bert Metz

304. 1048799107.txt
###################################################################################

From: Earth Government <earthgov@shaw.ca>
Subject: Press release from Earth Government and April Newsletter
Date: Thu, 27 Mar 2003 16:05:07 -0800

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 dictature, what
is it? It does not say much about democracy at the UN. More like a dictature 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

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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).

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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

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

References

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2. http://members.shaw.ca/earthgov/NewsA.htm
3. http://members.shaw.ca/earthgov/EarthGovernment.htm
4. http://www.telusplanet.net/public/gdufour/
5. http://members.shaw.ca/earthgov
6. http://www.telusplanet.net/public/gdufour/
7. http://members.shaw.ca/earthgov
8. mailto:gdufour@globalcommunitywebnet.com
9. mailto:gdufour@telusplanet.net
10. mailto:earthgov@shaw.ca

305. 1049745840.txt
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From: "Eystein Jansen" <eystein.jansen@geo.uib.no>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Re: Re: Holclim follow up
Date: Mon 7 Apr 2003 16:04

Dear Keith.
I had a chat with Dominique Reynaud on this matter today here in Nice. His impression is the same, but added that he thinks Brussels would insist on a NoE rather than an IP. If we wish to have an IP it needs lobbying it seems. He told about the meeting in Brussels inJune. I am not invited as far as I can tell. Dominique mentioned that Nick Shackleton would be there and I will talk with him. The key thing would be to sort out what the most exciting science our community can offer when we integrate the communities.
In terms of meetings it seems to depend alittle of what comes out of the June meeting in Brusseks.
Cheers
Eystein
>---- Original Message ---
>From: Keith Briffa <k.briffa@uea.ac.uk>
>To: Eystein Jansen <eystein.jansen@geo.uib.no>
>Subject: Re: Holclim follow up
>
>
>Eystein

>your point is exactly correct , that only one project (and I believe it=20
>should be an IP) will be allowed and with the shrinking general scale of=20
>these things, it likely needs to be very clearly focused (on integrating=20
>evidence and providing some state-of-the-art product on climate history and=
>=20
>its causes) . I am not in Nice (have to go to 2 other meetings in May) . I=
>=20
>am still leaning towards your institute co-ordinating this . I have not=20
>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=
>=20
>million Euro project and many people are likely to be disappointed . I have=
>=20
>to be in Brussels for a meeting with Brelen in June . What are you thinking=
>=20
>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=
>=20
>> next week, in order for us to exchange some ideas about how to proceed=20
>> for FP6. Recent rumors says that the palaeoclimate variablity item is in=
>=20
>> the books for the third call, and that the call will be issued by the=20
>> turn of the year, thus we should start discussing how to proceed. So far=
>=20
>> my DOCC initiative is dormant, and I am more inclined to develop or take=
>=20
>> part in developing an IP if the call for proposals allow for one. But the=
>=20
>> size of these IPs seems to be diminishing, hence a careful focussing=20
>> needs to be undertaken in order for there to be resources for the science=
>=20
>> teams. I would be happy to discuss idea with you on this in Nice or=20
>> sometime else if you=B4re not there.
>>
>>Cheers,
>>Eystein
>>
>>
>>
>>Eystein Jansen
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>>Bjerknes Centre for Climate Research
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>--
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>http://www.cru.uea.ac.uk/cru/people/briffa/
>
>

306. 1051156418.txt
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##########

From: Tom Wigley <wigley@ucar.edu>
To: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme
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Mosley-Thompson <thompson4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>,
"Greg.Ayers" <Greg.Ayers@csiro.au>
Subject: My turn
Date: Wed, 23 Apr 2003 23:53:38 -0600

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
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.

307. 1051190249.txt
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From: Tom Wigley <wigley@ucar.edu>
To: Timothy Carter <tim.carter@ymparisto.fi>
Subject: Re: Java climate model
Date: Thu, 24 Apr 2003 09:17:29 -0600
Cc: Mike Hulme <m.hulme@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

Tim,

I know about what Matthews has done. He did so without contacting Sarah
or me. He uses a statistical emulation method that can never account for
the full range of uncertainties. I would not trust it outside the
calibration zone -- so I doubt that it can work well for (e.g.)
stabilization cases. As far as I know it has not been peer reviewed.
Furthermore, unless he has illegally got hold of the TAR version of the
model, what he has done can only be an emulation of the SAR version.

Personally, I regard this as junk science (i.e., not science at all).

Matthews is doing the community a considerable disservice.

Tom.

PS Re CR, I do not know the best way to handle the specifics of the
editoring. Hans von Storch is partly to blame -- he encourages the
publication of crap science 'in order to stimulate debate'. One approach
is to go direct to the publishers and point out the fact that their
journal is perceived as being a medium for disseminating misinformation
under the guise of refereed work. I use the word 'perceived' here, since
whether it is true or not is not what the publishers care about -- it is
how the journal is seen by the community that counts.

I think we could get a large group of highly credentialed scientists to
sign such a letter -- 50+ people.

Note that I am copying this view only to Mike Hulme and Phil Jones.
Mike's idea to get editorial board members to resign will probably not
work -- must get rid of von Storch too, otherwise holes will eventually
fill up with people like Legates, Balling, Lindzen, Michaels, Singer,
etc. I have heard that the publishers are not happy with von Storch, so
the above approach might remove that hurdle too.

_______________________________

_______________________________

Timothy Carter wrote:
>
> Dear Tom,

>
> Since you were online yesterday contributing to the "Climate Research"
> discussion, I figured that you might be in town to give your views on the
> Java Climate Model which, I understand, is based in large part on MAGICC:
>
> http://chooseclimate.org/jcm/
>
> and seems to be getting considerable exposure amongst the policy community
> now that Ben Matthews (was he a student of yours at UEA?) has made this
> available online.
>
> I wondered if this has been subjected to "peer review" by the people whose
> models it is based on or anyone else, since I have Ministry people here in
> Finland asking me if this type of tool is something they should think of
> using during the negotiating process!
>
> It's certainly a smart piece of software, though it seems to have
> irritating bugs, like returning to the default state when any little thing
> is adjusted. What is critically important, though, is that it can do what
> it is advertising. If it can't, then the careful work done offline by
> people such as yourself, could be undermined.
>
> Any thoughts?
>
> Best regards from a sunny though cool Helsinki.
>
> Tim
>
> P.S. On the CR issue, I agree that a rebuttal seems to be the only method
> of addressing the problem (I communicated this to Mike yesterday morning),
> and I wonder if a review of the refereeing policy is in order. The only way
> I can think of would be for all papers to go through two Editors rather
> than one, the former to have overall responsibility, the latter to provide
> a second opinion on a paper and reviewers' comments prior to publication. A
> General Editor would be needed to adjudicate in the event of disagreement.
> Of course, this could then slow down the review process enormously.
> However, without an editorial board to vote someone off, how can suspect
> Editors be removed except by the Publisher (in this case, Inter-Research).

308. 1051202354.txt
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: mark.eakin@noaa.gov
Subject: Re: My turn
Date: Thu, 24 Apr 2003 12:39:14 -0400
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"

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<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 sprit 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. 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,
>>>Tom.
>>
>>______________________________________________________________
>> Professor Michael E. Mann
>> Department of Environmental Sciences, Clark Hall
>> University of Virginia
>> Charlottesville, VA 22903
>>_______________________________________________________________________
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>> http://www.evsc.virginia.edu/faculty/people/mann.shtml
>
>
>--
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>http://www.ngdc.noaa.gov/paleo/paleo.html
>
>

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From: j.salinger@niwa.co.nz
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 <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>, "Michael E. Mann" <mann@virginia.edu>, 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 <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>, Tom Wigley <wigley@ucar.edu>
Subject: And again from the south!
Date: Thu, 24 Apr 2003 20:28:20 +1200

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. 2319-1222, 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
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> in the above example -- then this is an advantage.
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> time on this?
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>
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> 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 .....
>
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> referees, all reputable atmospheric scientists, agreed it should be
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>
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> of individuals with bona fide scientific credentials who could be used
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> 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
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P O Box 109 695
Newmarket, Auckland
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Tel + 64 9 375 2053  Fax + 64 9 375 2051
e-mail:  j.salinger@niwa.co.nz
**********************************************************

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#################################################################################
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential
Date: Tue Apr 29 13:55:38 2003

   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
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
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,
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Fax: +44-1603-507784
[1]http://www.cru.uea.ac.uk/cru/people/briffa/

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==================================
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==================================

--
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Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784
[2]http://www.cru.uea.ac.uk/cru/people/briffa[3]/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

311. 1051915601.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: belated thanks for review and questions
Date: Fri, 02 May 2003 18:46:41 -0400

HI Keith,
No problem, I know how hectic the past couple months have been for you, so no apologizes
necessary whatsoever!
Call me old fashioned, but I still tend to prefer the "blind" reviewer convention, so I'd
prefer to remain anonymous unless you think that revealing my identity would be help in any
particular way.
I agree w/ your take on this--a journal like GRL is probably more appropriate, or even
"Climatic Change" because a number of similar papers have been published there in the past
(by folks like Nychka, Bloomfield, and others). I'm not sure if Steve Schneider is sick and
tired of those papers though...
Please don't hesitate to let me know if I can be of any additional help w/ this.
Looking forward to seeing you one of these days,
mike
At 02:36 PM 5/2/2003 +0100, you wrote:

Mike
in hassling another reviewer , I realised that I did not thank you properly for the
review you did of the manuscript by Gil-Alana (fractionally integrated techniques used
to show increased persistence in global temperature record in 20th century). So this is
by way of thanks and to ask whether you wish me to reveal your name to the reviewer
(considering you make some very helpful suggestions for further analysis)? I would
otherwise assume no. As it happens I can not get a response from the other reviewer -
but rather than prolong the wait for the submitter , I am tempted (on the basis of my
reading also) to just send your comments and reject the manuscript as it is - I suppose
they could resubmit a major rework following your suggestions - but I tend to the
opinion that it would be better suited to another journal anyway - GRL comes to mind.
What do you think
Cheers
Keith
--
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[2]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential
Date: Mon May 12 17:26:29 2003

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
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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References

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2. http://www.cru.uea.ac.uk/cru/people/briffa/
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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: Clivar Conference 2004
Date: Tue May 20 14:57:55 2003

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,
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 scientific
progress and associated applications of CLIVAR to the participants of the conference.
It would be very helpful if you could to 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

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References

1. http://www.clivar2004.org/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.cru.uea.ac.uk/cru/people/briffa/
4. http://www.cru.uea.ac.uk/cru/people/briffa/

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu
Subject: Re: Soon et al. paper
Date: Tue May 20 16:07:41 2003
Cc: Jerry Meehl <meehl@ucar.edu>, Caspar Ammann <ammann@ucar.edu>, mann@virginia.edu

Mike and Tom and others
My silence to do with the specific issue of the Soon and Baliunas conveys general strong
agreement with all the general remarks (and restatement of many in various forms ) by Tom
Crowley, Mike Mann, Neville Nichols and now Tom Wigley regarding the scientific value of

the paper and its obvious methodological flaws.
I have to say that I tended towards the "who cares" camp , in as much as those who are
concerned about the science should see through it anyway . I also admit to thinking that
some of you seem a little paranoid (especially in the implication that Climate Research is
a pro sceptic journal) but I am changing my mind regarding the way the "meaning" of the BS
paper is being presented to the wider public - in response to some very poor recent
reporting in the British press and several requests from the US that indicate that those of
you who work there can not simply rely on the weight of good science eventually showing
through as regards the public perception . As Tom W. states , there are uncertainties and
"difficulties" with our current knowledge of Hemispheric temperature histories and valid
criticisms or shortcomings in much of our work. This is the nature of the beast - and I
have been loathe to become embroiled in polarised debates that force too simplistic a
presentation of the state of the art or "consensus view". Having read Tom W's and Mike's
latest statements I now agree about the need to make some public comment on BS . (I too
have given my personal view of the work to David Appell who I assume is writing a balanced
view of this paper for Scientific American). I see little need to get involved in a over
detailed critic of all the points in the paper , because I am not sure what audience would
benefit from it, but the points made by those I listed above could usefully be fashioned
into a simple letter to Climate Research, signed by those who wish. This would then go on
record as a simple statement of refutation of the method employed and corresponding
limitation of the work for informing the "global warming " debate . This could be quickly
citable when talking to the media.
The one additional point I would make that seems to have been overlooked in the discussions
up to now , is the invalidity of assuming that the existence of a global Medieval Warm
period , even if shown to be as warm as the current climate , somehow negates the
possibility of enhanced greenhouse warming. The business of constructing a reliable climate
history is only one part of establishing the relative roles of natural and anthropogenic
forcings, now and in the future. Without reference to the roles of natural forcings in
recent and past times , comparisons with other periods are of very limited value anyway.
So I agree with Tom and Mike that something needs to go "on record" . The various papers
apparently in production, regardless of their individual emphasis or approaches, will find
their way in to the literature and the next IPCC can sift and present their message(s) as
it wishes., but in the meantime , why not a simple statement of the shortcomings of the BS

paper as they have been listed in these messages and why not in Climate Research?
Keith
At 05:04 PM 5/16/03 -0400, Michael E. Mann wrote:

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 "artillary" (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 diagreement 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)
(3) If the identified warm blips in their MWE were are 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 absurdem. 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.

______________________________________________________________

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: craig.wallace@uea.ac.uk
Subject: Fwd: Re: reminder
Date: Thu May 22 09:34:54 2003

Date: Wed, 21 May 2003 13:38:24 -0400
To: Keith Briffa <k.briffa@uea.ac.uk>
From: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: reminder
Hi Keith,
Busy, busy, busy as usual. Here are the lats and lons.

| LAT | LON | SITE | COORDINATES IN DECIMAL DEGREES |
|---|---|---|---|
| 52.220 | -117.23 | ATHABASCA | |
| 36.000 | -118.33 | BOREAL | |
| 68.160 | -133.20 | CAMPHILL | |
| 57.000 | 18.500 | GOTLAND | |
| 63.500 | 13.500 | JAEMTLAND | |
| 66.680 | 82.300 | MANGAZEJA | |

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     48.280  98.920          MONGOLIA
     66.830  65.670          POLAR URALS
     57.500  -76.000         QUEBEC
     72.000  102.00          TAYMIR
     47.000  11.000          TIROL
     68.220  19.720          TORNETRASK
     37.000  -118.42         UPPER WRIGHT
     67.450  142.62          ZHASCHIVIERSK
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I will get the data to you next week. I have to off to Rob Wilson's thesis defense now.
Cheers,
Ed

.. about the review and the data ( or at least accurate lats and longs while waiting)
cheers
Keith
--
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[2]http://www.cru.uea.ac.uk/cru/people/briffa[3]/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

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From: Mike Hulme <m.hulme@uea.ac.uk>
To: simon.shackley@umist.ac.uk, mgrc@ceh.ac.uk
Subject: Re: thresholds and CO2 leakage
Date: Thu May 22 11:18:31 2003

Cc: tlent@ceh.ac.uk, tim.cockerill@sunderland.ac.uk, shol@bgs.ac.uk,
kevin.anderson@umist.ac.uk

Simon,
Some comments to your questions below ......
At 13:46 20/05/2003 +0100, Simon J Shackley wrote:

dear Melvin, Tim, Mike, Tim, Sam and Kevin
For our analysis of acceptable leakage rates of carbon dioxide from geological storage sites, we can use the data provided in Lenton & Cannell CC paper I think. In particular, we could use your finding that to limit warming to under 0.2oC per decade, rate of increase of fossil fuel emissions has to be limited to under 0.03 GtC/yr/yr. This would seem sufficient to avoid the peak warming which occurs in about 2250 under the IS92a emissions scenario (figure 1(c)). Is the 0.2oc / decade threshold widely accepted in the science community however?

This threshold (0.2/decade; 2degC absolute by 2100) is the most commonly cited in science-policy circles. The EU have formally adopted it as a preferred target. It's origin however is less than obvious and it's adequacy difficult to establish. And of course it also depends whether this is carried out to 2200 - the impacts of 4degC by 2200 is not the equivalent of impacts of 2degC by 2100.
My personal view is that there is much circular argument here. The first GCM experiments in the 1980s were 2xCO2 equilibrium, i.e., 550ppmv (cf. 275ppmv pre-industrial). Thus much early work used these scenarios. 550ppmv is also a commonly cited target for no other reason than this. A 60% reduction in CO2 is broadly commensurate with 550ppm stabilisation (admittedly, the range is wide coz of C cycle uncertainty; but 60% is mid-range). And (again mid-range) 550ppm leads to about a 2degC global warming, which by 2100 is 0.2degC/decade. Independent arguments for 0.2deg/decade exist for sure - e.g. rate of ecosystem migration - but as we all know (and have pointed out in our paper on external and internal definitions of dangerous climate change), no single metric is adequate.
My feeling is that the 2degC (0.2deg/decade) mantra is as much related to the early mind-set of 2xCO2 GCM experiments as it is rooted in any more substantive reasoning. One might also point out of course that the world has been warming at about 0.15degC/decade now for three decades (since the 1970s) - has this been acceptable/dangerous?

