CRU CORRESPONDENCE
##
##########

156. 0947541692.txt
##
##########

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Simon.Shackley@umist.ac.uk
Subject: Re: industrial and commercial contacts
Date: Mon Jan 10 17:01:32 2000

Simon,

I have talked with Tim O'Riordan and others here today and Tim has a wealth of contacts he is prepared to help with. Four specific ones from Tim are:

- Charlotte Grezo, BP Fuel Options (possibly on the Assessment Panel. She is also on the ESRC Research Priorities Board), but someone Tim can easily talk with. There are others in BP Tim knows too.
- Richard Sykes, Head of Environment Division at Shell International
- Chris Laing, Managing Director, Laing Construction (also maybe someone at Bovis)
- ??, someone high-up in Unilever whose name escapes me.

And then Simon Gerrard here in our Risk Unit suggested the following personal contacts:

- ??, someone senior at AMEC Engineering in Yarmouth (involved with North Sea industry and wind energy)
- Richard Powell, Director of the East of England Development Board

You can add these to your list and I can ensure that Tim and Simon feed the right material through once finalised.

I will phone tomorrow re. the texts.

Cheers,

Mike

At 20:30 07/01/00 BST, you wrote:
>dear colleagues
>
>re: List of Industrial and Commercial Contacts to Elicit Support
>from for the Tyndall Centre
>
>This is the list so far. Our contact person is given in brackets
>afterwards. There is some discussion on whether we
>should restict ourselves to board level contacts - hence Dlugolecki
>is not board level but highly knowledgeable about climate change.
>I think people such as that, who are well known for their climate
>change interests, are worth writing to for support. There may be
>less value in writing to lesser known personnel at a non-board level.
>
>SPRU has offered to elicit support from their energy programme
>sponsors which will help beef things up. (Frans: is the Alsthom
>contact the same as Nick Jenkin's below? Also, do you have a BP
>Amoco contact? The name I've come up with is Paul Rutter, chief
>engineer, but he is not a personal contact]
>
>We could probably do with some more names from the financial sector.
>Does anyone know any investment bankers?

>
>Please send additional names as quickly as possible so we can
>finalise the list.
>
>I am sending a draft of the generic version of the letter eliciting
>support and the 2 page summary to Mike to look over. Then this can be
>used as a basis for letter writing by the Tyndall contact (the person
>in brackets).
>
>Mr Alan Wood CEO Siemens plc   [Nick Jenkins]
>Mr Mike Hughes CE Midlands Electricity (Visiting Prof at UMIST) [Nick
>Jenkins]
>Mr Keith Taylor, Chairman and CEO of Esso UK   (John
>Shepherd]
>Mr Brian Duckworth, Managing Director, Severn-Trent Water
>[Mike Hulme]
>Dr Jeremy Leggett, Director, Solar Century [Mike Hulme]
>Mr Brian Ford, Director of Quality, United Utilities plc [Simon
>Shackley]
>Dr Andrew Dlugolecki, CGU   [Jean Palutikof]
>Dr Ted Ellis, VP Building Products, Pilkington plc [Simon Shackley]
>Mr Mervyn Pedalty, CEO, Cooperative Bank plc [Simon Shackley]
>
>
>Possibles:
>Mr John Loughhead, Technology Director ALSTOM   [Nick Jenkins]
>Mr Edward Hyams, Managing Director Eastern Generation   [Nick
>Jenkins]
>Dr David Parry, Director Power Technology Centre, Powergen
>[Nick Jenkins]
>Mike Townsend, Director, The Woodland Trust [Melvin
>Cannell]
>Mr Paul Rutter, BP Amoco [via Terry Lazenby, UMIST]
>
>With kind regards
>
>Simon Shackley
>
>
>
>
>

157. 0947802707.txt
###
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru,ifor@krsk.infotel.ru,fritz.schweingruber@wsl.ch
Subject: EC contract proposal
Date: Thu Jan 13 17:31:47 2000
Cc: t.osborn@uea.ac.uk

Hi Stepan and Eugene (Eugene are you getting these messages?)
       You will have the first idea of things now and soon the first forms will come
which must be filled in and signed and stamped and returned here by FAX and as soon
as possible by REAL mail. The original forms must be submitted from here in
February. This message is to reiterate that the reviewing process this time is going
to look very carefully at the reakdown of costs in relation to precise tasks. There
is even a section of the form that asks for proportional costs associated with
individual deliverables. Therefore it is important to specify (at least for the
sake of the plan) precisely what work can be done and the person hour costs,

materials, travel, fieldwork, equipment (corers, durable equipment like computer ,GPS, etc: consumable costs like xray film etc.etc.) . I need you to think in terms of intensive sampling of modern and sub-fossil wood with the emphasis on major contributions to extending the network in Russia both ringwidth (in Ekaterinburg) and a major part of the densitometry , perhaps of Russian and non-Russian samples(?) (in EKaterinburg). THIS IS NOT TO SAY I AM ASSUMING YOU ARE ONLY DATA PROVIDERS . I do not look on you in this way. It is simply that I have to make a strong @SPECIAL CASE@ for your both being partners and the relatively large funds that I have suggested must be convincingly justified. Your involvement is crucial on the scientific side and I will emphasise this strongly. But it is also important to display to referees what the money will go on. Hence yoy need to suggest various options to me in terms of possible sampling work, laboratory work and analysis and cost out these different options to cover different possible plans. We will then sort out an optimum one . You must budget realistically for travel, fieldwork travel and equipment - which I believe are expensive. ALso note our earlier message as regards travel to Europe. I would very much appreciate help with up to date information on state of the art of the Russian data for background, potential of new areas or your ideas of where best to concentrate updating work.
In both Yamal and Taimyr , the continued work on the long chronologies to greatly increase sample numbers is still very high on my lisy of priorities and the work Stepan (and Rashit) are doing to reconstruct tree-line changes on a detailed resolution is very very important. So please try to think about the details of new sampling sites(need bigger sample numbers with different age trees at each to look at age-dependent growth chages); best areas needing updating; subfossil continuation; real numbers for different cost options and start to interact with me and Tim (and Fritz) re the possible distribution of densitometry work. Finally, Eugene, I think your comments on the ring structure and using inpu from simulations and model (GCM) data are important. Can we factor in some exploratory work on this or is it better to do it as part of a separate proposal - I have two more in mind in the coming months (one to NERC in UK and one to the Leverhulme Foundation - more about these later).

for now that better be all

best wishes
Keith (p.s please copy all replies to Tim)

158. 0950712852.txt
##

From: "Sujata Gupta" <sujatag@teri.res.in>
To: <m.hulme@uea.ac.uk>
Subject: Re: Tyndall Centre bid
Date: Wed, 16 Feb 2000 09:54:12 +0530
Cc: <ritu.kumar@commonwealth.int >, "R K Pachauri" <pachauri@teri.res.in>

Dear Mike

Thank you for sending the outline bid submitted last October. After reviewing the document, my colleagues and I were of the view that TERI should go non-exclusive. Our primary interest is to be part of the project and given that we (TERI) would have the role of an affiliate in both the bids, it was decided that we go non-exclusive.

We understand that the outline bid is confidential and I can assure you that it will not be shared with anyone outside the concerned colleagues at TERI. Also, I assure you of all possible support TERI can provide in developing the final bid. We look forward to a fruitful association with you on the project.

Wishing you all the best in securing the bid.

Kind regards

Sujata

Sujata Gupta Ph.D.
Fellow and Dean
Policy Analysis Division
****
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****
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Tel. 460 1550 or 462 2246   City code 11
Web www.teriin.org

>>> Mike Hulme <m.hulme@uea.ac.uk> 02/12/00 11:56PM >>>
Dear Sujata,

I attach a copy of our outline bid from last October - it is now evolving rapidly of course in preparation for final submission. This gives you a quick idea about our Consortium and plans. You will also see the names and institutes of our partners. May I re-iterate that this document is confidential and must not be disclosed to anyone outside your immediate colleagues in TERI.

TERI was *not* listed as a formal co-applicant (non-UK institutions are not eligible to be formal co-applicants), but was listed as an 'affiliated organisation' along with about 10 others here in the UK. We would propose to do the same in the final bid, but say a little bit more about where and how TERI would interact with us were we to win the Centre.

If you decide to remain exclusively with our bid, then I will send you the first draft of our final submission during the next week - this will indicate more details about our research programmes and where TERI may be seen to interact with us as a key overseas collaborator.

However, if you decide to join with both bids - Imperial and UEA - then we will simply continue to list you as a collaborator, but we could not then agree to any further interaction over the next 2 weeks.

Best Regards,

Mike

At 10:45 10/02/00 +0530, you wrote:
>Dear Mike
>
>Thank you for your email. I appreciate your understanding of our position. TERI is essentially interested in working on the project. I can assure you that we will not disclose any information provided by you to the other finalist or anyone else for that matter and maintain strict confidentiality.
>

>However, I did not receive the original bid document or an outline of the proposal. We are not clear if TERI has been listed as a partner up-front or has been mentioned as an associate. I would greatly appreciate it if you could let me know TERI's status in the original document. This will help in our taking a decision on the exclusivity front, as yet we are still debating on the matter and have not reverted to the Imperial team. Also, who are the other members of the team headed by you.
>
>We look forward to working with you and hope we are able to reach a decision which is mutually beneficial.
>
>Best wishes
>
>Sujata
>
>
>
>Sujata Gupta Ph.D.
>Fellow and Dean
>Policy Analysis Division

>T E R I
>New Delhi - 110 003 / India
>Fax 462 1770 or 463 2609   Country code 91
>Tel. 460 1550 or 462 2246   City code 11
>Web www.teriin.org
>
>>>> Mike Hulme <m.hulme@uea.ac.uk> 02/08/00 01:49AM >>>
>Dear Sujata,
>
>I have consulted with colleagues in our Consortium and we consider the
>following to be the position .....
>
>- we clearly would prefer TERI to affiliate to only one of the two
>finalists, and obviously we prefer that one to be our bid. This is
>espeically the case since we made our initial approach to you last
>September when there were still seven bids in the making; no-one else
>approached you at that stage and therefore we feel we have some preference
>through prior approach.
>
>- we recognise that *you* may now consider it in your interest to affiliate
>to both finalists to cover yourselves either way (although *we* consider
>there are strong grounds for you not to do so). This is your choice of
>course, although were you to do this then I must point out the following
>two consequences:
>
>a) since I believe I sent you last October/November a copy of our outline
>bid for the Centre I would need to insist that you do not divulge the
>contents of this outline to Imperial College. This is clearly a case of
>professional integrity which we are sure you understand.
>
>b) if you indicate that you are also joining with Imperial then this
>effectively precludes any further dialogue between us over the remaining 3
>weeks before submission. All that we would be able to do would be to name
>you and your expertise in our submission rather than engage you

>interactively in shaping 1-2 of our ideas (which was my original intention
>as our final bid shapes up).
>
>Please let me know how you wish to proceed - either way, I look forward to
>a fruitful association between us in the event of our bid succeeding with
>the UK Research Councils.
>
>Best regards,
>
>Mike
>
>At 16:00 01/02/00 +0530, you wrote:
>>Dear Mike,
>>
>>TERI has a presence in London as of 25 January. My colleague Dr Ritu Kumar
>there has been approached by the consortia led by Imperial College
>>for TERI to join them. I am writing to explore the possibility of TERI
>joining both consortia on a non-exclusive basis. This would of course imply
>that we do not share/participate in the preparation of the bid. Any inputs
>provided by TERI would be common to both consortia, unless it was in
>response to a specific request by a particular partner.
>>
>>As we have committed to you first, we will revert to Imperial College for
>a non-exclusive tie-up, only after discussing the matter with you.
>>
>>I am copying this email to my colleague Dr Kumar.
>>
>>Looking forward to hearing from you.
>>
>>Regards
>>
>>Sujata
>>
>>
>>
>>
>>
>>
>>Sujata Gupta Ph.D.
>>Fellow and Dean
>>Policy Analysis Division
>>**
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>>Tel. 460 1550 or 462 2246   City code 11
>>Web www.teriin.org
>>
>>>>> Mike Hulme <m.hulme@uea.ac.uk> 01/19/00 02:52PM >>>
>>Thank you Sujata ...... I will keep you informed about our needs for
>>bidding for the UK Climate Change Centre.
>>
>>And it *was* me that you had a conversation with in Canberra about
>>reviewers for Chapter 3 on scenarios. I will forward your suggestion on to
>>the TSU II.

>>
>>Regards,
>>
>>Mike
>>
>>
>>At 11:56 19/01/00 +0530, you wrote:
>>>Dear Dr Hulme
>>>
>>>TERI will be happy to provide sole support to the consortium led by you
>>and UEA. I was on travel and hence could not respond earlier. Please let
>>me know if we can assist in any way in the preparation of the bid.
>>>
>>>If I recollect we had a discussion on a possible reviewer for the
>>scenarios chapter from India who was thus far not involved with the IPCC
>>process. I can suggest the name of Dr Shreekant Gupta at the Delhi School
>>of Economics, New Delhi. It is quite possible that I had this discussion
>>with Tom Downing. Please let me know if I am communicating to the wrong
>>person on this matter.
>>>
>>>Best wishes for the new year
>>>
>>>Sujata
>>>
>>>
>>>
>>>Sujata Gupta Ph.D.
>>>Fellow and Dean
>>>Policy Analysis Division
>>>**
>>>TERI's SILVER JUBILEE CONFERENCES
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>>>**
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>>>New Delhi - 110 003 / India
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>>>Tel. 460 1550 or 462 2246   City code 11
>>>Web www.teriin.org
>>>
>>>>>> Mike Hulme <m.hulme@uea.ac.uk> 01/05/00 06:54PM >>>
>>>Dear Colleague,
>>>
>>>Thank you very much for your support for our bid to run the new UK Climate
>>>Change Centre being established by three of our national research councils.
>>> We have heard that just two of the seven outline bids have been invited to
>>>submit detailed proposals and that the Consortium led by UEA is one of
>>>these two. Final bids are required by 29th February. The UEA-led bid
>>>proposes the new Centre to be called the Tyndall Centre for Climate Change
>>>Research (named after the 19th century British physicist who experimented
>>>with the radiative properties of greenhouse gases, John Tyndall).
>>>
>>>Assuming you are happy to continue sole support for our initiative, and on
>>>the undertaking that you do not disclose our outline bid to other parties
>>>who may be aligned with the other finalist (a Consortium led by Imperial
>>>College and involving the Environmental Change Institute at Oxford and the
>>>U. Edinburgh), then I will send you a copy of our outline proposal.
>>>
>>>There are a number of aspects of this outline bid that we will change and

>>>develop before 29th Feb. and it may be that I am back in contact with you
>>>to ask for some additional text of support about some concrete ways the UK
>>>Tyndall Centre could collaborate with your organisation.
>>>
>>>We would also, of course, welcome any suggestions you may have about such
>>>future collaboration.
>>>
>>>Best wishes for the New Year,
>>>
>>>Mike
>>>
>>>
>>>***
***
>>>****
>>>Dr Mike Hulme
>>>Reader in Climatology   tel: +44 1603 593162
>>>Climatic Research Unit   fax: +44 1603 507784
>>>School of Environmental Science   email: m.hulme@uea.ac.uk
>>>University of East Anglia   web site:
>http://www.cru.uea.ac.uk/~mikeh/
>>>Norwich NR4 7TJ
>>>***
***
>>>****
>>> The estimated annual mean temperature in Central England for 1999 is +1.16
>>>degC   above the 1961-90 average, the warmest year recorded in 341
>>>years
>>>   ***
>>>   The estimated global-mean surface air temperature anomaly for
1999 is
>>>   +0.33 deg C above the 1961-90 average, the 5th warmest year yet
>recorded
>>>***
***
>>>****
>>>   Neither of these estimates have yet been confirmed
>>>   **
>>>
>>>
>>***
>>Dr Mike Hulme
>>Reader in Climatology   tel: +44 1603 593162
>>Climatic Research Unit   fax: +44 1603 507784
>>School of Environmental Science   email: m.hulme@uea.ac.uk
>>University of East Anglia   web site:
http://www.cru.uea.ac.uk/~mikeh/
>>Norwich NR4 7TJ
>>***
>>The unconfirmed annual mean temperature in Central England for 1999 was
>+1.16
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>>   ***
>> The unconfirmed global-mean surface air temperature anomaly for 1999 was
>> +0.33 deg C above the 1961-90 average, the 5th warmest year yet recorded
>>***
>>
>>
>***
>Dr Mike Hulme
>Reader in Climatology   tel: +44 1603 593162
>Climatic Research Unit   fax: +44 1603 507784
>School of Environmental Science   email: m.hulme@uea.ac.uk

>University of East Anglia   web site: http://www.cru.uea.ac.uk/~mikeh/
>Norwich NR4 7TJ
>**
>The unconfirmed annual mean temperature in Central England for 1999 was
+1.16
>   degC above the 1961-90 average, the warmest year recorded in 341 years
>   **
> The unconfirmed global-mean surface air temperature anomaly for 1999 was
> +0.33 deg C above the 1961-90 average, the 5th warmest year yet recorded
>**
>
>

159. 0951431850.txt
##
##########

From: John Shepherd <John.G.Shepherd@soc.soton.ac.uk>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: BGS, Esso, & CV for Tyndall bid
Date: Thu, 24 Feb 2000 17:37:30 +0000

Mike

BGS are now on board, so please leave them in the text : I have drafted a letter for David Falvey to sign and sent it. I hope we shall get it back in time...

The Esso (Exxon-Mobil) situation is still promising, but they're having to get clearance from HQ in the USA (my best contact retired (with cancer) just a few weeks ago, so we've had to work around the new CE, to whom all this is news...). They know the deadline and will do their best for us.

Finally, my short informal CV is attached, as requested.

Hope the drafting is coming together well.

John
Attachment Converted: "c:\eudora\attach\JGS_CV_informal.doc"

160. 0951763817.txt
##
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: newest reconstruction
Date: Mon Feb 28 13:50:17 2000
Cc: k.briffa@uea, t.osborn@uea

At 11:56 25/02/00 -0500, you wrote:
>I need your newest northern hemisphere density-based tree-ring reconstruction
>and appropriate reference for updating IPCC. Please send in ASCII format as
>soon as possible so we can incorporate. I hope all is well. Thanks,

Hi Mike

Keith asked me to get back to you on this. The reconstruction is the same as the one I sent on the 5th October 1999, but I'm sending it again in case that e-mail isn't handy. The reconstruction has now been published, in the following paper:

Briffa K.R. (2000) Annual climate variability in the Holocene: interpreting the message of ancient trees. Quaternary Science Reviews 19, 87-105.

This paper does not, however, give full details about how the reconstruction was obtained. The details are not yet published, but will soon be submitted:

Briffa KR, Osborn TJ, Schweingruber FH, Harris IC, Jones PD, Shiyatov SG and Vaganov EA (2000) Low-frequency temperature variations from a northern tree-ring density network. In preparation (to be submitted to Journal of Geophysical Research).

Details about the file I'm sending you (repeated from 5th Oct 99):

The data are attached to this e-mail. They go from 1402 to 1994, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use. I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves. The data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep tempratures averaged over all land grid boxes (that have observed data) that are north of 20N. As such, the mean of our reconstruction over 1881-1960 matches the mean of the observed target series over the same period. Since the observed series consists of degrees C anomalies wrt to 1961-90, we say that the reconstructed series also represents degrees C anomalies wrt to 1961-90.

(I've already truncated the series at 1960 because of the problems with the recent period.)

Best regards

Tim

161. 0951977522.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Frank Oldfield <frank.oldfield@pages.unibe.ch>
Subject: Re: PAGES QSR volume
Date: Thu Mar 2 01:12:02 2000
Cc: matti.saarnisto@gsf.fi, brigham-grette@geo.umass.edu, D.Jewson@ulst.ac.uk, keith.alverson@pages.unibe.ch, fritz.schweingruber@wsl.ch

Hi Frank
I have two names - one of which you know well. First , I strongly urge that one copy be sent to

Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia

This is the home of the Laboratory of Dendrochronology , headed by Dr. Stepan G. Shiyatov and I would suggest you consign the book to him, or through him , to a genearl library if one exists.

e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

I know they have very limited resources but they will make real use of the volume .
They are genuinely active and the work they do is truly 'world class'. You will
remember also that one of their younger scientists (Rashit Hantemirov) won a prize
in London at the Open Science meeting for his poster on the long Yamal chronology.
This group gets my first and strongest vote.

My other suggestion is to send one to Eugene Vaganov's Institute of Forest.
They are not so strapped for resources as the Ekaterinburg lab. but they are large
and have many active areas of research and the book would get a wide audience.

