

PETITIONERS' EXEMPLARS (PE)

PE-1

Date: Thu, 27 Mar 2008 10:28:38 +0000

From: Phil Jones <p.jones@uea.ac.uk>
To: trenbert@ucar.edu,"Jonathan Overpeck" <jto@u.arizona.edu>
Subject: Re: Fwd: ukweatherworld
Date: Thu, 27 Mar 2008 10:28:38 +0000
Cc: mann@multiproxy.evsc.virginia.edu,santer1@ltnl.gov, "Susan Solomon"
<susan.solomon@noaa.gov>

<x-flowed>

Peck et al,

I recall meeting David Deeming at a meeting years ago (~10). He worked in boreholes then. I've seen his name on several of the skeptic websites.

Kevin's idea is a possibility. I wouldn't post on the website 'ukweatherworld'.

The person who sent you this is likely far worse. This is David Holland. He is a UK citizen who send countless letters to his MP in the UK, writes in Energy & Environment about the biased IPCC and has also been hassling John Mitchell about his role as Review Editor for Ch 6. You might want to talk to John about how he's responding. He has been making requests under our FOI about the letters Review Editors sent when signing off. I'm sure Susan is aware of this. He's also made requests for similar letters re WG2 and maybe 3. Keith has been in contact with John about this.

I've also seen the quote about getting rid of the MWP - it would seem to go back many years, maybe even to around the TAR. I've no idea where it came from. I didn't say it!

I've written a piece for RMS [popular journal Weather on the MWP and LIA - from a UK perspective. It is due out in June. I can send if you want.

I'm away all next week - with Mike. PaleoENSO meeting in Tahiti - you can't turn those sorts of meetings down!

Cheers

Phil

At 23:15 26/03/2008, Kevin Trenberth wrote:

>Hi Jon

>There is a lot to be said for ignoring such a thing. But I understand the
>frustration. An alternative approach is to write a blog on this topic of
>the medieval warm period and post it at a neutral site and then refer
>enquiries to that link. You would have a choice of directly confronting
>the statements or making a more general statement, presumably that such a
>thing is real but was more regional and not as warm as most recent times.
>This approach would not then acknowledge that particular person, except
>indirectly.

>

>A possible neutral site might be blogs.nature.com/climatefeedback/
>I posted a number of blogs there last year but not this year. I can send
>you the contact person if you are interested and you can make the case
>that they should post the blog.

>

>Good luck

>Kevin

>

PETITIONERS' EXEMPLARS (PE)

>
>> Hi Phil, Kevin, Mike, Susan and Ben - I'm looking
>> for some IPCC-related advice, so thanks in
>> advance. The email below recently came in and I
>> googled "We have to get rid of the warm medieval
>> period" and "Overpeck" and indeed, there is a
>> person David Deeming that attributes the quote to
>> an email from me. He apparently did mention the
>> quote (but I don't think me) in a Senate hearing.
>> His "news" (often with attribution to me) appears
>> to be getting widespread coverage on the
>> internet. It is upsetting.
>>
>> I have no memory of emailing w/ him, nor any
>> record of doing so (I need to do an exhaustive
>> search I guess), nor any memory of him period. I
>> assume it is possible that I emailed w/ him long
>> ago, and that he's taking the quote out of
>> context, since know I would never have said what
>> he's saying I would have, at least in the context
>> he is implying.
>>
>> Any idea what my reaction should be? I usually
>> ignore this kind of misinformation, but I can
>> imagine that it could take on a life of it's own
>> and that I might want to deal with it now, rather
>> than later. I could - as the person below
>> suggests - make a quick statement on a web site
>> that the attribution to me is false, but I
>> suspect that this Deeming guy could then produce
>> a fake email. I would then say it's fake. Or just
>> ignore? Or something else?
>>
>> I googled Deeming, and from the first page of
>> hits got the sense that he's not your average
>> university professor... to put it lightly.
>>
>> Again, thanks for any advice - I'd really like
>> this to not blow up into something that creates
>> grief for me, the IPCC, or the community. It is
>> bogus.
>>
>> Best, Peck
>>
>>
>>>X-Sieve: CMU Sieve 2.3
>>>Reply-To: "David Holland" <d.holland@theiet.org>
>>>From: "David Holland" <d.holland@theiet.org>
>>>To: <jto@u.arizona.edu>
>>>Subject: ukweatherworld
>>>Date: Mon, 24 Mar 2008 08:39:10 -0000
>>>
>>>Dear Dr Overpeck,
>>>
>>>
>>>

PETITIONERS' EXEMPLARS (PE)

> >>I recall David Deeming giving evidence to a
> >>Senate hearing to the effect that he had
> >>received an email including a remark to the
> >>effect "We have to get rid of the warm medieval
> >>period". I have now seen several comment web
> >>pages attribute the email to your. Some serious
> >>and well moderated pages like
> >>ukweatherworld would welcome a post from you if
> >>the attribution is untrue and would, I feel
> >>sure, remove it if you were to ask them to. I am
> >>sure that many other blogs would report your
> >>denial. Is there any reason you have not issued
> >>a denial?
> >>
> >>
> >>
> >>David Holland
> >
> >
> > --
> > Jonathan T. Overpeck
> > Director, Institute for the Study of Planet Earth
> > Professor, Department of Geosciences
> > Professor, Department of Atmospheric Sciences
> >
> > Mail and Fedex Address:
> >
> > Institute for the Study of Planet Earth
> > 715 N. Park Ave. 2nd Floor
> > University of Arizona
> > Tucson, AZ 85721
> > direct tel: +1 520 622-9065 begin_of_the_skype_highlighting +1 520 622-9065
end_of_the_skype_highlighting
> > fax: +1 520 792-8795
> > <http://www.geo.arizona.edu/dges/>
> > <http://www.ispe.arizona.edu/>
> _____

>Kevin Trenberth
>Climate Analysis Section, NCAR
>PO Box 3000
>Boulder CO 80307
>ph 303 497 1318 begin_of_the_skype_highlighting 303 497 1318
end_of_the_skype_highlighting
><http://www.cgd.ucar.edu/cas/trenbert.html>

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

</x-flowed>

PETITIONERS' EXEMPLARS (PE)

PE-2

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

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

To: f14@zedat.fu-berlin.de

Subject: Re: program code

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

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

Dear Irina,

The code we used in Mann/Bradley/Hughes 1998 was not changed or "improved", but there may be different versions of the code floating around, and in a previous email to Uli Cubasch, I indicated that I was not sure the version you have (from Tim Osborn), is identical to the version we used in our original paper (it would require some work on my part to insure it gives precisely the same results, and I don't have the time to do that). I suspect, however, that the code is the same as the one we used in our paper and any differences, if they exist, should be minor (as long as the code compiles and runs correctly on the platform you have--the possible platform-dependence of fortran is a potential cause for concern here).

Numerous people have coded up our method independently, including Ed Zorita, w/ whom I believe your group has a close collaboration, and my graduate student Zhang has successfully coded this up independently in Matlab (its a short script, which didn't take Zhang long to write anyway). I'm copying this message to Zhang, so that he can provide you with his matlab version of the code if you are interested. Because Zhang's version is in Matlab, it should run correctly, independently of the particular platform (an advantage over the fortran code) [As an aside, on a pedagogical note, I would still encourage you to code this up yourself].

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

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

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

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', Geophysical Research Letters, 29 (10), 1501, doi: 10.1029/2001GL014554, 2002.

available at:

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

Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., Climate Field Reconstruction Under Stationary and Nonstationary Forcing, Journal of Climate, 16, 462-479, 2003.

available at:

[3]<ftp://holocene.evsc.virginia.edu/pub/mann/Rutherfordetal-Jclim03.pdf>

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

I hope you find this information of help.

Best of luck w/ your research,

mike mann

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

PETITIONERS' EXEMPLARS (PE)

Dear Michael,

I believe that you have not heard about me as yet. My name is Irina Fast. Since the January 2003 I am a PhD student at the Free University in Berlin in the framework of the EU-Project SOAP. My supervisor is Ulrich Cubasch.

At the SOAP's start-up meeting it was proposed to use your multiproxy calibration method (published in 1998) for the joint analysis of model simulations and proxydata.

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

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

I apologise for my mistakes in this letter.

Best regards

Irina Fast

--

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

e-mail: f14@zedat.fu-berlin.de

phone: +49 (0)30 838 711 22 begin_of_the_skype_highlighting +49 (0)30 838 711 22

end_of_the_skype_highlighting

fax: +49 (0)30 838 711 60

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

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

References

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

PETITIONERS' EXEMPLARS (PE)

PE-3

Date: Thu, 24 May 2001 11:33:02 -0400

From: "Michael E. Mann" <mann@virginia.edu>
To: christy@nsstc.uah.edu
Subject: Re: FYI: Fwd: Re: IPCC
Date: Thu, 24 May 2001 11:33:02 -0400
Cc: rbradley@geo.umass.edu, tkarl@ncdc.noaa.gov, tom crowley <tom@ocean.tamu.edu>, mhughes@ltrr.arizona.edu, jto@u.arizona.edu, rbradley@geo.umass.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk, "Folland, Chris" <ckfolland@meto.gov.uk>, jouzel@lsce.saclay.cea.fr, trenbert@cgd.ucar.edu, steig@ess.washington.edu, mann@virginia.edu

<x-flowed>

John:

For future reference, I think its also important to clarify for you what the Dahl-Jensen, Clow et al borehole results actually show (see Dahl-Jensen et al, "Past Temperatures Directly from the Greenland Ice Sheet", Science, 282, October 1998).

In fact, the results show that the amplitude of variability over the past 1000+ years differs by a factor of 2 between the GRIP and Dye 3 borehole estimates (the latter only 865 km to the south). This is an example of extreme regional-scale variability, which should give pause to those who want to draw large-scale inferences.

However, even more importantly, they show in the case of Dye 3, the mid 20th century warm period in the record actually exceeds the Medieval warm peak! (see Fig 4, lower panel, blue curve). So here we have two temperature histories less than 1000 km apart in Greenland, which give different stories regarding the level of Medieval warmth, with at least one of the histories conforming precisely to the hemispheric trends presented in IPCC chapter 2 (note that in the chapter, we actually discuss the evidence of conflicting temperature trends in Greenland, though not specifically referring to Dahl-Jensen et al).

So do I understand correctly that you are referring to the results of Dahl-Jensen et al as conflicting with what we say in the chapter? At the face of it, this argument has no merit whatsoever. I think we should all use a better explanation from you, since you seem to be arguing publically that the Dahl-Jensen et al record undermines what we've said in the chapter.

Thanks in advance,

mike

p.s. I've cc'd in Eric Steig, a collaborator of Clow's and a Greenland & Antarctic Ice Core expert, to make sure my facts above have been presented accurately. Perhaps Eric would be kind enough to forward my email to Gary Clow, and Gary can let us know directly if he disagrees with any of my remarks above.

At 03:30 PM 5/23/01 -0400, Michael E. Mann wrote:

>John,

>

>I appreciate your reply.

>

PETITIONERS' EXEMPLARS (PE)

>However, I don't agree at all w/ your assessment. It was determined early
>on that the ice core borehole results would be discussed in the context of
>the millennial-scale variability section, as they arguably don't have the
>resolution to address the timescales relevant to the past 1000 years. So
>this was in Jean's domain, not mine, and if the cross-references between
>the sections aren't clear enough in that regard, that is indeed our fault.

>

>However, there is considerable discussion of the fact that the
>Arctic/North Atlantic regions are inappropriate for inferences into
>hemispheric-scale temperature patterns, and this remains fundamentally
>from any reasonable treatment of the underlying climate dynamics that
>influence that region.