Should we also be looking at a 0.1oC / decade threshold as well?

I would regard this threshold as a very conservative (or radical - depending on how you look at it) one

Since we are only looking at the UK we will need to translate the 0.03 GtC figure into allowable rate of increase (presumably decrease) of European emissions and then pro-rata to the UK. IPCC SRES Emissions scenarios would provide some basis for doing these calculations and i'll have a look at the data they provide. Alternatively / in addition, we could use the Contraction

and Convergence model of the GCI to calculate 'acceptable' rates
of change (decreasing) of UK emissions into the next millenium.
In Lenton & Cannell, the authors argue that: 'Early consideration
should be given to leaving a fraction of fossil carbon unused, and/or
to carbon capture and storage'. One implication of the work on
leakage from geological storage sites is that the suggestion to use
CCS to lessen eventual warming might not hold on longer
timescales, depending on the rate of leakage. So does any one
have any idea on what fraction of fossil carbon should be left in the
ground so as to provide a cap on the eventual warming on long time
scales (3000 years say)? Is there an 'accepted' threshold for
eventual warming which is 'safe' and to which society can adapt?
If so, what does this threshold tell us about how much carbon has
to be left in the ground? A simpler way forward for us might again
be to use Contraction & Convergence to provide us with an
acceptable absolute level of emissions from the UK on long
millenial timescales and to work backwards from that figure to
calculate acceptable leakage rates for the UK.
Thanks for any help you can provide
Simon

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From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Pritchard, Norah" <norah.pritchard@metoffice.com>
Subject: Re: IPCC WG2 AR4 draft outlines - WGII outline & Chapters 2 and 13
Date: Mon Jun 2 13:49:07 2003

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 an 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
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 Techical 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 there fore 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,
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<<AR4_outline27May_2scen_v1.doc>> <<AR4 WG2 summary final.doc>>

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From: Scott Rutherford <srutherford@gso.uri.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>
Subject: revised NH comparison manuscript
Date: Tue, 3 Jun 2003 14:51:09 -0400
Cc: Mike Mann <mann@virginia.edu>

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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 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

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Attachment Converted: "c:\eudora\attach\nhcomparison_v7_1.doc"
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_______________________________________________
Scott Rutherford

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From: "Michael E. Mann" <mann@virginia.edu>
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>
Subject: Re: Prospective Eos piece?
Date: Wed, 04 Jun 2003 10:17:57 -0400
Cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.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 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 where 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
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 rectanges 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 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 ih 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,
 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
globab 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.
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 Ihave 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
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 regrds,
Judy Jacobs

______________________________________________________________
Professor Michael E. Mann
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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

______________________________________________________________
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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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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
Attachment Converted: "c:\eudora\attach\MannPersp20021.pdf"

References

1. http://eos-submit.agu.org/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

320. 1054748574.txt
###################################################################################
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential REALLY URGENT
Date: Wed Jun  4 13:42:54 2003

I am really sorry but I have to nag about that review - Confidentially I now need a hard
and if required extensive case for rejecting - to support Dave Stahle's and really as soon
as you can. Please
Keith
At 08:00 AM 5/28/03 -0400, you wrote:

Hi Keith,
Okay, here is a zipped archive containing Jan's ring-width measurement series. The
directory names are:
random
all
slope
flat
"All" contains files with "all" series; "slope" has those series Jan reckoned had
curvilinear growth trends; "flat" has those series with linear growth trends; "random"
are those series that Jan chose not to use. Note that I had to pull out the Mongolia
data set. I would love to give you it, but Gordon would go nuts if he found out. I don't
know any way around this problem.
The file names are:
01ath   Athabasca
02bor   Boreal
03cam   Camphill
04que   Quebec
05upp   Upper Wright
06got   Gotland
07jae   Jaemtland
08lau   Lauenen (site not used in paper)
09tir   Tirol
10tor   Tornestrask
11man   Mangazeja
13pol   Polar Urals
14tay   Taymir
15zha   Zhaschiviersk
I can't put my hands on the derived RCS indices for these sites just now, but I can find
them if you want them. This at least gives you the basic data and how it was partitioned
by Jan. I did not participate in this stage of the analysis, so any questions about it
should be directed to Jan.
Cheers,
Ed
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Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964  USA
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Phone:  845-365-8618
Fax:    845-365-8152
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[1]http://www.cru.uea.ac.uk/cru/people/briffa[2]/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/

321. 1054756929.txt
###############################################################################
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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential REALLY URGENT
Date: Wed Jun  4 16:02:09 2003

Hi Big Boy
You just caught me as I was about to slope off after a brutal day - we spent all day
yesterday interviewing for a job we have and then someone accepted it - and now Janice
tells us we don't have the money to pay at therate the job was advertised for! This attack
sounds like the last straw- from what you say it is a waste of time my looking at it but
send a copy anyway. The file you have is an old version of a reconstruction output for one
Tornetrask reconstruction - if it was labelled something like 990 it is the original Nature
one , but 997 (i Think//1) would make it the Climate Dynamics one . Trouble is I will have
to go back and find out which . Please ring if I haven't my tomorrow to remind me - and
concentrate on the review for now. I will also talk about an extended nearby data set
(temp) that might allow a longer more rigorous validation . Kirsten has just done Math GCSE
and Amy her driving test so I have to go and picjk them up. I will looke at the file and be
ready with an answer by midday my time. the best and a beer til then
Keith
At 09:50 AM 6/4/03 -0400, you wrote:

Hi Keith,
Okay, today. Promise! Now something to ask from you. Actually somewhat important too. I
got a paper to review (submitted to the Journal of Agricultural, Biological, and
Environmental Sciences), written by a Korean guy and someone from Berkeley, that claims
that the method of reconstruction that we use in dendroclimatology (reverse regression)
is wrong, biased, lousy, horrible, etc. They use your Tornetrask recon as the main
whipping boy. I have a file that you gave me in 1993 that comes from your 1992 paper.

Below is part of that file. Is this the right one? Also, is it possible to resurrect the
column headings? I would like to play with it in an effort to refute their claims.
If published as is, this paper could really do some damage. It is also an ugly paper to
review because it is rather mathematical, with a lot of Box-Jenkins stuff in it. It
won't be easy to dismiss out of hand as the math appears to be correct theoretically,
but it suffers from the classic problem of pointing out theoretical deficiencies,
without showing that their improved inverse regression method is actually better in a
practical sense. So they do lots of monte carlo stuff that shows the superiority of
their method and the deficiencies of our way of doing things, but NEVER actually show
how their method would change the Tornetrask reconstruction from what you produced.
Your assistance here is greatly appreciated. Otherwise, I will let Tornetrask sink into
the melting permafrost of northern Sweden (just kidding of course).
Cheers,
Ed
TORNETRASK RECONSTRUCTION

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 500 | 1.24 | -9.99 | 0.00 | 0.16 | 0.81 | 0.31 | | | |
| 501 | 0.38 | -9.99 | 0.00 | 0.25 | 0.81 | 0.39 | | | |
| 502 | 0.51 | -9.99 | 0.00 | 0.08 | 0.81 | 0.25 | | | |
| 503 | 0.14 | -9.99 | 0.00 | 0.19 | 0.81 | 0.34 | | | |
| 504 | -1.32 | -9.99 | 0.00 | 0.19 | 0.81 | 0.34 | | | |
| 505 | -0.65 | -9.99 | 0.00 | 0.08 | 0.81 | 0.25 | | | |
| 506 | -0.19 | -9.99 | 0.00 | 0.07 | 0.81 | 0.24 | | | |
| 507 | 0.55 | -9.99 | 0.00 | 0.19 | 0.81 | 0.33 | | | |
| 508 | 0.54 | -9.99 | 0.00 | 0.16 | 0.81 | 0.31 | | | |
| 509 | 0.93 | -9.99 | 0.00 | 0.11 | 0.81 | 0.27 | | | |
| 510 | 0.02 | -9.99 | 0.00 | 0.14 | 0.81 | 0.29 | | | |
| 511 | -1.62 | -9.99 | 0.00 | 0.20 | 0.81 | 0.35 | | | |
| 512 | -0.01 | -9.99 | 0.00 | 0.13 | 0.81 | 0.28 | | | |
| 513 | 1.00 | -9.99 | 0.00 | 0.11 | 0.81 | 0.27 | | | |
| 514 | 0.10 | -9.99 | 0.00 | 0.14 | 0.81 | 0.29 | | | |
| 515 | -0.96 | -9.99 | 0.00 | 0.11 | 0.81 | 0.26 | | | |
| 516 | -0.08 | -9.99 | 0.00 | 0.12 | 0.81 | 0.27 | | | |
| 517 | 0.35 | -9.99 | 0.00 | 0.09 | 0.85 | 0.25 | | | |
| 518 | 0.30 | -9.99 | 0.00 | 0.10 | 0.85 | 0.26 | | | |
| 519 | 0.55 | -9.99 | 0.00 | 0.10 | 0.85 | 0.26 | | | |
| 520 | -0.19 | -9.99 | 0.00 | 0.10 | 0.85 | 0.26 | | | |
| 521 | -0.84 | -9.99 | 0.00 | 0.23 | 0.85 | 0.38 | | | |
| 522 | -0.83 | -9.99 | 0.00 | 0.23 | 0.85 | 0.37 | | | |
| 523 | 0.05 | -9.99 | 0.00 | 0.07 | 0.85 | 0.24 | | | |
| 524 | -0.27 | -9.99 | 0.00 | 0.08 | 0.85 | 0.25 | | | |
| 525 | 0.14 | -9.99 | 0.00 | 0.07 | 0.85 | 0.24 | | | |
| 526 | 0.01 | -9.99 | 0.00 | 0.10 | 0.85 | 0.25 | | | |
| 527 | -0.31 | -9.99 | 0.00 | 0.13 | 0.85 | 0.28 | | | |
| 528 | 0.46 | -9.99 | 0.00 | 0.09 | 0.85 | 0.25 | | | |
| 529 | 0.01 | -9.99 | 0.00 | 0.09 | 0.85 | 0.25 | | | |

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| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 1848 | 0.10 | -9.99 | 0.00 | 0.09 | 1.00 | 0.24 | | | |
| 1849 | -0.39 | -9.99 | 0.00 | 0.14 | 1.00 | 0.28 | | | |
| 1850 | 0.55 | -9.99 | 0.00 | 0.16 | 1.00 | 0.29 | | | |
| 1851 | 0.04 | -9.99 | 0.00 | 0.13 | 1.00 | 0.27 | 1.92 | 0.96 | -1.98 |

```
-1.24
     -1.41  -0.35
      1852    0.68   -9.99    0.00    0.12    1.00    0.26  -2.82    0.59    1.66
1.95
     2.12   0.70
      1853    0.67   -9.99    0.00    0.14    1.00    0.28  -2.23    0.24    2.27
1.64
     -0.33   0.32
      1854    1.13   -9.99    0.00    0.14    1.00    0.27   0.21    1.57    0.89
2.47
     2.11   1.45
      1855    0.05   -9.99    0.00    0.15    1.00    0.29  -0.74   -0.80    0.24
4.19
     -0.16   0.55
      1856   -1.41   -9.99    0.00    0.19    1.00    0.33  -0.48   -1.24   -1.37
-0.34
     -2.55  -1.20
      1857   -0.30   -9.99    0.00    0.19    1.00    0.32  -1.13   -0.78   -1.39
-0.23
     2.44  -0.22
      1858    0.81   -9.99    0.00    0.15    1.00    0.28  -0.63    0.48    1.37
2.74
     2.72   1.34
      1859   -0.60   -9.99    0.00    0.10    1.00    0.25  -1.28    0.73    1.04
0.10
     0.16   0.15
      1860    0.49   -9.99    0.00    0.10    1.00    0.24  -0.41   -1.37    0.62
0.42
     0.17  -0.11
      1861    0.73   -9.99    0.00    0.10    1.00    0.24  -1.19   -2.59    1.54
2.27
     0.33   0.07
      1862   -0.15   -9.99    0.00    0.06    1.00    0.22  -0.06    0.50   -1.16
-2.08
     -1.95  -0.95
      1863    0.03   -9.99    0.00    0.08    1.00    0.23   1.00   -0.79    0.18
-1.72
     -0.60  -0.39
      1864   -0.50   -9.99    0.00    0.11    1.00    0.25  -0.49   -3.34    0.26
0.74
     -2.40  -1.05
      1865   -0.32   -9.99    0.00    0.07    1.00    0.22   0.10    0.14   -2.96
1.61
     -1.31  -0.48
      1866   -0.37   -9.99    0.00    0.10    1.00    0.24   0.29   -1.99    0.67
-1.17
     0.67  -0.31
      1867   -1.03   -9.99    0.00    0.12    1.00    0.26  -2.83   -5.37   -2.59
-0.62
     -0.31  -2.34
      1868   -0.28   -9.99    0.00    0.16    1.00    0.29  -0.02    1.04   -0.36
1.72
     2.78   1.03
      1869   -0.84   -9.99    0.00    0.10    1.00    0.25   1.21   -1.14   -1.40
0.53
     -0.63  -0.29
      1870   -0.25   -9.99    0.00    0.12    1.00    0.26   1.33   -0.70   -0.27
1.12
     -0.36   0.22
      1871   -0.59   -9.99    0.00    0.10    1.00    0.24  -2.34   -2.32   -2.34
1.12
     -0.09  -1.19
      1872    0.44   -9.99    0.00    0.10    1.00    0.25   0.80    0.57    1.16
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1.32
-0.34 0.70
1873 0.52 -9.99 0.00 0.14 1.00 0.28 -1.97 -2.50 0.82
1.38
0.12 -0.43
1874 -0.54 -9.99 0.00 0.11 1.00 0.25 0.25 -2.24 -1.15
0.15
-1.06 -0.81
1875 0.36 -9.99 0.00 0.09 1.00 0.24 -1.96 0.36 0.00
0.87
-0.33 -0.21
1876 0.46 -0.15 0.61 0.12 1.00 0.25 -0.70 -3.06 1.93
0.74
0.34 -0.15
1877 -0.98 -1.74 0.76 0.14 1.00 0.28 -3.31 -2.70 -1.18
0.26
-1.76 -1.74
1878 -0.04 -0.19 0.15 0.08 1.00 0.23 1.02 -0.30 0.16
-1.71
-0.12 -0.19
1879 0.20 -0.41 0.62 0.10 1.00 0.25 -1.24 -0.19 -1.09
-0.64
1.09 -0.41
1880 -1.05 0.14 -1.19 0.17 1.00 0.31 0.17 -0.53 -0.70
-0.20
1.94 0.14
1881 -1.34 -1.88 0.54 0.17 1.00 0.30 -3.66 -2.02 -1.35
-1.07
-1.32 -1.88
1882 0.30 0.37 -0.08 0.16 1.00 0.30 -0.32 0.21 -0.36
0.56
1.78 0.37
1883 1.13 0.24 0.89 0.13 1.00 0.26 0.49 -0.08 0.99
0.52
-0.70 0.24
1884 0.00 -0.80 0.80 0.14 1.00 0.27 -0.80 -1.99 -1.15
0.32
-0.39 -0.80
1885 -1.26 -1.25 -0.01 0.14 1.00 0.28 -0.29 -2.26 -2.34
0.42
-1.76 -1.25
1886 -0.24 0.10 -0.34 0.15 1.00 0.28 0.69 -0.55 -0.01
0.13
0.24 0.10
1887 -0.83 -0.40 -0.43 0.14 1.00 0.27 -0.10 0.23 -1.01
-0.12
-1.02 -0.40
1888 -0.79 -1.69 0.90 0.12 1.00 0.26 -2.95 -1.85 -1.37
-1.05
-1.25 -1.69
1889 0.28 0.71 -0.43 0.08 1.00 0.23 -0.46 2.98 2.28
-0.40
-0.84 0.71
1890 0.47 0.22 0.25 0.08 1.00 0.23 1.06 2.04 -0.58
-1.18
-0.26 0.22
1891 -0.55 -0.49 -0.06 0.16 1.00 0.30 -0.43 -0.38 -1.74
1.24
-1.12 -0.49
1892 -1.58 -1.46 -0.12 0.16 1.00 0.29 -0.95 -1.55 -2.20
-1.24
-1.36 -1.46
1893 -0.61 -0.60 -0.01 0.10 1.00 0.24 -0.46 -1.17 -0.48

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-0.07
     -0.80  -0.60
      1894    0.53    0.79   -0.26    0.09    1.00    0.24    2.61    0.07    0.50
1.18
     -0.40   0.79
      1895    0.68    0.38    0.30    0.09    1.00    0.24   -0.15    2.19    0.78
-0.66
     -0.24   0.38
      1896    0.06    0.47   -0.41    0.11    1.00    0.25   -0.04   -0.30    1.40
2.02
     -0.73   0.47
      1897    0.71    1.01   -0.30    0.13    1.00    0.27    0.90    2.20   -0.20
1.10
     1.05   1.01
      1898    0.10   -0.61    0.71    0.12    1.00    0.25   -1.06   -0.20   -0.16
-1.03
     -0.60  -0.61
      1899   -1.36   -0.84   -0.53    0.17    1.00    0.31   -0.98   -1.95   -1.85
2.38
     -1.79  -0.84
      1900   -0.38   -0.89    0.51    0.18    1.00    0.31   -1.31   -2.02   -0.02
-1.11
     -0.01  -0.89
      1901    0.85    1.32   -0.47    0.17    1.00    0.30    0.76    0.56    1.05
3.24
     1.00   1.32
      1902   -1.59   -2.44    0.85    0.19    1.00    0.33   -2.71   -2.33   -2.44
-2.52
     -2.22  -2.44
      1903   -1.27   -0.42   -0.85    0.20    1.00    0.33    0.36    0.14   -0.37
-1.02
     -1.22  -0.42
      1904   -1.52   -1.11   -0.42    0.15    1.00    0.29    0.77   -1.61   -1.73
-1.64
     -1.32  -1.11
      1905   -0.45   -0.06   -0.39    0.08    1.00    0.23   -1.29    0.69    1.41
0.05
     -1.16  -0.06
      1906   -0.44    0.55   -0.98    0.08    1.00    0.23    1.44    1.74    0.34
0.69
     -1.47   0.55
      1907   -0.40   -1.10    0.69    0.07    1.00    0.23    0.24   -2.05   -0.31
-0.70
     -2.67  -1.10
      1908   -0.15   -0.55    0.41    0.11    1.00    0.25    0.36   -1.22   -1.31
-0.22
     -0.38  -0.55
      1909   -0.77   -1.71    0.94    0.09    1.00    0.24   -2.54   -3.21   -1.26
-0.51
     -1.03  -1.71
      1910   -0.16    0.00   -0.16    0.09    1.00    0.24    1.18    0.91   -0.19
-0.60
     -1.32   0.00
      1911   -0.38    0.02   -0.40    0.09    1.00    0.24   -0.37    1.25   -1.34
-0.55
     1.12   0.02
      1912    0.06   -0.23    0.29    0.06    1.00    0.22   -1.32   -0.99    0.16
0.79
     0.20  -0.23
      1913    0.08    0.29   -0.21    0.07    1.00    0.22    1.68    0.02   -1.15
0.99
     -0.07   0.29
      1914    0.09    0.84   -0.75    0.07    1.00    0.22    1.51   -0.37    0.47
```