Eugene's email is
ifor@krsk.infotel.ru

Then there is the question of getting them there . The post is not reliable. You
might send then to Fritz Schweingruber's laboratory from where they could be picked
up or carried to Russia ?
Hope this helps
best wishes
        Keith

e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

At 12:58 PM 3/2/00 +0100, Frank Oldfield wrote:
>Dear Keith, Julie, Matti and David,
>
>We are compiling a list of people and/or institutions in the former USSR to
>whom we should send FREE copies of the PAGES Open Science Meeting Special
>issue of Quaternary Science Reviews. For this, we need some help and advice
>in the way of key addresses and contacts. Where it seems best to send the
>book to a library we'd quite like to inform at least one key academic in
>the Institution that we are doing this. Where we are sending to an
>individual, we need to be able to trust in a degree of collegiality and we
>shall indicate that we want to be sure the book will be made as widely
>available as possible. We do not anticipate being able to send more than 10
>or so copies for free; others may be available at a reduced rate at the end
>of the year. This means a selective and carefully compiled 'hit list' is
>required.
>
>Over to you - we need your help.
>
>Many thanks,
>
>Frank
>
>____
>Frank Oldfield
>
>Executive Director
>PAGES IPO
>Barenplatz 2
>CH-3011 Bern, Switzerland
>
>e-mail: frank.oldfield@pages.unibe.ch
>
>Phone: +41 31 312 3133; Fax: +41 31 312 3168
>http://www.pages.unibe.ch/pages.html
>

>
>

162. 0952106664.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Shaopeng Huang <shaopeng@geo.lsa.umich.edu>,hpollack@geo.lsa.umich.edu
Subject: Nature paper and beyond
Date: Fri, 03 Mar 2000 13:04:24 +0000
Cc: mann@multiproxy.evsc.virginia.edu,tom@ocean.tamu.edu,k.briffa@uea.ac.uk,
t.osborn@uea.ac.uk

Dear Shaopeng and Henry,
First, congratulations on the Nature paper. Can you send me some reprints when you get them ?

I was at a meeting this week with Tom Crowley and we were discussing ways to reconcile the high-freq proxies with your borehole data. Here are a couple of our thoughts. Involving Mike Mann and others here in CRU, as they all have an input.

1. I've shown that the borehole data in Europe agree well with the long instrumental data in both the UK and Europe. The biggest differences/problems seem to come with the North American borehole data, which show the 16/17/18th data much cooler than the European/Asian/African data in the 16/17th century. I'm still reminded by the potential effects of land-use changes, principally in the eastern US, which could be making your North American series too cool. I realise you've taken great care with the selection, but this is a nagging doubt and will be picked up by the few skeptics trying to divide us all about the course of change over the last millennium. Is it possible to subdivide the North American borehole data into regions where we can be confident of no land-use changes (possibly and thinking aloud say Canada and the western US and Alaska) ? The aim of this (possibly joint work) is to try and reconcile the low- and high-freq proxies. Tom Crowley has a series for the NH where he's combined about 20 series (a few of which are in Mike's and the series we've produced here but he has over half the series from less-well resolved proxies - shallow marine and lake sediments) and he gets something very similar to Mike and CRU.

2. As all our (Mike, Tom and CRU) all show that the first few centuries of the millennium were cooler than the 20th century, we will come in for some flak from the skeptics saying we're wrong because everyone knows it was warmer in the Medieval period. We can show why we believe we are correct with independent data from glacial advances and even slower responding proxies, however, what are the chances of putting together a group of a very few borhole series that are deep enough to get the last 1000 years. Basically trying to head off criticisms of the IPCC chapter, but good science in that we will be rewriting people's perceived wisdom about the course of temperature change over the past millennium. It is important as studies of the millennium will help to show that the levels of natural variability from models are reasonable. Tom has run his EBM with current best estimates of past forcing (Be-10 as a proxy for solar output and Alan Robock's ice core volcanic index) and this produces a series similar to all series of the last 1000 years.

The above is just ideas of how we, as a group, could/should try and reduce criticisms etc over the next year or so. Nothing is sacred. Your North American borehole series could be correct as it is annual and most of the high-freq proxy series respond mainly to summer variations. Is yours really annual when there is a marked seasonal snow cover season ?

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit   Telephone +44 (0) 1603 592090
School of Environmental Sciences   Fax +44 (0) 1603 507784
University of East Anglia
Norwich   Email   p.jones@uea.ac.uk
NR4 7TJ
UK

----

163. 0952619617.txt
##
##########

From: "ifor" <ifor@krsk.infotel.ru>
To: "Briffa Keith" <K.Briffa@uea.ac.uk>
Subject: No Subject
Date: Thu, 9 Mar 2000 11:33:37 +0700

Dear Keith,

we Mukhtar and me are definitely out from Abisko workshop,

so you are free to present any material suitable.

Make the same in France, no problem with permission.

Best withes, Gene.

From ???@??? Wed Mar 08 20:29:20 2000 Received: from [139.222.230.3]
(helo=mailgate3.uea.ac.uk) by mailserver1.uea.ac.uk with smtp (Exim 3.02 #1) id
12SxCi-0001SB-00 for f023@smtp.uea.ac.uk; Thu, 09 Mar 2000 07:17:52 +0000
Received: from
DarkOne.ural.net [195.64.192.49] by mailgate3.uea.ac.uk with esmtp (Exim 1.73 #1)
id
12Sx7z-00020G-00; Thu, 9 Mar 2000 07:12:59 +0000 Received: from relay.uran.ru
(atreyu.ural.net [195.19.137.69]) by DarkOne.ural.net (8.10.0/eTn) with ESMTP id
e297CwJ06512 for ; Thu, 9 Mar 2000 12:12:58 +0500 (ES) Received: from
ipae.uran.ru
([195.19.128.15]) by relay.uran.ru (8.9.3/eTn) with SMTP id MAA56670 for ; Thu, 9
Mar 2000
12:12:49 +0500 (ES) Received: from mail.ipae.uran.ru (rashit.ipae.uran.ru
[195.19.135.143]
) by ipae.uran.ru (Hethmon Brothers Smtpd) ; Thu, 9 Mar 2000 12:16:06 +0500 Date:
Thu, 9
Mar 2000 12:15:07 +0500 From: Rashit Hantemirov X-Mailer: The Bat! (v1.00 Build
1311)
Registered to Andy Malyshev Reply-To: Rashit Hantemirov Organization: IPAE
Priority: Normal
Message-ID: <3511.000309@ipae.uran.ru> To: Keith Briffa Subject: Re: meeting in
Sweden
References: <3.0.1.32.20000308021839.00746228@pop.uea.ac.uk> Mime-Version: 1.0
Content-Type: text/plain; charset=us-ascii Content-Transfer-Encoding: 7bit

Status: Dear
   Keith, I'm glad that chance to see you in Sweden has arisen, because I will hardly come to
   Mendoza. I was invited to Abisko under curious circumstances and was pleasantly surprised
   seeing you among participants. I apologize if my participating give you trouble with
   preparing your paper. I'm going to present results of tree line reconstruction in Yamal,
   based on about 50 radiocarbon data (from 9500 BP) and about 500 samples dated using Yamal
   chronology (from 7000 BP). May be some short-scale falls in summer temperature will be
   examined as a potential cause of tree line recession. Organizers will pay for my travel,
   accommodation and food (otherwise I could not come to Sweden). I don't know about other
   participants. Best regards, Rashit M. Hantemirov Lab. of Dendrochronology Institute of
   Plant and Animal Ecology 8 Marta St., 202 Ekaterinburg, 620144, Russia e-mail:
   rashit@ipae.uran.ru Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92

164. 0954268691.txt
##

From: Trevor Davies <t.d.davies@uea.ac.uk>
To: r.k.turner@uea,g.bentham@uea,t.oriordan@uea,n.pidgeon@uea,p.jones@uea,
j.palutikof@uea,n.adger@uea,i.bateman@uea,m.hulme@uea,a.lovett@uea
Subject: JIF news
Date: Tue, 28 Mar 2000 13:38:11 +0100

We have heard from ESRC that the ICER bid has been successful. We are to be funded at a "reduced level", although we don't know what that is yet. Our guess is that it will be close to the 10 million we were asked to approach (the revised bid was about 12.5 million).

Well done everyone.

The letter asks us not to make any public announcement, publicity or press releases until 4 April, when there will be a JIF press conference (altho we are encouraged to prepare the press as soon as possible). Please, therefore, continue to regard this information as confidential as far as the outside world is concerned - I shall ask the Press Office to do the necessary.

I will send a note out to all faculty later this afternoon.

Trevor

+++++++++++++++++++++++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719
+++++++++++++++++++++++++

165. 0955699514.txt
###
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru,ifor@krsk.infotel.ru
Subject: Mendoza, intas
Date: Fri Apr 14 04:05:14 2000

Dear Stepan and Eugene
I was very much looking forward to seeing you both and talking over progress and future plans. I am very sorry that you were not able to attend the Mondoza meeting. I used my introductory talk for the long chronology session to illustrate the great progress and important potential of the Yamal and Taimyr work - and gave a clear indication of the quality and world significane of the continuing research at Ekaterinburg and Krasnoyarsk , and the work of Rashit and Muchtar.
Please also let me appologise that Fritz may have been over zealous in requesting receipts for the small amount of money he is to forward to you. I have received these but it was not my intention that he should keep this money until the receipts were to hand. I hope no offence was taken and I am sorry that this money has not been forwarded earlier. I have asked him to send it straight away. Also I hope Stepan that you are now well. I am now back as you see and my first job is to write and send the INTAS report . I will forward copies as soon as it is complete.
I have heard nothing about our proposal to the European Commission but I am not confident.
I will be sending your manuscripts back with comments in the near future for the Holocene issue.
It is my greatest hope that collaboration is continued between us even if our latest application fails and I will do my very best to find other sources of support in the future. I really want to understand more about the cell growth model and the link between long term changes in treelines and the lack of very long term evidence of climate change in our ring width and density chronologies. Please let us stay more closely in touch in the future.
my very best wishes
Keith

166. 0956161482.txt
###
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Christoph Schmutz <schmutz@giub.unibe.ch>
Subject: Re: Your recent GRL paper (fwd)
Date: Wed, 19 Apr 2000 12:24:42 -0400
Cc: drdendro@ldeo.columbia.edu, Juerg Luterbacher <juerg@giub.unibe.ch>, Elena Xoplaki <xoplaki@giub.unibe.ch>, Heinz Wanner <wanner@giub.unibe.ch>, Dimitrios Gyalistras <gyalistr@giub.unibe.ch>, mann@multiproxy.evsc.virginia.edu, cullen@ldeo.columbia.edu, druidrd@ldeo.columbia.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk, christian.pfister@hist.unibe.ch

Christoph,

I have time for just a few brief comments. I'll leave Ed and the others to follow up if they wish...

mike mann

At 05:13 PM 4/19/00 +0200, you wrote:
>
>Dear Prof. Cook
>

>I have received your comments and the comments of Prof. Mann (Juerg
>kindly forwarded me the messages).
>
>First I would like to point out that our paper clearly has the intention
>to contribute in a constructive way to the discussion of proxy-based
>climate reconstructions. This was the reason for fitting available
>proxy-based indices onto J, in order to assess the potential of the
>complementary information in the proxy data. In fact, we need proxy-data
>to go further back. But it is essential to know the limitations and there
>ARE obviously major limitations.
>
>As you mentioned, there might be some non-stationarities in the NAO.
>

Hmmm. I *think* what Ed actually meant is that if one samples e.g. only a subset of the quadrapole set of temperature "lobes" of the NAO (especially, if one samples only, say, one of them--the European one), then one will necessarily be seeing a combination of the NAO, and any other climate patterns that have a distinct regional overprint in that region. In the case of Europe, there are several. So the "nonstationarity" isn't in the *true* NAO, it is
an the attempt to *define* the NAO in terms of an insufficent subsample of regions influence by it.

>However, the signature of the NAO shows to be quite robust for most of the
>20.th century. As you said, we do not know if there is in fact a probably
>strongly biased signal towards the European continent back in time.
>

>I have downloaded the preprint paper by Cullen et al. In a first overview
>it seems to me that one of my main conclusions, which states that it is
>important to use the complementary information in the data is confirmed by
>their work. In fact this was already one of the conclusions in the
>Luterbacher et al. 1999 paper (number of used predictors are an important
>factor for the obtained skill).
>
>It would have been nice to find the Luterbacher et al. 1999 index in the
>analyses of the mentioned Cullen et al. paper (e.g. in the Tables 1 to 3).

In fact, the Cullen et al paper was originally written and submitted well before the paper you cite (GRL has an extremely fast turnaround time relative to Paleoceanography), and it wouldn't have been appropriate for Heidi Cullen to redo all the analyses using this additional index, at the time the paper was already in review/in press.

>
>The loss of skill (1840-1873) found in table 3 of the mentioned Cullen et
>al. paper implies again that proxy-based index reconstructions have to be
>verified rigorously in the pre-1850 period. The Luterbacher et al. 1999
>index might give some help for the validation of proxy-based
>reconstruction attempts. This index will be open to the public after the
>EGS2000 conference. (http://www.giub.unibe.ch/klimet)
>
>Since I'm not a specialist in tree-ring proxy-data you could probably
>better explain the following questions that I (honestly) can not explain:
>
>Why are the different proxy-indices not significantly correlated back in
>time (if one considers a serious significance testing procedure) on the
>interannual and decadal time-scale?

Hmmm. I'm not sure how you come to this conclusion from the results we show. Several proxy indices are in fact quite significantly correlated (the

Appenzeller index is the only one that doesn't show close correlation with
the others).

>How is it possible (from a biological and physical point of view) to
>relate the mid- and high latitude tree-ring density and width to the
>main winter circulation pattern in Europe?
>