>

>The various hemispheric temperature reconstructions discussed in our
>chapter (the emphasis was on the commonality between them), including Mann
>et al, Jones et al, Briffa et al, Crowley and Lowery, and others, make
>considerable use of just about all of the available reliable low-res and
>high-res paleo data available, and come to a clear consensus regarding the
>relative warmth of the Medieval period at the hemispheric/global scale.
>Crowley's modeling results come to the same conclusion, and it entirely
>independent of
>any empirical paleoclimate reconstructions.

>

>You misrepresent the Mann et al reconstruction--it is not based on "tree
>rings", but uses all high-resolution proxy information commonly available.
>We have shown, in fact, that our reconstruction is robust to the
>inclusion/exclusion of tree ring information. The Crowley and Lowery
>reconstruction, which is discussed in our chapter, makes use of almost no
>tree ring data, and employs lower-resolution proxy indicators, including
>the very records (Keigwin, Lamb's central England temperature record,
>GISP2 018) that are often used to argue for a warmer MWP, and yet comes to
>the same conclusion. And Tom shows that when averaged across the
>hemisphere, a warmer-than-present-day MWP just doesn't hold up.

>

>Our treatment of this subject in the chapter was far more careful, far
>more inclusive and detailed, and far more nuanced than you give us credit
>for. Your comments below remain disturbingly selective and myopic, and we
>have dealt w/ similar comments many times over...

>

>If ABC is looking to do a hatchet job on IPCC so be it (this doesn't
>surprise me--Stossel has an abysmal record in his treatment of
>environmental issues, from what I had heard), but I'll be very disturbed
>if you turn out to have played into this in a way that is unfair to your
>co-authors on chapter 2, and your colleagues in general. This wouldn't
>have surprised me coming from certain individuals, but I honestly expected
>more from you...

>

>Mike

>

>>Date: Wed, 23 May 2001 13:50:49 -0500
>>From: John Christy <christy@nsstc.uah.edu>
>>X-Mailer: Mozilla 4.04 (Macintosh; I; PPC)
>>To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
>>Subject: Re: IPCC

>>

>>Hi Mike:

PETITIONERS' EXEMPLARS (PE)

>>

>>Here's what happened. ABC News 20/20 with Stossel wanted me to be part of a segment that will air at the end of June on the climate change issue. Specifically the piece will be dealing with the alarmist rhetoric that tends to be found in the media. I am more than happy to talk about that because I've been very disappointed with what has gone on even with respect to some of the IPCC elders and their pronouncements for forthcoming disasters.

>>

>>In one of the pre-interviews they asked about the "Hockey Stick". I told them of my doubts about the intercentury precision of the record, especially the early part, and that other records suggested the period 1000 years ago was warmer. I remember saying that "you must give the author credit for including the large error bars for that time series in the figure." I also specifically said that the most precise record of century scale precision, Greenland Borehole temps, was very important to note but that the figure was not in the IPCC. I then looked quickly at the IPCC reference list and saw the citation of Dahl-Jensen and assumed that it was at least commented on in the 1000 year time series material and told ABC as much.

>>

>>ABC called back a few days later and said they couldn't find a reference to the Greenland stuff in the IPCC discussion of the past 1000 years. So I read the final version, and ABC was right. I said this was an omission that should not have happened - and that I take part of the blame because I had mentioned it at each of our Lead Author meetings.

>>

>>Last Thursday night, I was one of the guys flown to NY City for the taping of the show. There was only one question on this particular issue (it was even after Stossel had left the room) and I gave much the same answer as I indicated above (as best as I can remember)- that the "Hockey Stick" (I don't think I used the term "Hockey Stick", and I'm almost positive I did not mention your name at any point) is one realization of temperatures but that other data are not included and that I had thought the "other" data were clearly mentioned in the IPCC, but weren't. I mentioned the large error bars (as a credit to you) and that I was partly to blame for this omission. If they use my remark, they could slice and dice it to make it as provocative as possible.

>>

>>Four of us were taped for almost 2 hours, and from this they will select about 8 minutes, so I doubt my remarks will make the show. When Stossel came back in after all was said and done, he said to me that I might be a good scientist but I didn't have the emotion and passion necessary to excite the audience. In one way, that is a compliment I suppose. I think Pat M. will have a good chunk of air time (I don't remember whether he added any comments on the 1000-year time series, but he may have).

>>

>>Whatever is shown, just keep it in context. There is no way a clear scientific point with all the caveats and uncertainties can come across in such venues. However, I do agree with Stossel's premise (though I don't know what the piece will actually look like so I may be disappointed) that the dose of climate change disasters that have been dumped on the average citizen is designed to be overly alarmist and could lead us to make some bad policy decisions. (I've got a good story about the writers of the TIME cover piece a couple of months ago that

PETITIONERS' EXEMPLARS (PE)

>>proves they were not out to discuss the issue but to ignore science and
>>influence government.)

>>

>>It is not bad science to look at arguably the most precise measure of a
>>point temperature (actually two boreholes) when that point shows a 600+
>>year period of greater warmth than today. On that time scale, the
>>equivalent spatial scale is much larger than any of the regional
>>oscillations we now identify. But, there are several other (admittedly
>>less robust) measures that suggest greater warmth 1000 years ago that
>>are outside the N. Atlantic area. I just don't think tree rings, if
>>averaged over a century, can tell us which century was warmest. We've
>>never had two complete, independent centuries of global instrumental
>>data (separated by more than one century) to even test this idea. (By
>>the way, I came to my own conclusions long before Broekers piece
>>appeared.) This is an area of further work that I promoted to the NRC
>>about 2 months ago (more funding for Paleo work to assess intercentury
>>precision of all proxy records.)

>>

>>Regarding the IPCC. The IPCC TAR is good, but it is not perfect nor
>>sacred and is open to criticism as any document should be. In some
>>cases it is already outdated. Some of the story lines used to generate
>>high temperature changes are simply ridiculous. The IPCC is us. We are
>>under no gag rule to keep our thoughts to ourselves. I thought our
>>chapter turned out pretty good overall, and I attribute that to the
>>open, working relationship we all had (some other chapter groups did not
>>experience this) and to the tireless efforts of our convening lead
>>authors.

>>

>>Good to hear from you.

>>

>>John C.

>>

>>

>>--

>>*****

>>John R. Christy

>>Director, Earth System Science Center voice: 256-961-7763 begin_of_the_skype_highlighting
256-961-7763 end_of_the_skype_highlighting

>>Professor, Atmospheric Science fax: 256-961-7751

>>Alabama State Climatologist

>>University of Alabama in Huntsville

>><http://www.atmos.uah.edu/atmos/christy.html>

>>

>>Mail: University of Alabama in Huntsville, Huntsville AL 35899

>>Express: NSSTC/ESSC 320 Sparkman Dr., Huntsville AL 35805

>

>

> 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 begin_of_the_skype_highlighting
7770 end_of_the_skype_highlighting FAX: (804) 982-2137

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

>

(804) 924-

PETITIONERS' EXEMPLARS (PE)

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 begin_of_the_skype_highlighting (804) 924-
7770 end_of_the_skype_highlighting FAX: (804) 982-2137
<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

</x-flowed>

PETITIONERS' EXEMPLARS (PE)

PE-4 Date: Wed, 22 Sep 1999 12:58:14 +0100

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>,k.briffa@uea.ac.uk
Subject: Re: IPCC revisions
Date: Wed, 22 Sep 1999 12:58:14 +0100
Cc: ckfolland@meto.gov.uk,tkarl@ncdc.noaa.gov

Mike,

Been away in Japan the last week or so. Malcolm was there in a wheelchair because of his ruptured achilles. We both mentioned the lack of evidence for global scale change related to the MWE and LIA, but all the later Japanese speakers kept saying the same old things.

As for the TAR Chap 2 it seems somewhat arbitrary division to exclude the tree-ring only reconstructions. Keith's reconstruction is of a different character to other tree-ring work as it is as 'hemispheric in scale' as possible so is unlike any other tree-ring related work that is reported upon.

If we go as is suggested then there would be two diagrams - one simpler one with just Mann et al and Jones et al and in another section Briffa et al. This might make it somewhat awkward for the reader trying to put them into context.

The most important bit of the proxy section is the general discussion of 'Was there an MWE and a LIA' drawing all the strands together. Keith and I would be happy to look through any revisions of the section if there is time.

One other thing, did you bring up the possibility of having a proxy-only chapter (albeit short) for the next assessment ?

On Venice I suggested to Peck that you and Keith give talks on the reconstructions - frank and honest etc emphasising issues and I lead a discussion with you both and the rest of those there where the issues can be addressed (ie I would like to get the views of other proxy types and the modellers/detectors there). I suggested to Peck that this was early in the week as I have to leave on the Thursday to go to the last day of a Working Group meeting of the Climate Change Detection group in Geneva (a joint WMO Commission for Climatology/CLIVAR). I hope to report on the main findings of the Venice meeting.

Another issue I would like to raise is availability of all the series you use in your reconstructions. That old chestnut again !

How is life in Charlottesville ? Do you ever bump into Michaels or is always off giving skeptical talks ?

Tim Osborn is making great progress with his NERC grant and will be looking into dates soon for coming to see you.

Cheers
Phil

PETITIONERS' EXEMPLARS (PE)

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

PETITIONERS' EXEMPLARS (PE)

PE-5

Date: Thu, 23 Sep 1999 13:34:14 -0400

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:34:14 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks for your comments Phil,

They look quite reasonable, and I will seek to incorporate them. I'll need Keith's comments by tomorrow morning (my time) at the very latest if I am to have time to assess them and incorporate them.

Some important specifics:

1) I am definitely using the version of the Briffa et al series you sent in which Keith had restandardized to retain *more* low-frequency variability relative to the one shown by Briffa et al (1998). So already, the reconstruction I'm using is one-step removed from the published series (as far as I know!) and that makes our use of even this series a bit tenuous in my mind, but I'm happy to do it and let the reviewers tell us if they see any problem. If I understand you correctly, there is yet a new version of this series that is two steps removed from Briffa et al (1998)? Frankly, at this stage I think we have to go w/ what we have (please see Ian Macadam's plot when it is available--I think the story it tells isn't all that bad, actually) for the time being. Things as you say will change following review anyways.

2) One other thing--I'm actually averse to shortening the section on sediments. Even if they haven't contributed to some of the multiproxy studies (they certainly *did* contribute to Overpeck et al!) there are some important results there irrespective of the role of the proxies in multiproxy studies. Lets, again, wait for reviews before shortening this...

3) We could eliminate the map of the boreholes, although I actually think it is essential to see what the contributing spatial sampling (and, accordingly, the potential bias of that sampling in determining "global mean temperature") actually is. So I vote for keeping it for the time being. Again, it's an extremity that we can afford to lose if necessary in the end..

4) One important note on references: We don't have time at this late stage to dig up incomplete citations, so you'll need to give me full citations for any suggested added references (e.g. the Villalba paper). FYI, the Crowley and Lowery paper is Tom's Ambio paper. He observes a mean warming of about 0.5 C since the 17th century giving us yet another datapoint in the scatter of estimates...

5) I agree, the ranking of centuries is more specific than it needs to be. I don't know what I was thinking. You sure that didn't come from the text you originally contributed?? In any case, we can eliminate much of it in my

PETITIONERS' EXEMPLARS (PE)

opinion too...