3.50
-0.93 0.84
1915 0.11 -0.91 1.01 0.06 1.00 0.22 -0.20 -1.59 -2.40
0.61
-0.95 -0.91
1916 -0.35 -0.51 0.16 0.13 1.00 0.26 0.46 -1.26 -1.37
1.65
-2.04 -0.51
1917 0.18 -0.02 0.20 0.11 1.00 0.25 -1.95 -1.60 1.89
-0.78
2.35 -0.02
1918 0.71 -0.39 1.10 0.10 1.00 0.24 1.11 -0.49 -1.73
0.68
-1.52 -0.39
1919 -0.09 0.12 -0.21 0.07 1.00 0.22 -0.88 1.29 0.09
1.87
-1.79 0.12
1920 0.33 0.85 -0.52 0.07 1.00 0.22 2.05 2.16 -0.36
0.93
-0.51 0.85
1921 0.29 0.75 -0.46 0.10 1.00 0.24 3.97 2.43 -0.68
-1.35
-0.62 0.75
1922 0.66 -0.23 0.89 0.12 1.00 0.26 -0.60 0.22 0.00
0.12
-0.88 -0.23
1923 -0.66 -1.84 1.19 0.12 1.00 0.26 -1.53 -1.74 -3.76
0.02
-2.20 -1.84
1924 0.49 -0.46 0.95 0.08 1.00 0.23 -1.60 -0.68 -1.93
0.64
1.25 -0.46
1925 0.30 1.10 -0.80 0.12 1.00 0.26 1.66 0.70 -0.63
3.49
0.30 1.10
1926 0.47 0.06 0.41 0.10 1.00 0.24 -0.06 -0.51 0.02
0.75
0.12 0.06
1927 0.23 0.10 0.14 0.11 1.00 0.25 -0.58 -2.17 -1.54
3.18
1.60 0.10
1928 -0.82 -1.21 0.39 0.11 1.00 0.25 0.42 -0.20 -3.05
-2.14
-1.09 -1.21
1929 0.00 -1.25 1.26 0.15 1.00 0.28 -3.24 0.57 -1.51
-1.02
-1.06 -1.25
1930 1.00 1.42 -0.42 0.16 1.00 0.29 1.78 1.81 0.59
1.58
1.34 1.42
1931 -0.67 -0.21 -0.46 0.08 1.00 0.23 -0.29 1.18 -2.95
1.21
-0.20 -0.21
1932 -0.32 0.27 -0.59 0.08 1.00 0.23 0.54 0.03 -1.68
1.74
0.74 0.27
1933 0.65 0.36 0.29 0.12 1.00 0.26 -0.33 -0.86 1.64
1.77
-0.43 0.36
1934 0.56 0.98 -0.42 0.12 1.00 0.26 0.37 1.88 -0.48
1.88
1.27 0.98
1935 -0.56 -0.37 -0.20 0.09 1.00 0.24 0.30 -1.94 0.11

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-0.05
     -0.25  -0.37
      1936   -0.09    1.48   -1.57    0.19    1.00    0.33    0.03    1.84    2.96
1.86
     0.71   1.48
      1937    1.77    2.39   -0.62    0.19    1.00    0.32    2.82    2.55    1.32
2.26
     3.01   2.39
      1938    0.58    0.91   -0.33    0.09    1.00    0.24    0.59   -0.07   -0.60
2.49
     2.14   0.91
      1939    0.31    0.71   -0.40    0.08    1.00    0.23   -0.22   -0.15    0.04
0.99
     2.88   0.71
      1940    0.20    0.42   -0.22    0.15    1.00    0.28   -0.95    2.26    0.72
0.67
     -0.60   0.42
      1941   -0.03   -0.20    0.17    0.14    1.00    0.28   -2.00   -1.34   -1.20
3.70
     -0.17  -0.20
      1942    0.11   -0.50    0.61    0.08    1.00    0.23    0.14   -1.04   -1.47
-0.32
     0.20  -0.50
      1943    0.36    0.69   -0.33    0.07    1.00    0.22    1.55    0.88    0.99
0.69
     -0.64   0.69
      1944    0.12   -0.50    0.62    0.10    1.00    0.24   -1.67   -1.25   -1.58
1.18
     0.83  -0.50
      1945    0.57    0.71   -0.14    0.10    1.00    0.25    1.21   -0.53   -0.86
1.81
     1.91   0.71
      1946    0.48    0.64   -0.16    0.09    1.00    0.24    1.17    0.28   -0.18
1.62
     0.31   0.64
      1947    0.69    1.20   -0.51    0.10    1.00    0.24    0.18    1.48    1.69
1.43
     1.20   1.20
      1948    0.00    0.67   -0.67    0.08    1.00    0.23    2.10    1.66    0.03
0.74
     -1.18   0.67
      1949   -0.21    0.11   -0.32    0.14    1.00    0.27    1.26    1.76   -1.34
-0.14
     -1.01   0.11
      1950    0.83    0.73    0.09    0.10    1.00    0.24    2.24    0.91   -0.14
-0.52
     1.18   0.73
      1951   -0.13   -0.34    0.21    0.07    1.00    0.22    0.78   -1.83   -1.25
-1.23
     1.84  -0.34
      1952   -0.13   -0.38    0.25    0.12    1.00    0.26    1.78   -0.91   -1.17
-0.26
     -1.34  -0.38
      1953    0.95    1.11   -0.16    0.11    1.00    0.25    1.80    0.21    3.01
0.37
     0.16   1.11
      1954    0.12    0.32   -0.20    0.10    1.00    0.24   -0.60    2.11   -0.57
0.70
     -0.05   0.32
      1955    0.02   -0.76    0.77    0.09    1.00    0.24   -2.65   -2.42   -2.22
1.42
     2.09  -0.76
      1956   -0.26   -0.94    0.68    0.07    1.00    0.22   -2.32    0.39    0.12
```

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-0.73
     -2.15  -0.94
      1957   -0.15   -0.31    0.16    0.07    1.00    0.22  -0.09  -0.53  -2.06
1.32
     -0.19  -0.31
      1958   -0.08   -0.90    0.82    0.09    1.00    0.24  -1.29  -1.07  -1.05
-0.77
     -0.31  -0.90
      1959    0.83    0.98   -0.16    0.15    1.00    0.28   1.03   0.66   0.44
1.32
     1.47   0.98
      1960    1.13    1.02    0.11    0.13    1.00    0.27   0.63   1.88   0.92
1.39
     0.29   1.02
      1961    0.05    0.17   -0.11    0.10    1.00    0.25  -0.12   0.10   1.47
0.19
     -0.81   0.17
      1962   -0.45   -1.01    0.56    0.09    1.00    0.24   1.27  -0.52  -2.15
-1.65
     -2.00  -1.01
      1963    0.11    0.79   -0.68    0.18    1.00    0.31   0.43   3.15  -0.33
-0.07
     0.77   0.79
      1964   -0.21   -0.09   -0.13    0.15    1.00    0.28   0.64   1.02  -0.78
-0.42
     -0.90  -0.09
      1965   -0.82   -0.82    0.00    0.10    1.00    0.24   0.62  -1.64  -0.03
-1.74
     -1.30  -0.82
      1966    0.07   -0.13    0.20    0.06    1.00    0.22  -2.47   0.26   1.97
0.46
     -0.87  -0.13
      1967   -0.22    0.21   -0.44    0.08    1.00    0.23   0.69   0.29  -0.80
0.13
     0.75   0.21
      1968   -0.57    0.10   -0.67    0.13    1.00    0.27   1.18  -1.20   1.37
-1.07
     0.22   0.10
      1969    0.55    0.54    0.01    0.08    1.00    0.23   0.21  -0.61   0.90
0.37
     1.82   0.54
      1970    0.37    0.40   -0.04    0.10    1.00    0.24  -1.25   0.51   2.27
0.05
     0.44   0.40
      1971   -0.31   -0.12   -0.19    0.07    1.00    0.22  -0.71   0.81  -0.64
0.03
     -0.07  -0.12
      1972    0.25    1.18   -0.94    0.08    1.00    0.23   0.18   0.44   1.62
3.00
     0.68   1.18
      1973    0.30    0.85   -0.55    0.10    0.99    0.25  -0.02   0.76   1.31
2.85
     -0.66   0.85
      1974    0.07    0.12   -0.05    0.11    0.99    0.25   0.86  -0.41   0.62
-0.30
     -0.18   0.12
      1975   -0.49    0.51   -1.00    0.08    0.99    0.23   0.45   1.72  -1.09
0.62
     0.84   0.51
      1976    0.08   -9.99    0.00    0.07    0.99    0.22  -0.28   1.72  -1.36
-0.23
     0.05  -0.02
      1977   -0.33   -9.99    0.00    0.08    0.99    0.23  -1.05  -0.01  -0.50
```

-0.90
-0.65 -0.62
1978 -0.30 -9.99 0.00 0.07 0.96 0.23 -0.98 0.92 0.14
-0.48
-1.07 -0.29
1979 0.06 -9.99 0.00 0.12 0.95 0.26 -0.73 0.75 1.02
-0.83
0.07 0.06
1980 0.93 -9.99 0.00 0.13 0.95 0.26 1.42 -0.37 1.23
1.02
-0.36 0.59

I am really sorry but I have to nag about that review - Confidentially I now need a hard
and if required extensive case for rejecting - to support Dave Stahle's and really as
soon as you can. Please
Keith
At 08:00 AM 5/28/03 -0400, you wrote:

Hi Keith,
Okay, here is a zipped archive containing Jan's ring-width measurement series. The
directory names are:
random
all
slope
flat
"All" contains files with "all" series; "slope" has those series Jan reckoned had
curvilinear growth trends; "flat" has those series with linear growth trends; "random"
are those series that Jan chose not to use. Note that I had to pull out the Mongolia
data set. I would love to give you it, but Gordon would go nuts if he found out. I don't
know any way around this problem.
The file names are:
01ath Athabasca
02bor Boreal
03cam Camphill
04que Quebec
05upp Upper Wright
06got Gotland
07jae Jaemtland
08lau Lauenen (site not used in paper)
09tir Tirol
10tor Tornestrask
11man Mangazeja
13pol Polar Urals
14tay Taymir
15zha Zhaschiviersk
I can't put my hands on the derived RCS indices for these sites just now, but I can find
them if you want them. This at least gives you the basic data and how it was partitioned
by Jan. I did not participate in this stage of the analysis, so any questions about it
should be directed to Jan.
Cheers,
Ed
--
==================================

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[2]http://www.cru.uea.ac.uk/cru/people/briffa[3]/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

322. 1054757526.txt
##########################################################################################

From: "Michael E. Mann" <mann@virginia.edu>
To: rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>
Subject: Fwd: Re: Prospective Eos piece?
Date: Wed, 04 Jun 2003 16:12:06 -0400
Cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.edu>

Dear All,

I've attached a draft (attached word document), incorporating many of the suggestions,
wording, etc. I've already recieved from various of you. Some specific
comments/inquiries/requests for help indicated in yellow highlighting. Waiting to hear
back from Peck and Tom C (guys: if you're out there, can you give a holler, to let me know
your disposition? thanks). Otherwise everyone else has indicated they're on board.
I've been in touch w/ Judy Jacobs at AGU to clarify the ground rules. Apparently we *can*
refer, where necessary, to press releases, parenthetically in the piece. I think this is
important in our case because there is a subtle, but important, distinction between what
the papers actual purport to show, and what the authors (and their promoters) have
*claimed* they show (e.g. in the Harvard-Smithsonian press release). We need to draw out
this distinction-I sent Judy my paragraph on that, and she said it looks fine--so
apparently its kosher.
I've avoided any reference to unpublished work however (e.g. Mann and Jones), because this
opens up a can of worms. We can nicely make use of work that Keith has already done to
provide a suggestion of the longer-term (past 2K) changes, for greater context...
Re, references--we necessarily have to go well over the normal 10 or so, because part of
the strength of our piece is the wealth of recent studies supporting our basic conclusions.
Judy said that's ok too--especially since our text is short (by about 100 words) relative
to the official (1200 word) limit. So we should try to keep it that way..ie, we need to
play a zero-sum game, as much as possible, with any suggested revisions.
Re figures, Scott Rutherford has generously offered to help prepare a draft of figure 1
which I'll send on to everyone once its available.
I've also described, in the figure caption, my concept of Figure 2--clearly it would be
helpful if Phil and Ray could collaborate on the preparation of this one (guys?).
Looking forward to comments, and suggested revisions. I'll just accumulate these from
everyone in whatever form you prefer to provide them (emailed comments, word file w/ track
changes or highlighting of changes used, etc) and try to prepare a revised draft once I've
heard back from everyone.
Thanks again to everyone for their willingness to help with this and to be involved with
this,
mike

Date: Wed, 04 Jun 2003 10:17:57 -0400
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>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Prospective Eos piece?
Cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.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 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 where
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 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
rectanges
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
     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 ih 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,
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
globab 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.
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 Ihave 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
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 regrds,
Judy Jacobs

______________________________________________________________
Professor Michael E. Mann

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

References

1. http://eos-submit.agu.org/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

323. 1055004012.txt
####################################################################################
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: Revised Version!
Date: Sat, 07 Jun 2003 12:40:12 -0400
Cc: "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>, Tom Wigley
<wigley@ucar.edu>, jto@u.arizona.edu, Scott Rutherford <srutherford@gso.uri.edu>,
mann@virginia.edu

Thanks Kevin,
Those are helpful--Tom C. has returned from travels and will be providing
comments shortly.
Will incorporate those and any others I receive into a revised version, which I
hope to
send out (w/ Figure 1 included) tonight or tomorrow,
mike
p.s. Tom W is taking the lead on preparing a companion, more targeted commentary,
to be
submitted to "Climate Research". Any one else interested should contact Tom...
At 05:16 PM 6/6/2003 -0600, Kevin Trenberth wrote:

Good job. I am attaching marked up copy with few suggestions.
Kevin
Michael E. Mann wrote:

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
thinks 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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References

1. mailto:mann@virginia.edu%A0
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. mailto:trenbert@ucar.edu
4. http://www.cgd.ucar.edu/cas/
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

324. 1055258297.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford <srutherford@gso.uri.edu>
Subject: Re: Figure 1
Date: Tue, 10 Jun 2003 11:18:17 -0400
Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

   Sounds great on all counts.
   Kevin's comments are all good ones,
   mike
   At 04:09 PM 6/10/2003 +0100, Phil Jones wrote:

      Scott,
         Seems OK. we will send both figures and the text for one last look through
today.
      Trying now to incorporate Kevin's comments.
      Cheers
      Phil
     At 10:48 10/06/03 -0400, Scott Rutherford wrote:

     Phil and others,
     Here is a revised figure. What do you think?
     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 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 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 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

---

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fax: (401) 874-6811
snail mail:
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[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

325. 1055269567.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Scott Rutherford <srutherford@gso.uri.edu>
Subject: Re: EOS text
Date: Tue, 10 Jun 2003 14:26:07 -0400
Cc: phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

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,
Keith, Tim and I have been at this for part of the day. Scott has also
redrawn Fig 1.
Attached is the latest draft, which includes Kevin's from about 1 hour ago, but not
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
Prof. Phil Jones
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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.cgd.ucar.edu/cas/
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3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

326. 1055273033.txt
####################################################################################################

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: possible rewording of section of letter?
Date: Tue Jun 10 15:23:53 2003

thanks and all now ok
Keith
At 10:30 AM 6/10/03 -0400, you wrote:

Hi Keith,
no problem...Responses below. let me know what you think...
thanks,
mike
At 03:01 PM 6/10/2003 +0100, Keith Briffa wrote:

thanks for that Mike - sorry but just a few more questions
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 qualifyer is needed - perhaps "for the most part , agree well " ?