I'm sure Ed and Keith can point you to the relevant wealth of literature on
this.
>
>
>Sincerely yours, Christoph Schmutz
>
>
>> From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
>> To: Ed Cook <drdendro@ldeo.columbia.edu>,
>>   Juerg Luterbacher <juerg@giub.unibe.ch>
>> Cc: cullen@ldeo.columbia.edu, druidrd@ldeo.columbia.edu, p.jones@uea.ac.uk,
>>   k.briffa@uea.ac.uk
>> Subject: Re: Your recent GRL paper
>>
>> Thanks for your comments Ed,
>>
>> I agree with them, and think this needs to be looked into further. I would
>> encourage those who haven't yet, to take a look at the Cullen et al
>> manuscript which covers the same territory and comes to somewhat different
>> conclusions. The manuscript is now in-press in Paleoceanography, and is
>> available in
>> preprint form here (both as postscript and pdf file):
>>
>> http://rainbow.ldeo.columbia.edu/climategroup/papers/
>>
>> Would be interested in peoples thoughts.
>>
>> regards,
>>
>> mike
>> At 04:34 PM 4/18/00 -0400, Ed Cook wrote:
>> >Dear Juerg,
>> >
>> >I have just completed reading your most recent GRL paper (Schmutz et al.,
>> >2000) on NAO reconstructions in which you show that proxy-based NAO
>> >reconstructions are probably wanting. It is not possible to strongly defend
>> >my reconstruction at this time (indeed I was extremely cautious in my
>> >description of it with regards to over-fitting problems, etc.). However, I
>> >do think that there are some issues that have not been fully explored,
>> >which could help explain some of the non-stationarity in the relationships
>> >found between your index and mine (at least) based on proxy data alone.
>> >First, my NAO reconstruction is based on 6 North American and 4 European
>> >tree-ring chronologies. Because the putuative NAO information in these
>> >records spans the North Atlantic and nicely brackets the NAO centers of
>> >action as we know them now, they potentially contain past information that
>> >is missing from a purely European-based estimate of NAO. This could occur
>> >if the NAO did not affect climate on both sides of the North Atlantic in
>> >the same roughly symmetric way back in time as it does now. If this were
>> >the case (and we have no way of knowing that now as far as I know), then it
>> >is conceivable that your L index is excessively biased towards Europe, as
>> >would be the extended Jones SLP index. If so, any comparisons between your
>> >L index and my proxy index with the Jones index would be hopelessly biased
>> >in your favor. This is not to say that my reconstruction is as good as
>> >yours, but it might not be as bad as your results indicate either.
>> >

```
>> >Indeed, I did make some effort to "verify" my reconstruction against early
>> >instrumental records, with somewhat contradictory and potentially
>> >interesting results. Over the 1841-1873 period, my record correlates
>> >significantly with Stykkisholmer SLP (-0.456) and Oslo temperatures
>> >(0.323), but not Bermuda SLP (0.156) and Central England temperatures
>> >(0.211). The "appearance" of significant verification with only the more
>> >northerly instrumental records may be telling us something about
>> >differences in circulation and SSTs over the North Atlantic from what is
>> >now the case. This could affect the way in which the NAO affects climate
>> >jointly over North America and Europe. Of course, when I added some earlier
>> >observations (same stations) to the verification tests (Table 4 of my
>> >paper), the results weakened considerably. So, maybe this means that my NAO
>> >reconstruction is indeed poor. However, I must admit to having doubts about
>> >the quality of the early instrumental records despite the great efforts
>> >made to homogenize and correct them. This is especially the case with
>> >regards to low-frequency variability, but can also extend to individual
>> >values as well. I talked with Phil Jones about one suspect datum in the
>> >early portion of his extended NAO record that largely destroys any
>> >correlation with proxy-based NAO estimates (the sign of the instrumental
>> >index appears to be wrong to me). Yet, Phil is convinced that that datum is
>> >good and he may very well be right. Either way, more robust methods of
>> >association between series may be jusitified to guard anomalous values.
>> >
>> >Last year I asked you to please send my your reconstruction of the NAO (L).
>> >I never received it and ask you again to please send it.
>> >
>> >Regards,
>> >
>> >Ed
>> >
>> >
>> >
>> >
>> >
>> >
>> >
>> ______________________________________________________________________
>>                     Professor Michael E. Mann
>>           Department of Environmental Sciences, Clark Hall
>>                      University of Virginia
>>                     Charlottesville, VA 22903
>> ______________________________________________________________________
>> e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804) 982-2137
>>        http://www.evsc.virginia.edu/faculty/people/mann.html
>>
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>>
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>
>
>~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
>    Christoph Schmutz
>    Climatology and Meteorology          Tel:  (+41) (0)31 631 88 68
>    Institute of Geography               Fax:  (+41) (0)31 631 85 11
>    University of Bern
>    Hallerstrasse 12
>    CH-3012 Bern                      E-Mail:  schmutz@giub.unibe.ch
>
>~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
>
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>
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Professor Michael E. Mann
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____
e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804) 982-2137
http://www.evsc.virginia.edu/faculty/people/mann.html

167. 0957536665.txt
##

From: Mike Hulme <m.hulme@uea.ac.uk>
To: t.d.davies
Subject: ESSO
Date: Fri May 5 10:24:25 2000

>Date: Fri, 05 May 2000 10:04:21 +0100
>To: shepherd
>From: Mike Hulme <m.hulme@uea.ac.uk>
>Subject: ESSO
>
>John,
>
>I can make a London lunch on either 19 or 20, but with a strong preference for 20th. Trevor could also make both days if necessary. By then we will have got further with the Tyndall contract so it would useful to talk with Esso (do you have a copy of the Exxonmobil booklet referred to?).
>
>Let me know how this proceeds,
>
>Mike
>
>

168. 0959187643.txt
##

From: John Shepherd <j.g.shepherd@soc.soton.ac.uk>
To: t.d.davies@uea.ac.uk
Subject: Re: ESSO
Date: Wed, 24 May 2000 13:00:43 +0100
Cc: Mike Hulme <m.hulme@uea.ac.uk>

Trevor
I gather you're going to collect the free lunch(?) with Esso ! I agree witrh Mike's analysis : i.e. there's room for some constructive dialogue...

See you on the 1014 from Ipswich (0940 from Norwich), for a kick-off at 12 noon ??

John

At 14:07 19/05/00 +0100, Mike Hulme wrote:
>John,
>
>It will be Trevor on the 19th for ESSO - too tricky for my schedule. I
>will pass the Esso booklet onto Trevor.
>

>Esso have selectively quoted to (over)-emphasise the uncertainties re.
>climate change, but at least they have moved beyond denial and recognise
>that potential unknown long-term risks may require tangible short-term
>actions. Seems to be some room for negotiation over what research needs
>doing. I would think Tyndall should have an open mind about this and try
>to find the slants that would appeal to Esso. Uncertainty and risk
>analysis and C sequestration may be the sort of things that appeal.
>
>See you Wednesday,
>
>Mike
>
>At 16:23 10/05/00 +0100, you wrote:
>>Mike
>>   Despite my efforts Esso have gone firm on 19th (to fit the schedule of
>>their man from the USA). Can you decide between you who should come (I
>>suggest one is enough) : it'll be lunchtime somewhere in London. I shall
>>be travelling from Ipswich (it's my week for the Aldeburgh Festival) so we
>>could possibly meet on the train there ??
>>
>>   Copies of the Esso booklet arrived yesterday and are now on their way to
>>you... I read it last night and wrote "misleading" and "wrong" in the
>>margins in quite a few places !
>>
>>   John
>>
>>At 10:04 05/05/00 +0100, you wrote:
>>>John,
>>>
>>>I can make a London lunch on either 19 or 20, but with a strong preference
>>>for 20th. Trevor could also make both days if necessary. By then we will
>>>have got further with the Tyndall contract so it would useful to talk with
>>>Esso (do you have a copy of the Exxonmobil booklet referred to?).
>>>
>>>Let me know how this proceeds,
>>>
>>>Mike
>>>
>>>
>>>
>>
>

169. 0962366892.txt
##
##########

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Noguer, Maria" <mnoguer@meto.gov.uk>,'tar10 ' <tar10@egs.uct.ac.za>
Subject: Re: Precipitation map for the Box
Date: Fri, 30 Jun 2000 08:08:12 +0100

<x-flowed>
Dear Chapter 10,

Sorry I missed out on the meeting.

In general I like the proposed Figure and suggested Box contents (and I
particularly agree that the diversity of downscaling methods and results
precludes using them as a basis of consolidated regional conclusions). I
also agree with others that it looks better with the +- signs
included. However, there are 2-3 points that concern me, mostly from the

perspective of climate scenarios (Chapter 13 - and also Chapter 9).

- it needs to be made very clear if any numbers are cited in the Box (e.g. 2-6degC for continental warming) that these refer to only *one* forcing scenario, namely 1% p.a.

- rather than talk about GHG and SUL I would suggest the more conventional nomenclature of GG and GS (the SUL runs are not just SUL forcing of course, which might give that impression).

- another very important caveat concerns the GS (SUL) results - these all stem from IS92a type aerosol forcing a la IPCC SAR. Most of the new SRES forcings used in TAR and Chapter 9 for example have much smaller or even positive SO4 forcing relative to 1990. In principle this could actually switch the sign of the precip. changes in some regions. There is the danger of inconsistency here between Chapter 9 (TAR aerosol effects) and Chapter 10 Box (SAR aerosol effects) if this is not carefully explained. For example, in CAM and JJA it appears that aerosols switch the P change from 'strongly negative' to being 'uncertain' - but this is only for IS92a aerosol forcing: it is not a conclusion that would be valid for SRES aerosol forcing!

- as Filippo says, another key uncertainty not represented in the Box is forcing uncertainty - again, Chapter 9 present a wide range of Tglobal results, part of which relates to prior assumptions about which SRES forcing materialises. We do a disservice if we give the impression in Chapter 10 Box that these regional responses are independent of what future forcing materialises. For example, under the lowest SRES forcing (B1) the precip. response in some regions would revert back to being very small and therefore indistinguishable from noise.

- with regard to temperature and Filippo's comment, Chapter 9 has global maps of T change, averaged across the standard set of AOGCM experiments (ranges are also shown). This is in effect the information being sought-for by readers of Chapter 10 is it not. I would have thought that back-references in the Box to Figure 9.9 would be sensible.

See you all in Victoria,

Mike

At 14:35 27/06/00 +0100, Noguer, Maria wrote:
>Dear all,
>
>Here are two examples that Paul has put together regarding the map of
>changes in precipitation drawn from Figure 10.5
>Do you think it works? Please send me any suggestions that you may.
>
> <<Fig01a.pdf>> <<Fig01b.pdf>>
>
>Regards,
>
>Maria
>
>***
>Dr. Maria Noguer
>IPCC WGI Technical Support Unit
>Hadley Centre for Climate Prediction and Research
>The Met Office
>London Road
>Bracknell
>Berkshire, RG12 2SY
>UK

>
>Tel: +44 (0) 1344 854938
>Fax: +44 (0) 1344 856912
>e-mail: mnoguer@meto.gov.uk
>www.met-office.gov.uk
>www.ipcc.ch
>**
>
>
>

</x-flowed>

170. 0962724639.txt
###
##########

From: stepan <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Manuskript of papes
Date: Tue, 4 Jul 2000 11:30:39 +0600
Reply-to: stepan <stepan@ipae.uran.ru>
Cc: t.osborn@uea.ac.uk

Dear Keith and Tim,

Thank you for the papers which I have received some days ago. They produced an impression on me. It is really a big job. I do not have time now to evaluate in details the results obtained. I want to make two remarks only.

First, I think, that the method of standardisation is very interesting, but it is disputable for the regions and sites where trees grow under extreme climatic conditions, for example at the polar timberline in Siberia. In such conditions the shape of age curve and the age of maximum growth are very changeable in different trees growing at the same site. It will be very interesting if you can present the age curve obtained for one such site, for example for the North Taymir Peninsula.

Second, I do not agree that in the northern Siberia the 15th century summers were warmer than those observed in the 20th century, at least in the Western and Middle Siberia. May be it is a result of stundartisation?

We suggest to inscibe in list of references the next papers:

1. Vaganov E.A., Shiyatov S.G., Mazepa V.S. Dendroclimatic study in Ural-Siberian Subarctic. - Novosibirsk "Nauka", Siberian Publishing Firm RAS, 1996. - 246 p. (in Russian).

2. Mazepa V.S. Influence of Precipitations on Tree-Ring Growth of Coniferous in Subarctic Regions of Eurasia //Lesovedenie, No. 6, 1999. - P.14-21. (in Russian).

Abstract. Influence of precipitation on tree-ring variability of coniferous trees in Subarctic regions of Eurasia has been shown. Depending on the region, significant ecological factor for tree growth are precipitation of autumn-winter, winter-spring and summer periods. Ecological explanation of such influence has been given. On the base of relationships between tree-rings and rainfall the reconstructions of precipitation in different regions of

Subarctic for last 200 years have been developed.

3. Mazepa V.S. Spatial Reconstruction of Summer Air Temperature in the North of the West Siberia since 1690 on the base of Tree-Ring Data. //Siberian ecological journal, No. 2, 1999. - P.175-183. (in Russian).

Abstract. Opportunity of annual reconstruction of summer thermal conditions from Polar Urals (64-68°N, 64-68°E) up to Yenisei River (66-70°N, 86-89°E) is caused by high and sufficiently stable relationship between coniferous tree growth (Larix sibirica, Picea obovata) and corresponding climatic factors. Percent variance in tree-ring chronologies explained by climate (June-July temperature) in this extreme for growth of trees area reaches 50%. Spatial reconstruction of air summer temperature on the base of point reconstruction for 11 corresponding meteostations has been developed. Analysis of reconstructed temperatures has shown their significant changes for last 300 years. The most strong fall of temperatures was observed in XIX century, but rise in temperature was observed in XVIII and XX centuries.

4. Mazepa V.S. Dendroclimatic reconstructing air summer temperatures since 1690 in subarctic regions of siberia. //Problems of ecological monitoring and ecosystem modelling, Volume XVII. - St.Petersburg Gidrometeoizdat, 2000. - P.170-187. (in Russian).

Abstract. The further development of many-year dendroclimatic study carried out in subarctic regions of Siberia and on the polar timberline, is given in this paper. Climatic factors determining the year-to-year and many-yeared tree-ring width variability were revealed, using multiple regression models. The spatial year-to-year reconstruction of air summer temperatures was made on the base of available dendroclimatic network. The reliability of spatial summer temperatures reconstruction in the boreal zone of the Urals and Siberia was evaluated. The temporal dendroclimatic zoning of the area investigated was carried out according to the chronology similarity. The regional border changes, depending on warm and cold periods, were shown. Five regional chronologies showing the nature of summer months thermic regime variability were developed. Extremely cold and warm periods were revealed. The coldest periods are: the first half of XVII and XIX centuries. The warmest periods are: the second half of XVII, XVIII and middle of XX centuries.

To-day R. Hantemirov and A. Surkov will go to the Yamal Peninsula for subfossil wood collecting. I and V. Mazepa will go to the Polar Ural Mountains in some days.

Best regards,
Stepan mailto:stepan@ipae.uran.ru

171. 0962818260.txt
##

From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: m.hulme@uea.ac.uk
Subject: Shell
Date: Wed, 05 Jul 2000 13:31:00 +0100

Reply-to: m.kelly@uea.ac.uk
Cc: t.oriordan@uea.ac.uk, t.o'riordan@uea.ac.uk

Mike
Had a very good meeting with Shell yesterday. Only a minor part of the agenda, but I expect they will accept an invitation to act as a strategic partner and will contribute to a studentship fund though under certain conditions. I now have to wait for the top-level soundings at their end after the meeting to result in a response. We, however, have to discuss asap what a strategic partnership means, what a studentship fund is, etc, etc. By email? In person?
I hear that Shell's name came up at the TC meeting. I'm ccing this to Tim who I think was involved in that discussion so all concerned know not to make an independent approach at this stage without consulting me!
I'm talking to Shell International's climate change team but this approach will do equally for the new foundation as it's only one step or so off Shell's equivalent of a board level. I do know a little about the Fdn and what kind of projects they are looking for. It could be relevant for the new building, incidentally, though opinions are mixed as to whether it's within the remit.
Regards
Mick
____

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Email: m.kelly@uea.ac.uk
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____

172. 0963233839.txt
##
##########

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: Frank Oldfield <frank.oldfield@pages.unibe.ch>
Subject: Re: the ghost of futures past
Date: Mon, 10 Jul 2000 08:57:19 -0400
Cc: alverson@pages.unibe.ch, jto@u.arizona.edu, k.briffa@uea.ac.uk, mhughes@ltrr.arizona.edu, pedersen@eos.ubc.ca, whitlock@oregon.uoregon.edu, mann@multiproxy.evsc.virginia.edu

<x-flowed>Sorry this kept you awake...but I have also found it a rather alarming graph. First, a disclaimer/explanation.
The graph patches together 3 things: Mann et al NH mean annual temps + 2 sigma standard error for AD1000-1980, + instrumental data for 1981-1998 + IPCC ("do not quote, do not cite" projections for GLOBAL temperature for the next 100 years, relative to 1998. The range of shading represents several models of projected emissions scenarios as input to GCMs, but the GCM mean global temperature output (as I understand it) was then reproduced by Sarah Raper's energy balance model, and it is those values that are plotted. Keith pointed this out to me; I need to go back & read the IPCC TAR to understand why they did that, but it makes no difference to the first order result....neither does it matter that the projection is global rather than NH....the important point is that the range of estimates far exceeds the range estimated by Mann et al in their reconstruction. Keith also said that the Hadley Center GCM runs are being archived at CRU, so it ought to be possible to get that data and simply compute the NH variability

for the projected period & add that to the figure, but it will not add much real information. However, getting such data would allow us to extract (say) a summer regional series for the Arctic and to then plot it versus the Holocene melt record from Agassiz ice cap....or....well, you can see other possiblities.

[......At this point Keith Alverson throws up his hands in despair at the ignorance of non-model amateurs...]

But there are real questions to be asked of the paleo reconstruction. First, I should point out that we calibrated versus 1902-1980, then "verified" the approach using an independent data set for 1854-1901. The results were good, giving me confidence that if we had a comparable proxy data set for post-1980 (we don't!) our proxy-based reconstruction would capture that period well. Unfortunately, the proxy network we used has not been updated, and furthermore there are many/some/ tree ring sites where there has been a "decoupling" between the long-term relationship between climate and tree growth, so that things fall apart in recent decades....this makes it very difficult to demonstrate what I just claimed. We can only call on evidence from many other proxies for "unprecedented" states in recent years (e.g. glaciers, isotopes in tropical ice etc..). But there are (at least) two other problems -- Keith Briffa points out that the very strong trend in the 20th century calibration period accounts for much of the success of our calibration and makes it unlikely that we would be able be able to reconstruct such an extraordinary period as the 1990s with much success (I may be mis-quoting him somewhat, but that is the general thrust of his criticism). Indeed, in the verification period, the biggest "miss" was an apparently very warm year in the late 19th century that we did not get right at all. This makes criticisms of the "antis" difficult to respond to (they have not yet risen to this level of sophistication, but they are "on the scent"). Furthermore, it may be that Mann et al simply don't have the long-term trend right, due to underestimation of low frequency info. in the (very few) proxies that we used. We tried to demonstrate that this was not a problem of the tree ring data we used by re-running the reconstruction with & without tree rings, and indeed the two efforts were very similar -- but we could only do this back to about 1700. Whether we have the 1000 year trend right is far less certain (& one reason why I hedge my bets on whether there were any periods in Medieval times that might have been "warm", to the irritation of my co-authors!). So, possibly if you crank up the trend over 1000 years, you find that the envelope of uncertainty is comparable with at least some of the future scenarios, which of course begs the question as to what the likely forcing was 1000 years ago. (My money is firmly on an increase in solar irradiance, based on the 10-Be data..). Another issue is whether we have estimated the totality of uncertainty in the long-term data set used -- maybe the envelope is really much larger, due to inherent characteristics of the proxy data themselves....again this would cause the past and future envelopes to overlap.

In Ch 7 we will try to discuss some of these issues, in the limited space available. Perhaps the best thing at this stage is to simply point out the inherent uncertainties and point the way towards how these uncertainties can be reduced. Malcolm & I are working with Mike Mann to do just that.

I would welcome other thoughts and comments on any of this!

Ray

At 01:34 PM 7/10/00 +0200, you wrote:
>Salut mes amis,
>

>I've lost sleep fussing about the figure coupling Mann et al. (or any
>alternative climate-history time series) to the IPCC scenarios. It seems to
>me to encapsulate the whole past-future philosophical dilemma that bugs me
>on and off (Ray - don't stop reading just yet!), to provide potentially the
>most powerful peg to hang much of PAGES future on, at least in the eyes of
>funding agents, and, by the same token, to offer more hostages to fortune
>for the politically motivated and malicious. It also links closely to the
>concept of being inside or outside 'the envelope' - which begs all kinds of
>notions of definition. Given what I see as its its prime importance, I
>therefore feel the need to understand the whole thing better. I don't know
>how to help move things forward and my ideas, if they have any effect at
>all, will probably do the reverse. At least I might get more sleep having
>unloaded them, so here goes......
>
>The questions in my mind centre round the following issues. If I've got any
>one of them wrong, what follows in each section can be disregarded or (more
>kindly) set straight for my benefit.
>
>1. How can we justify bridging proxy-based reconstruction via the last bit
>of instrumental time series to future model-based scenarios.
>
>2. How can the incompatibilities and logical inconsistencies inherent in
>the past-future comparisons be reduced?
>
>3. More specifically, what forms of translation between what we know about
>the past and the scenarios developed for the future deal adequately with
>uncertainty and variability on either side of the 'contemporary hinge' in a
>way that improves comparability across the hinge.
>
>4. Which, if any, scenarios place our future in or out of 'the envelope'
>in terms of experienced climate as distinct from calculated forcing? This
>idea of an envelope is an engaging concept, easy to state in a quick and
>sexy way (therefore both attractive and dangerous); the future could leave
>us hoisted by our own petard unless it is given a lot more thought.
>
>1. I am more or less assuming that this can already be addressed from data
>available and calculations completed, by pointing to robust calibration
>over the chosen time interval and perhaps looking separately at variability
>pre 1970, if the last 3 decades really do seem to have distorted the
>response signatures for whatever reasons. I imagine developing this line of
>argument could feed into the 'detection' theme in significant ways.
>
>2 & 3. This is where life gets complicated. For the past we have biases,
>error bars that combine sources of uncertainty, and temporal variability.
>For the future we have no variability, simply a smooth, mean, monotonic
>trend to a target 'equilibrium' date. Bandwidths of uncertainty reflect
>model construction and behaviour. So we are comparing apples and oranges
>when we make any statement about the significance of the past record for
>the future on the basis of the graph. Are there ways of partially