On the whole, I have never been under the assumption that you and I would have independently assessed the evidence quite the same way. I would hope we would have come up w/ the same key points, and so your comments in that regard are reassuring. I feel confident in my ability to defend the science that is presented here, so let the reviews fall where they may. I'm sure we will be forced to admit some changes, as well as "minority viewpoints" and alternative interpretations along the way. That's what will make this all interesting...

mike

At 05:20 PM 9/23/99 +0100, Phil Jones wrote:

- >
- > Mike,
- > Here are my thoughts on the text you sent. Keith will be sending some
- > as well hopefully later today. One important aspect Keith will address is
- > whether you're using the latest Briffa et al curve. We know you're not but
- > the one with the greater low frequency and therefore much better chance of
- > looking much better with the other two series, isn't yet published. We know
- > it looks better in plots we have here.
- >
- > Specifics :
- >
- > p1 line 10 - say mid-19th century rather than the 20th century
- >
- > lines 18-20 - seems a bit too much here with three refs on laminated
- > sediments.
- >
- > line 46 Add Briffa et al (1998b) to Cook(1995).
- >
- > p2 line 59 - I would suggest changing 'a particularly' to 'the most' .
- >
- > line 64 - I would add a reference here to the paper by Crowley and
- > Kim (1999) in GRL (July) where this aspect is also discussed.
- >
- > p3 line 101 - I would add Argentina as well as Chile adding a ref to
- > Villalba (1990) in QR.
- >
- > line 108 change 'key' to 'vital'
- >
- > line 119 'have providing' to 'provide' . There are several instances
- > where the text doesn't read that well. I suspect as there are several
- > iterations to go it is not that important yet !
- >
- > The coral section is just about the right size now and is justly
- > devoid of references !
- >
- > p4 line 151 I would add a reference here to Morgan and van Ommen (1997)
- > 'Seasonality in late-Holocene climate from ice core records',
- > The Holocene 7, 351-4. This is the Law Dome core which is the best
- > available with regards to dating in either hemisphere. It should be
- > there.
- >
- > As with the coral section the ice core section expresses some

PETITIONERS' EXEMPLARS (PE)

- > cautionary notes with regard to dating etc which I think are justified.
- > I suspect the contrast with the tree-ring section will draw some
- > criticism. Just a warning !
- >
- > As none of the multiproxy reconstructions use any sediment information
- > this section seems overlarge and could be reduced.
- >
- > p189 century-scale add in the 'y'
- >
- > p5 The borehole section is also a bit overlong. I don't know whether the
- > map really adds something. Not that vehement on this.
- >
- > With respect to comparing high and low frequency aspects the diagram
- > comparing CET with the UK boreholes is now out. I've sent a copy to
- > Chris. It is in :
- >
- > Jones PD, 1999 : Classics in physical geography revisited - Manley's
- > CET series. Progress in Physical Geography 23, 425-428.
- >
- > line 245 the 'is' is not needed.
- >
- > p6 I still think that a reference to Raper et al (1996) would be good
- > here. This models a glacier in northern Sweden using the northern
- > Fennoscandian temperature reconstructions since AD 500. Again it shows
- > how a low frequency estimate (the glacial snout position) can be compared
- > with a high-frequency temperature reconstruction from trees.
- >
- > Raper, SCB, Briffa KR and Wigley TML, 1996: Glacial change in northern
- > Sweden from AD 500: a simple geometric model of Storglaciaren. Journal
- > of Glaciology 42, 341-351.
- >
- > line 268 IPCC(1996) earlier - is it 95 or 96
- >
- > p 7 line 295 I would like to add my paper in Reviews of Geophysics in 1999
- > as that also says that 1998 was likely to be the warmest year of the
- > millennium.
- >
- > line 334 I would like to see Bradley (1999). I must get a copy from
- > Ray in Venice.
- >
- > p7-9 All need a careful read through for English and the arguments.
- >
- > At the bottom of p8 I think you make too much of the differences in the
- > ranking of the centuries. The boreholes would agree with my series with
- > the 17th being colder than the 19th, although they may not be able to
- > resolve the timescales then.
- >
- > Is the Crowley and Lowery (1999) the paper Tom's submitted to Ambio ?
- >
- > I've not commented much on this final section as again I suspect there
- > are many things you will have to justify in the next two sets of reviews.
- > On the whole I think most is OK and I support the final paragraph. I
- > don't believe the astronomical argument as an explanation over the
- > last 1000 years but we can differ on that.
- >
- > I know I would have written this final section 2.3.3 somewhat differently

PETITIONERS' EXEMPLARS (PE)

> with different emphases and slants but the basic final conclusion would
> have been the same.
>
> Cheers
> Phil
>
>
>
>
>Prof. Phil Jones
>Climatic Research Unit Telephone +44 (0) 1603 592090 begin_of_the_skype_highlighting
>+44 (0) 1603 592090 end_of_the_skype_highlighting
>School of Environmental Sciences Fax +44 (0) 1603 507784
>University of East Anglia
>Norwich Email p.jones@uea.ac.uk
>NR4 7TJ
>UK
>
>-----
>
>
>
>

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

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

PETITIONERS' EXEMPLARS (PE)

PE-6

Date: Mon, 10 Jul 2000 08:57:19 -0400

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 possibilities.

[.....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 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

PETITIONERS' EXEMPLARS (PE)

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?

>

PETITIONERS' EXEMPLARS (PE)

>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 natural variability in reasonably realistic ways and perhaps

PETITIONERS' EXEMPLARS (PE)

>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

>

>

>

>Frank Oldfield

>

>Executive Director

>PAGES IPO

>Barenplatz 2

>CH-3011 Bern, Switzerland

>

>e-mail: frank.oldfield@pages.unibe.ch

>

>Phone: +41 31 312 3133 begin_of_the_skype_highlighting +41 31 312 3133

end_of_the_skype_highlighting; Fax: +41 31 312 3168

><http://www.pages.unibe.ch/pages.html>

>

Raymond S. Bradley

Professor and Head of Department

Department of Geosciences

University of Massachusetts

Amherst, MA 01003-5820

Tel: 413-545-2120 begin_of_the_skype_highlighting 413-545-2120

end_of_the_skype_highlighting

Fax: 413-545-1200

Climate System Research Center: 413-545-0659 begin_of_the_skype_highlighting 413-545-0659

end_of_the_skype_highlighting

Climate System Research Center Web Site:

<<<http://www.geo.umass.edu/climate/climate.html>><http://www.geo.umass.edu/climate/climate.html>

Paleoclimatology Book Web Site (1999):

<<http://www.geo.umass.edu/climate/paleo/html>><http://www.geo.umass.edu/climate/paleo/html>

</x-flowed>

PETITIONERS' EXEMPLARS (PE)

PE-7

Date: Tue, 05 Oct 1999 16:18:29 +0100

From: Tim Osborn <t.osborn@uea.ac.uk>
To: mann@virginia.edu,imacadam@meto.gov.uk
Subject: Briffa et al. series for IPCC figure
Date: Tue, 05 Oct 1999 16:18:29 +0100
Cc: k.briffa@uea,p.jones@uea

Dear Mike and Ian

Keith has asked me to send you a timeseries for the IPCC multi-proxy reconstruction figure, to replace the one you currently have. The data are attached to this e-mail. They go from 1402 to 1995, 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 raw data are the same as used in Briffa et al. (1998), the Nature paper that I think you have the reference for already. They are analysed in a different way, to retain the low-frequency variations. In this sense, it is one-step removed from Briffa et al. (1998). It is not two-steps removed from Briffa et al. (1998), since the new series is simply a *replacement* for the one that you have been using, rather than being one-step further.

A new manuscript is in preparation describing this alternative analysis method, the calibration of the resulting series, and their comparison with other reconstructions. We are considering submitting this manuscript to J. Geophys. Res. when it is ready, but for now it is best cited as:
Briffa KR, Osborn TJ, Schweingruber FH, Harris IC and Jones PD (1999) Extracting low-frequency temperature variations from a northern tree-ring density network. In preparation.
Keith will be sending you a copy of the manuscript when it is nearer to completion.

I have also attached a PS file showing the original Briffa et al. (1998) curve, with annotation of cold years associated with known volcanic eruptions. Overlain on this, you will see a green curve. This is the new series with a 40-yr filter through it. This is just so that you can see what it should look like (**ignore the temperature scale on this figure**, since the baseline is non-standard).

With regard to the baseline, the data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep temperatures averaged over all land grid boxes with 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. One could, of course, shift the mean of our reconstruction so that it matched the observed series over a different period - say 1931-60 - but I don't see that this improves things. Indeed, if the non-temperature signal that causes the decline in tree-ring density begins before 1960, then a short 1931-60 period might yield a more biased result than using a longer 1881-1960 period.

If you have any queries regarding this replacement data, then please e-mail

PETITIONERS' EXEMPLARS (PE)

me and/or Keith.

Best regards

Tim

Calibrated against observed Apr-Sep temperature over 1881-1960
averaged over all land grid boxes north of 20N

Year Reconstructed temperature anomaly (degrees C wrt 1961-90)

1402	-0.283
1403	-0.334
1404	-0.286
1405	-0.350
1406	-0.152
1407	-0.124
1408	-0.220
1409	-0.175
1410	-0.100
1411	-0.129
1412	-0.226
1413	-0.115
1414	-0.386
1415	-0.319
1416	-0.277
1417	-0.136
1418	-0.172
1419	-0.294
1420	-0.280
1421	-0.335
1422	-0.406
1423	-0.312
1424	-0.207
1425	-0.136
1426	-0.354
1427	-0.222
1428	-0.305
1429	-0.322
1430	-0.282
1431	-0.143
1432	-0.212
1433	-0.234
1434	-0.076
1435	-0.309
1436	-0.411
1437	-0.122
1438	-0.272
1439	-0.159
1440	-0.330
1441	-0.160
1442	-0.105
1443	-0.080
1444	-0.308
1445	-0.138
1446	-0.317

PETITIONERS' EXEMPLARS (PE)

1447	-0.270
1448	-0.301
1449	-0.357
1450	-0.137
1451	-0.183
1452	-0.207
1453	-0.485
1454	-0.265
1455	-0.358
1456	-0.241
1457	-0.199
1458	-0.366
1459	-0.397
1460	-0.252
1461	-0.230
1462	-0.252
1463	-0.209
1464	-0.174
1465	-0.174
1466	-0.280
1467	-0.256
1468	-0.256
1469	-0.222
1470	-0.237
1471	-0.094
1472	-0.122
1473	-0.056
1474	-0.320
1475	-0.376
1476	-0.133
1477	-0.075
1478	0.037
1479	-0.161
1480	-0.379
1481	-0.513
1482	-0.286
1483	-0.354
1484	-0.327
1485	-0.208
1486	-0.125
1487	-0.380
1488	-0.193
1489	-0.245
1490	-0.466
1491	-0.244
1492	-0.146
1493	-0.278
1494	-0.394
1495	-0.526
1496	-0.275
1497	-0.264
1498	-0.233
1499	-0.169
1500	-0.128
1501	-0.415
1502	-0.306

PETITIONERS' EXEMPLARS (PE)

1503	0.011
1504	-0.013
1505	-0.378
1506	-0.226
1507	-0.428
1508	-0.192
1509	-0.312
1510	-0.157
1511	-0.162
1512	-0.188
1513	-0.135
1514	-0.418
1515	-0.258
1516	-0.381
1517	-0.134
1518	-0.180
1519	-0.166
1520	-0.035
1521	-0.384
1522	-0.302
1523	-0.541
1524	-0.371
1525	-0.183
1526	-0.289
1527	-0.224
1528	-0.247
1529	-0.432
1530	-0.291
1531	-0.467
1532	-0.343
1533	-0.586
1534	-0.183
1535	-0.417
1536	-0.350
1537	-0.257
1538	-0.451
1539	-0.398
1540	-0.497
1541	-0.406
1542	-0.584
1543	-0.448
1544	-0.317
1545	-0.312
1546	-0.289
1547	-0.114
1548	-0.459
1549	-0.335
1550	-0.009
1551	-0.074
1552	-0.047
1553	-0.207
1554	-0.285
1555	-0.116
1556	-0.141
1557	-0.419
1558	-0.174

PETITIONERS' EXEMPLARS (PE)