Yes, "remarkably" is an overstatement given that, as you say, Bauer et al does stray
some bit.
How about simply:
"Agree with the proxy-based reconstructions within estimated uncertainties (Figure 1)".

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 much of the the 10th,11th and 12th centuries"

yep, better...

but also , where we say [just below] "the specific periods of cold and warm apparent for
Europe differ significantly from those for the Northern Hemisphere as a whole." , to
what evidence of European anomalies are we referring?

ahh--I left that open-ended, for Phil and you guys to deal with as you see best. I was
anticipating that Figure 2 would include an appropriate proxy series or two for Europe
(CET, Fennoscandia?) that would make this point. But why don't you guys revise the

wording, as necessary, based on Figure 2?

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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

327. 1055512559.txt
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From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: EOS text
Date: Fri, 13 Jun 2003 09:55:59 -0600
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

<x-flowed>
Hi all
On isotopes, see the paper by Werner et al (briefly discussed in our
Science perspectives) showing that isotopes don't sample the deep winter
well as there is inadequate precip then in Greenland during the past.
I had to send this as I have been getting 2 of everything and I so I
adjusted the cc list.
Kevin

Phil Jones wrote:

>
>  Tom,
>     The W. Greenland series is based on a stack of 6 isotope series -
> see chapter by
>  Fisher et al in book from 1996 by Jones, Bradley and Jouzel.
> Correlation of this series
>  with Greenland Annual temps is 0.58 on annual timescale over 1901-80.
> It is one of the
>  better ones of the series in Fig 2. Others are better with different
> seasons, but this one
>  is good for annual.  The averaging of the 6 sites improves it a lot.

>
>  Cheers
>  Phil
>
>
>
> At 08:51 13/06/03 -0600, Tom Wigley wrote:
>
>> Phil,
>>
>> If W Greenland is based on isotopes, I note that the correlation
>> between these and temperature is very low. Do we really want to
>> perpetuate the myth that ice core isotopes are a good proxy for
>> temperature?
>>
>> Tom.
>> ___________________________
>>
>> Phil Jones wrote:
>>
>>>
>>>>
>>>>  Dear All,
>>>
>>>            Keith, Tim and I have been at this for part of the day.
>>> Scott has also redrawn Fig 1.
>>>  Attached is the latest draft, which includes Kevin's from about 1
>>> hour ago, but not 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
>>>
>>>
>>> Prof. Phil Jones
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328. 1056133160.txt
######################################################################################

From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: VERY VERY IMPORTANT
Date: Fri, 20 Jun 2003 14:19:20 -0400
Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

Hi Phil et al,
Re, Malcolm co-authorship--big oversight on my part. Can you ask Ellen if we can add his
name (i.e., just say it was 'accidentally left off'), where it belongs alphabetically in
the list.
I've talked to Malcolm on the phone. The PC #1 *is* the right one--but Malcolm has raised
the valid point that we need to cover our behinds on what was done here, lest we be
vulnerable to the snipings of the Idsos and co (i.e., that non-climatic influences on
recent growth were nominally dealt w/, as in MBH99).
Malcolm is supposed to be sending some text to Phil.
So, can we incorporate his small bit of text, and add his name, and then resubmit to AGU
ASAP?
Thanks all for all the help here. Now, I better get back to my newlywed wife!
mike
At 05:25 PM 6/20/2003 +0100, Phil Jones wrote:

Mike,
Malcolm has just called Keith. He's been with Ray. Apart from probably being a
little
miffed off he's not on the article, he says that the W. US series in Figure 2 is wrong.
He says
it looks the first PC (which I said it was), but that this isn't the corrected

one (for
CO2 growth
effects). Can you check whether it is the right one?  Malcolm says that Idso (who was
on
E&E) will say that the increase in that series is not climatic but due to
fertilization. This
would not look good obviously. Idso was on a paper with Don Graybill re fertilisation
effects
on bristlecones.
If you need to send a revised series for this top series in Fig 2 then send it to
Tim.
Tim has done this plot so can make the alterations if another series is needed. If you
think
that the series is OK then we'll leave it.  If you do change it will affect Fig 2 of
the GRL also
but probably not to any noticeable effect - at least at the size the plot will be.
Tim will send round the copyright forms to all and reprint forms. Tell Tim if you
want any.
Seems like the pdf will do.
Cheers
Phil
PS Tell Lorraine I'm not always emailing you - but Malcolm thought the above was
important.
I assumed you would have sent the corrected one you used in GRL in 1999.

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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

329. 1056440026.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Ellen Mosley-Thompson <thompson.4@osu.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: 2003ES000354 Decision Letter
Date: Tue, 24 Jun 2003 03:33:46 -0400

Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

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) .
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
(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
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For information on the production process, please contact Shermonta 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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Norwich                        Email    p.jones@uea.ac.uk
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Professor Michael E. Mann
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[5]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.agu.org/sci_soc/policy/climate_change_position.html
2. http://www.agu.org/pubs/journal_forms/EosCopyright.pdf
3. http://www.agu.org/pubs/journal_forms/EosReprint_orders.pdf
4. http://www.adobe.com/prodindex/acrobat/readstep.html
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

330. 1056477710.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@ucar.edu>
Subject: Re: bradley comment

Date: Tue, 24 Jun 2003 14:01:50 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, mann@virginia.edu

Tim,
I suggest we let Eos size the figures, etc. Then, in the end, we can simply substitute a
version of Figure 2 w/ the correlations added at the proof stage. Anything else will slow
down the publication of the manuscript unnecessarily, in my opinion.
Phil and I have already discussed--we agree that the low weight given to the record in the
Mann and Jones composite treats the record appropriately...
mike
At 02:37 PM 6/24/2003 +0100, Tim Osborn wrote:

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 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.

Dr Timothy J Osborn
Climatic Research Unit

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web:      [1]http://www.cru.uea.ac.uk/~timo/
sunclock: [2]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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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

References

1. http://www.cru.uea.ac.uk/~timo/
2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

331. 1056477985.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Raymond S. Bradley" <rbradley@geo.umass.edu>
Subject: Re: ice cores/China series (FYI)
Date: Tue, 24 Jun 2003 14:06:25 -0400
Cc: mann@virginia.edu

Thanks Keith,
I just read your email after reading the others. We actually eliminate records with
negative correlations (this is mentioned breifly 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
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
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Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784

[1]http://www.cru.uea.ac.uk/cru/people/briffa/

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

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml

332. 1056478635.txt
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From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: Nguyen Huu Ninh (cered@hn.vnn.vn)
Subject: NOAA funding
Date: Tue, 24 Jun 2003 14:17:15 +0000

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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

____________________________________________

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Email: m.kelly@uea.ac.uk
Web: http://www.cru.uea.ac.uk/tiempo/
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From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Asher Minns" <A.Minns@uea.ac.uk>
Subject: Re: From Prof. Pachauri
Date: Thu Jun 26 15:04:29 2003

Asher,
Spoke with Sinclair-Wilson from Earthscan yesterday about this and we agreed one or two
things. We should take next steps on this after the Assembly business has died down.
Mike
At 07:51 19/06/2003 +0100, you wrote:

Mike, this message below id fresh-in from RK Pachauri. He seems keen, and we have been given a direct contact at TERI. He has made a few interesting suggestions on content, though nothing on funding as of yet.
Asher
------------------------------
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
----- Original Message -----
From: "R K Pachauri" <pachauri@teri.res.in>
To: <tyndall@uea.ac.uk>
Cc: "Ulka Kelkar" <ulkak@teri.res.in>
Sent: Thursday, June 19, 2003 7:34 AM
Subject: Dear Prof. Hulme
Dear Prof. Hulme,
Thank you for your letter proposing that the Tyndall Centre and TERI jointly produce a series of yearbooks on climate change. May I congratulate you on this excellent idea! I am convinced that a market exists for precisely such a publication, and am delighted that you thought of TERI as a partner in this venture.
I am putting down some initial thoughts on the proposed publication and the

suggested contents that you had sent.
While there is a lot of information and related data available on climate change, it is scattered. On the one hand we have the IPCC assessment on the state of knowledge about climate change, and on the other the WMO's annual bulletins. Similarly, the UNFCCC compiles GHG inventory information from periodically submitted National Communications, while the IEA presents annual fuel combustion emission statistics. In such a scenario, the metier of our Yearbook would be to synthesise the current knowledge on climate change. As mentioned in your note, it would present this information in a clear and visually appealing manner. Moreover, it would go into climate change issues in more detail than say, the annual World Resources brought out by WRI.
The Foreword - and perhaps an Emerging Issues section at the end of the book - could comment on scientific and political issues, which are otherwise not discussed in either the IPCC Reports or in the types of publications mentioned above.
In the draft table of contents, there are two sections that are slightly different in character from the others. In the chapter on national policies, we may choose between alternative structures:
1 By Annex I country
2 By type of policy/instrument (e.g. CDM, international trading regimes, taxation, etc)
The proposed chapter on Social Change and Adaptation is important to complete the set of topics/issues covered in the Yearbook, but is probably the most complex in terms of scope/structure. One option that we could discuss is to cover adaptation policies not in chapter 7, but in chapter 9, and to highlight studies of community and local government level implementation.
With such a scope, the media would also be an important part of the audience for this yearbook
I do appreciate that producing this Yearbook would involve significant commitment in terms of time and effort if all relevant literature is to be reviewed. However, by teaming up authors from our two organisations, I am confident that we will provide an impartial yet balanced North-South perspective to the Yearbook. For specialised subjects, like the chapter on business, we may even think of invited chapters, by say the WBCSD.
You may also be interested to know that TERI also brings out a yearbook focusing on India, called the TERI Energy Directory, Database, and Yearbook (TEDDY). This publication has a readership of 15000-20000, reaching out to government, corporates, individual researchers, and libraries in India and overseas.
These are just some initial thoughts, and my colleagues can be in touch with your team to develop this outline further. Ms Ulka Kelkar (ulkak@teri.res.in) will coordinate this effort on behalf of TERI.
We look forward to working with you on this Yearbook.
With kind regards,
Yours sincerely,
R.K. Pachauri

References

1. http://www.tyndall.ac.uk/

334. 1056986548.txt
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From: Jenny Duckmanton <jmd4@york.ac.uk>
To: Mick Kelly <m.kelly@uea.ac.uk>
Subject: Re: Tiempo final invoice
Date: Mon, 30 Jun 2003 11:22:28 +0100
Cc: "Duckmanton, Jenny" <jmd4@york.ac.uk>, "Kuylenstierna, Johan" <jck1@york.ac.uk>

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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/
> ____________________________________________

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From: "Kuylenstierna, J.C." <jck1@york.ac.uk>
To: Mick Kelly <m.kelly@uea.ac.uk>
Subject: New tiempo cpsts
Date: Mon, 30 Jun 2003 18:25:29 +0100

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Hi Mick,

Sara has sugested 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

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
--
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       +44 1904 432897 (general)
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Email.: jck1@york.ac.uk

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From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: 'dean.env@uea.ac.uk'

Subject: Museum of Climate Change
Date: Wed, 02 Jul 2003 13:17:11 +0000

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Trevor
A quick update:
1. I'm arranging a meeting between our team and the Museums Service (including I hope the director) late July to discuss next stage. I'll consult Chris Flack about possible dates. They are ready to push ahead with the next stage.
2. N County Council now appear well and truly behind the project and want to bring development responsibility into their Economic Development Unit. Good news in terms of political will, but some concern about loss of control and transformation into a tourism project.
Think we need to resolve how best this initiative might relate to the linking CRED initiative, as discussed, and reach understanding with Museums Service sooner rather than later? Unless it's premature?
Finally, Melissa Burgan, ex MSc student, now with NCC transport division is very impressed with way CRED has been taken seriously by county council politicos. I assume her assessment is accurate!
Mick

______________________________________________

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Email: m.kelly@uea.ac.uk
Web: http://www.cru.uea.ac.uk/tiempo/
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From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: FP6-news?
Date: Fri, 4 Jul 2003 21:29:43 +0200

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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 hould 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 , Starkel and
>Zorita
>(in place of Von Storch). The rationale for the meeting was nothing
>more than The EC (Hans Brelen) 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
>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
>>>>variablity 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
>>>>
>>>>
>>>>
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>>>>tel: +4755583491/secr:+4755589803/fax:+4755584330
>>>>eystein.jansen@geo.uib.no, www.bjerknes.uib.no
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______________________________________________________________
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Fax:    +47-55-584330
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The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts
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From: Keith Alverson <keith.alverson@pages.unibe.ch>
To: Rick Battarbee <r.battarbee@geog.ucl.ac.uk>, Eystein Jansen
<eystein.jansen@geol.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: fp6
Date: Mon, 07 Jul 2003 09:57:05 +0200

Dear Rick, Keith and Eystein,

It is certainly good news that FP6 will have a climate change and paleo related call. My personal feeling is that whatever paleo proposal(s) eventually do go in that it would be a good thing to specifically include the PAGES office in Bern as a participant in the network. This would, I believe, help the network by providing an international context and the many PAGES resources for outreach within Europe, and inclusion of non-europeans. On the other side of the coin, PAGES is currently seeking to broaden our support base beyond USA and Switzerland and participation in an EU framework proposal would be an ideal way to do this, given the strong representation of European scientists within the PAGES community. If, however, you have reason to believe that explicit inclusion of the PAGES office in the list of partner organizations would reduce the chance of success of such a proposal, then of course don't do it. Basically, I would much appreciate being kept in the loop with your plans and am happy to participate, and offer the help of PAGES, in any way I that you deem useful.