>overcoming this by developing different interactions between past data and
>future models?
>
>My own thinking runs as follows: Take variability. Do we need to wait for
>models to capture this before building it into future scenarios? This seems
>unnecessary to me, especially since past variability will be the validation
>target for the models. Is there really no way of building past variability
>into the future projections? One approach would be to first smooth the
>past record on the same time-span as the future scenarios. This would get
>us to first base in terms of comparability, but a very dull and pretty
>useless first base in and of itself. It would, however, allow all kinds of
>calculations of inter-annual variability relative to a mean time line of
>the 'right' length. This in turn could be used in several ways, for
>example:

>   - build the total range of past variability into the uncertainty
>bands of each future scenario.
>   - take the 30,50 or 100 year period (depending on the scenario for
>comparison) during which
>   there was the greatest net variability, or the greatest net fall
>in Temperature, or the
>   greatest net increase in T. and superimpose/add this data-based
>variability on the mean
>   trends.
>   - take the n-greatest positive anomalies relative to the trend and
>use them to define an upper
>   limit of natural variability to compare with the (to my mind)
>more realistic future scenarios.
>
>These and cleverer variants I cannot begin to think up seem to me to hold
>out the possibility of linking future projections of GHG forcing with what
>we know about natrual variability in reasonably realistic ways and perhaps
>even of redefining the 'past data-future scenario' relationship in ways
>that benefit both the paleo-community and the quality of future
>projections.
>
>4. I also think the above kinds of exercise might eventually lead us
>towards a better definition of 'the envelope' and more confidence in
>deciding what is outside and what is not. The same sort of approach can be
>taken towards projections of P/E I imagine and, more particularly, at
>regional rather than global or hemispheric level.
>
>Sorry if all this sounds stupid or obvious. I got afflicted with the 'need
>to share' bug.
>
>Frank
>
>
>____
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mate/climate.html
Paleoclimatology Book Web Site (1999):
<http://www.geo.umass.edu/climate/paleo/html>http://www.geo.umass.edu/climat
e/paleo/html

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173. 0963250650.txt
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##########

From: "Michael E. Mann" <mann@holocene.evsc.virginia.edu>
To: Frank Oldfield <frank.oldfield@pages.unibe.ch>
Subject: Re: the ghost of futures past
Date: Mon, 10 Jul 2000 13:37:30 -0400
Cc: rbradley@geo.umass.edu, jto@u.arizona.edu, keith.alverson@pages.unibe.ch,
k.briffa@uea.ac.uk, pedersen@eos.ubc.ca, mhughes@ltrr.arizona.edu,
whitlock@oregon.uoregon.edu

Thanks Frank,

My apologies...

Sorry, no, I hadn't looked in detail at your original email to Ray,
only his response, and simply wanted to note that others have already
jumped on this bandwagon, so Ray deserves neither all the blame,
nor all the glory, depending on your perspective :)

And, as I stated, IPCC clearly considers such a plot not appropriate
for prime time--so you won't see anything like this in the TAR.

WHat I find most useful, howevever, along the lines of what you discuss, is
using empirical reconstructions as a baseline for comparison against model
simulations of both free and forced variability. A number of studies have
attempted this recently, and the results are encouraging from the point of
view that (a) the coupled models appear to be getting the internal
variability of mean global/hemispheric temperatures about right [this leads
us in the
direction of having greater faith in future scenarios from such models]
and (b) the models,forced with paleoestimates of past volcanic, solar, and
GHG radiative forcings, appear to be able to explain more than 50% of the
variance in the paleo temperature reconstructions. A paper to appear in
this Friday's "Science" by Tom Crowley describes some impressive results
along these lines.

It is agreed that hydrological change and regional temperature anomalies
superimposed on any large-scale temperature changes are of key
importance from any practical point of view. And I think this
is what we're all working towards, more regionally detailed
reconstructions of climate fields (temperature, drought, slp, etc.)
in past centuries. Clearly more high-resolution proxy evidence is
necessary, in both time and space. I make many of these very points in a
"Perspectives"
article also to appear in Science on Friday, accompanying Tom Crowley's
article.

Will appreciate any comments on it. Hope the above provides some
clarification.

cheers,

mike

At 06:59 PM 7/10/00 +0200, you wrote:
>Hi Mike,
>
>Not sure if your reply implied you were taking my points seriously or not -
>I'm not even sure if Ray sent them on to you or you just received his

>reply! My reactions to the graphs on the website are that the temperature
>one does not address my points (but it does not aim to and I fully agree
>that if the projections are sufficiently reliable it hardly needs to!),
>that P/E is likely to be much more important than temperature per se and
>that the historical sea-level curve is not really acceptable - very much
>more high resolution work needs to be done on that before we have any real
>sense of past variability on decadal to century timescales.
>
>Cheers,
>
>Frank
>
>____
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>
>
>
>At 06:59 PM 7/10/00 +0200, Frank Oldfield wrote:
>Hi Mike,
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>Not sure if your reply implied you were taking my points seriously or not -
>I'm not even sure if Ray sent them on to you or you just received his
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>one does not address my points (but it does not aim to and I fully agree
>that if the projections are sufficiently reliable it hardly needs to!),
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>that the historical sea-level curve is not really acceptable - very much
>more high resolution work needs to be done on that before we have any real
>sense of past variability on decadal to century timescales.
>
>Cheers,
>
>Frank
>
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http://www.evsc.virginia.edu/faculty/people/mann.html

174. 0965139790.txt
###
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: keith.alverson@pages.unibe.ch
Subject: glossy
Date: Tue Aug 1 10:23:10 2000

Keith
I've sent you a few slides taken by Hakan Grudd as promised . I think these should be supplemented by a bit of a colourful timeseries - part of a chronology. It could be a piece of the Tornetrask series (northern Sweden) from where the pictures are taken - but I think a section of the 3-region average (Tornetrask,Yamal, Taimyr) possibly showing the 563 A.D. would be better. So I am sending a couple post script files and a suggested colour scheme. What do you think? I suggest a one hundred year section of the average series , showing annual values. Note that in these Figures , A.D. 536 is marked by a filled triange. Just showing the initiation of a dramatic cooling in A.D. 536 and the widespread cold summers of the 540's (a major vocano? if perhaps not as David Keys makes out in his recent book), or a comet (as Mike Baille says in his?) , is quite appealing.
Keith

175. 0965416206.txt
###
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: joos <joos@climate.unibe.ch>
Subject: Re: climate reconstructions
Date: Fri Aug 4 15:10:06 2000

Dear Fortunat
I am pleased to hear from you. I have still not been in touch about the data I showed you
in Vienna! As for your question - of course I will send the series you mention - but it is
only an average of three regional tree-ring chronologies (Northern Sweden, Yamal,Taimyr)
and not calibrated in terms of temperature. Nevertheless, it is representative of summer
warmth over a large Russian region, We have recently submitted a paper describing a
different standardization approach (for preserving low frequency variance) applied to a
big high-latitude network of tree-density data. This yields regional (up to 600-year)
calibrated reconstructions and a hemispheric curve - all representing april-sept season. I
have asked my colleague Tim Osborn here to send the data and a copy of the papers to you, I
am on the verge of leaving for 2 weeks so if you need more information contact him.
As for other areas of the world - Phil Jones has an alternative Hemisphere curve

and there
are some southern hemisphere chronologies (temp. sensitive). There are short
precip
reconstructions for several spots - but systematic Palmer Drought Indices for the
U.S. from
about 1700. I will be happy to talk on the phone about all these in two weeks.
best wishes
Keith
At 11:01 AM 7/19/00 +0200, you wrote:

Dear Keith,
How are you? Hope everything is going well.
I am writing because I am interested in your climate reconstruction for
the last millennium.
The Etheridge ice core data of CO2 indicate that CO2 was below average
in the 17th and 18th centuries by a few ppm. Very few (1-2 points) of
ice core C13 data (Francey tellus, 99) suggest that this drawdown was
caused by additional terrestrial carbon storage (Joos et al, GRL, 99;
Trudinger, Tellus, 99). We try to investigate this suggestion using the
Lund-Potsdam-Jena dynamical global vegetation model (LPJ-DGVM).
A diploma student of mine, Philippe Bruegger, has used the Mann et al
annual mean temperature patterns (2 EOFs only) in combination with the
Etheridge CO2 record to drive the LPJ model. Instead of absorbing
carbon, the model is releasing carbon due to a reduced CO2 fertilization
effect in the model that outweights any climatic effects. Thus, the
model results is clearly not compatible with the ice core results.
Obviously, the study is hampered by the limitation of the climate
reconstruction (as well as by the few C13 ice core data). Instead of
changes in monthly values of Temp and precip (and cloud cover) changes
in ANNUAL mean temperature were used to force LPJ.
Could you or Phil Jones provide alternative forcing fields that focus
e.g. more on summer temperature? Any info about precipitation?
I would also appreciate very much to obtain reprints of your most recent
articles, namely the article in Quaternary Science Rev. 2000.
Thanks for any help you can provide.
Regards, Fortunat
--
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References

1. http://www.climate.unibe.ch/~joos/

176. 0965671134.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: tom crowley <tom@ocean.tamu.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: mill records
Date: Mon Aug 7 13:58:54 2000

Tom and Mike,
What Tom said is essentially correct. Tim Osborn here recalibrated
each series, as a composite, against the same NH series for the April-
Sept average north of 20N (using land only data). All this does is

rescale the series as it is simple regression (y=ax+b). Because y is based on temps wrt 61-90 this means that the axis is then wrt 61-90. Doing this we can then add the same instrumental temp series. It also brings the series together and the web page was just for illustrative purposes. For Mike's series you get pretty much the same result by subtracting 0.12 from Mike's numbers as this is the difference between Mike's base period and 1961-90.
There is nothing sinister going on ! I'll summarise this to Rob.

Cheers
Phil

PS I seem to be stirring up loads of emails about historical data. You are both on those emails so you can see what crap is being written and my (time wasting for me) replies. Apologies for replying. I should know better and keep quiet. We can all expect more of this if IPCC stays in roughly the same form pre-Victoria. It's relatively easy to knock historical records, so as long as it gets no worse than this we'll be fine.

177. 0965750123.txt
##

From: "S. Fred Singer" <singer@sepp.org>
To: "Raymond S. Bradley" <rbradley@geo.umass.edu>
Subject: Re:Your msg about climate/energy policy
Date: Tue, 08 Aug 2000 11:55:23 -0400
Cc: mann@virginia.edu, pjm8x@rootboy.nhes.com

<x-flowed>Dear Ray

You sent me this op-ed (?) (Letter to editor?) about the need to convert the US from a carbon-based economy to a hydrogen-based economy. I can't guess why you wanted me to know your views, but it does help me to better understand what motivates your scientific work and judgment. It also throws some doubt about your impartiality in promoting the "hockey stick' temperature curve that a number of us have been critical of.

In any case, I doubt if espousal of this energy policy will help BP and ARCO discover a source of hydrogen somewhere.

You quote the "progressive" Business Council approvingly: "We accept the views of most scientists that enough is known about the science and environmental impacts of climate change for us to take actions to address its consequences." And from BP chairman : "the time to consider the policy dimensions of policy change is not when the link between greenhouse gases and climate change is conclusively proven, but when the possibility cannot be discounted and is taken seriously by the society of which we are part."

I note that BP and ARCO are still out there exploring for oil; they don't seem to be quite ready yet to put real money where their mouth is.

You call for the US to take leadership in stabilizing the climate. Perhaps the government will turn to you to learn how to do this. A far less ambitious goal would be to stabilize the atmospheric concentration of CO2. According to the IPCC this would require an emission reduction of 60 to 80 percent (with respect to 1990) --- WORLDWIDE.

Have you ever considered the consequences of such a policy -- assuming it could really be adopted?

Best wishes ,

Fred
****
At 10:34 AM 8/1/00 -0400, you wrote:

> WASHINGTON, DC -- In August 1997, a few months before the Kyoto
> Conference on Climate Change, the Global Climate Coalition (GCC)
> helped launch a massive advertising campaign designed to prevent the
> United States from endorsing any meaningful agreement to reduce global
> carbon emissions. This group included in its ranks some of the world's most
> powerful corporations and trade associations involved with fossil
> fuels. The
> campaign effectively undermined public support of U.S. efforts to lead the
> international effort to stabilize climate.
> While the public image of the GCC was that of a unified group, there was
> dissent. John Browne, Chairman of British Petroleum, on May 19, 1997,
> announced that "the time to consider the policy dimensions of policy change
> is not when the link between greenhouse gases and climate change is
> conclusively proven, but when the possibility cannot be discounted and is
> taken seriously by the society of which we are part. We in BP have
> reached that point."
> BP withdrew from the Global Climate Coalition. Dupont had already left.
> The following year, Royal Dutch Shell left.
> In 1999, Ford withdrew from the GCC. A company spokesman noted,
> "Over the course of time, membership in the Global Climate Coalition has
> become something of an impediment for Ford Motor Company to
> achieving our environmental objectives."
> In rapid succession in the early months of 2000, Daimler Chrysler, Texaco,
> and General Motors announced that they too were leaving the Coalition.
> This accelerating exodus reflected the conflict emerging within GCC ranks
> between firms that were clinging to the past and those that were planning
> for the future.
> Some of the exiting companies, such as BP Amoco, Shell, and Dupont,
> joined a progressive new group, the Business Environmental Leadership
> Council, which says, "We accept the views of most scientists that enough is
> known about the science and environmental impacts of climate change for
> us to take actions to address its consequences."
> Membership requires companies to have programs for reducing carbon
> emissions. BP Amoco, for example, plans to bring its carbon emissions to
> 10 percent below its 1990 level by 2010, exceeding the Kyoto goal of
> roughly 5 percent for industrial countries.
> Dupont has already cut its 1990 greenhouse gas emissions by 45 percent
> and plans to reduce them by 65 percent by 2010.
> There is a growing acceptance among the key energy players that the
> world is in the early stages of the transition from a carbon-based to a
> hydrogen-based energy economy. In February 1999, ARCO CEO
> Michael Bowlin said, "We've embarked on the beginning of the Last Days
> of the Age of Oil." He then discussed the need to convert our
> carbon-based energy economy into a hydrogen-based energy economy.
> With the organization that so effectively undermined U.S. leadership in
> Kyoto no longer a dominant player in the global climate debate, the
> stage is
> set for the United States to resume leadership of the global climate
> stabilization effort.

>Raymond S. Bradley
>Professor and Head of Department
>Department of Geosciences
>University of Massachusetts
>Amherst, MA 01003-5820
>
>Tel: 413-545-2120
>Fax: 413-545-1200
>Climate System Research Center: 413-545-0659

>Climate System Research Center Web Page:
><http://www.geo.umass.edu/climate/climate.html>
>Paleoclimatology Book Web Site (1999):
>http://www.geo.umass.edu/climate/paleo/html

S. Fred Singer, President
Science & Environmental Policy Project
9812 Doulton Court
Fairfax, VA 22032
http://www.sepp.org
Tel: 703-503-5064
e-fax 815-461-7448 (your fax will be sent as email to my
computer)

****
"The improver of natural knowledge absolutely refuses
to acknowledge authority, as such. For him, scepticism
is the highest of duties; blind faith the one unpardonable sin."
Thomas H. Huxley
****
  "That theory is worthless. It isn't even wrong!" - W. Pauli
****

</x-flowed>

178. 0966015630.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: Re: FW: Mann etal
Date: Fri, 11 Aug 2000 13:40:30 +0100
Cc: jfbmitchell@meto.gov.uk,k.briffa@uea.ac.uk

Chris and John (and Mike for info),
   I'm basically reiterating Mike's email. There seem to be two lots of
suggestions doing the rounds. Both are basically groundless.

1. Recent paleo doesn't show warming.

   This basically stems back to Keith Briffa's paper in Nature in 1998
   (Vol 391, pp678-682). In this it was shown that northern boreal forest
   conifers don't pick up all the observed warming since about the late
   1950s. It was suggested that some other factor or a combination of
   factors related to human-induced pollution (e.g. nitrogen deposition,
   higher levels of CO2, ozone depletion etc). Hence in a new paper
   submitted to JGR recently we develop a new standardization approach
   (called age banding) and produce a large-scale reconstruction
   (calibrated over the period 1881-1960 against NH land north of 20N)
   back to 1402. If you want a copy of this can you email Keith and he'll
   send copies once he's back from holiday.

   This background is to illustrate how Singer et al distort things. The
   new reconstruction only runs to 1960 as did earlier ones based solely
   on tree-ring density. All the other long series (Mike's, Tom Crowley's
   and mine) include other proxy information (ice cores, corals,
   historical records, sediments and early instrumental records as well as
   tree-ring width data, which are only marginally affected). All these
   series end around 1980 or in the early 1980s. We don't have paleo data

for much of the last 20 years. It would require tremendous effort and resources to update a lot of the paleo series because they were collected during the 1970s/early 1980s.

It is possible to add the instrumental series on from about 1980 (Mike sought of did this in his Nature article to say 1998 was the warmest of the millennium - and I did something similar in Rev. Geophys.) but there is no way Singer can say the proxy data doesn't record the last 20 years of warming, as we don't have enough of the proxy series after about 1980.

http://www.co2.science.org/edit/editor.html takes the argument further saying that as trees don't see all the warming since about 1960 the instrumental records recently must be in error (i.e. this group believes the trees and not the instrumental records). This piece by Idso and Idso seems to want to have the argument whichever suits them.

2. Everyone knows it was cooler during the Little Ice Age and warmer in the Medieval Warm Period.

All of the millennial-long reconstructions show these features, but they are just less pronounced than people believed in the 1960s and 1970s, when there was much less paleo data and its spatial extent was limited to the eastern US/N.Atlantic/European and Far East areas. The issue seems to revolve around the average temperatures we have for earlier centuries in the millennium. I use the argument that for the instrumental period we need sites located over much of the NH (land and marine) regions in order to claim we have a reasonable record for the whole hemisphere. We wouldn't dream of extending the NH series based on longer European records and in the extreme just CET, so with the paleo data we need records from as many regions as possible. The coverage still could be better, but it is far better than it was 25 years ago, when the ideas embodied in the MWP and LIA became sort of mainstream.

The typical comments I've heard, generally relate to the MWP, and say that crops and vines were grown further north than they are now (the vines grown in York in Viking times etc). Similarly, statements about frost fairs and freezing of the Baltic so armies could cross etc. Frost fairs on the Thames in London occurred more readily because the tidal limit was at the old London Bridge (the 5ft weir under it). The bridge was rebuilt around the 1840s and the frost fairs stopped. If statements continue to be based on historical accounts they will be easy to knock down with all the usual phrases such as the need for contemporary sources, reliable chroniclers and annalists, who witnessed the events rather than through hearsay. As you all know various people in CRU (maybe less so now) have considerable experience in dealing with this type of data. Christian Pfister also has a lifetime of experience of this. There is a paper coming out from the CRU conference with a reconstruction of summer and winter temps for Holland back to about AD 800, which shows the 20th century warmer than all others. Evidence is sparser before 1400 but the workers at KNMI (Aryan van Engelen et al.) take all this into account.

I hope this is of use and hasn't been a total waste of time.

In Victoria last month, did you discuss how the policymaker's summary will report the millennial temperature series ? Are there any tentative phrases you're working on a la Balance of evidence etc ? Is Chapter 12 thinking of a new sentence to supercede the above ? Any sentence on the millennium record should be in Ch. 2.

Cheers
Phil

Prof. Phil Jones
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School of Environmental Sciences   Fax +44 (0) 1603 507784
University of East Anglia
Norwich   Email   p.jones@uea.ac.uk
NR4 7TJ
UK

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179. 0966633586.txt
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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Benjamin Santer <e782144@popgun.llnl.gov>
Subject: JGR paper
Date: Fri Aug 18 17:19:46 2000

Ben,
Here a few main points about the paper. I've ignored minor English/wording things I spotted.

p4 It seems better to put the other anthro forcings before the natural get discussed. (top of page). ie Other heteorogeneous.. sentence should be before Stratospheric aerosols.

p4 Bottom. Could reference Delworth et al to illustrate the 'perfect' model argument. They reproduced reality 1 out of 5 attempts.

p5 Don't like phenomenology of ENSO, change to ENSO sequences ?

p6 middle. Emphasise that withe models you can look at a lot longer series.

p6 bottom. Whether the model was really 'perfect' Michaels would find some problem.

p7 2/3rd way down. Say something about Santer et al (2000a).

p9 Don't think you need to say you got the SOI from CRU.

p10 ECHAM4 has solar, but how much does it change by. Or is it constant ?

p11 end of 2. Presumably in combining the SAT and SST you used anomalies. Worthwhile saying.

p12-15 Section 3 gets to read like a recipe. It is important, but it might be better as an Appendix. Also I guess the amount of detail depends on success of other submissions. I think the section needs reworking a bit as the style changes somewhat.

Have you considered whether alpha and tau and t(ramp) can differ by a month between the surface and 2LT.

The lag you use is 7 months. The science paper of Tom's uses 6 months.

In the later tables I wasn't clear how raw and nofilt relate to each other. I guess all the Tables need longer captions with more explanation. I couldn't figure out what the () numbers referred to in the Tables.

p17 I wonder if it's possible to show in a diagram that the iterative scheme works and you're getting to a global rather local minimum.

p19 The higher 'ratios' get nearer to my 2, but only at the high end.

p20 The last 4 numbers in Table 3 have been multiplied by 0.1 .

p23 An interesting aside would be to show in one of the Tables how much change in the observations is due to volcanoes (ie show how much cooling due to this there has been). People will quote this value. It shows that 'natural' factors (solar/volcanoes) have led to cooling as solar effects will be very small over this time.

p24 Emphasise later that models and obs all show 2LT level changes more than surface.

p24 Say something about how good ECHAM4 is for ENSO, or refer to a paper.

p25-33 All good stuff, but it does take a time to read. Not a very helpful comment, I know, but I'm being a referee.

p33 Does Fig 7 use the same data as in Fig 5 ? One shwing things through time, the other as a distribution.