1559	-0.465
1560	-0.287
1561	-0.169
1562	-0.231
1563	-0.270
1564	-0.347
1565	-0.116
1566	-0.202
1567	-0.278
1568	-0.445
1569	-0.488
1570	-0.465
1571	-0.434
1572	-0.674
1573	-0.324
1574	-0.493
1575	-0.273
1576	-0.623
1577	-0.483
1578	-0.521
1579	-0.551
1580	-0.473
1581	-0.436
1582	-0.382
1583	-0.345
1584	-0.280
1585	-0.565
1586	-0.409
1587	-0.580
1588	-0.530
1589	-0.534
1590	-0.354
1591	-0.377
1592	-0.407
1593	-0.337
1594	-0.591
1595	-0.459
1596	-0.436
1597	-0.475
1598	-0.152
1599	-0.134
1600	-0.381
1601	-1.169
1602	-0.403
1603	-0.414
1604	-0.472
1605	-0.393
1606	-0.564
1607	-0.529
1608	-0.822
1609	-0.789
1610	-0.617
1611	-0.681
1612	-0.670
1613	-0.364
1614	-0.733

PETITIONERS' EXEMPLARS (PE)

1615	-0.428
1616	-0.698
1617	-0.479
1618	-0.485
1619	-0.524
1620	-0.706
1621	-0.671
1622	-0.714
1623	-0.662
1624	-0.387
1625	-0.566
1626	-0.671
1627	-0.665
1628	-0.759
1629	-0.654
1630	-0.379
1631	-0.466
1632	-0.330
1633	-0.377
1634	-0.521
1635	-0.222
1636	-0.265
1637	-0.252
1638	-0.396
1639	-0.382
1640	-0.400
1641	-1.152
1642	-1.067
1643	-1.092
1644	-0.649
1645	-0.588
1646	-0.632
1647	-0.554
1648	-0.368
1649	-0.572
1650	-0.215
1651	-0.317
1652	-0.529
1653	-0.268
1654	-0.343
1655	-0.400
1656	-0.372
1657	-0.332
1658	-0.359
1659	-0.182
1660	-0.260
1661	-0.258
1662	-0.433
1663	-0.433
1664	-0.353
1665	-0.440
1666	-0.837
1667	-0.857
1668	-0.816
1669	-0.779
1670	-0.871

PETITIONERS' EXEMPLARS (PE)

1671	-0.463
1672	-0.434
1673	-0.631
1674	-0.663
1675	-0.870
1676	-0.523
1677	-0.670
1678	-0.794
1679	-0.768
1680	-0.701
1681	-0.380
1682	-0.518
1683	-0.364
1684	-0.369
1685	-0.688
1686	-0.178
1687	-0.481
1688	-0.351
1689	-0.229
1690	-0.254
1691	-0.221
1692	-0.545
1693	-0.263
1694	-0.316
1695	-0.955
1696	-0.816
1697	-0.687
1698	-1.054
1699	-1.005
1700	-0.630
1701	-0.818
1702	-0.510
1703	-0.377
1704	-0.420
1705	-0.527
1706	-0.328
1707	-0.257
1708	-0.465
1709	-0.493
1710	-0.288
1711	-0.344
1712	-0.345
1713	-0.242
1714	-0.390
1715	-0.305
1716	-0.390
1717	-0.309
1718	-0.270
1719	-0.194
1720	-0.110
1721	-0.427
1722	0.005
1723	-0.193
1724	-0.249
1725	-0.497
1726	-0.381

PETITIONERS' EXEMPLARS (PE)

1727	-0.241
1728	-0.133
1729	-0.261
1730	-0.633
1731	-0.723
1732	-0.426
1733	-0.371
1734	-0.104
1735	-0.373
1736	-0.330
1737	-0.206
1738	-0.557
1739	-0.291
1740	-0.734
1741	-0.594
1742	-0.808
1743	-0.378
1744	-0.372
1745	-0.418
1746	-0.501
1747	-0.150
1748	-0.389
1749	-0.328
1750	-0.168
1751	-0.343
1752	-0.227
1753	-0.218
1754	-0.377
1755	-0.328
1756	-0.221
1757	-0.259
1758	-0.431
1759	-0.340
1760	-0.335
1761	-0.261
1762	-0.466
1763	-0.291
1764	-0.473
1765	-0.378
1766	-0.212
1767	-0.429
1768	-0.544
1769	-0.343
1770	-0.341
1771	-0.265
1772	-0.547
1773	-0.421
1774	-0.048
1775	-0.289
1776	-0.186
1777	-0.288
1778	-0.178
1779	-0.550
1780	-0.339
1781	-0.251
1782	-0.164

PETITIONERS' EXEMPLARS (PE)

1783	-0.757
1784	-0.142
1785	-0.141
1786	-0.179
1787	-0.432
1788	-0.207
1789	-0.235
1790	-0.612
1791	-0.163
1792	-0.086
1793	-0.023
1794	-0.030
1795	-0.243
1796	-0.028
1797	-0.565
1798	-0.049
1799	-0.228
1800	-0.287
1801	-0.413
1802	-0.117
1803	0.020
1804	0.036
1805	-0.094
1806	-0.251
1807	-0.089
1808	-0.241
1809	-0.460
1810	-0.582
1811	-0.353
1812	-0.459
1813	-0.545
1814	-0.458
1815	-0.588
1816	-0.855
1817	-0.861
1818	-0.629
1819	-0.680
1820	-0.289
1821	-0.351
1822	-0.159
1823	-0.246
1824	-0.276
1825	-0.263
1826	-0.140
1827	-0.293
1828	-0.033
1829	-0.087
1830	-0.173
1831	-0.045
1832	-0.621
1833	-0.660
1834	-0.141
1835	-0.647
1836	-0.775
1837	-0.771
1838	-0.359

PETITIONERS' EXEMPLARS (PE)

1839	-0.267
1840	-0.144
1841	-0.077
1842	-0.337
1843	-0.435
1844	-0.101
1845	-0.412
1846	0.106
1847	-0.079
1848	-0.346
1849	-0.393
1850	-0.261
1851	-0.165
1852	-0.100
1853	-0.174
1854	-0.138
1855	-0.418
1856	-0.250
1857	-0.538
1858	-0.126
1859	-0.195
1860	-0.231
1861	-0.029
1862	-0.555
1863	-0.303
1864	-0.407
1865	-0.256
1866	-0.437
1867	-0.413
1868	-0.119
1869	-0.321
1870	-0.213
1871	-0.352
1872	-0.163
1873	-0.183
1874	-0.372
1875	-0.247
1876	-0.487
1877	-0.192
1878	0.120
1879	-0.152
1880	-0.346
1881	-0.184
1882	-0.200
1883	-0.183
1884	-0.717
1885	-0.534
1886	-0.485
1887	-0.281
1888	-0.261
1889	-0.153
1890	-0.341
1891	-0.313
1892	-0.138
1893	-0.301
1894	-0.134

PETITIONERS' EXEMPLARS (PE)

1895	-0.128
1896	-0.241
1897	-0.016
1898	0.065
1899	-0.574
1900	-0.218
1901	-0.049
1902	-0.287
1903	-0.142
1904	-0.205
1905	-0.308
1906	-0.034
1907	-0.412
1908	-0.048
1909	-0.214
1910	-0.147
1911	-0.194
1912	-0.631
1913	-0.161
1914	-0.294
1915	-0.074
1916	-0.277
1917	-0.297
1918	-0.460
1919	-0.013
1920	-0.272
1921	-0.114
1922	-0.036
1923	-0.305
1924	-0.141
1925	-0.258
1926	-0.115
1927	-0.198
1928	-0.018
1929	-0.161
1930	0.086
1931	0.104
1932	0.081
1933	-0.057
1934	0.007
1935	-0.037
1936	-0.019
1937	0.060
1938	0.163
1939	-0.075
1940	0.113
1941	-0.200
1942	0.128
1943	0.053
1944	-0.080
1945	0.059
1946	-0.016
1947	-0.188
1948	-0.038
1949	-0.107
1950	-0.269

PETITIONERS' EXEMPLARS (PE)

1951 -0.100
1952 -0.118
1953 0.161
1954 -0.235
1955 -0.127
1956 -0.308
1957 -0.194
1958 -0.308
1959 -0.224
1960 0.076
1961 -0.104
1962 -0.289
1963 -0.173
1964 -0.479
1965 -0.474
1966 -0.171
1967 -0.200
1968 -0.599
1969 -0.355
1970 -0.353
1971 -0.328
1972 -0.563
1973 -0.262
1974 -0.336
1975 -0.507
1976 -0.558
1977 -0.363
1978 -0.698
1979 -0.289
1980 -0.612
1981 -0.195
1982 -0.522
1983 -0.234
1984 -0.335
1985 -0.423
1986 -0.430
1987 -0.424
1988 -0.161
1989 -0.286
1990 -0.275
1991 -0.169
1992 -0.175
1993 -0.341
1994 -0.320

Attachment Converted: "c:\eudora\attach\Briffa et al.ps"

Dr Timothy J Osborn | phone: +44 1603 592089 begin_of_the_skype_highlighting +44
1603 592089 end_of_the_skype_highlighting
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>

PETITIONERS' EXEMPLARS (PE)

PE-8

Date: Mon, 13 Dec 2004 11:53:16 -0500

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #3: Stendel et al paper (submitted)
Date: Mon, 13 Dec 2004 11:53:16 -0500

Keith,

Attached is the Stendel et al paper (submitted to "Climate Dynamics" last month) and a corrected version of their Figure 3 (using the correct Mann and Jones NH series).

The importance of this paper is that they use the same model as von Storch (higher resolution in fact), and get a temperature history that looks much like the reconstructions/other models. Also, they appear to get the negative NAO pattern in the Maunder Minimum, which von Storch et al do not...

Again, this should be referenceable in the zero order draft, but would be good to contact Martin Stendel first about this...

Mike

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

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

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

Attachment Converted: "c:\eudora\attach\stendel_et_al_ClimDyn.pdf" Attachment Converted: "c:\eudora\attach\nh-extend.pdf"

References

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

PETITIONERS' EXEMPLARS (PE)

PE-9 Date: Tue, 16 Nov 1999 13:31:15 +0000

From: Phil Jones <p.jones@uea.ac.uk>
To: ray bradley <rbradley@geo.umass.edu>, mann@virginia.edu, mhughes@ltrr.arizona.edu
Subject: Diagram for WMO Statement
Date: Tue, 16 Nov 1999 13:31:15 +0000
Cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

Dear Ray, Mike and Malcolm,

Once Tim's got a diagram here we'll send that either later today or first thing tomorrow.

I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) amd from 1961 for Keith's to hide the decline. Mike's series got the annual land and marine values while the other two got April-Sept for NH land N of 20N. The latter two are real for 1999, while the estimate for 1999 for NH combined is +0.44C wrt 61-90. The Global estimate for 1999 with data through Oct is +0.35C cf. 0.57 for 1998.

Thanks for the comments, Ray.

Cheers
Phil

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

PETITIONERS' EXEMPLARS (PE)

PE-10 Date: Thu, 30 Dec 2004 09:22:02 -0500

From: Phil Jones <p.jones@uea.ac.uk>
To: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk
Subject: Fwd: Re: Fw: Rutherford et al. [2004]
Date: Tue, 04 Jan 2005 11:22:31 +0000

FYI.

Just look at the attachment. Don't refer to it or send it on to anybody yet. I guess you could refer to it in the IPCC Chapter - you will have to some day !

Cheers
Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 6.1.1.1

Date: Thu, 30 Dec 2004 09:22:02 -0500

To: Phil Jones <p.jones@uea.ac.uk>
From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Fw: Rutherford et al. [2004]

X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean

X-UEA-MailScanner-SpamScore: s

Phil,

I would immediately delete anything you receive from this fraud.