Keith

on 07/04/2003 08:08 PM, Rick Battarbee at r.battarbee@geog.ucl.ac.uk wrote:

> Dear all,
>
> We have just come to the end of a very rewarding and successful HOLIVAR
> training course here with a very good bunch of young scientists from across
> Europe all involved in some aspect of  high resolution Holocene change and
> embracing climate modelling, and climate reconstruction both from marine
> and continental records.  We shall be putting details on the HOLIVAR
> website soon.  (I should also say that Andy Lotter's workshop in April on
> age modelling was also very successful, and details are now on the web)
>
> I will produce a more detailed report on HOLIVAR activities and plans for
> the future shortly, and there should be plenty to discuss at our next
> Steering Committee meeting on October 3rd (please check your diaries -
> Innsbruck October 3rd).
>
> The main reason for writing, however, is to alert you to the probability of
> a call for proposals on climate change by the EU in FP6 for 2004, and the
> need for us to begin thinking again about an integrated project based on
> HOLIVAR.  If you remember Keith Briffa submitted on behalf of the HOLIVAR
> community an Expression of Interest called HOLCLIM that found much favour
> at the time with the EU.  Although I have not spoken at length with Keith
> about this I'm sure he is keen to see a project based on HOLCLIM taken
> forwards.
>
> Whilst we can not be sure of the detailed wording of the call I think it is
> nevertheless not too soon to begin designing the project  It would be very
> useful to have your thoughts on how to proceed so that we can prepare a
> document for discussion on October 3rd.  One issue is the potential overlap
> with DOCC.  Eystein, what is your view on this?  I'm sure there will be
> only one "palaeo" project funded and therefore if we simply followed the
> original intentions, HOLCLIM and DOCC would be in competition.  And putting
> the two together would be difficult, HOLCLIM is an IP, and DOCC a NoE and

> the research community potentially involved would be huge, especially in
> relation to the budget which may be no more than 10 million euros.
>
> Please let me have your views, and then I will get together with Keith and
> come up with some kind of proposed way forwards for the meeting in October.
>
> Best wishes to all,
>
> Rick
> Professor R.W. Battarbee
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> Tel. +44 (0)20 7679 7582, Fax +44 (0)20 7679 7565
> http://www.geog.ucl.ac.uk/ecrc/
>

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Tel (cell): +41 79 705 65 36
Fax: +41 31 312 31 68

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From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>, rls@email.unc.edu
Subject: More on Climate Research.....
Date: Fri, 11 Jul 2003 12:40:57 -0700
Cc: Tom Wigley <wigley@ucar.edu>, "Michael E. Mann" <mann@virginia.edu>, Mike Hulme <m.hulme@uea.ac.uk>

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 autcorrelation 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 reviwers 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
>
> 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
> >>>>--
> >>>>--------------------------------------------------
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> >>>>--------------------------------------------------

> >>>
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> >>                    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
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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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> UK
> ----------------------------------------------------------------------------
>
>   ------------------------------------------------------------------------------
>              Name: CR.txt
>    CR.txt    Type: Plain Text (text/plain)
>          Encoding: quoted-printable

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----------------------------------------------------------------------------
PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!

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Program for Climate Model Diagnosis and Intercomparison
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----------------------------------------------------------------------------

340. 1057944829.txt
####################################################################################
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Tom Crowley <tcrowley@duke.edu>
Subject: Re: Fwd: Re: Fwd: Re: Climate Research
Date: Fri Jul 11 13:33:49 2003

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)!
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
O18 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
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
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 an 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 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 the any of the four referees I used (but is curiously similar to points raided 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 paleoclimatolgist, 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
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 salesmen showing expected rate of return from a market
bottom - not precisely false, but rather in the realm of sleight-of-
hand.
Regards
Chris

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References

1. http://amavis.org/
2. http://www.cru.uea.ac.uk/~timo/
3. http://www.cru.uea.ac.uk/~timo/sunclock.htm

341. 1058275977.txt
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From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: revised NH comparison manuscript
Date: Tue, 15 Jul 2003 09:32:57 -0400

<x-flowed>
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 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
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>>University of Rhode Island
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>>phone: (401) 874-6599
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>>Narragansett, RI 02882
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>--
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>University of East Anglia
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>http://www.cru.uea.ac.uk/cru/people/briffa/
>
>Attachment converted: Macintosh HD:nhcomparison_v7_1.doc (WDBN/MSWD)
>(0008AC53)

--
==================================
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Email:  drdendro@ldeo.columbia.edu
Phone:  845-365-8618
Fax:    845-365-8152
==================================
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342. 1058898765.txt
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##########

From: "Michael E. Mann" <mann@virginia.edu>
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>
Subject: letter to Senate
Date: Tue, 22 Jul 2003 14:32:45 -0400

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
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml
Attachment Converted: "c:\eudora\attach\EOS.senate letter-final.doc"

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

343. 1058906971.txt
####################################################################################

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: letter to Senate
Date: Tue, 22 Jul 2003 16:49:31 -0700
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>, 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>

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

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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

Attachment converted: Macintosh HD:EOS.senate letter-final.doc (WDBN/MSWD) (00055FCF)

--

Jonathan T. Overpeck
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Professor, Department of Geosciences
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Tucson, AZ 85721
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fax: +1 520 792-8795
http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
http://www.ispe.arizona.edu/

344. 1059005592.txt
##########################################################################################

From: Tom Wigley <wigley@ucar.edu>
To: Michael Oppenheimer <omichael@Princeton.EDU>
Subject: Re: letter to Senate
Date: Wed, 23 Jul 2003 20:13:12 -0600
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>

<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 conciousness of people in Congress, and is given prominence in some publications emanating from that sector. The work appears to have the imprimateur 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.
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>>>>Thanks in advance,
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>>>>Michael M and Michael O
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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
>>>
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>>>>(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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>>>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/
>>

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345. 1059664704.txt
####################################################################################
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: reconstruction errors

Date: Thu, 31 Jul 2003 11:18:24 -0400

Tim,
Attached are the calibration residual series for experiments based on available networks
back to:
AD 1000
AD 1400
AD 1600
I can't find the one for the network back to 1820! But basically, you'll see that the
residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd
case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them
up. In any case, the incremental changes are modest after 1600--its pretty clear that key
predictors drop out before AD 1600, hence the redness of the residuals, and the notably
larger uncertainties farther back...
You only want to look at the first column (year) and second column (residual) of the files.
I can't even remember what the other columns are!
Let me know if that helps. Thanks,
mike
p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on
this, I'm providing these for your own personal use, since you're a trusted colleague. So
please don't pass this along to others without checking w/ me first. This is the sort of
"dirty laundry" one doesn't want to fall into the hands of those who might potentially try
to distort things...
At 02:58 PM 7/31/2003 +0100, you wrote:

Thanks for the explanation, Mike.  Now I see it, it looks familiar - so perhaps you've
explained it to me previously (if you have, then sorry for asking twice!).
I now understand how you compute them in theory.  I have two further questions though
(sorry):
(1) how do you compute them in practise?  Do you actually integrate the spectrum of the
residuals?
(2) how would I estimate an uncertainty for a particular band of time scales (e.g.
decadal to secular, f=0.0 to 0.1)?  If integrating the spectrum of the residuals, I
wonder whether integrating from f=0 to f=0.02 and then f=0.02 to (e.g.) f=0.1 (note this
last limit has changed) would give me the right error for time scales of 10 years and
longer (i.e. for a 10-yr low pass filter)?  The way I had planned to do this was to
assume the residuals could be modelled as a first order autoregressive process, with
lag-1 autocorrelation r1=0.0 after 1600 (essentially white) and r1=??? before 1600.  Do
you know what the lag-1 autocorrelation of the residuals is for the network that goes
back to 1000 AD?
The stuff back 2000 years will be interesting, though the GCM runs we're

starting to
look at go back only 500 (Hadley Centre) or 1000 (German groups), so MBH99 seems fine
for now.
Cheers
Tim
At 14:28 31/07/2003, you wrote:

Tim,
The one-sigma *total* uncertainty is determined from adding the low f and high f
components of uncertainty in quadrature. The low f and high f uncertainties aren't
uncertainties for a particular (e.g. 30 year or 40-year) running mean,they are band
integrated estimates of uncertainties (high-frequency band from f=0 to f=0.02,
low-frequency band from f=0.02 to f=0.5 cycle/year) taking into account the spectrum of
the residual variance (the broadband or "white noise" mean of which is the nominal
variance of the calibration residuals)
Alternatively, one could calculate uncertainties for a particular timescale average
using the standard deviation of the calibration residuals, and applying a square-root-N'
argument (where N' is the effective degrees of freedom in the calibration residuals). I
believed I did this at one point, and got similar results.
Let me know if this needs further clarification. Thanks,
mike
p.s. you might want to try to using Mann and Jones N. Hem if you're going back further
than AD 1000? Crowley has some EBM results now back to 0 AD, and is in the process of
comparing w/  that. SHould be interesting...
At 02:04 PM 7/31/2003 +0100, you wrote:

Hi Mike,
we've recently been making plans with Simon Tett at the Hadley Centre for comparing
model simulations with various climate reconstructions, including the MBH98 and MBH99
Northern Hemisphere temperatures.  I was stressing the importance of including
uncertainty estimates in the comparison and that the error estimates should depend on
the timescale (e.g. smoothing filter or running mean) that had been applied.
I then looked at the file that I have been using for the uncertainties associated with
MBH99 (see attachment), which I must have got from you some time ago.  Column 1 is year,
2 is the "raw" standard error, 3 is 2*SE.
But what are columns 4 and 5?  I've been plotting column 4, labelled "1 sig (lowf)" when
plotted your smoothed reconstruction, assuming that this is the error appropriate to
low-pass filtered data.  I'd also assumed that the last column "1 sig (highf)" was
appropriate to high-pass filtered data.  I also noticed that the sum of the squared high
and low errors equalled the square of the raw error, which is nice.
But I've realised that I don't understand how you estimate these errors, nor what time
scale the lowf and highf cutoff uses (maybe 40-year smoothed as in the IPCC

plots?).
From MBH99 it sounds like post-1600 you assume uncorrelated gaussian calibration
residuals. In which case you would expect the errors for a 40-year mean to be reduced
by sqrt(40). This doesn't seem to match the values in the attached file. Pre-1600 you
take into account that the residuals are autocorrelated (red noise rather than white),
so presumably the reduction is less than sqrt(40), but some factor (how do you compute
this?).
The reason for my questions is that I would like to (1) check whether I've been doing
the right thing in using column 4 of the attached file with your smoothed
reconstruction, and (2) I'd like to estimate the errors for a range of time scales, so I
can compare decadal means, 30-year means, 50-year means etc.
Thanks in advance for any help you can give me here.
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: [1]http://www.cru.uea.ac.uk/~timo/
sunclock: [2]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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sunclock: [5]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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[6]http://www.evsc.virginia.edu/faculty/people/mann.shtml
Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\nh-ad1000-resid.dat" Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\nh-ad1400-resid.dat" Attachment Converted:
"c:\documents and settings\tim osborn\my

documents\eudora\attach\nh-ad1600-resid.dat"

References

1. http://www.cru.uea.ac.uk/~timo/
2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.cru.uea.ac.uk/~timo/
5. http://www.cru.uea.ac.uk/~timo/sunclock.htm
6. http://www.evsc.virginia.edu/faculty/people/mann.shtml

346. 1059674663.txt
####################################################################################################

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: reconstruction errors
Date: Thu Jul 31 14:04:23 2003

Hi Mike,

we've recently been making plans with Simon Tett at the Hadley Centre for comparing model simulations with various climate reconstructions, including the MBH98 and MBH99 Northern Hemisphere temperatures. I was stressing the importance of including uncertainty estimates in the comparison and that the error estimates should depend on the timescale (e.g. smoothing filter or running mean) that had been applied.

I then looked at the file that I have been using for the uncertainties associated with MBH99 (see attachment), which I must have got from you some time ago. Column 1 is year, 2 is the "raw" standard error, 3 is 2*SE.

But what are columns 4 and 5? I've been plotting column 4, labelled "1 sig (lowf)" when plotted your smoothed reconstruction, assuming that this is the error appropriate to low-pass filtered data. I'd also assumed that the last column "1 sig (highf)" was appropriate to high-pass filtered data. I also noticed that the sum of the squared high and low errors equalled the square of the raw error, which is nice.

But I've realised that I don't understand how you estimate these errors, nor what time scale the lowf and highf cutoff uses (maybe 40-year smoothed as in the IPCC plots?). From MBH99 it sounds like post-1600 you assume uncorrelated gaussian calibration residuals. In which case you would expect the errors for a 40-year mean to be reduced by sqrt(40). This doesn't seem to match the values in the attached file. Pre-1600 you take into account that the residuals are autocorrelated (red noise rather than white), so presumably the reduction is less than sqrt(40), but some factor (how do you compute this?).

The reason for my questions is that I would like to (1) check whether I've been doing the right thing in using column 4 of the attached file with your smoothed reconstruction, and (2) I'd like to estimate the errors for a range of time scales, so I can compare decadal means, 30-year means, 50-year means etc.

Thanks in advance for any help you can give me here.

Tim

347. 1059762275.txt
####################################################################################################

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: reconstruction errors
Date: Fri Aug  1 14:24:35 2003

Thanks very much for helping me out with this Mike.  Rest assured that the data won't be passed on to anyone else.  I'll let you know if I use them to compute uncertainties at different time scales.
Cheers
Tim
At 16:18 31/07/2003, you wrote:

Tim,
Attached are the calibration residual series for experiments based on available networks back to:
AD 1000
AD 1400
AD 1600
I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...
You only want to look at the first column (year) and second column (residual) of the files. I can't even remember what the other columns are!
Let me know if that helps. Thanks,
mike
p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

348. 1060002347.txt
############################################################################################

From: "Michael E. Mann" <mann@virginia.edu>
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>
Subject: RE: Recent climate sceptic research and the journal Climate Research
Date: Mon, 04 Aug 2003 09:05:47 -0400
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,

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 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
>>>>  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
>
********************************************
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,
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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
[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"

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Simon Tett <simon.tett@metoffice.com>,Keith Briffa <k.briffa@uea.ac.uk>, Philip Brohan <philip.brohan@metoffice.com>
Subject: Re: Uncertainty in model-paleo uncertainty
Date: Mon, 04 Aug 2003 14:30:35 +0100

<x-flowed>
Simon & Philip,

here's some thoughts on uncertainty...

At 10:42 04/08/2003, Simon Tett wrote:
>1) Calibration uncertainty -- there is some uncertainty in the
>relationship between proxy and temperature.
>2) Residual noise -- the proxyies do not capture large-scale temperature
>variability perfectly.
>3) Internal-climate variability in "real" life -- there is some chaotic
>variability in the real climate system

>4) Internal-climate variability in the model -- ditto!
>
>3) & 4) I suggest we estimate from HadCM3 -- model var agrees well with
>paleo var so can't be too far wrong!

Yes, I'm happy that we use (3) and (4) from the model. If you use a short baseline to take the anomalies from, then the internal variability comes in twice in each case, both in comparing the baseline mean and the anomaly. We can minimise this by using a long baseline.

>1) & 2) are, to some extent related, as calibration is estimate by
>regression -- thus minimising residual var (2). Nicest thing to do would
>be to estimate residual from indep. data but I don't think there is enough.....

The uncertainties that we've published with our regional and quasi-hemispheric reconstructions attempt to take both (1) and (2) in account already. Thus I use the standard errors on the two regression coefficients (for the linear regression of the sub-continental regions) and the standard errors on all multiple regression coefficients (for the quasi-Northern Hemisphere series). And then I incorporate the variance of the calibration residuals too (i.e., item (2)), modelled as first-order autoregressive terms. The appendix of the Briffa part 1 paper (page 755-757 is the appendix) in the Holocene special issue paper gives an explanation of this. Others quite often ignore (1) and just use the residuals to quantify reconstruction error, but (1) can be important especially for big anomalies (because the regression slope error is multiplied by the predicted anomaly). (1) can be difficult to quantify, of course, using some multi-variate techniques like Mann and Luterbacher use.

The regression standard errors (1) are of course computed from the calibration period. Our published errors also use the residual variance (2) computed from this calibration period. It is possible to compute (2) from independent data, but as you say we are limited by data. AND I think that the residual variance from independent data would also incorporate some or all of error (1) (because that would contribute to differences between reconstruction and observation). I think it is better to keep the two terms separate and explicitly compute both, especially as their relative magnitudes can depend upon time scale (i.e., time averaging the data).

Am I right in thinking that the error in the *observed* record would, if taken into account, result in *reduced* reconstruction errors, because the residual variance (2) would not all be assumed to be reconstruction error - some would be observation error? But I suppose that the regression coefficient errors (1) would get larger to compensate? Anyway, we don't currently consider observed errors.