p35 PCM crept into the Hamburg section, so it should be said here when the GISS section starts.

p38 Quantify the volcanic cooling. I mentioned this earlier.

p39 Not clear what independent components are wrt Smith et al (2000).

p42 Surface data has errors too.

p43 The last sentence of the acknowledgements is like a red rag to a bull for Michaels. Even the perceptive adjective will not placate him.

Have to go home now. I think I've covered most things I noticed.

Have a good weekend !

Cheers
Phil

180. 0967041809.txt
##

From: Stephen H Schneider <shs@stanford.edu>
To: tkarl@ncdc.noaa.gov
Subject: Re: THC collapse
Date: Wed, 23 Aug 2000 10:43:29 -0700 (PDT)
Cc: Thomas Stocker <stocker@climate.unibe.ch>, Jerry Meehl <meehl@meeker.ucar.edu>, Timothy Carter <tim.carter@vyh.fi>, maureen.joseph@eci.ox.ac.uk, lindam@ucar.edu, m.hulme@uea.ac.uk, peter.whetton@dar.csiro.au, giorgi@ictp.trieste.it, cubasch@dkrz.de, ckfolland@meto.gov.uk, hewitson@egs.uct.ac.za, "Stouffer, Ron" <rjs@gfdl.gov>, DEASTERL@ncdc.noaa.gov

Great Tom, I think we are converging to much clearer meanings across
various cultures here. Please get the inconclusive out! By the way,
"possible" still has some logical issues as it is true for very large or
very small probabilities in principle, but if you define it clearly it is
probably OK--but "quite possible" conveys medium confidence better--but
then why not use medium confidence, as the 3 rounds of review over the
guidance paper concluded after going through exactly the kinds of
disucssions were having now. Thanks, Steve

On Wed, 23 Aug 2000 tkarl@ncdc.noaa.gov wrote:

>
>
> Steve, I agree with your assesement of inconclusive --- quite possible is
> much better and we use 'possible' in the US National Assessment. Surveys
> has shown that the term 'possible' is interpreted in this range by the
> public.
>
> Tom
>
>
>
>
> Stephen H Schneider <shs@stanford.edu> on 08/23/2000 03:02:33 AM
>
>
>
> To: Thomas Stocker <stocker@climate.unibe.ch>
>
> cc: Jerry Meehl <meehl@meeker.ucar.edu>, Timothy Carter
>   <tim.carter@vyh.fi>, maureen.joseph@eci.ox.ac.uk,
>   lindam@ucar.edu, m.hulme@uea.ac.uk,
>   peter.whetton@dar.csiro.au, giorgi@ictp.trieste.it,
>   Tom Karl/NCDC, cubasch@dkrz.de,
>   ckfolland@meto.gov.uk, hewitson@egs.uct.ac.za,
>   "Stouffer, Ron" <rjs@gfdl.gov>
>
>
>
> Subject: Re: THC collapse
>
>
>
>
>
>
>
>
>
> Hello all. I appreciate the improvement in the table from WG 1,
> particularly the inclusion of symmetrical confidence levels--but please
> get rid of the ridiculous "inconclusive" for the .34 to .66 subjective
> probability range. It will convey a completely differnt meaning to lay
> persons--read decisionmakers--since that probability range represents
> medium levels of confidence, not rare events. A phrase like "quite
> possible" is closer to popular lexicon, but inconclusive applies as well
> to very likely or very unlikely events and is undoubtedly going to be
> misinterpreted on the outside. I also appreciate the addition of
> increasing huricane intensities with warming moving out of the catch all
> less than .66 category it was in the SOD.
>   I do have some concerns with the THC issue as dealt with here--echoing
> the comments of Tim Carter and Thomas Stocker. I fully agree that the

> likelihood of a complete collapse in the THC by 2100 is very remote, but
> to leave it at that is very misleading to policymakers given than there is
> both empirical and modeling evidence that such events can be triggered by
> phenomena in one century, but the occurrence of the event may be delayed
> a century or two more. Given also that the likelihood of a collapse
> depends on several uncertain parameters--CO2 stabilization level, CO2
> buildup rate, climate sensitivity, hydrological sensitivity and initial
> THC overturning rates, it is inconceivable to me that we could be 99% sure
> of anything--implied by the "exceptionally unlikely" label--given the
> plausibility of an unhappy combo of climate sensitivity, slower than
> current A/OGCMs initial THC strength and more rapid CO2 increase
> scenarios. Also, if 21st century actions could trigger 22nd century
> irreversible consequences, it would be irresponsible of us to not mention
> this possibility in a footnote at least, and not to simply let the matter
> rest with a very low likelihood of a collapse wholly within the 21st
> century. So my view is to add a footnote to this effect and be sure to
> convey the many paramenters that are uncertain which determine the
> likelihood of this event.
>   Thanks again for the good work on this improtant table. Cheers, Steve
>
>
> On Wed, 23 Aug 2000, Thomas Stocker wrote:
>
> > DEar Jerry, Tim and Ron et al
> >
> > I agree that an abrupt collapse - abrupt meaning within less than a
> decade, say
> > - has not been simulated by any climate model (3D and intermediate
> complexity)
> > in response to increasing CO2. Some models do show for longer
> integrations a
> > complete collapse that occurs within about 100-150 years. If you put that
> into
> > context of the apparent stability of THC during the last 10,000 years or
> so,
> > this is pretty "abrupt".
> >
> > Following up on the discussion regarding THC collapse, I think the
> statement Ron
> > apparently added to Ch9 needs to be made more specific. In order to keep
> Ch7 and
> > Ch9 consistent, I propose to Ron the following revision:
> >
> > "It seems that the likelihood of a collapse of the THC by year 2100 is
> less
> > than previously thought in the SAR based on the AOGCM results to date."
> >
> > There is really no model basis to extend this statement beyond 2100 as
> evidenced
> > by the figures that we show in TAR. There are many models that now run up
> to
> > 2060, some up to 2100, but very few longer.
> >
> > Also I should add for your information, that we add to Ch7 a sentence:
> >
> > "Models with reduced THC appear to be more susceptible for a
> > shutdown."
> >
> > Models indicate that the THC becomes more susceptible to collapse if
> previously
> > reduced (GFDL results by Tziperman, Science 97 and JPO 99). This is
> important as
> > "collapse unlikely by 2100" should not tempt people to conclude that THC

> > collapse is hence not an issue. The contrary is true: reduction means
> > destabilisation.
> >
> > Best regards
> >
> > thomas
> > --
> > --
> > Thomas Stocker
> > Climate and Environmental Physics   stocker@climate.unibe.ch
> > Physics Institute, University of Bern   phone: +41 31 631 44 64
> > Sidlerstrasse 5   NEW   fax: +41 31 631 87 42
> > 3012 Bern, Switzerland   http://www.climate.unibe.ch/~stocker
> > --
> >
>
> ------
> Stephen H. Schneider
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> Stanford University
> Stanford, CA 94305-5020 U.S.A.
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> Tel: (650)725-9978
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181. 0967231160.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mhughes@ltrr.arizona.edu
Subject: Re: cool bristlecone, etc
Date: Fri Aug 25 15:19:20 2000

Hi again Malcolm
I am forwarding the data in another message (from Tim). I am sending the whole lot for
simplicity. Please don't pass on until we hear whether the paper is accepted or not.
Remember that , although they are strongly correlated with them, these data are not
identical in the high frequency domain to the equivalent data standardised using say a
Hugershoff function. The main purpose here was to extract long-timescale variations and I

still consider the inter annual to decadal variability to be better defined using the
'traditional' approach. For a first look anyway these are fine
best wishes
Keith
At 04:14 AM 8/24/00 -0700, you wrote:

Dear Keith,
It was good to talk with you this morning. This is a reminder about sending your Western North America banded record as you suggested. I suspect that you are right to think that it would eventually be best to use a customized banded set, but as a start, I think it would be good to compare the WNW record with the mean series Graybill and Idso used in their 1993 paper, and with the single site Campito Mountain record. I'll start with a simple graphical comparison and then move to comparing waveforms extracted by, for example, SSA.
My hope is that we can fairly rapidly generate a note to something like GRL or JoC's new short format, putting a believable version of these records out there for general use.
Please reply to the mhughes@ltrr.arizona.edu address. I'm sending it from my other address as well as a 'belt-and-braces' approach because of recent e-mail problems. Looking forward to working on this with you, Cheers, Malcolm
Malcolm Hughes
Professor of Dendrochronology
Laboratory of Tree-Ring Research
University of Arizona
Tucson, AZ 85721
520-621-6470
fax 520-621-8229

182. 0968127296.txt
##

From: "Ben Matthews" <ben@chooseclimate.org>
To: "Mike Hulme" <m.hulme@uea.ac.uk>
Subject: Re: interactive climate science-policy website,
Date: Tue, 5 Sep 2000 00:14:56 +0100
Reply-to: "Ben Matthews" <ben@chooseclimate.org>

Dear Mike,

Regarding my last mesage,

In case you wonder about my background, I have attached a 2-page version of my CV, in rich-text format, file bjhmcv2.rtf

My experience, ranging from laboratory work with CO2 fluxes and marine algae, through to organising events at the UN climate negotiations, combined with a strong mathematical and linguistic background, is a somewhat unusual combination which perhaps makes me more a "jack of all trades" than a specialist. On the other hand, this has given me an interdisciplinary overview which may be valuable for bridging the gap between science and policy, appreciating dilemmas and uncertainties, and communicating these around the world.

However, Kyoto left me very disillusioned by the apparent lack of connection between climate science and policy -in the protocol there was not one sentence discussing what we need to do to stabilise the climate in the long term, based on scientific predictions. This made me wonder, what is the use of my intricate research on air-sea CO2 exchange, if the policymakers ignore even the most basic knowledge? I left UEA and started working at home, developing interactive web graphics showing the link between per-capita emissions and global climate change. Eventually, I realised that working alone was neither effective nor sustainable, and this has led to unfortunate personal circumstances. Now I need the stimulus of working again in a team, in an institute, even if this requires sacrificing of my own ideas. I am not just looking for a "job", it is more important to me, to rejoin the research community, and feel I am making the best use of my skills. I hope you can help, if only to discuss the possibilites.

I have also attached a zip package containing the interactive java applets which I developed,
it's only 90K including supporting webpages and historical data.
Once unzipped (all in one directory), you have to open the file "starthere.html" in any java-enabled web browser.
I can send a self-extracting windows version if you prefer, on the other hand you may find it easier just to look at the website
www.chooseclimate.org/applet/
Currently, this uses only very crude formulae loosely based on IPCC SAR and GCI's C&C, -but the presentation is unique: you can adjust the parameters just by dragging controls with a mouse, and all the linked plots respond instantly. It's hard to describe in words, which is why I encourage you to have a look.

Ben

****
Dr Ben Matthews ben@chooseclimate.org,

----- Original Message -----
From: Mike Hulme <m.hulme@uea.ac.uk>
To: Ben Matthews <ben@chooseclimate.org>
Sent: 04 September 2000 13:38
Subject: Re: interactive climate science-policy website,

> Thanks for this note Ben.
>
> I would be interested in talking about your ideas at some stage,
> particularly in relation
> to our outreach strategy. We are appointing a Communications Manager very
> soon and you are
> welcome to attend the presentations as listed below:
>
> I would suggest that we arrange a meeting a little further down the line,
> once the Centre
> has started operating in its new premises after 2 October.
>
> Mike
>
> ______________________________________

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Attachment Converted: "c:\eudora\attach\bjhmcv2.rtf"

183. 0968367517.txt
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From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: j.kohler@econ.cam.ac.uk, m.hulme@uea.ac.uk, simon.shackley@umist.ac.uk
Subject: Tyndall RP2 proposal, final version
Date: Thu, 07 Sep 2000 18:58:37 +0100
Reply-to: m.kelly@uea.ac.uk
Cc: n.adger@uea.ac.uk

Dear Mike
I have attached the final version of the RP2 outline proposal on the interaction between the flexible mechanisms and the WTO trade rules.
Please jettison the previous draft.
As noted earlier, Neil and I see this project as delivering multiple benefits to the Tyndall Centre on the basis of a limited, 'value-added' investment, not least in terms of tying Shell International to the Centre.
We also highlight the suggestion of a workshop on common themes to be held in a couple of years' time to link related projects across the research programmes (though this is not covered by the current proposal).
Regards
Mick

Mick Kelly   Climatic Research Unit
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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184. 0968450529.txt
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From: "Griggs, Dave" <djgriggs@meto.gov.uk>
To: 'TAR CLA list' <tar_cla@meto.gov.uk>, 'TAR LA list' <tar_la@meto.gov.uk>
Subject: Uncertainties again
Date: Fri, 08 Sep 2000 18:02:09 +0100
Cc: 'TAR Review Editors' <tar_re@meto.gov.uk>, "'Watson, Bob'" <rwatson@worldbank.org>, "'Moss, Richard'" <richard.moss@pnl.gov>, "'Houghton, Sir John'" <jthoughton@ipccwg1.demon.co.uk>, "'Albritton, Dan'" <aldiroff@al.noaa.gov>, "'Swart, Rob'" <Rob.Swart@rivm.nl>, "'Leary, Neil'" <nleary@usgcrp.gov>, "'McCarthy, Jim'" <James_j_mccarthy@harvard.edu>, "'Stone, John'" <john.stone@ec.gc.ca>, "'shs@leland.stanford.edu'" <shs@leland.stanford.edu>, "'m.manning@niwa.cri.nz'" <m.manning@niwa.cri.nz>

Dear CLAs/LAs

As you all know, in my Victoria follow-up e-mail of 2 August I presented a summary of the agreement we reached in Victoria on a common use of terminology to express degree of likelihood in the TAR. At that time the word or term to be used for the central box of 33 to 66% had not been agreed and the word "inconclusive" was proposed for that category. Since that time there has been a lengthy discussion, including Working Groups II and III, regarding the best word to be used in this category. To cut a long story short the term we would now like you to use for this middle category is "medium likelihood". I am sorry I have not been able to canvas this around all of you but from the discussions this term was agreed by all to be the best compromise. In particular, it clearly maintains the scale as one of degrees of likelihood, whereas inconclusive could be confused as to whether a degree of likelihood was being expressed or whether there was insufficient information to conclude a likelihood. I attach a table showing what should now be the final scale.

During the discussions it became clear that in addition to making likelihood statements it is sometimes more appropriate to express statements in terms of a degree of confidence, and indeed several chapters use this terminology. As you know the Uncertainties Guidance paper by Richard Moss and Steve Schneider recommends a scale of confidence from Very Low to Very High confidence. WGII in particular are using this scale and so I would ask that, if you choose to express things in terms of a level of confidence, that you use the terms as they are laid down in the guidance paper. This in no way affects the use of the likelihood scale where this is more appropriate. For example, if we are highly uncertain how well a model handles a particular process, we may have "very low confidence" in a model result which is highly dependent on this process. If we have no other corroborating evidence we may therefore conclude that there is insufficient information to assign a likelihood in this case. By following the guidance paper when expressing a level of confidence we will hopefully improve the consistency between the two reports. Incidentally, if there are instances in the WGII report where they are able express degrees of likelihood they are going to try and use our scale.

Thirdly, there has been a lot of discussion about the impression which the likelihood scale, if taken out of context, could give for low likelihood, high consequence events, such as a disintegration of the WAIS or a shutdown of the THC in the next 100years. Please bear in mind that policymakers must balance likelihood and consequence in deciding whether or not to take action. Therefore please take extra care when considering text for these types of issues as simply expressing them as "extremely unlikely" does not give the full picture. For example, you could say an aircraft was "extremely unlikely" to crash on its next flight but if there was a 1% chance I would not fly on it. While it is a true statement the right balance is only achieved when the consequence is also brought in to put the risk in context.

I apologise for this late change to our scale but I hope you all agree that it is an improvement. If anything is not clear about any of the above please do not hesitate to contact me.

Best regards

Dave

<<Agreed terminology2.doc>>

Dr David Griggs
IPCC WGI Technical Support Unit
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Met. Office
London Road
Bracknell
Berks, RG12 2SY
UK

Tel +44 (0)1344 856615
Fax: +44 (0)1344 856912
Email: djgriggs@meto.gov.uk

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185. 0968691929.txt
##

From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: m.hulme@uea.ac.uk, t.oriordan@uea.ac.uk
Subject: Shell International
Date: Mon, 11 Sep 2000 13:05:29 +0100
Reply-to: m.kelly@uea.ac.uk

Mike and Tim
Notes from the meeting with Shell International attached.
Sorry about the delay.
I suspect that the climate change team in Shell International is probably the best route through to funding from elsewhere in the organisation including the foundation as they seem to have good access to the top levels.
Mick

Mick Kelly Climatic Research Unit
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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186. 0968705882.txt
##

From: GIORGI FILIPPO <giorgi@ictp.trieste.it>
To: Chapter 10 LAs -- Congbin Fu <fcb@ast590.tea.ac.cn>, GIORGI FILIPPO <giorgi@ictp.trieste.it>, Bruce Hewitson <hewitson@egs.uct.ac.za>, Mike Hulme <m.hulme@uea.ac.uk>, Jens Christensen <jhc@dmi.min.dk>, Linda Mearns <lindam@ucar.edu>, Richard Jones <rgjones@meto.gov.uk>, Hans von Storch <storch@gkss.de>, Peter Whetton <phw@dar.csiro.au>
Subject: On "what to do?"
Date: Mon, 11 Sep 2000 16:58:02 +0200 (MET DST)

Dear All

we heard the opinions of most LAs, namely Jens, Richard, Linda, Peter, and Hans as well as some interesting interpretations of my email (Linda says:
" You seem to be assuming that the most desirable result is
if the SRES results have no contrasts with the IS92a results.

I don't understand your reasoning on this." I do not have any particular desire
on the new data. We said that one thing to look at was the agreement with
the old data and thus I noticed that relaxing the criteria would yield a greater
agreement). I would say that a broad range of opinions was covered, from
one where the SRES should essentially be commented upon concerning their
agreement with the old data to one in which all the old stuff should be
replaced with SRES stuff. Some people want to make the BOX more central,
others want to get rid of it.

Given this, I would like to add my own opinion developed through the weekend.

First let me say that in general, as my own opinion, I feel rather
unconfortable about using not only unpublished but also un reviewed
material as the backbone of our conclusions (or any conclusions).
I realize that chapter 9
is including SRES stuff, and thus we can and need to do that too, but the
fact is that in doing so the rules of IPCC have been softened to the point
that in this way the IPCC is not any more an assessment of published science
(which is its proclaimed goal) but production of results.
The softened condition that the models
themself have to be published does not even apply because the Japanese
model for example is very different from the published one which gave results
not even close to the actual outlier version (in the old dataset the CCC model
was the outlier). Essentially, I feel that at this point there are very
little rules and almost
anything goes. I think this will set a dangerous precedent which might mine the
IPCC credibility, and I am a bit unconfortable
that now nearly everybody seems to think that it is just ok to do this.
Anyways, this is only my opinion for what it is worth.

Going to the problem at hand, I have a proposal that is in between the
two extreme positions. I think the SRES runs should be included and
highlighted in the chapter, but should not be the only source of our
conclusions, partially also for the reasons I state above (I seem to
remember that in Chapter 9 the SRES results were only a small section
in the whole chapter in which it was said that they essentially confirmed
previous findings). Also let me say
that, as it currently stands, the box is essentially meaningless, because
it simply repeats what is already said in the executive summary.
With these premises here is my proposal:

1) We leave 10.3 more or less as it is, a discussion of published science
on model behavior, uncertainty, some climate change runs.
Perhaps we shorten it or something like that. I am not in favor of
presenting Giorgi and Francisco-type plots for the SRES runs for the simple
reason that they do not convey effectively what readers want. Proof is that we
had all the plots there and we were accused of not having any results in
the chapter !! I think people want something more direct, i.e. plots similar
to the +/- one we had proposed in the BOX.

2) We make the BOX only with SRES results, i.e. the BOX becomes a
summary presentation of the SRES projections. In this way we accomplish
several objectives: we highlight the SRES results in a way that is of direct
impact (after all this is what working group II people are really interested
in); we can explicitly state that the results are preliminary and sort
of differentiate them from the more IPCC-proper chapter material (of course
we are not going to say so); we
have a natural place for the BOX (end of 10.3), do not need to rewrite
the whole thing and just need to make the proper connections with the
rest of the chapter. All and all I think this is a feasible and clean
solution. The rest of the material in the old box (sections a and c) was
really just general material repetitive of what we were saying in various

other part of the chapter.

3) In the executive summary we summarize what we believe are the confident patterns from the combination of old and new runs.

As to what should the SRES box look like. I hear people liked a lot our +/- plot, so we do the same types of plots, both for precip and temperature, one for the A2 and one for B2 scenarios, plus one or two paragraphs explaining the plots. This will portray agreement not only across models but also across what are now considered plausible scenarios. We can easily fit 4 plots in a page
and if need be fit the 1-2 paragraphs on another page (I do not see anything wrong with a 1.5 page box).
For precipitation I think the old criteria are fine. For temperature this is what I propose. In the precip plots we had 4 sub-categories, (+, - large change, small change) plus the inconclusive, or whatever we decided to call it. Similarly, we could do 4 categories here
1) Amplification positive, 2) Amplification negative (i.e. less than the global average), 3) strong amplification (> 50%), 4) small amplification (between 25 and 50 %). I cannot visualize it at the moment, but I think this will work to figures analogous to the precip ones. Correct me if I am wrong.

To the two technical issues:

1) Do we soften our requirement, i.e. from n-1 to n-2 model agreement?
I do not feel strongly about it but am more in favor of not softening the criterion. We are looking for confidence and model agreement and should have stringent requirements on it.

2) Do we include the outliers in the analysis? I say yes, not having time for more detailed analysis as to why they should not be included.
In Chapter 9 they are presented as bracketing the answers not as being wrong.
This is the problem of not having published research on this. perhaps a paper would have excluded them on scientific grounds, but can we at this point?
I am not sure we can have solid enough foundations to legitimate it.
Besides, I have done analysis without them as well and things did not change almost at all.

To the operational issues:

1) I agree there is no time for a paper to be delivered before the Sept. 26 deadline. After the deadline however, and with some calm, I think we should have a paper on it.

2) Meeting or conference call. I myself am not keen on a meeting of the Europeans. Jens is not back until the end of the week, which means the meeting would have to be during the last week before deadline. With all that is still left to do on the chapter and other internal committments I have,
I certainly could do without spending 2 days to do this (which is always the minimum it takes me to get anywhere and back)and I cannot do it over the weekend since I am not here.
It sounds like we would have to contact people by phone
anyways (see Peter and Linda's messages), so why not a conference call directly?
>From the technical viewpoint Linda seems to be the best person to organize this. As soon as Jens is back perhaps? (Jens if you can read this can you let us know when this is possible?).