You've probably seen now the paper by Wahl and Ammann which independently exposes McIntyre and McKittrick for what it is--pure crap. Of course, we've already done this on "RealClimate", but Wahl and Ammann is peer-reviewed and independent of us. I've attached it in case you haven't seen (please don't pass it along to others yet). It should be in press shortly. Meanwhile, I would NOT RESPOND to this guy. As you know, only bad things can come of that. The last thing this guy cares about is honest debate--he is funded by the same people as Singer, Michaels, etc...

Other than this distraction, I hope you're enjoying the holidays too...

talk to you soon,

mike

At 09:02 AM 12/30/2004, you wrote:

Mike,

FYI. Just in for an hour or so today as still off until Jan 4.

Not replied to this - too much else with IPCC etc. Not read this in detail - just printed it off.

Have a good New Year's Eve.

Cheers
Phil

From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>

To: "Phil Jones" <p.jones@uea.ac.uk>

Subject: Fw: Rutherford et al. [2004]

Date: Wed, 29 Dec 2004 10:08:18 -0500

X-Mailer: Microsoft Outlook Express 6.00.2800.1158

X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean

Dear Phil,

I have noticed the following statements in Rutherford et al [2004], in which you are a co-author. As compared with some of your co-authors, I get the impression that, while

PETITIONERS' EXEMPLARS (PE)

you feel very strongly about your views, you are also concerned with getting to the bottom of matters and are less concerned with scoring meaningless debating points. In this spirit, I draw your attention to some incorrect statements in Rutherford et al. [2004] concerning our material. There is really a quite serious problem with the PC methods in MBH98 and the comments made in Rutherford et al [2004] are really quite misleading. For the reasons set out below, I request that these comments be removed from the manuscript.

Regards, Steve McIntyre

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

From: [1]Steve McIntyre

To: [2]David Randall

Cc: [3]Scott Rutherford ; [4]Paul Kushner ; [5]Cindy Carrick ; [6]Ross McKitrick

Sent: Tuesday, December 28, 2004 1:48 PM

Subject: Rutherford et al. [2004]

Dear Dr. Randall,

Recently, at the website [7]www.realclimate.org, Michael Mann publicized a submission by Rutherford et al. to Journal of Climate, entitled Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season, and Target Domain. This paper contains some untrue statements and mischaracterizations regarding criticisms we (McIntyre and McKitrick) made of Mann et al. (1998) [MBH98] in a 2003 paper and subsequent exchanges under the auspices of Nature. We are writing to request that these untrue statements be removed from the paper before any further processing of the document by Journal of Climate takes place.

First, Rutherford et al. states that McIntyre and McKitrick [2003] used an incorrect version of the Mann et al. (1998) proxy indicator dataset. The history of this matter is summarized below (all relevant emails and other documentation are available at [8]<http://www.climate2003.com/file.issues.htm> .

In April 2003, we requested from Mann the FTP location of the dataset used in MBH98. Mann advised me that he was unable to recall the location of this dataset and referred the request to Rutherford. Rutherford eventually directed us to a file (pcproxy.txt) located at a URL at Manns FTP site. In using this data file, we noticed numerous problems with it, not least with the principal component series. We sought specific confirmation from Mann that this dataset was the one used in MBH98; Mann said that he was too busy to respond to this or any other inquiry. Because of the many problems in this data set, we undertook a complete new re-collation of the data, using the list of data sources in the SI to MBH98 and using original archived versions wherever possible. After publication of McIntyre and McKitrick [2003], Mann said that dataset at his FTP site to which we had been referred was an incorrect version of the data and that this version had been prepared especially for me; through a blog, he provided a new URL which he now claimed to contain the correct data set. The file creation date of the incorrect version was in 2002, long prior to my first request for data, clearly disproving his assertion that it was prepared in response to my request. Mann and/or Rutherford then deleted this incorrect version with its date evidence from his FTP site.

It is false and misleading for Rutherford et al. to now allege that we used the wrong dataset. We used the dataset they directed us to at their FTP site. More importantly, for our analysis, to avoid the problems with the principal component series, we re-collated the tree ring data identified in MBH98 from ITRDB archives, calculated fresh principal component series; in addition, we re-collated other proxy data from archived

PETITIONERS' EXEMPLARS (PE)

versions wherever possible. Thus, our own calculations were not affected by the errors in the supplied file as we did NOT use the incorrect version in our calculations. To suggest otherwise, as is done in Rutherford et al [2004], is highly misleading. To date, no source code or other evidence has been provided to fully demonstrate that the incorrect version (now deleted) did not infect some of Manns and Rutherfords other work. In this respect, we note that the now deleted file pcproxy.txt occurs in a legend in a graphic at Rutherfords website, indicating possible use elsewhere by Rutherford of the incorrect version.

Accordingly, we request that the above claim be removed from the manuscript.

Secondly, Rutherford et al. [2004] argues that the difference between MBH98 results and MM03 results occurs because of our misunderstanding of a stepwise procedure in MBH98 for the calculation of principal component series for tree ring networks. Again, this claim is misleading on its face. While our 2003 paper did not implement the (then undisclosed) stepwise procedure, as soon as this matter was raised in subsequent correspondence in November 2003, we implemented it and we continued to observe the discrepancies in principal component series and final results. The current manuscript ignores a refereed exchange at Nature in which we specifically clarified (in response to a reviewers question) that we had obtained such results while using the exact stepwise procedure described in MBH98. Mann is aware of this refereed exchange.

The reason for the difference between our results and MBH98 results is primarily due to the fact that the tree ring principal component series in MBH98 cannot be replicated using a conventional principal components method. The MBH98 principal component series can only be replicated by standardizing on a short segment a procedure nowhere mentioned in MBH98 and only recently acknowledged in the SI to the Corrigendum of Mann et al. [Nature 2004] in response to our concerns on the subject expressed to Nature. In effect, MBH98 did not use a conventional centered PC calculation, but used an uncentered PC calculation on de-centered data. The impact of this method is the subject of ongoing controversy, which is well-known to the authors, but the existence of the method in MBH98 is no longer in doubt. In discussions of PC calculations in 2004 exchanged with the authors through Nature, we implemented the stepwise procedures of MBH98 referred to in the present manuscript and demonstrated that important differences remain even with stepwise procedures, as long as the uncentered and decentered methods of MBH98 are used. The differences in PC series resulting from using centered and uncentered series has been fully agreed to by all parties in the Nature exchange, although the parties continue to disagree on the ultimate effect on final NH temperature calculations. Accordingly, the discussion in Rutherford et al. [2004] is very incomplete and misleading in this respect. While we recognize that Mann et al. have argued that they can salvage MBH98-type results using alternative methodologies (e.g. increasing the number of PC series used in the 1400-1450 period), these salvage efforts are themselves a matter of controversy and do not validate the claims being put forward in the Rutherford et al. paper.

Accordingly we ask that this claim also be deleted from the manuscript.

Regards,
Stephen McIntyre and Ross McKittrick

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090 begin_of_the_skype_highlighting
+44 (0) 1603 592090 end_of_the_skype_highlighting
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia

PETITIONERS' EXEMPLARS (PE)

Norwich
NR4 7TJ
UK

Email p.jones@uea.ac.uk

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

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

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

Attachment Converted: "c:\documents and settings\tim osborn\my documents\europa\attach\Wahl_MBH_Recreation_JClimLett_Nov22.pdf"

References

1. <mailto:stephen.mcintyre@utoronto.ca>
2. <mailto:randall@atmos.colostate.edu>
3. <mailto:srutherford@rwu.edu>
4. <mailto:j.climate@atmosp.physics.utoronto.ca>
5. <mailto:cindy@atmos.colostate.edu>
6. <mailto:rmckitri@uoguelph.ca>
7. <http://www.realclimate.org/>
8. <http://www.climate2003.com/file.issues.htm>
9. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

PETITIONERS' EXEMPLARS (PE)

PE-11

Date: Fri, 12 Apr 2002 12:32:33 -0400

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Your letter to Science
Date: Fri, 12 Apr 2002 12:32:33 -0400
Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

Whoaah...Please don't put words in my mouth Keith, especially such inflammatory word! I was not attributing the entirety of "spin" here (which is of a pretty massive scale) to you! And I said I think such "spin", where it has occurred, is EITHER sloppy OR disingenuous. You chose to assume I was talking about you in specific, and that I was attributing the latter rather than the former. My actual words don't bear this out. In the case of the Briffa & Osborn piece, I actually tend to believe that sloppiness was the main problem. In other cases of "spin" (e.g., the skeptics web pages of Daly and his ilk) it is most clearly disingenuous...I don't equate you with Daly and those folks by any stretch of the imagination. Hopefully, you know that I respect you quite a bit as a scientist! But in this case, I think you were sloppy. And the sloppiness had a real cost...

And as to whether or not your statements about IPCC are fair (I didn't use the word "disservice!"), I'll leave that to each to decide. But personally, I think they were unfair, because they opened up IPCC to criticism that is not merited by what is actually said or shown in the IPCC report. Other IPCC authors who have contacted me feel the same way, and perhaps there may be an official response on the part of IPCC authors. I don't know.

But I agree that any further discussion ought to take place in the peer-reviewed literature,

Mike

At 05:09 PM 4/12/02 +0100, Keith Briffa wrote:

I agree with the sentiments expressed by Tom . However, in his latest message Mike clearly says that our perspectives piece did the IPCC a disservice. He then accuses us of spinning the ECS paper to say that MBH is an underestimate of what it purports to be and that we have been sloppy and disingenuous. Frankly this is too much to take . I am not going to let this ruin my weekend so I wait until I have calmed down and find time next week to write a response. In the meantime I just wanted to note that I disagree with these comments. Perhaps the best place to continue this discussion is in the peer review literature.

Keith

At 11:11 AM 4/12/02 -0400, Michael E. Mann wrote:

Ed and others,

I thought I too should chime in here one last time...

I'll leave it to you, Malcolm, Keith and others to debate out the issue of any additional uncertainties, biases, etc. that might arise from RCS in the presence of limited samples. That is beyond my range of expertise. But since this is a new and relatively untested approach, and it is on the basis of this approach that other estimates are being argued to be "underestimates", we would indeed have been remiss now to point this out in our letter.

The wording "perilous" perhaps should be changed, by I very much stand by the overall sentiment expressed by Malcolm in our piece with regard to RCS.

One very important additional point that Malcolm makes in his message is that conservative estimates of uncertainties, appropriate additional caveats, etc. were indeed all provided in MBH99, and I have always been careful to interpret our results in the context of these uncertainties and caveats. IPCC '2001 was careful to do so to, and

PETITIONERS' EXEMPLARS (PE)

based its conclusions within the context of the uncertainties (hence the choice of the conservative term "likely" in describing the apparently unprecedented nature of late 20th century warmth) and, moreover, on the collective results of many independent reconstructions. Briffa & Osborn would have you believe that IPCC '2001's conclusions in this regard rested on MBH99 alone. Frankly, Keith and Tim, I believe that is unfair to the IPCC, whether or not one cares about being fair to MBH or not.

What is unfortunate here then is that Esper et al has been "spun" i to argue that MBH99 underestimates the quantity it purports to estimate, full Northern Hemisphere annual mean temperature. Given the readily acknowledged level of uncertainty in both estimates, combined with the "apples and oranges" nature of the comparison between the two (which I have sought to clarify in my letter to Science, and in my messages to you all, and the comparison plot I provided), I believe it is either sloppy or disingenuous reasoning to argue that this is the case. The fact that this sloppiness also readily serves the interests of the skeptics is quite unfortunate, but it is indeed beside the point!