Cheers

Tim

Dr Timothy J Osborn
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School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK

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web: http://www.cru.uea.ac.uk/~timo/
sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm

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From: "Stephan Singer" <SSinger@wwfepo.org>
To: <grassl@dkrz.de>,<klaus.hasselmann@dkrz.de>, <per.carstedt@ecosystem.se>, <mueller@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>
Subject: economic costs of european heat wave
Date: Wed, 06 Aug 2003 15:06:03 +0200
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>, <liam@wwfthai.org>, "Katherine Silverthorne" <Katherine.Silverthorne@WWFUS.ORG>, "Lara Hansen" <Lara.Hansen@WWFUS.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
**************************************************

**************************************************
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36 avenue de Tervuren Box 12
1040 Brussels, Belgium
Tel: +32-2-743-8817
Fax: +32-2-743-8819

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From: Tom Wigley <wigley@ucar.edu>
To: André Berger <berger@astr.ucl.ac.be>
Subject: Re: FW: Shaviv & Veizer in GSA Today
Date: Tue, 19 Aug 2003 09:00:33 -0600
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@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>, 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>

<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 Collegues,
>

> I admire the courage of Stefan and of all other colleagues who are
> willing to answer these highly controversed 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
> paper by Soon and Baliunas that denies globl 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
>> 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>
>> 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
>>
>>
> ***************************************************************************
> 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/>
> ***************************************************************************
>

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>,Tom Wigley <wigley@ucar.edu>, Tom Crowley <tcrowley@duke.edu>
Subject: Re: POLL ON SOON-BALIUNAS
Date: Tue, 19 Aug 2003 09:48:05 +0100
Cc: Keith Briffa <k.briffa@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>,Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>,Caspar Ammann <ammann@ucar.edu>, hegerl@duke.edu,mann@virginia.edu

Tom,
I once met Soon at a meeting organised by the ESA in Tenerife.  I think he gave a talk
-
but only think, so it wasn't memorable in any way. As you say they don't come to the
regular meetings like EGU/S, AGU, AMS etc.  I only went to Tenerife as the organisers paid
for me to go.
Citation ratings vary (there are several different scales/indicators as well) a lot
from year to year for most journals. I've never figured out how the counting is done wrt
the highly cited lists that Tom. W., Kevin and I are on.  Do only first authorships count
for
example?  Even with a common name like mine people still get it wrong and mistakes
persist.
Surprisingly Jim Hansen doesn't make the above list ([1]http://www.highlycited.com), but
then

he normally drops his E.
There are few more journals (QSR, Climate Change, IJC, AAR to give a few) where
paleo papers also appear.
Cheers
Phil

At 10:43 13/08/2003 -0400, Michael E. Mann wrote:

I checked this out prior to my senate hearing. Their science citations in the climate
literature are poor, as one would hope and expect.
Interestingly, they both drop their second initials when publishing in the climate
literature so that their names don't turn in up in ISI if you do a search on their
publications in the  astronomy literature (which use the full initials)--apparently,
they don't want their astronomy colleagues to be aware that they're moonlighting as
supposed climatologists...
Their numbers are better in the astronomy literature, though Soon's numbers even here
are mediocre.
Baliunas had some well-cited publications more than a decade ago. This is her work on
the use of sun-like stars as a model for solar variability, etc., which is well
referenced in the astrophysics community. However, most of these appear to be her Ph.D.
work, and appear to have been published w/ her Ph.D adviser.
Not much evidence however that she has made any useful, independent contribution since
then. There are some additional  papers she's published on time series analysis of solar
signals--looks like the kind of stuff you might expect to see from a graduate student
first-year research project....
In my opinion, its would be a mistake to evaluate these  on their citations numbers in
astronomy. We should focus on their numbers in the climate literature, which are the
only ones relevant when discussing the issue of how their work on climate is received by
their fellow scientists,
mike
At 08:15 AM 8/13/2003 -0600, Tom Wigley wrote:

Might be interesting to see how frequently Soon and Baliunas, individually, are cited
(as astronomers). Are they any good in their own fields?
Perhaps we could start referring to them as astrologers (excusable as ... 'oops, just a
typo')
Tom.
++++++++++++++++
Tom Crowley wrote:

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 or 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 there 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

______________________________________________________________
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References

1. http://www.highlycited.com/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml

353. 1061625894.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [Fwd: VS: [Climate Sceptics] Mann & Jones on 1800 yrs  proxies]

Date: Sat, 23 Aug 2003 04:04:54 -0400
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@ltrr.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

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. Bednarz 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.
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 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, its 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
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@yahoogroups.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" <wsoon@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
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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[2]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: reconstruction uncertainties
Date: Fri Aug 29 16:33:55 2003
Cc: k.briffa@uea.ac.uk
Attachments: Mann uncertainty.doc

Hi Mike,

after a few bits of holiday here and there, I've now had time to complete my (initial) approach to estimating reconstruction errors on your NH temperature reconstruction. This is all based on the calibration residuals that you kindly sent me a few weeks ago.

My rationale for doing this was that I wanted uncertainty/error estimates that were dependent on the time scale being considered (e.g. a decadal mean, an annual mean, a 30-year mean, etc.). I didn't think you had published timescale-dependent errors, hence my attempt.

A second reason is that I wanted to be able to model (i.e., stochastically generate) time series of the errors, with appropriate timescale characteristics. Again, I didn't think that I could get this from your published results.

The attached document summarises the progress I've made. There are a few questions I have, and I'm concerned that the reduction in uncertainty with increasing time scale is too great. Perhaps one should be ultra conservative and have no reduction with time scale? Yet surely there ought to be some cancelling of partly uncorrelated errors? The document is not meant to form part of any paper on this (I hope to use the errors in a paper, but the point of the paper is on trend detection, not estimating errors), it just seemed appropriate to write it up like this to inform you of what I've done so far.

Any comments or criticisms will be very useful.

Cheers

Tim

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From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: reconstruction uncertainties
Date: Tue, 02 Sep 2003 14:30:48 -0400
Cc: Scott Rutherford <srutherford@gso.uri.edu>, mann@virginia.edu

Hi Tim
Thanks for sending this. Unfortunately, I don't really have the time look into any of this
in detail, but let me offer the following additional explanation which will hopefully
clarify the nature of any differences between our results. I fear that I may not have been
clear enough in my previous explanation.
The reason that our uncertainty estimates reduce little fwith increasing

timescale for the
   earlier networks is that the effective degrees of freedom are diminished sharply by the
   redness of the calibration residuals for networks prior to AD 1600 and earlier. But unlike
   you, wee do not model the residuals as an AR process--this may the source of some of the
   differences.
   Back to AD 1600 (and later networks), the calibration residuals pass for "white noise" ,
   and the estimates follow simply from the residual uncalibrated variance, and the reduction
   of variance upon averaging follows standard sqrt(N) statistics.
   Prior to that, the networks failed the test. So we decomposed the calibration residuals
   into a "low-frequency" band (all timescales longer than 40 years which are not
   distinguishable from secular timescales, since I had a roughly 80 years series and was
   evaluating the spectrum using a multiple-taper estimate with a spectral bandwidth of +/-2
   Rayleigh frequencies). We then estimated the enhancement of unresolved variance in the
   low-frequency band relative to the nominal white noise level. The enhancement was about a
   factor of 5-6 or so for the earlier networks, as I recall. To get the component of
   uncertainty for the low-frequency band alone (timescales longer than 40 years), I simply
   took that enhancement factor x the nominal unresolved calibration variance x the bandwidth
   of the "low-frequency" band (0.025 cycle/year). This yields a reduction in variance that is
   far less than the nominal "sqrt N" reduction applied to the individual annual
   uncertainties. Of course,  one could calculate the equivalent N' (effective temporal
   degrees of freedom) that this implies in a model of the residuals as AR(1) red noise,  but
   we didn't take this approach. We modeled it as a simple step-increase spectrum (w/ the
   boundary at f=0.025 cycle/yr). Modeling the residuals as red noise would, my guess is,
   generally yield the same result, but it might have the effect of dampening the estimated
   enhancement of unresolved variance at the longest timescales. In any case, it should yield
   similar, but it would be very surprising if identical(!), results, consistent w/ your
   observations.
   My guess for the difference in the AD 1600 network is that, based on the spectrum test, we
   did not reject the white noise null hypothesis for the residuals. So there was no variance
   enhancement factor for that, or subsequent, networks. It would appear that your method
   argues for significant serial correlation in that case. Not sure why we come to different
   conclusions in this case (perhaps using different criteria for testing for the significance
   of redness in the spectrum/serial correlation), but that's probably the reason...
   I hope that clarifies this. Please keep me in the loop on this. I've copied to Scott, who
   may have some additional insights here, since we've been dealing w/ these issues now in the

RegEM estimates (Scott:did we ever reject the white noise null hypothesis in the residuals
for any of our proxy-based NH reconstrucitions in the paper submited to J. Climate? I don't
recall).
Thanks,
mike
At 04:33 PM 8/29/2003 +0100, you wrote:

Hi Mike,
after a few bits of holiday here and there, I've now had time to complete my (initial)
approach to estimating reconstruction errors on your NH temperature reconstruction.
This is all based on the calibration residuals that you kindly sent me a few weeks ago.
My rationale for doing this was that I wanted uncertainty/error estimates that were
dependent on the time scale being considered (e.g. a decadal mean, an annual mean, a
30-year mean, etc.).  I didn't think you had published timescale-dependent errors, hence
my attempt.
A second reason is that I wanted to be able to model (i.e., stochastically generate)
time series of the errors, with appropriate timescale characteristics.  Again, I didn't
think that I could get this from your published results.
The attached document summarises the progress I've made.  There are a few questions I
have, and I'm concerned that the reduction in uncertainty with increasing time scale is
too great.  Perhaps one should be ultra conservative and have no reduction with time
scale?  Yet surely there ought to be some cancelling of partly uncorrelated errors?  The
document is not meant to form part of any paper on this (I hope to use the errors in a
paper, but the point of the paper is on trend detection, not estimating errors), it just
seemed appropriate to write it up like this to inform you of what I've done so far.
Any comments or criticisms will be very useful.
Cheers
Tim
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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

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\Mann uncertainty.doc"

References

1. http://www.cru.uea.ac.uk/~timo/
2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: An idea to pass by you
Date: Wed, 3 Sep 2003 08:32:11 -0400

<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 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
--
==================================
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Phone:  845-365-8618
Fax:    845-365-8152
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Keith Briffa <k.briffa@uea.ac.uk>, Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Fwd: Re: Fwd: Soon & Baliunas
Date: Wed Sep  3 15:54:41 2003

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 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 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.
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 accuarately 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
> >  synchroneity 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
> >>
> >>
> >>
> >>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
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> >>7TJ        | sunclock: UK                        |
> >>[2]http://www.cru.uea.ac.uk/~timo/sunclock.htm
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> >--------------------------------------------------------------------
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> >
> >
> >Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
> >/CARO) (00016021)
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> --
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2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

358. 1062783293.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Something for the weekend !
Date: Fri, 05 Sep 2003 13:34:53 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>, mann@virginia.edu

sorry phil, one more relevant item. I've cc'd in Keith on this, since you had mentioned
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 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

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References

1. http://www.ltrr.arizona.edu/~dmeko/notes_8.pdf
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359. 1062784268.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Something for the weekend !
Date: Fri, 05 Sep 2003 13:51:08 -0400
Cc: Keith Briffa <k.briffa@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:

sorry phil, one more relevant item. I've cc'd in Keith on this, since you had mentioned
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3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Phil Jones <p.jones@uea.ac.uk>
To: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk,simon.tett@metoffice.com, peter.thorne@metoffice.com,chris.folland@metoffice.com, david.parker@metoffice.com

Subject: Fwd: rural/urban paper
Date: Mon, 15 Sep 2003 16:19:49 +0100

<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

>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>

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From: Irina Fast <f14@zedat.fu-berlin.de>
To: Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: COLD season T reconstruction
Date: Tue, 30 Sep 2003 14:24:57 +0200
Reply-to: f14@zedat.fu-berlin.de

Hi Tim, hi  Keith,

attached you can find my reconstruction of the cold season temperature anomalies. I have retained the 3rd, 4th, 5th and 6th EOFs for the whole time span (1500-1976). It seems to be a rather strange choice, but if I retain the 1st and/or 2nd EOFs the reconstructed T anomalies for Northern Europe are too large in comparison to observed anomalies.
You will see that calibration/verification skills are miserable. But it puts my mind to rest, if you say, that this is an expected result.

Last week you wrote :
>Please let us (me and Keith) know if you are happy with your implementation
>of the Mann et al. method.  I remember that you had some strange results
>when you applied it to the model simulations - did you solve those
>problems?  We might be able to help or provide advice if you still have
>problems with the method.
The problems I mentioned at the meeting in France arose if I applied my implementation of the method to the INSTRUMENTAL data and I tried to explain this effect through the gaps in the data. In the meantime I was able to eliminate to some degree this problem through the use of other fortran compiler and numeric library. I will prepare an slide with assesment of the performance of the current method implementation for "perfect proxy data" (i.e. instrumental data as proxy data).

And now some words to agenda
1) Antje Weisheimer will say initial greeting words and make all organisational announcments.
2) As you know, Ulrich take part in the analysis of the simulations  performed with ECHO-G by GKSS group. I am not sure, but maybe he will also present his ideas for further (in framework of SO&P reasonable) simulations, that can be conducted by FUB.

For the presentations both OHP and data projector are available.

Best redards
Irina
--
________________________________________________________
Irina Fast
Freie Universität Berlin
Institut für Meteorologie
Carl-Heinrich-Becker-Weg 6-10
D-12165 Berlin
Germany

phone:  +49 (0)30 838 712 21   fax: +49 (0)30 838 711 60
e-mail: f14@zedat.fu-berlin.de

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\rectemp_October-March1.dat"

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\rectemp_regave_October-March1.dat"

362. 1065125462.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: "Robert Matthews" <r.matthews@physics.org>
Subject: Re:

Date: Thu, 02 Oct 2003 16:11:02 -0400
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

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 corportation, 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:

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When the paper came out, some critics argued that the paper actually showed that there
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[1]http://www.co2science.org/journal/2003/v6n34c4.htm)

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Robert Matthews
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Robert Matthews
Science Correspondent, The Sunday Telegraph
C/o:  47 Victoria Road, Oxford, OX2 7QF
Email: [2]r.matthews@physics.org
Homepage: [3]www.ncrg.aston.ac.uk/People/
Tel: (+44)(0)1865 514 004 / Mob: 0790-651 9126
------------------------------------------------------------------------

______________________________________________________________
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

References

1. http://www.co2science.org/journal/2003/v6n34c4.htm
2. mailto:r.matthews@physics.org
3. http://www.ncrg.aston.ac.uk/People/
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: "Michael E. Mann" <mann@virginia.edu>
To: 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
Subject: Re:
Date: Thu, 02 Oct 2003 17:03:15 -0400

For those of you who haven't seen it, this is Robert Matthews last article on the topic.
Hence the fairly brusque tone taken...
mike
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 Nation's 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 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.