3) We just got the MPI data and the full CCC ones (I guess some was lost in the previous run). We need to incorporate these so we have all models

available. I and Bruce will interact on this.

4) I agree we should contact the TSU about it, but I also think we should have a proposal on it with less spread than current to present them.

Last but not least, please work on your section revisions (especially those who have nothing to do with the BOX) so at least we get that out of the way.

Cheers,   Filippo


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################################################################
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# Phone: + 39 040 2240 425                                     #
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# email: giorgi@ictp.trieste.it                                #
################################################################
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From: GIORGI FILIPPO <giorgi@ictp.trieste.it>
To: Chapter 10 LAs -- Congbin Fu <fcb@ast590.tea.ac.cn>, GIORGI FILIPPO <giorgi@ictp.trieste.it>, Bruce Hewitson <hewitson@egs.uct.ac.za>, Mike Hulme <m.hulme@uea.ac.uk>, Jens Christensen <jhc@dmi.min.dk>, Linda Mearns <lindam@ucar.edu>, Richard Jones <rgjones@meto.gov.uk>, Hans von Storch <storch@gkss.de>, Peter Whetton <phw@dar.csiro.au>
Subject: more on "what to do"
Date: Tue, 12 Sep 2000 11:53:20 +0200 (MET DST)

Dear All

I think I heard replies to my last proposal from most of you. I have also had a phone conversation with Linda. So let me try to summarize the situation

1) From the replies I got, it sounds like at least the basic idea of my proposal is viable. In particular I read an at least semi-consensus, and certainly some strong individual positions, that the SRES material , since it is unpublished (and remember unreviewed until now), should not be presented as our sole or even primary source of conclusions. Now, I share that position and in fact quite strongly. Presenting such material breaks the proclaimed IPCC rules. Now the rules have been softened for this case, but remember that there are people around who are paid to find faults in the IPCC process and the last thing I want to do is being accused of having broken the rules. I think the TSU people are too optimistic and casual in the way they change the rules during the process and expect people to accept that this "just" happens. Remember what happened to Ben Santer after the SAR. Besides, I myself think that material for a document as important as the TAR cannot be drawn from last-minute barely quality checked and un-peered reviewed material (people have barely looked at the MPI run that was completed last friday !!). It is up the the IPCC to better plan these things and avoid the mess. Be it as it may, unless somebody is strongly against this position, I will assume that we can proceed from this basis.

2) Having said the above, it is also clear the we can present the IPCC data in some format. Chapter 9 is doing it (remember also in their case the SRES stuff is only a minor component of the chapter) so we can and I think we should because it is relevant and important material,
but with the proper caveats clearly up front, i.e. that whatever we present is a preliminary analysis that has not undergone a publication process. It would be certainly strange and confusing to have the SRES discussed in Chapter 9 but not in our chapter in some form. Besides we went through a significant effort to get it and process it. I myself think that the SRES information is important to provide. It is
just unfortunate, but not surprising, that it came around too late.

3) So the question is at this point how do we present the SRES. I suggested not to incorporate it within the text of 10.3, since 10.3 is our assessment of published research which has undergone peer and government review. I stand strongly by that suggestion. Obviously 10.3 might need a bit of rewriting to make it flow better with possibly different conclusions but not more than that. I then suggested to make the Box an SRES Box including the +/- format figures (I thought we needed 4, i.e. two for each scenario, but Linda pointed out we really need only 2, one for precip and one for temperature each including the two scenariost). Now
this offers several advantages: we can say right up front that this is from a preliminary analysis; we can separate it cleanly from the rest of the "official " text; it gives direct info in a format that people seem to like. Two very legitimate comments were made on this. Peter said, if we give this more palatable format (the +/- figures)
only for SRES data would it not implicitly give it
too much attention? Linda said: why not present similar plots for the IS92 data? The obvious action which would address both of these concerns is to present similar plots for the IS92 data. This is certainly an option. The only problem I see is that I think the clear separation of published and unpublished results would be lost if we put it in the BOX. The alternative is to do those figures and put them in 10.3, leaving the SRES for the BOX. This could be a good option, although it might require significant effort. All and all, I am still in favor of an SRES-only box with a clear statement up front that gets us off the hook in case of problems (you can see it as a sort of disclaimer I guess).

So let's come to the next point: we need to decide on this and soon. The best way appears to be a conference call. Linda suggested thursday, which is fine with me. It now looks like Richard cannot organize this. So Linda I am afraid you are left with the organization of it. The call
would have to be during European-South African afternoon - US morning and I am afraid I am not sure what time in Australia. problems is: Jens can you make it? I think Jens is the person in the group most strongly opposed to presenting SRES data, so it important he is in the conference call. It is also critical that Peter participates, given he has been the main player in all this. Now here is my proposal:

Conference call on thursday 3 p.m. Trieste-Hamburg time, which means 4 p.m. Cape town time, 2 p.m. Bracknell time, 9 a.m. Boulder time, 8 a.m. Fairbanks time and ??? Australia time. Linda is this feasible for you to organize?
Is this ok for all? Conbin, are you available at all?

items of discussion:

Question 1): Do we do an SRES BOX with +/- figures?

Question 2): What are the technical details (n-1 vs. n-2 model agreement, inclusion of outliers, threshold for large vs. small vs. no change both for precip change and temperature amplification factor).

Question 3): Do we do similar figures for IS92 data which would either replace
             the current figures on IS92 in the text (I think this would be
             perfectly acceptable since it is simply a way to present
             in a different way published results).

Question 4): How do we incorporate the SRES results within the current
             executive summary

I hope that by thursday I will have all data to do all relevant figures.
I need to get CCC control and MPI-DMI data from Bruce and dig out the old
IS92 data. If not by thursday then hopefully by friday. Once I have the
data I can easily directly calculate all the thresholds necessary for
doing the relevant figures. I will then circulate all the material to you.
Needless to say that any data based on SRES that is circulated among us
should NOT go any further (except for the chapter of course) until we decide
what to do with it (a paper or something like that).

In the mean time, I will never tire to keep asking you to please work on
the section revisions and let's get those out of the way.

Cheers, Filippo

##
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188. 0968941827.txt
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From: "Whetton, Peter" <peter.whetton@dar.csiro.au>
To: 'Hans von Storch' <Hans.von.Storch@gkss.de>, Congbin Fu <fcb@ast590.tea.ac.cn>,
GIORGI FILIPPO <giorgi@ictp.trieste.it>, Bruce Hewitson <hewitson@egs.uct.ac.za>,
Mike Hulme <m.hulme@uea.ac.uk>, Jens Christensen <jhc@dmi.min.dk>, Linda Mearns
<lindam@ucar.edu>, Richard Jones <rgjones@meto.gov.uk>, "Whetton, Peter"
<peter.whetton@dar.csiro.au>
Subject: RE: n-1 / n-2
Date: Thu, 14 Sep 2000 10:30:27 +1100

Dear all,

It could be viewed that using n-1 for 9 models where we used n-1 for five
models before is an implicit change in the stringency of our criterion.
When we had five models, agreement (0/5, 1/5, 4/5 or 5/5) could be expected
37% of the time just by chance (ignoring the near zero case). With nine
models the equivalent figure for n-1 is only 3.5%, and it is still much
lower for n-2 (18%)... (assuming that my somewhat rusty probability
calculations are correct). It really depends on what we had understood the
purpose of the criterion to be. I am not certain how much this was
discussed.

Also, I would prefer Friday night as well if it means that more information will be available.

Cheers

Peter

-----Original Message-----
From: Hans von Storch [mailto:Hans.von.Storch@gkss.de]
Sent: Wednesday, 13 September 2000 19:48
To: Congbin Fu; GIORGI FILIPPO; Bruce Hewitson; Mike Hulme; Jens Christensen; Linda Mearns; Richard Jones; Hans von Storch; Peter Whetton
Subject: n-1 / n-2

Dear friends,

I have already indicated that I favour the n-1 version. Obviously, this choice is arbitrary, but it was made BEFORE we did the analysis. By changing the criterion AFTER we have seen the data, we may be targeted by critics for biased rules. Using material, which is unpublished and unreviewed is already a bit shacky (Hans Oerlemans is unwilling to participate in the IPCC process because of a similar incident in the 1995 report!).

Hans

--

Hans von Storch

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189. 0969308584.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: <wsh@unite.com.au>
Subject: Re: TAR
Date: Mon Sep 18 16:23:04 2000
Cc: ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov

Warwick,
I did not think I would get a chance today to look at the web page. I see what boxes you are referring to. The interpolation procedure cannot produce larger anomalies than neighbours (larger values in a single month). If you have found any of these I will investigate. If you are talking about larger trends then that is a different matter. Trends say in Fig 2.9 for the 1976-99 period require 16 years to have data and at least 10 months in each year. It is conceivable that at there are 24 years in this period that missing values in some boxes influence trend calculation.
I would expect this to be random across the globe.

Cheers
Phil

Warwick,
Been away. Just checked my program and the interpolation shouldn't produce larger anomalies than the neighbouring cells. So can you send me the cells, months and year of the two cells you've found ? If I have this I can check to see what has happened and answer (1).
As for (2) and (3) we compared all stations with neighbours and these two stations did not have problems when the work was done (around 1985/6).
I am not around much for the next 3 weeks but will be here most of this week and will try to answer (1) if I get more details. If you have the names of stations that you've compared Olenek and Verhojansk with I would appreciate that.

Cheers
Phil

At 05:13 AM 9/14/00 +1000, you wrote:
>Dear Phillip and Chris Folland (with your IPCC hat on),
>Some days ago Chris I emailed to Tom Karl and you replied re the grid cells
>in north Siberia with no stations, yet carrying red circle grid point
>anomalies in the TAR Fig 2.9 global maps. I even sent a gif file map
>showing the grid cells barren of stations greyed out. You said this was
>due to interpolation and referred me to Phillip and procedures described in
>a submitted paper. In the last couple of days I have put up a page
>detailing shortcomings in your TAR Fig 2.9 maps in the north Siberian
>region, everything is specified there with diagrams and numbered grid
>points.
>[1]   One issue is that two of the interpolated grid cells have larger
>anomalies than the parent cells !!!!?????
>This must be explained.
>[2] Another serious issue is that obvious non-homogenous warming in Olenek
>and Verhojansk is being interpolated through to adjoining grid cells with
>no stations, like cancer.
>[3] The third serious issue is that the urbanization affected trend from
>the Irkutsk grid cell neare Lake Baikal, looks to be interpolated into its
>western neighbour.
>
>I am sure there are many other cases of this, 2 and 3
> happening.
>Best regards,
>Warwick Hughes (I have sent this to CKF)
>

190. 0969618170.txt
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From: mhughes@ltrr.arizona.edu
To: tom crowley <tom@ocean.tamu.edu>
Subject: Re: old stuff
Date: Fri, 22 Sep 2000 06:22:50 -0700
Cc: <k.briffa@uea.ac.uk>

Dear Tom,
The difference between the Campito Mountain record and, for example, the one from the Polar Urals that you mention, is that there is no meaningful correlation between the Campito record and local temperature, whereas there is a

strong correlation in the Polar Urals case. I give references to the work reporting this phenomenon at the end of this message, but I'm afraid I'm missing the references to the technical comments that are being responded to in the last two. If you examine my Fig 1 closely you will see that the Campito record and Keith's reconstruction from wood density are extraordinarily similar until 1850. After that they differ not only in the lack of long-term trend in Keith's record, but in every other respect - the decadal-scale correlation breaks down. I tried to imply in my e-mail, but will now say it directly, that although a direct carbon dioxide effect is still the best candidate to explain this effect, it is far from proven. In any case, the relevant point is that there is no meaningful correlation with local temperature. Not all high-elevation tree-ring records from the West that might reflect temperature show this upward trend. It is only clear in the driest parts (western) of the region (the Great Basin), above about 3150 meters elevation, in trees old enough (>~800 years) to have lost most of their bark - 'stripbark' trees. As luck would have it, these are precisely the trees that give the chance to build temperature records for most of the Holocene. I am confident that, before AD1850, they do contain a record of decadal-scale growth season temperature variability. I am equally confident that, after that date, they are recording something else.
I'm split between Harvard Forest and UMASS these days, and my copy of your paper is not with me today. I'd be interested to know what the name of the site for the LaMarche central Colorado record was.
Cheers, Malcolm

Reference List

1. Graybill, Donald A., and Sherwood B. Idso. 1993. Detecting the Aerial Fertilization Effects of Atmospheric CO2 Enrichment in Tree-Ring Chronologies. Global Bioeochemical Cycles 7, no. 1: 81-95.
2. LaMarche , V. C., D. A. Graybill, H. C. Fritts, and M. R. Rose. 1984. Increasing Atmospheric Carbon Dioxide: Tree Ring Evidence for Growth Enhancement in Natural Vegetation. Science 225: 1019-21.
3. ---1986. Carbon Dioxide Enhancement of Tree Growth At High Elevations. Science 231: 859-60.
4. ---1986. Technical Comments: Carbon Dioxide Enhancement of Tree Growth At High Elevations. Science 231: 860.

Quoting tom crowley <tom@ocean.tamu.edu>:

> Dear Malcolm and Keith,
>
> as I discuss in my Ambio paper the "anomalous" late 19th century warming
> also occurs in a LaMarche tree ring record from central Colorado, the
> Urals
> record of Briffa, and the east China phenological temperature record of
> Zhu.
>
> Alpine glaciers also started to retreat in many regions around 1850,
> with
> 1/3 to 1/2 of their full retreat occurring before the warming that
> commenced about 1920.
>
> The Overpeck et al Arctic synthesis also discusses warming before 1920 -
> that record matches very closely the Mann et al reconstruction in other
> details back to 1600.
>
> Unpublished work by us on coral trends also suggests slight warming
> between
> about 1850-1920.
>

> So, are you sure that some CO2 fertilization is responsible for this?
> May
> we not actually be seeing a warming?
>
> Tom
>
>
>
> Thomas J. Crowley
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##########

From: Ben Santer <santer1@llnl.gov>
To: wigley@ucar.edu, roeckner@dkrz.de, ktaylor@zooks.llnl.gov, boyle@pcmdi.llnl.gov, sailes1@llnl.gov, p.jones@uea.ac.uk, doutriau@pcmdi.llnl.gov, jhansen@giss.nasa.gov, meehl@meeker.ucar.edu, bengtsson@dkrz.de
Subject: Status of our JGR paper
Date: Fri, 22 Sep 2000 12:36:38 -0700

Dear All,

I just wanted to keep you informed about the status of our draft JGR paper. First, thanks to all of you for your comments - they were very helpful. I am now in the process of revising the paper, and hope to have a new draft ready by Oct. 10th. After several discussions with Tom, I have decided to repeat the volcano/ENSO signal separation for the observed data and for the GSOP experiment.

The reason for this is that there was a conceptual flaw in what I had done previously. The flaw related to the determination of the "pre-eruption" reference temperature, used as a baseline for estimating the maximum volcanically-induced cooling. Let's call this baseline temperature "TBASE". Previously, I was estimating TBASE for Pinatubo and El Chichon from either the raw or Gauss-filtered temperature data at time t=0 (the eruption month).
If I was calculating TBASE from the filtered data, the estimate of TBASE was biased by "contamination" from post-eruption cooling. In other words, since I was using a 13-term Gaussian filter, temperature values from t=0 + 6 months were influencing TBASE, likely leading to an underestimate of the true TBASE value.
I've now modified the program so that TBASE is not computed from the filtered data; instead, it is an average of the temperature anomalies in the MREF months prior to the eruption. There is some sensitivity to the choice of MREF (I've been experiment with values ranging from 6-18 months), which again underscores the uncertainties inherent in separating ENSO and volcanic signals.

The maximum volcanically-induced cooling is still estimated using filtered data, but now I'm using a 5-term binomial filter rather than the 13-term Gaussian.

These changes require repeating most of the analyses in the paper. Preliminary results indicate that the revised estimation of TBASE increases the ratio of the Chichon/Pinatubo maximum coolings, and brings this closer to the ratio of the Chichon/Pinatubo radiative forcings.

Tom has also made a number of useful suggestions regarding reorganization and shortening of various sections of the manuscript. Hopefully the next iteration will be a little shorter than the current version of the paper!

I will be out of my office next week, but should be back by October 2nd.

With best regards, and thanks again for all your help,

Ben
--

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##########

From: Tom Wigley <wigley@ucar.edu>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: Status of our JGR paper
Date: Fri, 22 Sep 2000 15:47:37 -0600
Cc: roeckner@dkrz.de, ktaylor@zooks.llnl.gov, boyle@pcmdi.llnl.gov, sailes1@llnl.gov, p.jones@uea.ac.uk, doutriau@pcmdi.llnl.gov, jhansen@giss.nasa.gov, meehl@meeker.ucar.edu, bengtsson@dkrz.de

****
Ben (or, really, everybody else),

I don't know whether you have all seen the paper analyzing the observed data that Ben and I sent to J. Climate ?? This is where the JGR paper began, and it is useful to compare both papers. In the J. Climate paper we assessed the best fits using a subjective balance of raw and lowpass filtered results. The reason for this was because of the difficulty of setting up an automated procedure -- which is the problem that Ben is currently having to deal with. In the next iteration of the JGR paper, the reason for moving to a more automated procedure will be explained. Both the subjective and automated procedures have their advantages and disadvantages. The latter procedure, of course, is in no way 'objective'. Many subjective choices have to be made in setting up the procedure. This is why the word 'automated' is used above, rather than 'objective'.

If you have not seen the J. Climate paper, let me know and I will send you a copy. There is a companion paper that has been accepted by GRL that I will send at the same time.

Cheers, Tom.
****

Ben Santer wrote:
>
> Dear All,
>
> I just wanted to keep you informed about the status of our draft JGR paper.
> First, thanks to all of you for your comments - they were very helpful. I am now
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> was using a 13-term Gaussian filter, temperature values from t=0 + 6 months were
> influencing TBASE, likely leading to an underestimate of the true TBASE value.
> I've now modified the program so that TBASE is not computed from the filtered
> data; instead, it is an average of the temperature anomalies in the MREF months
> prior to the eruption. There is some sensitivity to the choice of MREF (I've
> been experiment with values ranging from 6-18 months), which again underscores
> the uncertainties inherent in separating ENSO and volcanic signals.
>
> The maximum volcanically-induced cooling is still estimated using filtered data,
> but now I'm using a 5-term binomial filter rather than the 13-term Gaussian.
>
> These changes require repeating most of the analyses in the paper. Preliminary
> results indicate that the revised estimation of TBASE increases the ratio of the
> Chichon/Pinatubo maximum coolings, and brings this closer to the ratio of the
> Chichon/Pinatubo radiative forcings.
>
> Tom has also made a number of useful suggestions regarding reorganization and
> shortening of various sections of the manuscript. Hopefully the next iteration
> will be a little shorter than the current version of the paper!
>
> I will be out of my office next week, but should be back by October 2nd.
>
> With best regards, and thanks again for all your help,
>
> Ben
> --
> --
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-264
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-7638
> FAX: (925) 422-7675
> email: santer1@llnl.gov
> --

--
****
Tom M.L. Wigley
Senior Scientist
ACACIA Program Director
National Center for Atmospheric Research
P.O. Box 3000

Boulder, CO 80307-3000
USA
Phone: 303-497-2690
Fax: 303-497-2699
E-mail: wigley@ucar.edu
Web: http://www.acacia.ucar.edu
****

193. 0969652335.txt
##
##########

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: k.briffa@uea.ac.uk, p.jones@uea.ac.uk
Subject: OOPS. RETURN EMAIL GLITCHES IN ORIGINAL
Date: Fri, 22 Sep 2000 15:52:15 -0400

>Date: Fri, 22 Sep 2000 15:50:05 -0400
>To: Tim Osborn <t.osborn@uea.ac.uk>
>From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
>Subject: Re: my visit
>Cc: srutherford@virginia.edu, k.briffa@uea, p.jones@uea
>Bcc: mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu
>In-Reply-To: <3.0.6.32.20000922092400.007ed450@pop.uea.ac.uk>
>References: <3.0.6.32.20000919101130.00aad100@multiproxy.evsc.virginia.
edu> <3.0.6.32.20000919135642.008114b0@pop.uea.ac.uk>
>
>HI Tim,
>
>Very busy, so just a short response for the time being.
>
>Regarding our MBH98 and GRL99 datasets, I'm pretty sure that Scott put those
>on anonymous ftp for you some months ago. So you *should* already have had
access to all the data we used. In fact, it was only a few select series of
Malcolm's that weren't made available from the get-go. So data has never
>been an issue for us. I'm happy to hear that it is not an issue for
you/keith/phil and that you are ready to make your density data available...
>
>A few points of clarification might help here:
>
>The revised method (based on ridge regression) is currently in development
as far as paleoreconstruction is concerned (we have a paper to be submitted
on application to the instrumental record only). We intend to test it on
synthetic proxy datasets (as described in my previous email) before
applying it to actual proxy data, so your visit, unfortunately, occurs at a
time that is too premature for comparison with results from this method.
Rather, we were hoping
>you shared some of the interest along the lines of
developmental/methodological
>issues.
>
>Comparison between warm-season reconstructions would be fine, but you should
>be aware of the extreme caveats with regard to our seasonal
reconstructions, as spelled out in detail in our "Earth Interactions"
article. We don't do nearly as well for warm-season or cold-season as for
annual-mean, and we believe this is consistent w/ the mix of seasonal
information contained in the multiproxy dataset. Obviously, things are
somewhat different for the more seasonally homogeneous density chronology
dataset. So to us, this comparison might not
>seem as worthwhile as it would for you all, but we can do it if all provisos
>and caveats are fully recognized and embraced from the start...
>

>The idea of testing wavelet methods of distinguish contributions on different timescales sounds like it is of interest to all of us, and perhaps we can
>move in that direction during your visit.
>
>In any case, we'll have more than enough to do, talk about, investigate, and no need to necessarily hammer it all out beforehand.
>
>Comments from others (Scott, Phil, Keith?) welcome,
>
>mike
>
>At 09:24 AM 9/22/00 +0100, Tim Osborn wrote:
>>At 10:11 19/09/00 -0400, you wrote:
>>>I will put you up at the "Red Roof Inn" for the 10 nights...
>>>Will have reservations made for you for the night of the 10th through 19th,
>>>checking out morning of the 20th...
>>
>>That sounds great. Thanks.
>>
>>
>>Mike,
>>
>>I've talked over various ideas with Keith and Phil (and I'm cc'ing this to
>>them as well as to Scott), and I've now made some slightly firmer/clearer
>>suggestions, combining your ideas and ours.
>>
>>(1) We're still keen to spend part of the time on reconstruction method