It would probably also be helpful for me to point out, without naming names, that many of our most prominent colleagues in the climate research community, as well government funding agency representatives, have personally contacted me over the past few weeks to express their dismay at the way they believe this study was spun. I won't get into the blame game, because there's more than enough of that to go around. But when the leaders of our scientific research community and our funding managers personally alert us that they believe the credibility of our field has been damaged, I think it is time for some serious reflection on this episode.

that's my final 2 cents,

Mike

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

e-mail: mann@virginia.edu Phone: (434) 924-7770 begin_of_the_skype_highlighting (434)
924-7770 end_of_the_skype_highlighting FAX: (434) 982-2137
[1][http://www.evsc.virginia.edu/faculty/people/mann.\[2\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[2].shtml)

References

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

PETITIONERS' EXEMPLARS (PE)

PE-12

Date: Mon, 13 Dec 2004 11:47:16 -0500

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #1: some background info first...
Date: Mon, 13 Dec 2004 11:47:16 -0500

Hi Keith,

Thanks again for your phone call, and the (informal) opportunity to help out where I can. I'm perfectly happy in that role (as an informal contributor and a formal reviewer, for example), if you and Peck, for example, are both comfortable with that.

First, "RealClimate" should be helpful. It deals w/ the skeptic claims, etc. but using the legitimate

peer-reviewed research as a basis for the discussion.

The "hockey stick" overview should be helpful:

[1]<http://www.realclimate.org/index.php?p=7>

as well as itemized responses to the various contrarian propaganda/myths:

[2]<http://www.realclimate.org/index.php?p=11>

and the specific discrediting of the claims of McIntyre and McKittrick, based both on our response to their rejected Nature comment:

[3]<http://www.realclimate.org/index.php?p=8>

and the discussion of the analysis in the Rutherford et al (2004) paper in press in Journal of Climate, that independently discredits them:

[4]<http://www.realclimate.org/index.php?p=10>

In the following emails, I'll attach some other materials (submitted papers) that deal w/ the McIntyre and McKittrick matter, and the von Storch matter,

Please let me know if there is anything we discussed that I forgot to provide you. Will also draft an email to the small group (you, me, Scott, Caspar, Gene) about the prospective additional RegEM/Mann et al method model analyses,

cheers,

Mike

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

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

References

1. <http://www.realclimate.org/index.php?p=7>
2. <http://www.realclimate.org/index.php?p=11>
3. <http://www.realclimate.org/index.php?p=8>
4. <http://www.realclimate.org/index.php?p=10>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

PETITIONERS' EXEMPLARS (PE)

PE-13

Date: Fri, 02 May 2003 18:46:41 -0400

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

Hi Keith,

No problem, I know how hectic the past couple months have been for you, so no apologies necessary whatsoever!

Call me old fashioned, but I still tend to prefer the "blind" reviewer convention, so I'd prefer to remain anonymous unless you think that revealing my identity would be help in any particular way.

I agree w/ your take on this--a journal like GRL is probably more appropriate, or even "Climatic Change" because a number of similar papers have been published there in the past (by folks like Nychka, Bloomfield, and others). I'm not sure if Steve Schneider is sick and tired of those papers though...

Please don't hesitate to let me know if I can be of any additional help w/ this.

Looking forward to seeing you one of these days,
mike

At 02:36 PM 5/2/2003 +0100, you wrote:

Mike

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

Cheers

Keith

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909 begin_of_the_skype_highlighting +44-1603-593909
end_of_the_skype_highlighting

Fax: +44-1603-507784

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

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

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

PETITIONERS' EXEMPLARS (PE)

References

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

PETITIONERS' EXEMPLARS (PE)

PE-14 Date: Fri, 02 May 2003 18:46:41 -0400

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

Hi Keith,

No problem, I know how hectic the past couple months have been for you, so no apologies necessary whatsoever!

Call me old fashioned, but I still tend to prefer the "blind" reviewer convention, so I'd prefer to remain anonymous unless you think that revealing my identity would be help in any particular way.

I agree w/ your take on this--a journal like GRL is probably more appropriate, or even "Climatic Change" because a number of similar papers have been published there in the past (by folks like Nychka, Bloomfield, and others). I'm not sure if Steve Schneider is sick and tired of those papers though...

Please don't hesitate to let me know if I can be of any additional help w/ this.

Looking forward to seeing you one of these days,
mike

At 02:36 PM 5/2/2003 +0100, you wrote:

Mike

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

Cheers

Keith

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909 begin_of_the_skype_highlighting +44-1603-593909
end_of_the_skype_highlighting

Fax: +44-1603-507784

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

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

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

PETITIONERS' EXEMPLARS (PE)

References

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

PETITIONERS' EXEMPLARS (PE)

PE-15

Date: Tue, 11 Mar 2003 08:14:49 -0500

From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu,
mhughes@lrr.arizona.edu, srutherford@gso.uri.edu, tcrowley@duke.edu
Subject: Re: Fwd: Soon & Baliunas
Date: Tue, 11 Mar 2003 08:14:49 -0500
Cc: k.briffa@uea.ac.uk, jto@u.arizona.edu, drdendro@ldeo.columbia.edu,
keith.alverson@pages.unibe.ch, mmaccrac@comcast.net, jto@u.arizona.edu, mann@virginia.edu

Thanks Phil,

(Tom: Congrats again!)

The Soon & Baliunas paper couldn't have cleared a 'legitimate' peer review process anywhere. That leaves only one possibility--that the peer-review process at Climate Research has been hijacked by a few skeptics on the editorial board. And it isn't just De Frietas, unfortunately I think this group also includes a member of my own department... The skeptics appear to have staged a 'coup' at "Climate Research" (it was a mediocre journal to begin with, but now its a mediocre journal with a definite 'purpose').

Folks might want to check out the editors and review editors:

[1]<http://www.int-res.com/journals/cr/crEditors.html>

In fact, Mike McCracken first pointed out this article to me, and he and I have discussed this a bit. I've cc'd Mike in on this as well, and I've included Peck too. I told Mike that I believed our only choice was to ignore this paper. They've already achieved what they wanted--the claim of a peer-reviewed paper. There is nothing we can do about that now, but the last thing we want to do is bring attention to this paper, which will be ignored by the community on the whole...

It is pretty clear that thee skeptics here have staged a bit of a coup, even in the presence of a number of reasonable folks on the editorial board (Whetton, Goodess, ...). My guess is that Von Storch is actually with them (frankly, he's an odd individual, and I'm not sure he isn't himself somewhat of a skeptic himself), and without Von Storch on their side, they would have a very forceful personality promoting their new vision.

There have been several papers by Pat Michaels, as well as the Soon & Baliunas paper, that couldn't get published in a reputable journal.

This was the danger of always criticising the skeptics for not publishing in the "peer-reviewed literature". Obviously, they found a solution to that--take over a journal! So what do we do about this? I think we have to stop considering "Climate Research" as a legitimate peer-reviewed journal. Perhaps we should encourage our colleagues in the climate research community to no longer submit to, or cite papers in, this journal. We would also need to consider what we tell or request of our more reasonable colleagues who currently sit on the editorial board...

What do others think?

mike

At 08:49 AM 3/11/2003 +0000, Phil Jones wrote:

Dear All,

Apologies for sending this again. I was expecting a stack of emails this morning in response, but I inadvertently left Mike off (mistake in pasting) and picked up Tom's old address. Tom is busy though with another offspring !

I looked briefly at the paper last night and it is appalling - worst word I can think of today without the mood pepper appearing on the email ! I'll have time to read more at the weekend as I'm coming to the US for the DoE CCPP meeting at Charleston. Added Ed, Peck and Keith A.

PETITIONERS' EXEMPLARS (PE)

onto this list as well. I would like to have time to rise to the bait, but I have so much else on at the moment. As a few of us will be at the EGS/AGU meet in Nice, we should consider what to do there.

The phrasing of the questions at the start of the paper determine the answer they get. They have no idea what multiproxy averaging does. By their logic, I could argue 1998 wasn't the warmest year globally, because it wasn't the warmest everywhere. With their LIA being 1300-1900 and their MWP 800-1300, there appears (at my quick first reading) no discussion of synchronicity of the cool/warm periods. Even with the instrumental record, the early and late 20th century warming periods are only significant locally at between 10-20% of grid boxes.

Writing this I am becoming more convinced we should do something - even if this is just to state once and for all what we mean by the LIA and MWP. I think the skeptics will use this paper to their own ends and it will set paleo back a number of years if it goes unchallenged.

I will be emailing the journal to tell them I'm having nothing more to do with it until they rid themselves of this troublesome editor. A CRU person is on the editorial board, but papers get dealt with by the editor assigned by Hans von Storch.

Cheers

Phil

Dear all,

Tim Osborn has just come across this. Best to ignore probably, so don't let it spoil your day. I've not looked at it yet. It results from this journal having a number of editors. The responsible one for this is a well-known skeptic in NZ. He has let a few papers through by Michaels and Gray in the past. I've had words with Hans von Storch about this, but got nowhere.

Another thing to discuss in Nice !

Cheers

Phil

X-Sender: f055@pop.uea.ac.uk

X-Mailer: QUALCOMM Windows Eudora Version 5.1

Date: Mon, 10 Mar 2003 14:32:14 +0000

To: p.jones@uea

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Soon & Baliunas

Dr Timothy J Osborn | phone: +44 1603 592089 begin_of_the_skype_highlighting
+44 1603 592089 end_of_the_skype_highlighting

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 _____ | [2]<http://www.cru.uea.ac.uk/~timo/>

Norwich NR4 7TJ | suncllock:

UK | [3]<http://www.cru.uea.ac.uk/~timo/suncllock.htm>

PETITIONERS' EXEMPLARS (PE)

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

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

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

References

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

PETITIONERS' EXEMPLARS (PE)

PE-16

Date: Thu, 24 Apr 2003 20:28:20 +1200

From: j.salinger@niwa.co.nz

To: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>, Danny Harvey <harvey@cirque.geog.utoronto.ca>, Ben Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby <rob.wilby@kcl.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider <shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>, "simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton" <peter.whetton@dar.csiro.au>, "c.goodess" <c.goodess@uea.ucar.edu>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Mark Eakin <mark.eakin@noaa.gov>, Scott Rutherford <srutherford@deschutes.geo.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmaccrac@comcast.net>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>, Tom Wigley <wigley@ucar.edu>

Subject: And again from the south!
Date: Thu, 24 Apr 2003 20:28:20 +1200

Dear friends and colleagues

This will be the last from me for the moment and I believe we are all arriving at a consensus voiced by Tom, Barrie, Neville et al., from excellent discussions.

Firstly both Danny and Tom have complained to de Freitas about his editorial decision, which does not uphold the principles of good science. Tom has shared the response. I would be curious to find out who the other four cited are - but a rebuttal would be excellent.

Ignoring bad science eventually reinforces the apparent 'truth' of that bad science in the public mind, if it is not corrected. As importantly, the 'bad science' published by CR is used by the sceptics' lobbies to 'prove' that there is no need for concern over climate change. Since the IPCC makes it quite clear that there are substantial grounds for concern about climate change, is it not partially the responsibility of climate science to make sure only satisfactorily peer-reviewed science appears in scientific publications? - and to refute any inadequately reviewed and wrong articles that do make their way through the peer review process?

I can understand the weariness which the ongoing sceptics' onslaught would induce in anyone, scientist or not. But that's no excuse for ignoring bad science. It won't go away, and the more we ignore it the more traction it will gain in the minds of the general public, and the UNFCCC negotiators. If science doesn't uphold the purity of science, who will?

We Australasians (including Tom as an ex pat) have suggested some courses of action. Over to you now in the north to assess the success of your initiatives, the various discussions and suggestions and arrive on a path ahead. I am happy to be part of it.