At 04:11 PM 10/2/2003 -0400, 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
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The comparisons made in our  paper are well explained therein, and your statements belie
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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 corportation, 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
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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

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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
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[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
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With many thanks for your patience with this
Robert Matthews

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Robert Matthews
Science Correspondent, The Sunday Telegraph
C/o:  47 Victoria Road, Oxford, OX2 7QF
Email: [2]r.matthews@physics.org
Homepage: [3]www.ncrg.aston.ac.uk/People/
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e-mail: mann@virginia.edu   Phone: (434) 924-7770   FAX: (434) 982-2137
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References

1. http://www.co2science.org/journal/2003/v6n34c4.htm
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3. http://www.ncrg.aston.ac.uk/People/
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, "Robert Matthews" <r.matthews@physics.org>
Subject: Re: Mann and Jones, climate of the last two millennia
Date: Fri Oct  3 09:56:06 2003
Cc: 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

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.
Regards
Tim
At 21:11 02/10/2003, Michael E. Mann wrote:

Dear Mr. Matthews,
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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.
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With many thanks for your patience with this
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References

1. http://www.co2science.org/journal/2003/v6n34c4.htm

365. 1065206624.txt
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Mann and Jones, climate of the last two millennia
Date: Fri Oct 3 14:43:44 2003

Hi Mike,
I agree completely with your analysis. I don't get so many requests as you, but even so
get enough to mean that I ignore most - I just pick a few at random to respond to. As Phil
is away, I picked this. He's already come back with a second request, which I answered,
but that's all he'll get from me. I'll
At 13:56 03/10/2003, you wrote:

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 dried to bog Ben

Santer
down with 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:

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Dr Timothy J Osborn
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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
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
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References

1. http://www.cru.uea.ac.uk/~timo/
2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

366. 1065636937.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: Fwd: EOS: Soon et al reply
Date: Wed, 08 Oct 2003 14:15:37 -0400
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, 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, p.jones@uea.ac.uk

Thanks Tom,
In fact, I'm almost done with a brief (<750 word) response that addresses all of these
issues, and I'll be looking forward to comments on this. Hope to send it out later today,
mike
At 12:05 PM 10/8/2003 -0600, Tom Wigley wrote:

Folks,
I agree with Kevin that any response should be brief.
On the second page of their comment, SBL quote some of the caveat statements in their
earlier papers. The irony is that they do not heed their own caveats. If taken
literally, all these proxy data problems would mean that one can draw no conclusions
about the existence or otherwise of the MWE or LIA as global phenomena. This is what we
say (I hope -- at least I have said this in the paper cited below) -- but our over-bold

skeptics say that these anomalous intervals *did* exist. You can't have it both ways --
and basically what BS are doing is a confidence trick.
What is still needed here is an analysis of the BS method to show that it could be used
to prove anything they wanted.
I am still concerned about 'our' dependence on treerings. Are our results really
dependent on one region pre 1400 as SNL state? Is the problem of nonclimate obfuscating
factors in the 20th century enough to screw up calibrations on moderate to long
timescales? If not, we need to state and document this clearly. Does this problem apply
to both widths and densities? Are the borehole data largely garbage? I recall a paper of
Mike's on this issue that I refereed last year -- and there was something in GRL (I
think) very recently pointing out some serious potential problems.
Finally, did we really say what SBL claim we did in their p. 1 point (2)? Surely the
primary motive for all of this paleo work is that it DOES have a bearing on
human-induced climate effects?
Tom.
+++++++++++++++++++++++++++++++++++++++++++++==
Michael E. Mann wrote:

Thanks Kevin,
I agree w/ your take on this. We need to come up with a short, but powerful rebuttal.
According to Judy Jacobs, we're only allowed 750 words, so we will need to be even more
sparing and precise in our words that in the original Eos piece. By the way, we have 3
weeks to submit (i.e., our response is due October 27).
We need to focus on the key new claims, while simply dismissing, by reference to earlier
writings, the recycled ones. The Kalnay et al paper seems to be the new darling of the
contrarians, and you're precise wording on this  will be very helpful. Phil, Tim and
others should be able to put to rest, in one or two sentences, the myths about urban
heat bias on the CRU record. A few words from Malcolm and Keith on the biological tree
growth effects would help too. The comments on the various paleo figures are confusing
and inconsistent, but from what I can tell, just plain wrong. I'll draft some words on
that.
I'll just continue to assimilate info and suggestions from everyone over the next week
or so, and then try to put this in the form a rough draft rebuttal to send out.
Thanks for your quick reply. Looking forward to hearing back from others,
mike
At 09:16 AM 10/6/2003 -0600, Kevin Trenberth wrote:

Hi Mike et al
Firstly, you should know that comments by myself and the group at NCDC (Vose et al) on
the Kalnay and Cai Nature paper were accepted (after a rebuttal and review process), and
then fine tuned.  But it is a slow process and Kalnay and Cai have yet to finalize their

rebuttal.  I am attaching FYI the "final" version of my comment.  NCDC deals with the
problems with the records.
My reaction to the reply is as follows:
The first page deals with comments on proxy records and their problems.  I think we
should agree that there are issues with proxy records, they are not the same as
instrumental records (which have their own problems), but they are all we have.
However, some are better than others (e.g. borehole) and annual or better resolution is
highly desirable in particular to make sure that anomalies are synchronous.  The records
are not really the issue here, it is there use (and abuse).
There are several charges about only US or Northern Europe that can be quickly dealt
with.  However the main points are on p 2.
We know from the observational record that global or hemispheric means are typically
small residuals of large anomalies of opposite signs so that large warm spots occur
simultaneously with large cold regions (witness last winter).
This fact means that we need high temporal resolution (annual or better) AND an ability
to compute hemispheric averages based on a network.  The Soon and Baliunas approach
fails dismally on both of these critical points.
BS point out that Fig 2 of Mann and Jones show some temperatures as high as those in the
20th C.  (They are wrong, do they mean Fig 2 of
M03?)   You can counter that by looking at China where this is far from true.
I would be inclined to respond with a fairly short minimalist but powerful rebuttal,
focussing mostly on the shortcomings of BS and not defending the M03 and other records.
It should point out (again) that their methodolgy is fundamentally flawed and their
conclusions are demonstrably wrong.  For this, the shorter the better.
Regards
Kevin
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
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> <[1]mailto:ammann@ucar.edu>, rbradley@geo.umass.edu
<[2]mailto:rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>
<[3]mailto:k.briffa@uea.ac.uk>, Tom Crowley, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>
<[4]mailto:mhughes@ltrr.arizona.edu>, omichael@princeton.edu
<[5]mailto:omichael@princeton.edu>, Tim Osborn <t.osborn@uea.ac.uk>
<[6]mailto:t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>
<[7]mailto:jto@u.arizona.edu>, Scott Rutherford <srutherford@rwu.edu>
<[8]mailto:srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>
<[9]mailto:trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>
<[10]mailto:wigley@ucar.edu>
From: "Michael E. Mann" <mann@virginia.edu> <[11]mailto:mann@virginia.edu>
Subject: Fwd: EOS: Soon et al reply
Comments?
Mike

Delivered-To: mem6u@virginia.edu <[12]mailto:mem6u@virginia.edu>
Date: Sat, 04 Oct 2003 12:33:04 -0400
From: Ellen Mosley-Thompson <thompson.4@osu.edu> <[13]mailto:thompson.4@osu.edu>
Subject: EOS: Soon et al reply
X-Sender: ethompso@pop.service.ohio-state.edu
<[14]mailto:ethompso@pop.service.ohio-state.edu>
To: "Michael E. Mann" <mann@virginia.edu> <[15]mailto:mann@virginia.edu>
Cc: lzirkel@agu.edu <[16]mailto:lzirkel@agu.edu>, jjacobs@agu.org
<[17]mailto: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 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

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References

1. mailto:ammann@ucar.edu
2. mailto:rbradley@geo.umass.edu
3. mailto:k.briffa@uea.ac.uk
4. mailto:mhughes@ltrr.arizona.edu
5. mailto:omichael@princeton.edu
6. mailto:t.osborn@uea.ac.uk
7. mailto:jto@u.arizona.edu
8. mailto:srutherford@rwu.edu
9. mailto:trenbert@cgd.ucar.edu
10. mailto:wigley@ucar.edu
11. mailto:mann@virginia.edu
12. mailto:mem6u@virginia.edu
13. mailto:thompson.4@osu.edu
14. mailto:ethompso@pop.service.ohio-state.edu
15. mailto:mann@virginia.edu
16. mailto:lzirkel@agu.edu
17. mailto:jjacobs@agu.org
18. mailto:mann@virginia.edu
19. http://www.evsc.virginia.edu/faculty/people/mann.shtml
20. mailto:mann@virginia.edu
21. http://www.evsc.virginia.edu/faculty/people/mann.shtml
22. mailto:trenbert@ucar.edu
23. http://www.cgd.ucar.edu/cas/
24. http://www.cgd.ucar.edu/cas/
25. http://www.evsc.virginia.edu/faculty/people/mann.shtml
26. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tom Crowley <tcrowley@duke.edu>
Subject: Re: draft
Date: Thu, 09 Oct 2003 14:16:31 -0400
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, 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

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 that 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 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
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

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--

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[3]http://www.evsc.virginia.edu/faculty/people/mann.[4]shtml

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

368. 1065785323.txt
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From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Jan Esper <esper@wsl.ch>
Subject: Re: data again
Date: Fri, 10 Oct 2003 07:28:43 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<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 *.rwl files
>>for the 27 western chronologies usedin Mann, Bradley, Hughes 1998 at the
>>following web location:
>>http://www.ltrr.arizona.edu/~fenbiao/For_Jan_27rwl/
>>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 theraw data  (the *.rwl 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
>>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 *rwl 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 presevation 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
>>*.rwl for each of the four chronologies. Gfo is Giant Forest, CSX is
>>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-karakorrum;
>>> >(2) 20 individual sites ranging from the lower to upper local
>>> >timber-lines
>>> 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
>>>  >Laboratory of Tree-Ring Research
>>>  >University of Arizona
>>>  >Tucson, AZ 85721
>>>  >520-621-6470
>>>  >fax 520-621-8229
>>>
>>>
>>>  --
>>>  Dr. Jan Esper
>>>  Swiss Federal Research Institute WSL
>>>  Zuercherstrasse 111, 8903 Birmensdorf
>>>  Switzerland
>>>  Phone: +41-1-739 2510
>>>  Fax:     +41-1-739 2215
>>>  Email:   esper@wsl.ch
>>>
>>>  ----- End forwarded message -----
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>>----- End forwarded message -----
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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
>>   esper\csxars.crn D:\Projects\SEQUOIA\for esper\gfo.rwl
>>   D:\Projects\SEQUOIA\for esper\gfoars.crn D:\Projects\SEQUOIA\for
>>   esper\mhf1.rwl D:\Projects\SEQUOIA\for esper\mhf2.rwl
>>   D:\Projects\SEQUOIA\for esper\MHF2ARS.CRN D:\Projects\SEQUOIA\for
>>   esper\MHF1ARS.CRN
>>------- End of forwarded message -------Malcolm
>>Hughes
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>>University of Arizona
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>--
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>Email:   esper@wsl.ch

--
==================================
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>

369. 1066073000.txt
####################################################################################
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: draft
Date: Mon Oct 13 15:23:20 2003
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>

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 -
   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

370. 1066075033.txt
##########################################################################################

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.osborn@uea.ac.uk
Subject: Fwd: minor explosion
Date: Mon Oct 13 15:57:13 2003

     X-Sender: esper@mail.wsl.ch

Date: Mon, 13 Oct 2003 15:21:03 +0200
To: Keith Briffa <k.briffa@uea.ac.uk>
From: Jan Esper <esper@wsl.ch>
Subject: minor explosion
Cc: Wilson Rob <rjwilson_dendro@blueyonder.co.uk>
Hi Keith
thank you for the message and the comments to the Siberia draft. We are intending to
finalize a draft when Rob is coming over and we go on a sampling trip to the Bavarian
Forest and E-Germany. We will then also discuss of data-overlap issue again and might
include some extra figure with our record re-calculated (without Tornetraesk and Polar
Ural).
However, I (Jan) an not sure that we should have another figure with only the Mann and
the (reduced) Esper series. Second, it seems that Mann used the density records from
these two sites only (not ring width). Lets see.
We would really like to send you the final draft, and ask you to become the fourth
author? We ask this not only because of the "minor explosion" that might happen, but
also because some of the arguments in the draft were made earlier by you anyway. What do
you think?
Take care
Jan and Dave
CC
R Wilson

Jan
with respect to the overlap problem we could agree to differ for now -I think the
problem is much more in the earlier period anyway but I suggest you go ahead and submit
it anyway. There are some minor wording points but nothing that affects the meaning. You
know that in my opinion the recent similarity in the records is driven by instrumental
data inclusion (or calibration against instrumental data) and that Mann's earlier data
are strongly biased towards summer and northern land signals. I think you will start a
minor explosion - but that is what science needs .
I looked at your tree-line data and thought them very interesting. In my opinion the way
you directed the interpretation was what drew your criticisms . For a climate journal
you should have been pointing out the complicated regional responses (to the temperature
record) rather than trying to state a simple overall response. The data are clearly
important and you should have no trouble publishing them if you rethink the approach to
the description (no work needed). I think Boreas or Arctic and Alpine Res. are better
targets though. I enjoyed  the discussions also and it is frustrating not to be able to
get up to speed with your other projects. I will get back to you when I have looked more
at the idea of the big review paper.

the very best to you and all
Keith
At 09:55 AM 10/8/03 +0200, Jan Esper wrote:

Hi Keith
with respect to our EOS draft, I am still thinking about the data overlap argument you
made.
1. I still believe that the overlap is not that significant, and that the significance
is changing dramatically with time (less in more recent centuries).
2. With respect to the aim of the paper, we do NOT intend to explain the similarity
between the records. We rather address that the recons differ in the lower frequency
domains AND are much more similar in the higher frequency domains. I believe that this
is crucial. (One could also say that we only address the dissimilarity, and the
arguments related to that.)
I appreciated the discussions we had very, very much (especially the one in the night
before the official meeting).
Take care
Jan
CC
D Frank
R Wilson
--
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Phone: +44-1603-593909
Fax: +44-1603-507784
[1]http://www.cru.uea.ac.uk/cru/people/briffa/

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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

371. 1066077412.txt
##########################################################################################

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: draft
Date: Mon Oct 13 16:36:52 2003
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

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
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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Boulder, CO 80307                               (303) 497 1333 (fax)

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References

1. mailto:mann@virginia.edu%A0
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. mailto:trenbert@ucar.edu
4. http://www.cgd.ucar.edu/cas/
5. http://www.cru.uea.ac.uk/cru/people/briffa/
6. http://www.cru.uea.ac.uk/cru/people/briffa/

372. 1066149334.txt
##########################################################################################

From: "Michael E. Mann" <mann@virginia.edu>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: Fwd: Re: draft
Date: Tue, 14 Oct 2003 12:35:34 -0400
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>

thanks Caspar,
I agree--its important to emphasize this point, and I'm glad you recognized that we were
underplaying it...
mike
At 10:25 AM 10/14/2003 -0600, Caspar Ammann wrote:

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 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 ....
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
lowpass
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.

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--

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--
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References

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4. mailto:mann@virginia.edu
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml
6. mailto:mann@virginia.edu
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8. mailto:ammann@ucar.edu
9. mailto:mann@virginia.edu
10. http://www.evsc.virginia.edu/faculty/people/mann.shtml
11. mailto:ammann@ucar.edu
12. http://www.evsc.virginia.edu/faculty/people/mann.shtml

373. 1066166844.txt
####################################################################################
##########

From: "Michael E. Mann" <mann@virginia.edu>
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
Subject: Fwd: Re: smoothing
Date: Tue, 14 Oct 2003 17:27:24 -0400

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
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

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[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: "Michael E. Mann" <mann@virginia.edu>
To: Malcolm Hughes <mhughes@ltrr.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>
Subject: Fwd: Correspondence on Harvard Crimson coverage of Soon / Baliunas views on climate
Date: Thu, 16 Oct 2003 16:43:41 -0400

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
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, mlm@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:
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
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,
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.
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 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
--------------------------------------------------------------------------------
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[4]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.techcentralstation.com/
2. mailto:john_holdren@harvard.edu
3. mailto:john_holdren@harvard.edu
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: evelyn.smith@noaa.gov, "Christopher D Miller" <Christopher.D.Miller@noaa.gov>
Subject: Fwd: confidential assessment of GC04-203
Date: Fri Oct 24 10:20:33 2003

Dear Evelyn and Chris,
re. proposal review GC04-203, Meko et al. "A synthesis of 19th century climate data for the
United States from paleo, archival and instrumental sources".
I have read the "Reviewer conflict of interest and confidentiality..." document and can
state that I have no conflict of interest and will abide by the confidentiality provisions
etc.
I reviewed a very similar proposal by this group 1 year ago, and enclose my review of that
proposal below. The new proposal has taken into account my two main concerns from last
time, which were:
(i) that creation only of a blended data set that contained a time varying mixture of proxy
and instrumental data would limit the usefulness because its quality would be time varying,
perhaps in an unquantified way, and independent study of errors between proxy and observed
data would be prevented; and
(ii) that the proposed work was not very innovative in terms of the applications for which
the new information would be used.
Both of these points have been addressed adequately and so I now rate it "Excellent (5)"
for scientific/technical merit, and "High (5)" for importance/relevance and applicability.
One issue that I would like to raise, however, is that the need for quantifying
uncertainty/error in the reconstructions/database is not given much coverage in the
proposal. It is mentioned, but not focused on. For many applications (testing models,
comparison with other reconstructions, detection of unusual climate trends/events),
explicitly quantified error estimates are essential. These often change magnitude through
time, and thus should be estimated in such a way as to allow this. They may also change

with time scale (often being lower for, e.g., a decadal mean than for a single year's
value), and again the error estimation method should capture this.  I do not think that
this issue detracts from the quality of the proposal.  Instead I am mentioning it in the
hope that this comment can be passed on to the proposers, in the event that the project is
funded, so that they can be prompted into placing the appropriate emphasis on quantifying
uncertainty.
Apologies for being late yet again, and best regards,
Tim