>>issues, since that is one of the specifics that our current funded project
>>needs to address. To avoid being too retrospective, we could do something
>>that combined both your Nature98 and your revised methods:
>>
>>(a) compare your summer/warm season reconstructions (old & new methods)
>>with our reconstructions of Apr-Sep temperature from tree-ring densities
>>(regional/hemispheric averages and spatial comparisons).
>>
>>(b) In (a), we would be comparing reconstructions based on different
>>palaeodata *and* different statistical reconstruction methods. So a better
>>approach would be to use your (old & new) methods with our tree-ring
>>density data set to reconstruct Apr-Sep temperature fields, and then
>>compare with our reconstructions. This would be a good way of comparing
>>methods.
>>
>>(c) We could exchange data/methods to continue comparisons after the end of
>>my visit. We would be keen, for example, to obtain your Nature98 & GRL99
>>datasets and software to play around with after my return. In exchange, we
>>can provide you with our tree-ring density data set and the reconstructions
>>that we have produced from it. Of course, such subsequent work would
>>continue to be collaborative, keeping each other informed/involved with the
>>work.
>>
>>(d) If the tree-ring density data provided useful "added value" to your
>>reconstructions (perhaps at the higher frequencies and providing finer
>>spatial detail?), then we could use an appropriate method (perhaps your new
>>revised one) to produce a new reconstruction using all palaeodata. Such a
>>reconstruction might prove to be an important and well-used product.
>>
>>(2) Of your two specific suggestions I quite strongly prefer the first.
>>The reason is that, again, our project specifically requires comparison of
>>palaeo and model data and the development of appropriate methods to do
>>this. Your first suggestion would take us along those lines. There are
>>two related strands here. The first is to use the model outputs to assess
>>the reliability of the reconstructions (i.e., following the ideas you laid

>>out in your e-mail), which is certainly of interest. The second is to use
>>the reconstructions to evaluate the model simulations of "natural"
>>variability. We've done some comparisons with the HadCM2 and HadCM3
>>simulations - I shall brings papers/results along. What we need to develop
>>further are ways of incorporating the paleo biases/errors in such
>>comparisons. We have begun this, but when I visit we might be able to come
>>up with better methods and apply them to Hadley Centre and/or GFDL
>>comparisons.
>>
>>Your second suggestion, while interesting, is less appealing at this stage,
>>principally because we won't have time to do everything. As it happens,
>>Keith and I have just submitted a paper (to that well-known(!) journal
>>"Dendrochronologia") about timescale-dependent calibration of tree-ring
>>data - I shall bring a copy with me. My feeling is that the quantity of
>>data overlap available for calibration would be a strongly limiting factor
>>in most timescale-dependent approaches, whether they use wavelets or some
>>other filtering-type approach. What interests me more would be the
>>application of wavelets to the full palaeorecords to facilitate in the
>>definition of timescale-dependent coherent patterns (PCs?), rather than
>>just to the calibration period. Anyway, we can talk these ideas over even
>>if there's no time to begin any work yet.
>>
>>I think that a chance to exchange preprints, data, and discuss ongoing
>>developments of our work and yours will, in itself, prove to be a useful
>>outcome of my visit.
>>
>>Best regards
>>
>>Tim
>>
>>
>>
>>Dr Timothy J Osborn | phone: +44 1603 592089
>>Senior Research Associate | fax: +44 1603 507784
>>Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
>>School of Environmental Sciences | web-site:
>>University of East Anglia __________| http://www.cru.uea.ac.uk/~timo/
>>Norwich NR4 7TJ | sunclock:
>>UK | http://www.cru.uea.ac.uk/~timo/sunclock.htm
>>
>>
>>
____
Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903
____
e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804) 982-2137
http://www.evsc.virginia.edu/faculty/people/mann.html

194. 0969891412.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@virginia.edu
Subject: No Subject
Date: Mon Sep 25 10:16:52 2000
Cc: t.osborn@uea.ac.uk,p.jones@uea.ac.uk

Dear Mike
I know Tim has communicated with you about plans for his visit to Virginia. We have discussed ideas here and I ,for one, am excited about the prospects of joint work. Thank you for agreeing to his visit and for taking the trouble to arrange things .
The purpose of this brief message is simply to reiterate what we said in our brief discussions in Venice - namely that it is our intention to work with you rather than in any sense of competition. Our motivation for wanting to do some of the detailed comparisons between the results of our work and your own is to understand the sources of uncertainty in both. We are also committed to doing some of this work by the terms of our current NERC grant. We wish to involve you as much as possible , get your advice , and solicit criticisms of our approach -especially in relation to the Palaeo-model comparisons .
Our EC proposal was not funded , but we wish to follow it up with another to PRESCIENT (a NERC Thematic Programme of research along the same lines), and again we would be happy to collaborate with you. Better two way communication between here and there would be a major help.
It is my feeling that the relatively short time Tim has with you , might be best spent getting to grips with the finer details of your "old" and "new" approaches, including the details and results of your other work that is only partly described in the publications (seasonal runs, different data sets etc.) and , most importantly, discussing approaches and philosophies for data-model comparison work. That way he could come away with some concrete plans , and the means of fulfilling them, on his return. Any time you can spare to discuss and liaise along these lines would be much appreciated. He has discussed the specifics of your suggestions and I am happy with the approach and prioritization he has expressed.
While he is with you , we can always exchange emails if any issues need wider discussion.
very best wishes
Keith

195. 0969912361.txt
##

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re:
Date: Mon, 25 Sep 2000 16:06:01 -0400
Cc: t.osborn@uea.ac.uk, p.jones@uea.ac.uk, srutherford@virginia.edu

HI Keith et al,

Thanks for your message.

This sounds fine. I do have to warn that with a full teaching courseload this semester, my own free time will necessarily be somewhat limited. Thus, Scott's involvement here will be key.

Scott has been dealing w/ the new methodology and analyses, and hence my concern w/ any plans that expect new analyses w/ our old methodology. The code is not especially user friendly, though Tim is welcome to use it. Scott will be able to devote a decent share of his time to these activities during Tim's visit, though this will necessarily have to be split with time devoted to activities that Scott is explicitly supported for by our NOAA grant (ie, the development of a synthetic proxy network from model data, and wavelet-based calibration methods, as detailed in my previous email).

So I'm sure we'll be able to find common ground. Tim will have free access to our data and codes, and can make the comparisons indicated below. We of course appreciate your willingness to make available to us the tree ring density data.

It may be interesting to do a (highly preliminary!) analysis of both proxy datasets with our expectation maximization ridge regression scheme, and that would certainly fit in well w/ both our agendas (your NERC grant, and our NOAA grant).

Hopefully, our 4-processor Dell server (running Linux) will be back up and running, so Scott can use our Sun server, while Tim will have the Dell server to himself if he needs it.

I hope the above all sounds good.

Best regards,

mike

At 10:16 AM 9/25/00 +0100, Keith Briffa wrote:
>Dear Mike
>   I know Tim has communicated with you about plans for his visit
>to Virginia. We have discussed ideas here and I ,for one, am excited about
>the prospects of joint work. Thank you for agreeing to his visit and for
>taking the trouble to arrange things .
>The purpose of this brief message is simply to reiterate what we said in
>our brief discussions in Venice - namely that it is our intention to work
>with you rather than in any sense of competition. Our motivation for
>wanting to do some of the detailed comparisons between the results of our
>work and your own is to understand the sources of uncertainty in both. We
>are also committed to doing some of this work by the terms of our current
>NERC grant.   We wish to involve you as much as possible , get your advice
>, and solicit criticisms of our approach -especially in relation to the
>Palaeo-model comparisons .
> Our EC proposal was not funded , but we wish to follow it up with another
>to PRESCIENT (a NERC Thematic Programme of research along the same lines),
>and again we would be happy to collaborate with you. Better two way
>communication between here and there would be a major help.
>   It is my feeling that the relatively short time Tim has with you ,
>might be best spent getting to grips with the finer details of your "old"
>and "new" approaches, including the details and results of your other work
>that is only partly described in the publications (seasonal
>runs, different data sets etc.) and , most importantly, discussing
>approaches and philosophies for data-model comparison work. That way he
>could come away with some concrete plans , and the means of fulfilling
>them, on his return. Any time you can spare to discuss and liaise along
>these lines would be much appreciated. He has discussed the specifics of
>your suggestions and I am happy with the approach and prioritization he has
>expressed.
>While he is with you , we can always exchange emails if any issues need
>wider discussion.
>very best wishes
>Keith
>--
>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090   Fax: +44-1603-507784
>
>
>

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http://www.evsc.virginia.edu/faculty/people/mann.html

196. 0970663670.txt
##
##########

From: John Daly <daly@vision.net.au>
To: Chick Keller <cfk@lanl.gov>
Subject: Re: Hockey Sticks References
Date: Wed, 04 Oct 2000 08:47:50 +1000
Reply-to: daly@vision.net.au
Cc: VINCENT GRAY <vinmary.gray@paradise.net.nz>, Onar Åm <onar@netpower.no>, "John L. Daly" <daly@microtech.com.au>, "P. Dietze" <p_dietze@t-online.de>, mmaccrac@usgcrp.gov, Michael E Mann <mann@virginia.edu>, rbradley@geo.umass.edu, wallace@atmos.washington.edu, Thomas Crowley <tom@ocean.tamu.edu>, Phil Jones <p.jones@uea.ac.uk>, sfbtett@meto.govt.uk, jarl.ahlbeck@abo.fi, richard@courtney01.compulink.co.uk, McKitrick <rmckit@css.uoguelph.ca>, Bjarnason <agust@rt.is>, Harry Priem <priem@dds.nl>, balberts@nas.edu

Dear all

Here's another MWE reference, originally announced by the Idso's. I looked up the abstract from the South African Journal of Science and here's its URL

http://www.gheiss.de/Personal/Abstracts/SAJS2000_Abstr.html

That puts the MWE and LIA into South Africa.

Cheers

John D.

--

John L. Daly
"Still Waiting For Greenhouse"
http://www.vision.net.au/~daly

"All science is numbers, but not all numbers is science"

197. 0970664328.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Myles Allen <M.R.Allen@rl.ac.uk>
Subject: Re: Observed temperature for IPCC power spectra
Date: Wed Oct 4 08:58:48 2000
Cc: Curtis Covey <covey1@llnl.gov>, santer1@llnl.gov, jfbmitchell@meto.gov.uk

Myles and Curt,
Attached are the NH and SH averages from the new variance corrected analyses (HadCRUTv). When the paper comes out in JGR (probably early next year) you'll see that variance correction is only possible from 1870. So in these files I've patched on the 1856-1869 data on the front so they are the same length. This early data is the same as the original version (HadCRUT). For the global series I still think the best way of producing this is by averaging the two hemispheres. HadCRUT is what

you all probably have - it is on the CRU web page. Again I would
produce the globe by averaging the hemispheres so what Chris Folland has
for the globe may differ slightly as the HadC produce this as one domain.
   The way the variance correction is achieved is by reducing the high-freq
variance of each grid-box series. This means that when I update the series
for 2000 some values for the last few years (1995-9) will be altered
slightly.
   I don't know much about Chapter 2, but I don't recollect there being
any power spectrum diagrams. Probably left for the detection chapter.
Do make sure the axes and units are well explained. Don't leave anything
for the skeptics to cling to !

Cheers
Phil

At 05:16 PM 10/3/00 +0100, Myles Allen wrote:
>Hi Phil,
>
>If you could send me the latest version that chapter 2 are using, that
>would be great -- I certainly won't pass it on nor use it for anything
>else. Subtle differences in processing do make a difference to the visual
>appearence of the plot, and even though these differences are inside the
>noise indicated by the error bar, you can bet potential critics will
>ignore that.
>
>Do you show a power spectrum of global temperatures in chapter 2, and if
>so, how was it computed? It would certainly be tidy to make sure both are
>processed in the same way.
>
>Myles
>--
>Myles R. Allen   Phone (RAL):   44-1235-446480
>Space Science & Technology Department Ph (Oxford):   44-1865-272085
>Rutherford Appleton Laboratory   Fax: 44-1235-445848
>Chilton, Didcot, OX11 0QX   e-mail: m.r.allen@rl.ac.uk
>United Kingdom   http://www.climate-dynamics.rl.ac.uk/
>--
>
>On Tue, 3 Oct 2000, Phil Jones wrote:
>
>>
>> Curt, Myles and anyone else,
>>   As the data on the web site has an end date of 1994, I suspect you
>> may have an earlier version of the surface data (different form of
>> gridding and maybe a few other differences in data usage), so I suggest
>> you use the latest one, which can be got from the CRU web site.
>>   If this relates to IPCC work, then Chapter 2 on the Observations is
>> going to go with a variance corrected version (corrects for changing
>> station numbers within individual grid boxes), but the effect of this
>> on the hemispheric and global temp series is small.
>>   If anyone wants this new version (HadCRUTv) then I can send the hemispheric
>> and global series by email. The 'normal' version (HadCRUT) is on the
>> CRU website. This naming and the variance correction procedures are
>> discussed in a paper which has been accepted by JGR. It will not be out for
>> a while, as I've not yet sent the camera ready columns to the AGU.
>>
>> Cheers
>> Phil
>>
>>
>>
>> At 03:45 PM 10/2/00 -0700, Curtis Covey wrote:
>> > Myles Allen wrote: Dear Curt, Can you give me the ancestry of the "ObsJ"

>> >global mean temperature series
>> > We need the source, start date (I think I can work it
>> >out by matching bumps, but it would be better to be sure) and how it's
>> >been detrended for the figure caption.
>> >
>> >>
>> >>> sent you, the data is
>> >>>the "Jones" set used by the IPCC for its Second (1995)
>> >>> It's
>> >>> processed this particular data that
>> >>> don't remember exactly who gave it to me: either Phil
>> >>>   should invite Phil's latest
>> >>>(including error bars) now that I'm updating our Web
>> >>>site http://www-pcmdi.llnl.gov/cmip/powerspec.html.   See attached
>> >>>PostScript graphic. Regards,
>> >>>Curt
>> >>>
>> >>
>> >   Attachment Converted: "c:\eudora\attach\tseries_obsJ.ps"
>> Prof. Phil Jones
>> Climatic Research Unit   Telephone +44 (0) 1603 592090
>> School of Environmental Sciences   Fax +44 (0) 1603 507784
>> University of East Anglia
>> Norwich   Email   p.jones@uea.ac.uk
>> NR4 7TJ
>> UK
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>> --
>>
>>
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>
>

198. 0970842624.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru,eavaganov@forest.akadem.ru
Subject: intas
Date: Fri Oct 6 10:30:24 2000

Stepan and Eugene
I have asked INTAS for an extension on the report period. Stepan some problem has now arisen regarding your final payment . I have asked Janet to sort this out and contact you directly.
I have to give an up to date report on chronology development and tree-line changes at the PAGES meeting in Avignon on October 24-26 and I would really appreciate some Figures that demonstrate the latest state-of-the-art in the Yamal and Taimyr (and any other good Russian evidence) . The focus of the meeting is High-resolution variability of the Holocene , and the long records and evidence of tree-line changes is particularly valuable. Later there will be some large review papers (with many authors) summarising the information from high latitudes, mid latitudes, the tropics etc. The form of these papers is not yet decided but you would be contributing authors. I am also (with Ray Bradley,Julie Cole and Malcolm Hughes) writing a Chapter on the last 10000 years (with a major emphasis on the last 1000) for the PAGES Synthesis book and I intend to include a summary Figure that includes your work - I hope this is O.K
Malcolm has just asked for a letter of support from me for a project he is submitting to NSF , in which I believe you are both involved. I have sent it to him.
I am still exploring when we can resubmit our own proposal to the EC, and I will

write an application to The Leverhulme trust before the end of this year. I am still discussing the Holocene ADVANCE-10K issue and I will be in touch about your papers.
best wishes
Keith

199. 0971129284.txt
##

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Yamal treeline figures
Date: Mon, 9 Oct 2000 18:08:04 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,

Stepan Shiyatov tell me that you need some figures concerning Yamal chronology and tree line dynamics to show somewhere in France.

Attached are archived files contained some figures.

File MAP - the map of region of research. Red dots - subfossil wood sites, green marks - recent northern border of larch along river valleys.

File FIGURES - in Excel format, contains several figures.
 Sheet "Values-10" - data on northernmost position of trees and
   number of trees dated for corresponding year (decadal step)
 Sheet "Treeline" - dynamics of treeline in Yamal during last 7000
   years reconstructed using about 1000 subfossil wood remains.
   Recent treeline position is about 67°34’.
   One year ago we supposed (C-14 data, Hantemirov, Shiyatov
   1999) that significant drop of treeline (the transition from
   "middle" to "late" Holocene) was about 1700-1600 AD. According
   new data it was earlier (about 2550 BC). May be it is because
   of lack of data from region northward of 68°N (only 25
   datings)?
 Sheet "Treeline and Nu" - treeline dynamics and number of dated
   trees. May be number of trees reflects the long scale climate
   fluctuations as well.
 Sheet "2600-all" - for last 4600 years: treeline dynamics,
   number of trees, 11 most cold summers for last 7000 years
   (according our version of reconstruction), most expressed
   frosts in July (reconstructed using junipers from Polar Urals,
   see file PATHOL, frost in 1626 BC - based on subfossil larch -
   you can put away it), summer temperatures reconstruction
   smoothed with 20- and 100-year filters (our version of
   reconstruction).
 Sheet "Values-2" - values for preceding figures, in 2-years
   step.
 Sheet "Yam-Ur-fig" - comparing of treeline data for Yamal and
   Polar Urals upper treeline dynamics (data by S.G.Shiyatov)
 Sheet "Yamal-Ural" - values for preceding figure, in 2-years
   step.
 Sheet "Treeline-std" - treeline dynamics and 50-year standard
   deviations of summer temperatures (our version of
   reconstruction). This figure shows surprising high negative
   correlation. However may be both of them just reflect long
   scale climate fluctuations?
 Sheet "Std" - 50-year standard deviations of summer

temperatures (our version of reconstruction) .

File PATHOL - in Excel format, contains data and figure on
pathological structures in tree rings of Siberian juniper
(Juniperus sibirica Burgsd.). According our data (Hantemirov et
al., 2000) the presence of frost rings provides evidence for
frosts that occurred in late June or first days of July (frost
rings in earlywood) and in the first half of July (frost rings in
late wood). Long term and pronounced temperature drop in the
middle of very warm period in the second half of July is the
factor responsible for wood density fluctuations (false rings).

Please let me know when you receive this. Some time large
messages get lost.

P.S. We (Eugene Vaganov, Stepan Shiyatov, Leonid Agafonov and I)
will be in Birmensdorf from 23 till 29 October. Are you going to
Switzerland after your meeting? We would be happy to see you
there.

Best regards,
Rashit M. Hantemirov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: rashit@ipae.uran.ru
Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92
Attachment Converted: "c:\eudora\attach\Map.zip"

Attachment Converted: "c:\eudora\attach\Figures1.zip"

Attachment Converted: "c:\eudora\attach\Pathol.zip"

200. 0971992541.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <T.Osborn@uea.ac.uk>
Subject: Re: JGR paper
Date: Thu Oct 19 17:55:41 2000

I am just having to go so I will think about the "should we?" . The answer to the "can we?"
is yes. I have spoken to the person organising the editorial review and she has promised me
she will get it to us in the next week or so. If we can get it back immediattely she says
we can make the December issue. Therefore it is possible to do the edits if it means very
little change to the text. I have also confirmed that we will pay 1500 dollars for the
colour and they say they are working on these now. I really want to get this into the 2000
so I can include it in the RAE. Ed is here now and has some great looking extended PDSI
reconstructions (1000 years) for the western US.
I am suspicious as to whether the negative trend in Mike's Hockey stick prior to the 20th
century is not at least partly the result of a trend in the long high elevation

western US
   trees he uses . Malcolm sent me some figures for the HIHOL meeting and in this work he cuts
   off the juvenile growth sections of the long tree data but does no detrending on the
   remainder. This might leave a linear age trend in these data. I remember that Mike in his
   long reconstruction , stated that the pc representing the western US stuff was essential
   for getting a verifiable result. Interesting , but only a diversion. We can discuss the JGR
   and other stuff in Avignon. Hope your weekend was a god one. I tend to agree a
   bout the NAO meeting- you could use the money (and perhaps time) to better effect.
   At 04:24 PM 10/19/00 +0100, you wrote:

Keith,
have you had to produce the camera-ready copy for the age-banded JGR paper yet? If not, then is it possible to make some minor changes to it? For the comparison with the Mann et al. reconstruction, I had previously just taken their land&marine full northern hemisphere mean annual temperature time series and re-calibrated it against the instrumental land north of 20N Apr-Sep mean temperature time series. Well, I have not taken the Mann et al. spatial temperature field reconstructions, and computed a land north of 20N area mean. I still have to re-calibrate it against the instrumental series because it is an annual rather than Apr-Sep mean. After doing all this, you'll be pleased to know that the final figure is only slightly different (the Mann et al. curve is very slightly more of an outlier during the 1500-1700 period, and is cooler and closer to observations post-1950, but not much different elsewhere). What does change, however, are the correlations. The correlations with instrumental data are slightly worse (from 0.76 to 0.73, and from 0.92 to 0.89 decadal), but I'm not sure that we show these anyway. But the cross-correlations between the Mann et al. and the other reconstructions (which we do show) are all stronger than previously - which now seems a little unfair on them.
Cross-correlations between unfiltered series:
Mann versus: Jones, Briffa (ABD), Briffa (Torn+Tai+Yam)
before: 0.47, 0.36, 0.33
now: 0.50, 0.37, 0.34
Cross-correlations between 50-yr smoothed series:
Mann versus: Jones, Briffa (ABD), Briffa (T+T+Y), Overpeck, Crowley
before: 0.78, 0.43, 0.50, 0.86, 0.76
now: 0.81, 0.51, 0.55, 0.86, 0.78
I don't have a copy of the paper in front of me, but the 'before' values should match those in one of the tables. Some of the 50-yr smoothed new values are noticeably stronger.
Can we make these changes still, or is it too late? And do you think we should?
Cheers
Tim

201. 0972415204.txt
##

From: Phil Jones <p.jones@uea.ac.uk>
To: Brendaw Morris <brendawmorris@earthlink.net>
Subject: Re: JOC Review
Date: Tue Oct 24 15:20:04 2000

Dear Brendaw,

My review of the paper JCL 3435 is attached. My recommendation is
to accept the paper subject to minor changes. I don't wish to see it
again. If there are any problems with the attachment, let me know and
I can fax the 2 pages.

Cheers
Phil Jones

At 06:58 PM 10/7/00 -0400, you wrote:
>Professor Michael Mann, Editor of Journal of Climate, has suggested you as a
>possible reviewer of a paper entitled "Differential ENSO and volcanic
>effects on surface and tropospheric temperatures" (JCL-3435 by T. M. L.
>Wigley and B. D. Santer.
>
>Would you please let me know whether or not you will be able to do this
>review? If you accept, we ask that you complete your review by 11/24/00 (if
>possible). Hard copy or e-mail copies of reviews are very acceptable.
>
>Also, if you accept, please send your complete address including telephone
>and fax numbers for our files. Thank you so much.
>
>If you are unable to do this review, suggestions of other potential