Warm wishes to all

PETITIONERS' EXEMPLARS (PE)

Jim

On 23 Apr 2003, at 23:53, Tom Wigley wrote:

> Dear friends,
>
> [Apologies to those I have missed who have been part of this email
> exchange -- although they may be glad to have been missed]
>
> I think Barrie Pittock has the right idea -- although there are some
> unique things about this situation. Barrie says
>
> (1) There are lots of bad papers out there
> (2) The best response is probably to write a 'rebuttal'
>
> to which I add
>
> (3) A published rebuttal will help IPCC authors in the 4AR.
>
> _____
>
> Let me give you an example. There was a paper a few years ago by
> Legates and Davis in GRL (vol. 24, pp. 2319-1222, 1997) that was
> nothing more than a direct and pointed criticism of some work by
> Santer and me -- yet neither of us was asked to review the paper. We
> complained, and GRL admitted it was poor judgment on the part of the
> editor. Eventually (> 2 years later) we wrote a response (GRL 27,
> 2973-2976, 2000). However, our response was more than just a rebuttal,
> it was an attempt to clarify some issues on detection. In doing things
> this way we tried to make it clear that the original Legates/Davis
> paper was an example of bad science (more bluntly, either sophomoric
> ignorance or deliberate misrepresentation).
>
> Any rebuttal must point out very clearly the flaws in the original
> paper. If some new science (or explanations) can be added -- as we did
> in the above example -- then this is an advantage.
>
> _____
>
> There is some personal judgment involved in deciding whether to rebut.
> Correcting bad science is the first concern. Responding to unfair
> personal criticisms is next. Third is the possible misrepresentation
> of the results by persons with ideological or political agendas. On
> the basis of these I think the Baliunas paper should be rebutted by
> persons with appropriate expertise. Names like Mann, Crowley, Briffa,
> Bradley, Jones, Hughes come to mind. Are these people willing to spend
> time on this?
>
> _____
>
> There are two other examples that I know of where I will probably be
> involved in writing a response.
>
> The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16,
> 10.1029/2002GL015345, 2002). I refereed a virtually identical paper

PETITIONERS' EXEMPLARS (PE)

- > for J. Climate, recommending rejection. All the other referees
- > recommended rejection too. The paper is truly appalling -- but somehow
- > it must have been poorly reviewed by GRL and slipped through the net.
- > I have no reason to believe that this was anything more than chance.
- > Nevertheless, my judgment is that the science is so bad that a
- > response is necessary.
- >
- > The second is the paper by Michaels et al. that was in Climate
- > Research (vol. 23, pp. 1–9, 2002). Danny Harvey and I refereed this
- > and said it should be rejected. We questioned the editor (deFreitas
- > again!) and he responded saying
- >
- > The MS was reviewed initially by five referees. ... The other three
- > referees, all reputable atmospheric scientists, agreed it should be
- > published subject to minor revision. Even then I used a sixth person
- > to help me decide. I took his advice and that of the three other
- > referees and sent the MS back for revision. It was later accepted for
- > publication. The refereeing process was more rigorous than usual.
- >
- > On the surface this looks to be above board -- although, as referees
- > who advised rejection it is clear that Danny and I should have been
- > kept in the loop and seen how our criticisms were responded to.
- >
- > It is possible that Danny and I might write a response to this paper
- > -- deFreitas has offered us this possibility.
- >
- > _____
- >
- > This second case gets to the crux of the matter. I suspect that
- > deFreitas deliberately chose other referees who are members of the
- > skeptics camp. I also suspect that he has done this on other
- > occasions. How to deal with this is unclear, since there are a number
- > of individuals with bona fide scientific credentials who could be used
- > by an unscrupulous editor to ensure that 'anti-greenhouse' science can
- > get through the peer review process (Legates, Balling, Lindzen,
- > Baliunas, Soon, and so on).
- >
- > The peer review process is being abused, but proving this would be
- > difficult.
- >
- > The best response is, I strongly believe, to rebut the bad science
- > that does get through.
- >
- > _____
- >
- > Jim Salinger raises the more personal issue of deFreitas. He is
- > clearly giving good science a bad name, but I do not think a barrage
- > of ad hominem attacks or letters is the best way to counter this.
- >
- > If Jim wishes to write a letter with multiple authors, I may be
- > willing to sign it, but I would not write such a letter myself.
- >
- > In this case, deFreitas is such a poor scientist that he may simply
- > disappear. I saw some work from his PhD, and it was awful (Pat
- > Michaels' PhD is at the same level).
- >

PETITIONERS' EXEMPLARS (PE)

> _____
>
> Best wishes to all,
> Tom.
>

Dr Jim Salinger, CRSNZ
NIWA
P O Box 109 695
Newmarket, Auckland
New Zealand
Tel + 64 9 375 2053 begin_of_the_skype_highlighting + 64 9 375 2053
end_of_the_skype_highlighting Fax + 64 9 375 2051
e-mail: j.salinger@niwa.co.nz

PETITIONERS' EXEMPLARS (PE)

PE-17

Date: Thu Feb 26 15:59:12 2004

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Crap Papers
Date: Thu Feb 26 15:59:12 2004

Mike,

Just agreed to review a paper for GRL - it is absolute rubbish. It is having a go at the CRU temperature data - not the latest vesion, but the one you used in MBH98 !! We added lots of data in for the region this person says has Urban Warming ! So easy review to do.

Sent Ben the Soon et al. paper and he wonders who reviews these sorts of things. Says GRL hasn't a clue with editors or reviewers. By chance they seem to have got the right person with the one just received.

Can I ask you something in CONFIDENCE - don't email around, especially not to Keith and Tim here. Have you reviewed any papers recently for Science that say that MBH98 and MJ03 have underestimated variability in the millennial record - from models or from some low-freq proxy data. Just a yes or no will do. Tim is reviewing them - I want

to make sure he takes my comments on board, but he wants to be squeaky clean with discussing them with others. So forget this email when you reply.

Cheers

Phil

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

PETITIONERS' EXEMPLARS (PE)

PE-18

Date: Fri Apr 5 17:17:55 2002

From: Keith Briffa <k.briffa@uea.ac.uk>

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, drdendro@ldeo.columbia.edu, rkerr@aaas.org, bhanson@aaas.org

Subject: Re: Briffa & Osborn piece

Date: Fri Apr 5 17:17:55 2002

Cc: Tim Osborn <t.osborn@uea.ac.uk>

Dear Mike, (and interested colleagues)

Given the list of people to whom you have chosen to circulate your message(s), we thought we should make a short, somewhat formal, response here. I am happy to reserve my informal response until we are face to face! We did not respond earlier because we had more pressing tasks to deal with. This is not the place to go into a long or over-detailed response to all of your comments but a few brief remarks might help to clear up a couple of misconceptions.

You consider our commentary on Ed and Jan's paper

"more flawed than even the paper itself"

on the basis that scaling the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues we discuss. In fact we did not do this. The curve labelled Mann99 in our figure was, in fact, based on the average of only the land areas, north of 20 degrees N, extracted from your spatially-resolved reconstructions. We then scaled it by calibration against the instrumental annual temperatures from the same region. This is, just as you stress in your comments on the Esper et al. paper, what should have been done. We think that this single point addresses virtually all of your concerns. We can, of course, argue about what this means for the pre-1400 part of your reconstruction, when only 1 EOF was reconstructed, but the essential message is that we did our best to exclude the tropics (and the oceans too!) from your series so that it could more readily be compared with the other records.

The fact that we have used only the extra-tropical land from your data is not clear from the text, so we can see why you may not have appreciated this, but we think you will concede that this fact negates much of what you say and that we acted "more correctly" than you realised. Blame *Science* for being so mean with their space allocation if you want! Remember that this was an unrefereed piece and we felt justified in concentrating on one issue; that of the importance of the method of scaling and its effect on apparent "absolute" reconstruction levels. In our draft, we went on to say that this was crucial for issues of simple model sensitivity studies and climate detection, citing the work of Tom Crowley and Myles Allen, but this fell foul of the editor's knife.

You also express concerns about the calibration of Esper et al. (e.g., you say "if the authors had instead used the actual (unsmoothed) instrumental record for the extratropical northern hemisphere to scale their record, their reconstruction would be much closer to MBH99").

This point is wholly consistent with our discussion in the perspective piece, and indeed we show that in absolute terms the records are closer when Esper et al. is calibrated using unsmoothed data but since the variance is also reduced, the significance of the differences may be just as high.

Finally, we have to say that we do not feel constrained in what we say to the media or write in the scientific or popular press, by what the sceptics will say or do with our results. We can only strive to do our best and address the issues honestly. Some "sceptics" have their own dishonest agenda - we have no doubt of that. If you believe that I, or Tim, have any other objective but to be open and honest about the uncertainties in the climate change debate, then I am disappointed in you also.

Best regards

Keith (and Tim)

At 12:39 PM 3/22/02 -0500, Michael E. Mann wrote:

PETITIONERS' EXEMPLARS (PE)

Keith and Tim,

Sadly, your piece on the Esper et al paper is more flawed than even the paper itself. Ed, the AP release that appeared in the papers was even worse. Apparently you allowed yourself to be quoted saying things that are inconsistent with what you told me you had said.

You three all should have known better. Keith and Tim: Arguing you can scale the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues you discuss, and this isn't even touched on in your piece. The evidence of course continues to mount (e.g., Hendy et al, Science, a couple weeks ago) that the tropical SST in the past centuries varied far more less in past centuries. Hendy et al specifically point out that there is little evidence of an LIA in the tropics in the data. The internal inconsistency here is remarkably ironic. The tropics play a very important part in our reconstruction, with half of the surface temperature estimate coming from latitudes below 30N. You know this, and in my opinion you have knowingly misrepresented our work in your piece. This will be all be straightened out in due course. In the meantime, there is a lot of damage control that needs to be done and, in my opinion, you've done a disservice to the honest discussions we had all had in the past, because you've misrepresented the evidence. Many of us are very concerned with how Science dropped the ball as far as the review process on this paper was concerned. This never should have been published in Science, for the reason's I outlined before (and have attached for those of you who haven't seen them). I have to wonder why the functioning of the review process broke down so overtly here,
Mike

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

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

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

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

References

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

PETITIONERS' EXEMPLARS (PE)

PE-19

Date: Mon Sep 10 20:34:13 2001

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Ed Cook <drdendro@ldeo.columbia.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Esper/Cook paper
Date: Mon Sep 10 20:34:13 2001
Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, Crowley_Hegerl <tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper <esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk

Ed

I still believe you are not showing sufficient comparisons with series besides the MBH ; necessary to demonstrate the true extent of "new" information in this work. At the very least this needs to acknowledge that other (and other tree-ring-based) series are out there , that use at least some of the data you employ , and use the RCS method to process may of their constituent series - i.e. the Northern chronology series shown in my QSR paper. What is similar and what is different in your series and this one?

You give the impression here that you are using the RCS and new data to demonstrate the possibility of getting more low frequency signal from tree-ring data - but then you base this on a comparison with MBH only. Surely what is needed here is to establish WHY MBH don't get as much LIA for example . By not showing that other tree-ring data that have also shown a LIA , and not exploring why MBH does not (despite using some of the same -and note -already RCS standardised data) is perhaps confusing rather than clarifying the issue.

When we discussed this here, I also suggested the need to show separate "north" and more "south" curves ,separated in your data set, to try to get at least some handle on the independent expression of the centennial trends in a region south of the over-exploited northern network . At the very least it should be clearly stated that many of the site data used here and in previous work (see our Science perspectives piece) are common and other series already produce more low-frequency signal than is implied in MBH .