Date: Thu, 24 Oct 2002 17:14:31 +0000
Subject: confidential assessment of GC03-512
From: Tim Osborn <t.osborn@uea.ac.uk>
To: <irma.dupree@noaa.gov>
CC: <t.osborn@uea.ac.uk>,
<christopher.d.miller@noaa.gov>
Dear Irma and Chris,
Re. proposal review GC03-512, PI: David Meko "A 19th century data catalog"
First of all, I confirm that there is no conflict of interest etc.
Now to my review...
(1) Scientific Merit
Rating: Good
Comments:
I completely agree with the rationale behind improving data sets of 19th century climate (see my comments below on "Relevance to climate change programme"), and the proposers have identified the most relevant data sources available for the US.  The objectives and workplan are generally reasonable, but I have rated it "good" rather than "very good" or "excellent" because it does not seem as scientifically innovative or challenging as it might.  Some particular concerns are highlighted below.
I am very wary about the proposed approach of integrating the data sources together to produce a single climate product.  Obviously the data sources have to be used in combination, for calibration of proxy data or for assessment of possibly dubious early instrumental data, *but* combining them all into a single product only will be very restrictive for future use, assessment, improvements.  Much better would be to produce intrumental-only series for whatever length is available, and tree-ring only series for the full length (i.e., into the late 19th and 20th centuries, despite the availability of instrumental data for these periods).  Blending them into a single analysis is of some, but limited, use and comparisons of different periods and with (e.g.) model simulations can only ever be done by taking into account error bars that vary dramatically in time and are only estimates of the "true" errors - and the error estimates may be underestimates if based only on residuals or covariances during the 20th century.
No mention is made of using the 19th century data to consider key issues such as difference between tree-ring and ground borehole temperatures (they differ more in the 19th century, in terms of trend, than in other centuries), possibly taking into account land-use change.  No mention is made of using the 19th century data to assess multi-century temperature reconstructions and why they differ.  These are issues of great importance.
No mention is investigating seasonal dependence of temperature changes, which are greater in existing temperature products during the 19th century than in the 20th century and which has important implications for the calibration of proxy (including tree-ring) data against summer or annual data and the need to more clearly define the true seasonal response of proxy data.
Despite these concerns, the proposed work is certainly worthy of funding and the extra items of interest that I mention above could be achieved using the

data generated here, in some future project.
(2) Relevance to climate change programme
Rating: High
Comments:
The 19th century is certainly of particular importance, not just for the reasons outlined in the proposal but also because this century shows some of the biggest disagreements in warming trend between various quasi-hemispheric temperature reconstructions and between proxy and instrumental data and between different seasons of instrumental data. Additional data sources are definitely required, and additional digitisation, homogenisation and intercomparison of data sets is necessary. For these reasons, work such as that proposed here is essential for helping to refine answers to questions such as how unusual is late twentieth century climate and detection of climate change signals against the noise of natural climate variability.
Best regards
Tim

376. 1067194064.txt
####################################################################################

From: "Michael E. Mann" <mann@virginia.edu>
To: 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>, 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>
Subject: CONFIDENTIAL Fwd:
Date: Sun, 26 Oct 2003 13:47:44 -0500
Cc: mann@virginia.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...."

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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

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

377. 1067450707.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: stocker@climate.unibe.ch, joos@climate.unibe.ch, knutti@climate.unibe.ch
Subject: some info you'll want to have...
Date: Wed, 29 Oct 2003 13:05:07 -0500
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

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 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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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: "c:\eudora\attach\Journalists.re.EandEfin-revised.doc"

References

1. http://www.davidappell.com/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml

378. 1067522573.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: "raymond s.bradley" <rbradley@geo.umass.edu>, mhughes@ltrr.arizona.edu, "Phil Jones" <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, mann@multiproxy.evsc.virginia.edu, Scott Rutherford <srutherford@rwu.edu>
Subject: Can you believe it???
Date: Thu, 30 Oct 2003 09:02:53 -0500

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

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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.

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  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 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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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/
2. ftp://ftp.ngdc.noaa.gov/paleo/treering/chronologies/northamerica/usa
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

379. 1067532918.txt
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From: "raymond s. bradley" <rbradley@geo.umass.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, p.jones@uea.ac.uk, k.briffa@uea.ac.uk
Subject: One way out....
Date: Thu, 30 Oct 2003 11:55:18 -0500
Cc: mann@multiproxy.evsc.virginia.edu, mhughes@ltrr.arizona.edu

<x-flowed>
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:
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

</x-flowed>

380. 1067542015.txt
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From: "Michael E. Mann" <mann@virginia.edu>
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
Subject: Re: One way out....
Date: Thu, 30 Oct 2003 14:26:55 -0500
Cc: mhughes@ltrr.arizona.edu

Hi Keith,
sorry--yes, I think the Nature idea would be great. Definitely give it a try!
thanks,
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 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 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,
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Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[2]http://www.cru.uea.ac.uk/cru/people/briffa/

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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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[5]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. file://c:\eudora\attach\nh_temp_rec.jpg/
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.cru.uea.ac.uk/cru/people/briffa/
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

381. 1067596623.txt
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From: "Michael E. Mann" <mann@virginia.edu>
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>
Subject: RE: CLIMLIST
Date: Fri, 31 Oct 2003 05:37:03 -0500
Cc: mhughes <mhughes@ltrr.arizona.edu>

Thanks very much Tim,
I was hoping that the revisions would ally 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. Takinig 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
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 McKitrick (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 witholding 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 innaccuracies or difficult-to-prove claims.
Best regards
Tim

______________________________________________________________
Professor Michael E. Mann
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_______________________________________________________________________
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[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

382. 1068239573.txt
##########################################################################################

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>,"Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Fwd: Re: McIntyre-McKitrick and Mann-Bradley-Hughes
Date: Fri, 07 Nov 2003 16:12:53 +0000

<x-flowed>

>From: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>
>Date: Fri, 7 Nov 2003 15:58:06 +0000
>To: Steve McIntyre <smcintyre@cgxenergy.com>
>Subject: Re: McIntyre-McKitrick and Mann-Bradley-Hughes
>Cc: L.A.Love@hull.ac.uk, Tim Osborn <t.osborn@uea.ac.uk>,
>         Ross McKitrick <rmckitri@uoguelph.ca>
>Priority: NORMAL
>X-Mailer: Execmail for Win32 5.1.1 Build (10)
>
>Dear Steve
>Please send your material for comment direct to Tim, Osborne.I
>would like to publish the whole debate early next year, but
>'respectful' comments in the meantime can only help and the CRU people
>seem genuinely interested and have integrity. I have never heard of
>such bad behaviour here as appears to have been the case between
>Sallie and Soon and the rest..the US adversarial system and too many
>egos??
>As you know ,the  contact is Tim Osborn <t.osborn@uea.ac.uk> and I take
>the liberty to forward this to him now. You seem to suggest that this
>is welcome and are making make direct comments on his remarks to me
>concerning your paper.
>
>We shall get the printed proof, as a  single electronic file today, and
>shall look through it early next week. I am sure you do not want to see
>your paper again?  I think that adding anymore now (the exchanges

>between you and Mann/Bradley and perhaps now Tim as well)  is  premature
>and we shall wait until the next issue. Mann is said to be writing
>something, but he has not yet contacted me, though I just hang up on
>that journalist Appell who keeps on ringing. I told him that I will
>deal only directly with Mann. What cheek, after threatening me with
>litigation...Just keep me in the loop. Thanks.
>
>Sonja
>PS .By the way The Economist has  taken up a previous paper from E&E
>(Castles and Henderson, the social science critique of teh emission
>scenarios), and teh Australian and UK Treasuries have become involved.
>I have not seen it yet. As you know, I have always argued that the real
>'driver' of teh IPCC deception, if that is the right word,  has been on
>teh social /technology forcing side,  with focus of WG III.
>
>In London I heard two days ago that the WTO might make ratification of
>Kyoto conditional for something Russia wants. The source was speaker
>from the Deutsche Bank, a Justin Mundy, former advisor to the EU
>Commission on EU-Russia coordination and once senior advisor to the
>European Centre for Nature Conservation, he also worked for the World
>Bank.)
>Sonja
>
>On Fri, 7 Nov 2003 09:50:33 -0500
>Steve McIntyre <smcintyre@cgxenergy.com> wrote:
>
> > Dear Sonja,
> >
> > > > The interesting thing about their preliminary response, however, is
> that it
> > > > indicates that the difference in results might be fully explained by a
> > > > simple error in not using many of the early tree-ring data.  If
> this is
> > > > confirmed by their fuller response, then, even though there may be
> some
> > > > problems with the proxy data used by Mann et al., it implies that
> these
> > > > problems do not actually make a lot of difference to the results -
> the main
> > > > difference comes from omitting the early tree-ring data.  A paper that
> > > > identifies some problems with the proxy data used by Mann et al. would
> > > > still be interesting, but if these problems made very little
> difference to
> > > > the results obtained, then it would be of rather minor importance.
> > >
> > > (1) IMHO the data issues rise above "some problems". When you're
> doing a prospectus, audit or engineering-level feasibility study, there
> is a concerted effort to eliminate every error.  I have never seen such
> sloppy data as MBH98.  Perhaps from my business experience, I am used to
> a more demanding approach to data integrity than the above comment
> suggests about academic studies. Even the MBH response criticizes us for
> failing to use obsolete data. How silly is that. Bradley has also said
> that an "audit" should use original data and should not verify against
> source data and says that I should know better. I think that my
> experience with audits and engineering studies is more substantial than
> Bradley's and this is an extraordinarily silly thing for him to
> say.   After the fact, one of the key mis-steps in the Bre-X fraud was
> the engineering report in which ore reserves were calculated using false
> data supplied to the consulting engineers by Bre-X, without any
> verification being carried out by the engineers.
> > > (2) There was not a "simple error" of simply not using many of the
> early tree-ring data. The early tree-ring data in question are principal
> components of North American tree ring sites and of Stahle/SWM (also

> North American) tree ring sites . MBH98 states that they used
> conventional principal components methods for temperature. They do not
> explicitly say that they used conventional principal components methods
> for tree ring regions, but, in the absence of disclosure otherwise, this
> is certainly the most reasonable interpretation of the public disclosure
> (leaving aside Mann's refusal to provide clarification in response to our
> inquiries on methods.) A "conventional" principal component calculation
> requires that there be no missing data. Accordingly this indicator became
> unavailable in the earlier years using conventional principal component
> calculations - it was not "left out".  MBH now disclose for the very
> first time that they used a "stepwise principal components approach",
> although this is nowhere disclosed in MBH98 or in the SI thereto. They
> have still not disclosed the rosters of principal components involved. If
> this method is material to their results, as they now state, then it was
> a material omission in their prior disclosure. It seems like a very
> strange rebuttal for MBH to say: you're at fault because we made a
> material non-disclosure on methodology in our papers. If I were in MBH's
> shoes, I would be embarrassed at this non-disclosure and mitigating the
> situation by making full disclosure now. . When you do a prospectus, you
> have to sign an affidavit that there are no material omissions.  I have
> approached disclosure questions on the basis that prospectus-level
> disclosure is the minimum level of public disclosure in this matter,
> assuming that this level of disclosure would be exceeded.
> >
> > (3)  I've redone calculations with a re-calculated US PC1 in and get
> results similar to those in E&E, rather than the MBH response.  This is
> not a guarantee that I have fully replicated still undisclosed MBH
> methodology.  However, MBH disclosure of their methodology is very
> inadequate and without full disclosure by MBH of their methods, it is
> possible to be somewhat at cross-purposes. This defective disclosure is
> entirely their responsibility. It should be remedied immediately through
> FTP disclosure of their computer programs and full description of their
> methodology.
> >
> > [snip]
> >
> > >
> > > > >>It is quite obvious that if the opinion of these three people
> from the
> > > > >>UK University of East Anglia concerning publication of teh M&M paper
> > > > >>had been sought and taken, there would not have been no publication.
> > > >
> > > > Then I suggest you read our commentary again, which does not state
> this at all.
> >
> >
> > Part 2 has been drafted and I would be delighted to obtain comments on
> it from UEA/CRU. Indeed, I think that it would be very constructive,
> since Part 2 is significantly more hard-edged than Part 1. Because we
> have stated that we would post up a reply to the MBH response, we would
> have to disclose something on our websites, but I'd be prepared to deal
> with this. Intuitively, full, true and plain disclosure would be to state
> that we have prepared a reply and submitted it to UEA/CRU for
> comments.  I think that the many data errors will be self-evident to
> UEA/CRU; we have organized our materials to show this, as will be the
> material non-disclosures on methodology by MBH. However, if they are
> prepared to comment, this would have to be agreed on very quickly as we
> are very close to finalizing our repy.
> >
> > Regards,
> > Steve
>
>----------------------

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</x-flowed>

383. 1068652882.txt
####################################################################################

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Keith Briffa" <k.briffa@uea.ac.uk>,"Phil Jones" <p.jones@uea.ac.uk>
Subject: Fwd: MBH98
Date: Wed, 12 Nov 2003 11:01:22 +0000

<x-flowed>
Keith and Phil,

you will have seen Stephen McIntyre's request to us.  We need to talk about it, though my initial feeling is that we should turn it down (with carefully worded/explained reason) as another interrim stage and prefer to make our input at the peer-review stage.

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 muddied 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!

Anyway, because McIntyre has now asked Mann directly for his data and programs, his request that *we* send McIntyre's request to Mann has been dropped (I would have said "no" anyway).

So it's just the second bit, that we review part 2 of this response, that needs to be answered.

Cheers

Tim

>From: "Steve McIntyre" <smcintyre@cgxenergy.com>

>To: "Michael E. Mann" <mann@virginia.edu>
>Cc: "Tim Osborn" <t.osborn@uea.ac.uk>,
>         "Ross McKitrick" <rmckitri@uoguelph.ca>
>Subject: MBH98
>Date: Tue, 11 Nov 2003 23:39:46 -0500
>
>November 11, 2003
>
>
>
>Professor Michael E. Mann
>
>School of Earth Sciences
>
>University of Virginia
>
>
>
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>
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>
>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
><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
>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 McKitrick
>
>
>
>
>cc: Timothy Osborn

Dr Timothy J Osborn
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384. 1069630979.txt
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##########

From: RichardSCourtney@aol.com
To: t.osborn@uea.ac.uk, m.allen1@physics.ox.ac.uk, Russell.Vose@noaa.gov
Subject: Re: Workshop: Reconciling Vertical Temperature Trends
Date: Sun, 23 Nov 2003 18:42:59 EST
Cc: trenbert@cgd.ucar.edu, timo.hameranta@pp.inet.fi, Thomas.R.Karl@noaa.gov,
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Dear All:
The excuses seem to be becoming desperate. Unjustified assertion that I fail to understand
"Myles' comments and/or work on trying the detect/attribute climate change" does not stop
the attribution study being an error. The problem is that I do understand what is being
done, and I am willing to say why it is GIGO.
Tim Allen said;
In a message dated 19/11/03 08:47:16 GMT Standard Time, m.allen1@physics.ox.ac.uk writes:

> I would just like
> to add that those of us working on climate change detection and attribution
> are careful to mask model simulations in the same way that the observations
> have been sampled, so these well-known dependencies of nominal trends on the
> trend-estimation technique have no bearing on formal detection and
> attribution results as quoted, for example, in the IPCC TAR.

I rejected this saying:
At 09:31 21/11/2003, RichardSCourtney@aol.com wrote:
>It cannot be known that the 'masking' does not generate additional
>spurious trends. Anyway, why assume the errors in the data sets are
>geographical and not?. The masking is a 'fix' applied to the model
>simulations to adjust them to fit the surface data known to contain
>spurious trends. This is simple GIGO.
Now, Tim Osborn says of my comment;
In a message dated 21/11/03 10:04:56 GMT Standard Time, t.osborn@uea.ac.uk writes:

> Richard's statement makes it clear, to me at least, that he misunderstands
> Myles' comments and/or work on trying the detect/attribute climate change.
> As far as I understand it, the masking is applied to the model to remove
> those locations/times when there are no observations. This is quite
> different to removing those locations which do not match, in some way, with
> the observations - that would clearly be the wrong thing to do. To mask
> those that have no observations, however, is clearly the right thing to do
> - what is the point of attempting to detect a simulated signal of climate
> change over some part of (e.g.) the Southern Ocean if there are no
> observations there in which to detect the expected signal? That would
> clearly be pointless.

Yes it would. And I fully understand Myles' comments. Indeed, my comments clearly and
unarguably relate to Myles comments. But, as my response states, Myles' comments do not
alter the fact that the masked data and the unmasked data contain demonstrated false
trends. And the masking may introduce other spurious trends. So, the conducted

attribution study is pointless because it is GIGO.  Ad hominem insults don't change that.
And nor does the use of peer review to block my publication of the facts of these matters.
Richard