>reviewers (and their e-mail addresses) for this paper would be greatly
>appreciated.
>
>Brenda W. Morris
>Editorial Assistant
>Journal of Climate
>
>
>
>
>

202. 0972499087.txt
##

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: Figures for revised version of paper
Date: Wed Oct 25 14:38:07 2000

Ben,
I hope the surgery next week goes OK. Ruth and I are going away next
week for a short break to Coldstream on the River Tweed. This was the
holiday cottage Matthew had planned to go to for his honeymoon, but the
fuel crisis around his wedding time precluded this. We were able to
negotiate the cottage for a later date, as we could get a refund or
claim on the insurance as a national emergency wasn't declared. So on
Nov 1 we will think about you !
I've listed off the diagrams and will take the text when it comes, but
I won't be able to send you any comments until the week of Nov 6.
Also just sent back comments to Mike Mann on the paper by Tom and
you factoring out ENSO and Volcanoes. Felt like writing red ink all over
it, but sent back a short publish suject to minor revision to Mike. This
is the first time I've ever reviewed one of Tom's or your papers !
Copy of what I sent is attached. I forgot to sign it before sending it !

    Again hope all is well later next week !

 Cheers
 Phil

At 06:37 PM 10/24/00 -0700, you wrote:
>Dear All,
>
>Sorry that it has taken me so long to revise our paper. As I mentioned in a
>previous email, I've had to repeat most of the calculations using an improved
>estimate of the pre-eruption reference level temperature (Tref). I've also had
>to look at the sensitivity of our results to uncertainties in Tref. I'd like to
>thank Tom for prompting me to take a critical look at this issue - it's an
>important one. I'd also like to thank the rest of you for all the comments that
>you've sent me. I hope I've addressed them adequately in the revised paper.
>
>Another major change is that, rather than giving results are based on a variety
>of different filtering options -- e.g., estimation of volcano parameters from
>unfiltered data (too noisy) and highly smoothed data (13-term Gaussian filter
>leads to underestimate of volcanically-induced cooling) -- we now only give
>results for our "best guess" filtering option,
>a five-term binomial filter. We still discuss sensitivities to tau (the volcanic
>signal decay time) and choice of ENSO index. Restricting attention to one
>filtering option reduces the length of Tables, and hopefully improves the
>clarity of the paper.
>
>I've rewritten the discussion of the iterative method, and we now make it clear
>that although this approach is automated, its implementation still involves a
>number of subjective decisions (filter choice, choice of averaging period for
>estimating pre-eruption reference temperature, choice of tau, etc.) Many of the
>changes made here attempt to address useful comments that I received from Tom.
>
>Lennart and Erich kindly provided me with the SLP data from the GSOP, GSO1 and
>GSO2 integrations. Recall that we did not have this data previously, and so our
>estimation of ENSO signals in GSO1 and GSO2 and of ENSO/volcano signals in GSOP
>was based on simulated Nino 3.4 SSTs only. We've now also used the (simulated)
>SOI to perform ENSO/volcano signal estimation.
>
>Section 5 (discussion of ECHAM4/OPYC results) has been completely rewritten,
>and the ordering of individual subsections should now be more logical. We
>discuss the simulated Pinatubo signal first, then the "ENSO component" of
>simulated temperature trends, and finally residual trends after the removal of
>volcano and ENSO effects.
>
>Today I'm sending you, as postscript attachments, the revised Figures for the
>paper. To simplify things I've encoded the Figure number at the top of the
>postscript file. I don't want to overload your mailboxes, so I'm sending the
>Figures in two separate mail messages. There should be 11 Figures in total.
>Tomorrow I'll send you the revised text of the paper and the Tables. Please let
>me know if you have any problems printing these files. Note that all Figures
>except Figure 7 are in color. Color is not essential for some of the Figures,
>and in the next day or two I'll prepare black-and-white versions of Figues 3, 5,
>6, 8, 9, 10 and 11. But for now I thought you might find it easier working with
>the color versions.
>
>I will be going in for surgery on November 1st, and am not sure how long it will
>be until I get back to my office. I realize that it may not be feasible to
>submit the paper before November 1st. But I'd really appreciate it if you could
>send me comments before November 1st. These will keep me occupied while I'm
>trying to get back on my feet!
>

>With best regards,
>
>Ben
>--
>--
>Benjamin D. Santer
>Program for Climate Model Diagnosis and Intercomparison
>Lawrence Livermore National Laboratory
>P.O. Box 808, Mail Stop L-264
>Livermore, CA 94550, U.S.A.
>Tel: (925) 422-7638
>FAX: (925) 422-7675
>email: santer1@llnl.gov
>--

203. 0972649870.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: Text and Tables of draft JGR paper
Date: Fri Oct 27 08:31:10 2000

Ben,
All received and printed. The weather forecast for the next few days is cold and windy, so I'll read this at the cottage in Coldstream. Hope everything goes OK later next week. I will email comments, hopefully on Nov 6, maybe Nov 7 if there is a lot of urgent things to do when I get back.

Cheers
Phil

At 05:18 PM 10/26/00 -0700, you wrote:
>Dear All,
>
>Here are the three postscript files with the title page, main text, and Tables
>for our draft JGR paper. Sorry it took me a bit longer to get these to you.
>Please let me know if you have any problems printing these files. You should
>already have all the Figures that I sent on Tuesday.
>
>I'll be in my office tomorrow and Monday and Tuesday of next week. After Tuesday
>the best way of getting in touch with me is by contacting PCMDI's secretary,
>Harriet Moxley (925-422-7638). I hope to be out of hospital and back in my
>office by November 10th. It would be nice if we could submit this paper shortly
>thereafter!
>
>With best regards, and thanks again for all your help,
>
>Ben
>--
>--
>Benjamin D. Santer
>Program for Climate Model Diagnosis and Intercomparison
>Lawrence Livermore National Laboratory
>P.O. Box 808, Mail Stop L-264

>Livermore, CA 94550, U.S.A.
>Tel: (925) 422-7638
>FAX: (925) 422-7675
>email: santer1@llnl.gov
>--
>Attachment Converted: "c:\eudora\attach\volcano_tables2.ps"
>
>Attachment Converted: "c:\eudora\attach\driver_maintext.ps"
>
>Attachment Converted: "c:\eudora\attach\driver_titlepage.ps"
>

204. 0973374325.txt
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##########

From: Mike Hulme <m.hulme@uea.ac.uk>
To: barker,vira
Subject: Fwd: BP funding
Date: Sat Nov 4 16:45:25 2000

Any idea who at Cambridge has been benefitting from this BP money?
Mike

From: "Simon J Shackley" <Mcysssjs@fs1.sm.umist.ac.uk>
Organization: umist
To: m.hulme@uea.ac.uk
Date: Thu, 2 Nov 2000 14:44:09 GMT
Subject: BP funding
Reply-to: Simon.Shackley@umist.ac.uk
CC: robin.smith@umist.ac.uk,
brian.launder@umist.ac.uk
Priority: normal
X-mailer: Pegasus Mail for Win32 (v3.12a)
dear TC colleagues
looks like BP have their cheque books out!   How can TC benefit from
this largesse? I wonder who has received this money within Cambridge
University?
Cheers, Simon
17) BP, FORD GIVE $20 MILLION FOR PRINCETON UNIVERSITY
EMISSIONS
STUDY
Auto.com/Bloomberg News
October 26, 2000
Internet: [1]http://www.auto.com/industry/iwirc26_20001026.htm
LONDON -- BP Amoco Plc, the world's No. 3 publicly traded oil
company, and Ford Motor Co. said they will give Princeton
University $20 million over 10 years to study ways to reduce
carbon-dioxide emissions from fossil fuels. BP said it will give
$15 million. Ford, the world's second-biggest automaker, is
donating $5 million. The gift is part of a partnership between the
companies aimed at addressing concerns about climate change.
Carbon dioxide is the most common of the greenhouse gases believed
to contribute to global warming.
London-based BP said it plans to give $85 million in the next
decade to universities in the U.S. and U.K. to study environmental
and energy issues. In the past two years, the company has pledged
$40 million to Cambridge University, $20 million to the University
of California at Berkeley and $10 million to the University of
Colorado at Boulder.

References

   1. http://www.auto.com/industry/iwirc26_20001026.htm

205. 0973867989.txt
###
##########

From: Eric.Steig@sas.upenn.edu (via the vacation program)
To: k.briffa@uea.ac.uk
Subject: away from my mail
Date: Fri, 10 Nov 2000 09:53:09 -0500 (EST)

I am away for a couple of days. This is an automatic reply. I will reply
to your mail regarding "reminder" when I return on Sunday.

206. 0974731263.txt
###
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: jjzeeberg <jzeebe1@uic.edu>
Subject: Re: temperature time series
Date: Mon Nov 20 09:41:03 2000

   Hi Jap
   please see the following - I have had the data put on my web site and I will
slowly put
   other data and Figures and Abstracts on there also. Let me know if you have a
problem
   downloading the data. Good luck
   Keith
   The data you want are included in those listed under -
   [1]http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
   At 03:56 PM 11/17/00 -0600, you wrote:

     Dear Mr Briffa,
     I remind you to send me your temperature reconstructions for northern
     Scandinavia and the Polar Urals.
     JaapJan Zeeberg
     At 02:55 PM 11/14/2000 +0000, you wrote:
     >Dear Jap
     >I am sorry , but your earlier message must have slipped through the net .
     >I will try to look out the data and send them to you in the next couple of
     >days or so. Please remind me on thursday if they have not arrived.Best wishes
     >Keith
     >
     >At 02:14 PM 11/13/00 -0600, you wrote:
     >
     >>Dear Dr Briffa,
     >>
     >>You may not have received this message the first time I sent it (30/10);
     >>
     >>I am a PhD-student at the University of Illinois, Chicago. I study the
     >>effect of North Atlantic modulated inputs of precipitation and summer warmth
     >>on the glacier mass balance of Novaya Zemlya. Results will appear in the
     >>January or March-issue of The Holocene.
     >>
     >>I would like to use your temperature reconstructions for the northern Urals
     >>and northern Fennoscandia published in Nature 376, p. 156-159 (1995). I
     >>plan to compare the temperature time series with grain size distributions of

>>three sediment cores obtained from Russkaya Gavan', a fjord at north Novaya
>>Zemlya. These cores span the past ~4 centuries.
>>
>>I could not find the requested time series in the NOAA data base and would
>>be grateful if you could provide them.
>>
>>Sincerely
>>
>>JaapJan Zeeberg
>>
>>
>>=======================================
>>JaapJan Zeeberg
>>
>>[2]http://www2.uic.edu/~jzeebe1/news.htm
>>
>>845 W. Taylor Street MC186
>>Chicago, IL 60607-7059, USA
>>
>>Phone: 312-996-3154
>>Fax: 312-413-2279
>>e-mail jzeebe1@uic.edu
>>
>>==
>
>--
>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090   Fax: +44-1603-507784
>
>

--
Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
Norwich, NR4 7TJ, United Kingdom
Phone: +44-1603-592090   Fax: +44-1603-507784

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
2. http://www2.uic.edu/~jzeebe1/news.htm

207. 0975693499.txt
##
##########

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: New tree-ring density data
Date: Fri, 01 Dec 2000 12:58:19 -0500
Cc: srutherford@virginia.edu, mann@virginia.edu

Scott, Tim,

Here's the abstract.

If the results pan out, then several us us may want to be discussing this
work on the talk circuit.

This is the first stab! Notice how safe (a very results-insensitive abstract!)

mike

XXVI General Assembly, Spring EGS Meeting

Comparison of Large-Scale Proxy-Based Temperature Reconstructions Over the Past Few Centuries

MANN, M.E.; RUTHERFORD, S; OSBORN, T.J.

OA28.0 Study of past climates: Climate of the past millennium

JOUZEL, J.; (co-conveners: JONES, P.D.; MANN, M.E.)

Comparison of Large-Scale Proxy-Based Temperature Reconstructions Over the Past Few Centuries

M.E. Mann(1), S. Rutherford(1), and T.J. Osborn(2)

(1) Univ. of Virginia, USA, (2) Climate Research Unit, Univ. East Anglia, UK

A promising approach to the problem of reconstructing patterns of past climate variability
involves the application of spatial climate field reconstruction (CFR) techniques to networks of proxy
climate indicators (e.g., Mann et al 1998;2000--see http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html).
This approach seeks to exploit the complimentary information in a diverse network of proxy indicators by determining the most consistent relationships between these
networks of data and the leading spatial patterns of climate variability during a recent "calibration" period of overlap with the modern instrumental record.
The calibrated relationship is then used to estimate large-scale patterns of climate variability
in the past from the proxy data. This method makes no assumptions regarding the relationship between a given proxy indicator and specific local annual/seasonal climate variable, but
does assume that the proxy indicator is tied to some combination of large-scale patterns of climate variability.
Alternatively, it is possible to estimate large-scale temperature patterns from a relatively homogenous network
of proxy climate indicators (e.g., tree-ring density data--see Briffa et al, 1998) by invoking
a local calibration between each climate indicator and the climate variable (e.g., summer temperature)
of interest. This approach is more conservative in the amount of information it seeks to
extract from the proxy data network, but it is free from assumptions regarding the large-scale
patterns of past climate variability. Recent reconstructions of Northern Hemisphere annual-mean and
warm-season temperature patterns using these respective approaches and data show some similarities,
but also some important differences. Here we investigate these differences more closely, examining
the sensitivity of Northern Hemisphere temperature pattern reconstructions to (a) the underlying
proxy data used, (b) the particular method used to estimate large-scale patterns from these data,
and (c) the target seasonality of the reconstruction. By controlling independently for each of these
three factors, we gain insight into the reasons for differences between various proxy-based estimates
of past large-scale temperature variability.

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

e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804) 982-2137
http://www.evsc.virginia.edu/faculty/people/mann.html

208. 0976640129.txt
##

From: Eric Steig <esteig@sas.upenn.edu>
To: <masson@lsce.saclay.cea.fr>, jouzel@lsce.saclay.cea.fr, ddj@gfy.ku.dk, fujii@pmg.nipr.ac.jp, tas.van.omnen@utas.edu.au, vimeux@lsce.saclay.cea.fr, <fisher@nrn1.NRCan.gc.ca>, <ethompso@magnus.acs.ohio-state.edu>, <Koerner@ess-dns2.gsc.nrcan.gc.ca>, edw@geophys.washignton.edu, clow@usgs.gov
Subject: No Subject
Date: Tue, 12 Dec 2000 11:55:29 -0500
Cc: <raynaud@glaciog.ujf-grenoble.fr>, k.briffa@uea.ac.uk, steig@geophys.washington.edu

Dear Colleagues

At the HIHOL meeting in Avignon in October, several of us (Steig, van Ommen, Dahl-Jensen, Vimeux) agreed to write a review paper addressing Holocene climate change as viewed from polar ice core records. The main task of writing and organizing this paper has fallen upon Tas and Eric, and we are writing to solicit your interest, support, and contribution. We would appreciate hearing from each of you with comments on our proposed plan, requests for clarification and (hopefully) data sets. We hope you will be interested in working with us on this project. Note that the deadline for completion is the end of March, 2001.

Although the question of Holocene climate change has obviously been addressed in numerous papers on individual ice core records (and most recently in the Masson et al. review of Antarctic records in QR), we believe that
it would be valuable to select the best-understood, best dated, polar ice core data from both hemispheres and put them in a single paper. We also think that the paper should be limited only to

1) data that address directly the TEMPERATURE history at high latitudes -- the information
we get from isotopes and from borehole reconstructions -- as opposed e.g. to atmospheric circulation changes that one gets from the chemistry record, and

2) discussion of the long-term variations, as opposed to short term variations such as the Little Ice Age.

The intention here is not to be exclusive of either people or ideas, but to limit the scope of the paper so that it is as definitive a document as possible. Of particular interest is the "simple" question of the timing and magnitude of the "thermal maximum", the subsequent Holocene cooling, and their relationship to insolation forcing. This was a major question at the HIHOL meeting and we do not believe it was adequately resolved there.

Our vision is a summary paper that not only reproduces already-published work, but that carefully quantifies the uncertainties inherent in each of the reconstructions. Of particular interest are the possible affects of elevation change on the records, and uncertainties in the timescales. We cannot say a priori what the conclusions of this paper will be. An example might be that the "thermal maximum" was actually warmer than present - a major issue of contention in the popular literature - and was more-or-less simultaneous in both polar regions. If this is correct, it will be a useful service to the paleoclimate community to demonstrate it. Alternatively, we may find after carefully looking at the data that we CANNOT reach such a conclusion. This would be an equally important result.

How should we proceed? Our suggestion is that those who are willing to participate send their favorite ice-core based temperature reconstructions to us, providing the best available timescales and a brief description of the uncertainties you ascribe to the reconstruction. We will compile the data and produce both 1) a single file containing all the data, and 2) a PDF figure comparing all the independent temperature reconstructions. We can then intitiate discussion around a common figure, so that everyone is looking at exactly the same information. The last 11,000 years would be considered the appropriate time interval to consider. We do not wish to confuse matters by including the glacial-interglacial transition!

Data sets that we think would be particularly important include the following. Note that we will probably need to include other authors. This is just a preliminary list and is not intended to exclude anyone. We are also aware that some of these data are so far unpublished but we hope that they could be included anyway, perhaps in "smoothed" form (?).

1) Isotope profiles from Vostok, Byrd (and EPICA, if possible), on the most-accepted timescales (Francoise).

2) Isotope profiles from Taylor Dome and Siple Dome, Dye 3 and GISP2 (Eric).

3) Isotope profile from Dome Fuji (Fujii)

3) Isotope profiles and borehole temperatures from Law Dome core(s) (Tas, Vin).*

4) Isotope data from GRIP (and from N-GRIP if possible) (Dorthe)

4) Borehole data from Taylor Dome, GISP2, Dye 3 (Gary, Ed).*

5) Borehole data from GRIP (and N-GRIP if possible) (Dorethe)

6) Isotope, meltlayer frequency, and borehole T data from the Canadian ice caps (David, Fritz)

7) Meltlayer data from other sites (GISP2 - Alley?)

*The Law and Taylor Dome records only go to mid Holocene but would still be very useful here!

Other suggestions for data sets and people to contact?

Again, please reply to this email with your comments, criticisms concerns, request for clarification and (hopefully) data sets!

Thanks!

Warm regards to all,

Eric Steig & Tas van Ommen

209. 0976807838.txt
##

From: Eric Steig <steig@geophys.washington.edu>
To: Valerie Masson-Delmotte <masson@lsce.saclay.cea.fr>, Eric Steig <esteig@sas.upenn.edu>
Subject: Re: HILOL "optima"?
Date: Thu, 14 Dec 2000 10:30:38 -0500
Cc: jouzel@lsce.saclay.cea.fr, ddj@gfy.ku.dk, fujii@pmg.nipr.ac.jp, tas.van.ommen@utas.edu.au, vimeux@lsce.saclay.cea.fr, fisher@nrn1.NRCan.gc.ca, ethompso@magnus.acs.ohio-state.edu, Koerner@ess-dns2.gsc.nrcan.gc.ca, edw@geophys.washignton.edu, clow@usgs.gov, raynaud@glaciog.ujf-grenoble.fr, k.briffa@uea.ac.uk, Valerie Masson <masson@lsce.saclay.cea.fr>

Valerie, Francoise et al.

We also were suprised by the "conclusion" that there was a 9-7 ka optimum. This probably arose from a statement by Greg Zielinski regarding the Arctic records. In any case, the article by Dominique and Kieth was just a rough draft -- we have pointed out the mistake to them and I expect we will all see a final version anyway!

Regarding the subject of the HIHOL paper, we agree that there are already many papers published that dicuss the temperature interpretation of isotopic records during the Holocene. What has not been done, however, is to include the best Holocene records from both polar regions in a single paper, nor to make a specific comparison of the timing and magnitude of the optimum (or optima). For example, the elevation effect on the long-term trends for East Antartica has been discussed (Masson et al., 2000) but not quantified. Of course quantifying this effect is difficult but our paper could put useful error estimates, for example, on the amount of cooling in the late Holocene. We do not of course wish to compete with Sigfus, but his paper will be more limited in geographic focus than ours and will include new data that we will not use. It would be good to include NGRIP borehole temperatures if we can, but this is not necessary. Even the GRIP and GISP2 records show very clearly the Holocene optimum. Our suggestion would be to let Dorethe decide on that, in consultation with Sigfus.

In our vision, one of the key features of the Holocene article will be its deliberately limited scope and confinement to observation rather than speculation about causes of climate change. We think that to involve modelers and oceanographers makes it difficult to keep the focus and is rather beyond the intended purposes of the Holocene volume. Keep in mind that modelling was looked at separately at the HIHOL meeting and we believe that the modelers at the meeting are planning their own contribution to the volume.

As mentioned earlier, we think the best way to get the paper going is to begin soon the process of simply collating data sets and putting them all on one graph. We can then discuss the details of the paper with the same image in front of each of us.

We hope that you can agree more-or-less with the above, and that others on our email list will also provide some input. We are of course open to further discussion!

Further comments?

Eric and Tas

At 12:07 PM 12/13/00 +0100, Valerie Masson-Delmotte wrote:
>
>Dear Eric and Tas, dear collegues,
>
>First, thank you for your initiative in motivating a comparison of ice
>isotope and borehole temperature records from both hemispheres from the
>Holocene. We think that it is important to position this work with respect
>to other related studies. There are in particular several papers already
>discussing the temperature interpretation of isotopic records during the
>Holocene (see below for Greenland; correcting the isotopic profiles in
>Antarctica from trends due to SST or ocean isotopic composition changes,
>based on the deuterium excess).
>
>As Dorthe will probably confirm, there is an ongoing work conducted by
>Sigfus Johnsen to be submitted to Journal of Quaternary Sciences next
>year, aiming at comparing all the Greenland Holocene temperature and
>isotopic profiles (including North GRIP).
>
>Therefore we think that it important to better define the scope of the
>HILOL possible paper (comparing north and south Holocene isotopic records
>and discussing the climate mechanisms involved) more than discussing the
>temperature imprint on water isotope records for instance.
>
>Second, we are still under the shock of the HILOL conclusions, mentionning
>a widespread Antarctic temperature optimum supposely seen in all ice cores
>between 9 and 7 ka BP! In our paper published in Quaternary Research in
>november 2000 (data presented by Francoise at HILOL), we had a careful
>comparison of 11 existing Holocene Antarctic isotopic records (but
>without Dome F, so without ice cores in the Atlantic sector). Although we
>had no control on the independent time scales of these ice cores, they are
>all precisely dated during the transition and there is no doubt from the
>simple view of the raw isotopic (deuterium or oxygen 18) data, that they
>all exhibit a clear optimum from 11.5 to 9 ka BP, followed by a relative
>minimum at around 8 ka BP. Now, the sites located around the Ross Sea show
>a mid Holocene optimum (8 to 6 ka BP), whereas in East Antarctica (apart
>from Dome C and Taylor Dome) a third "warm" interval can be seen later (6
>to 3 ka BP). This is why we were quite surprised to hear about an optimum
>between 9 and 7 ka BP in Antarctica.
>
>Last, if the HILOL possible paper is supposed to discuss the different
>timing of the major optima in the north and the south high latitudes,
>then it would greatly benefit from including climate modellers using
>intermediate complexity models (such as CLIMBER) and oceanographers (to
>discuss the possible role of changes in the north Atlantic circulation in
>the first half of the Holocene).
>
>In such a framework, we are obviously willing to participate in the
>climate mechanisms discussion and of course provide the isotopic data
>measured at LSCE (e.g. Dome B, Vostok, "old" Dome C and EPICA Dome C). For
>Byrd, you need to contact the Danish group.
>
>Sincerely,
>
>   Valerie and Francoise.
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>____
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