Sorry for this rushed comment but I wanted to get this point over as we had talked about it before but you don't seem to have taken it on board.

cheers

Keith

At 02:51 PM 9/10/01 -0400, Ed Cook wrote:

Hi Mike et al.,

Okay, here is an overlay plot of MBH vs. RCS, with RCS scaled to the 1900-1977 period of MBH, and with 95% confidence limits. This has been done for the 40-yr low-pass RCS data to be consistent with the low-pass MBH series you sent me. The 95% confidence limits of the RCS are also scaled appropriately. Since correlations with both instrumental and MBH are $O(0.95)$ after even 20-year smoothing because of the trend, the RCS limits are effectively based on the bootstrap 95% limits of the 14 chronologies. Assuming that the original RCS C.I.s are reasonably accurate (which I think they are), what is apparent (to me anyway) is that the confidence limits of MBH are uniformly narrower after AD 1600. Prior to that, they are comparable to RCS back to ca. AD 1200 where RCS C.I.s get bigger. Of course this is an odd comparison because the confidence limits are not derived the same way. However, I do think that they are somewhat informative nonetheless. What is also apparent is the much great amplitude of variability in the RCS estimates. This is consistent with the understanding that extratropical temperatures are more variable than tropical temperatures, which supports the idea that the MBH record does have more

PETITIONERS' EXEMPLARS (PE)

tropical temperature information in it. The other interesting thing about expressing the RCS data this way and overlaying it on MBH is the appearance that MBH is missing the LIA rather than the MWP, at least on multi-centennial timescales. This turns some of Broecker's criticism of the "hockey stick" on its head. I'm not sure where all this leads.

Any comments and further suggestions are welcome as long as they come in by tomorrow. I am definitely submitting the paper within a day or two.

Cheers,

Ed

=====

Dr. Edward R. Cook

Doherty Senior Scholar

Tree-Ring Laboratory

Lamont-Doherty Earth Observatory

Palisades, New York 10964 USA

Email: drdendro@ldeo.columbia.edu

Phone: 845-365-8618 begin_of_the_skype_highlighting

845-365-8618

end_of_the_skype_highlighting

Fax: 845-365-8152

=====

--

Professor Keith Briffa,

Climatic Research Unit

University of East Anglia

Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909 begin_of_the_skype_highlighting

+44-1603-593909

end_of_the_skype_highlighting

Fax: +44-1603-507784

[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

References

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

PETITIONERS' EXEMPLARS (PE)

PE-20

Date: Thu, 21 Mar 2002 16:39:06 -0500

From: "Michael E. Mann" <mann@virginia.edu>

To: drdendro@ldeo.columbia.edu

Subject: Esper et al paper

Date: Thu, 21 Mar 2002 16:39:06 -0500

Cc: k.briffa@uea.ac.uk, p.jones@uea.ac.uk, Tom Crowley <tcrowley@duke.edu>, t.osborn@uea.ac.uk, rbradley@geo.umass.edu, Malcolm Hughes <mhughes@litr.arizona.edu>

Dear Ed,

I'm really sorry I couldn't be more supportive of the final version of the manuscript. I fully expected to be able to be more positive in my assessment. I was frankly very disappointed when I saw the final version--it is overwhelmingly different from the version you shared with us originally. Sadly, it seems to have suffered, and not benefited, from the review process--a very odd scenario. I fault the reviewers as much (in fact more) than I fault you for this. There are some really basic problems that they didn't seem to catch. I hope neither you nor your co-authors take this personally.

I'm trying to be as diplomatic as I can be in my discussions w/ reporters, etc. but I really wish you hadn't sprung this on us w/ no warning of the dramatic changes that were made. I'm forced to be somewhat critical, because the flaws in some of your conclusions need to be pointed out, or they will be exploited by those w/ ulterior motives. You certainly must have foreseen this, as must have the reviewers. I'm very disappointed, very disappointed indeed.

I'm sharing my comments w/ Keith, Phil, Tim, Tom, Ray, and Malcolm. I am resisting the temptation to write a letter of response to Science, although my better judgement dictates that I should...

Mike

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

e-mail: mann@virginia.edu Phone: (434) 924-7770 begin_of_the_skype_highlighting (434) 924-7770 end_of_the_skype_highlighting FAX: (434) 982-2137
[1][http://www.evsc.virginia.edu/faculty/people/mann.\[2\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[2].shtml)
Attachment Converted: "c:\eudora\attach\treerings-comments.doc"

References

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

PETITIONERS' EXEMPLARS (PE)

PE-21

Date: Mon, 06 Jan 2003 09:13:24 -0500

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

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

Dear Scott and Mike,

Over the Christmas break I (finally!) had time to read the RegEM manuscript in detail. Phil had already read and annotated a copy - so I've added my annotations to that and will mail it to you today. Mike asked for comments to go to Scott, so please tell me which address I should use (Rhode Island or Virginia?).
I spoke to Keith and he has partly read it too, and will provide separate comments soon. Overall, I think the paper is a very nice piece of work and I'm pleased to be involved with it. The results regarding robustness with respect to proxy data, method, region and season are definitely good to publish.
Among the many comments annotated on the manuscript, a few are repeated here so that all authors may respond if they wish:

- (1) Given the overwhelming number of values in the Tables, I suggest halving them by dropping all the CE values (keeping just RE values). As the paper points out, getting the verification period mean right is rewarded by RE but not by CE. Since we are interested in changes in the mean, I don't think that's a problem. CE is fine in addition, but dropping it would provide benefits of reducing manuscript size - and especially the size of the tables.
- (2) The "mixed-hybrid" approach sounds dubious to me - more justification/explanation of why it is needed (and hence why it captures more variance than the simpler splitting into high- and low-frequency components method).
- (3) It is not clear to me that the paragraph and figure on the comparison with Esper et al. are either correct or necessary. They also are problematic because it would appear that we (Briffa & Osborn) were contradicting our earlier paper when in fact we aren't. The paper is already long and to remove these parts would therefore be helpful anyway. The comparison with Esper et al. is important - but much better dealt with in a separate paper where it could be developed in more detail and with more room to explain the approach and its implications.
- (4) I still hope to write up some more detailed comparisons of the reconstructions using just the MXD data but different methods and will let Mike/Scott know my plans on this soon.

Happy new year to you all.

Tim

Dr Timothy J Osborn | phone: +44 1603 592089 begin_of_the_skype_highlighting
+44 1603 592089 end_of_the_skype_highlighting
Senior Research Associate | fax: +44 1603 507784

PETITIONERS' EXEMPLARS (PE)

Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
School of Environmental Sciences | web-site:
University of East Anglia _____ | [1]<http://www.cru.uea.ac.uk/~timo/>
Norwich NR4 7TJ | sunclock:
UK | [2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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

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

References

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

PETITIONERS' EXEMPLARS (PE)

PE-22

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

From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: reconstruction errors
Date: Thu, 31 Jul 2003 11:18:24 -0400

Tim,

Attached are the calibration residual series for experiments based on available networks back to:

AD 1000

AD 1400

AD 1600

I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...

You only want to look at the first column (year) and second column (residual) of the files.

I can't even remember what the other columns are!

Let me know if that helps. Thanks,

mike

p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

At 02:58 PM 7/31/2003 +0100, you wrote:

Thanks for the explanation, Mike. Now I see it, it looks familiar - so perhaps you've explained it to me previously (if you have, then sorry for asking twice!).

I now understand how you compute them in theory. I have two further questions though (sorry):

(1) how do you compute them in practise? Do you actually integrate the spectrum of the residuals?

(2) how would I estimate an uncertainty for a particular band of time scales (e.g. decadal to secular, $f=0.0$ to 0.1)? If integrating the spectrum of the residuals, I wonder whether integrating from $f=0$ to $f=0.02$ and then $f=0.02$ to (e.g.) $f=0.1$ (note this last limit has changed) would give me the right error for time scales of 10 years and longer (i.e. for a 10-yr low pass filter)? The way I had planned to do this was to assume the residuals could be modelled as a first order autoregressive process, with lag-1 autocorrelation $r_1=0.0$ after 1600 (essentially white) and $r_1=???$ before 1600. Do you know what the lag-1 autocorrelation of the residuals is for the network that goes back to 1000 AD?

The stuff back 2000 years will be interesting, though the GCM runs we're starting to look at go back only 500 (Hadley Centre) or 1000 (German groups), so MBH99 seems fine for now.

Cheers

Tim

At 14:28 31/07/2003, you wrote:

Tim,

The one-sigma *total* uncertainty is determined from adding the low f and high f components of uncertainty in quadrature. The low f and high f uncertainties aren't uncertainties for a particular (e.g. 30 year or 40-year) running mean, they are band

PETITIONERS' EXEMPLARS (PE)

integrated estimates of uncertainties (high-frequency band from $f=0$ to $f=0.02$, low-frequency band from $f=0.02$ to $f=0.5$ cycle/year) taking into account the spectrum of the residual variance (the broadband or "white noise" mean of which is the nominal variance of the calibration residuals)

Alternatively, one could calculate uncertainties for a particular timescale average using the standard deviation of the calibration residuals, and applying a square-root-N' argument (where N' is the effective degrees of freedom in the calibration residuals). I believed I did this at one point, and got similar results.

Let me know if this needs further clarification. Thanks,
mike

p.s. you might want to try to using Mann and Jones N. Hem if you're going back further than AD 1000? Crowley has some EBM results now back to 0 AD, and is in the process of comparing w/ that. Sould be interesting...

At 02:04 PM 7/31/2003 +0100, you wrote:

Hi Mike,

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

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

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

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

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

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

Tim

Dr Timothy J Osborn

Climatic Research Unit

School of Environmental Sciences, University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089 begin_of_the_skype_highlighting +44 1603 592089

end_of_the_skype_highlighting

fax: +44 1603 507784

web: [1]<http://www.cru.uea.ac.uk/~timo/>

suncllock: [2]<http://www.cru.uea.ac.uk/~timo/suncllock.htm>

Professor Michael E. Mann

PETITIONERS' EXEMPLARS (PE)

Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

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

Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK
e-mail: t.osborn@uea.ac.uk
phone: +44 1603 592089 begin_of_the_skype_highlighting +44 1603 592089
end_of_the_skype_highlighting
fax: +44 1603 507784
web: [4]<http://www.cru.uea.ac.uk/~timo/>
sunclock: [5]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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

e-mail: mann@virginia.edu Phone: (434) 924-7770 begin_of_the_skype_highlighting (434) 924-
7770 end_of_the_skype_highlighting FAX: (434) 982-2137
[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\documents and settings\tim osborn\my
documents\leudora\attach\nh-ad1000-resid.dat" Attachment Converted: "c:\documents and
settings\tim osborn\my documents\leudora\attach\nh-ad1400-resid.dat" Attachment Converted:
"c:\documents and settings\tim osborn\my documents\leudora\attach\nh-ad1600-resid.dat"

References

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

PETITIONERS' EXEMPLARS (PE)

PE-23

Date: Wed Apr 27 09:06:53 2005

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: CCNet: DEBUNKING THE "DANGEROUS CLIMATE CHANGE" SCARE
Date: Wed Apr 27 09:06:53 2005

Mike,

Presumably you've seen all this - the forwarded email from Tim. I got this email from McIntyre a few days ago. As far as I'm concerned he has the data - sent ages ago. I'll tell him this, but that's all - no code. If I can find it, it is likely to be hundreds of lines of

uncommented fortran ! I recall the program did a lot more than just average the series.

I know why he can't replicate the results early on - it is because there was a variance correction for fewer series.

See you in Bern.

Cheers

Phil

Dear Phil,

In keeping with the spirit of your suggestions to look at some of the other multiproxy publications, I've been looking at Jones et al [1998]. The methodology here is obviously more straightforward than MBH98. However, while I have been able to substantially emulate your calculations, I have been unable to do so exactly. The differences are larger in the early periods.

Since I have been unable to replicate the results exactly based on available materials, I would appreciate a copy of the actual data set used in Jones et al [1998] as well as the code used in these calculations.

There is an interesting article on replication by Anderson et al., some distinguished economists, here [1]<http://research.stlouisfed.org/wp/2005/2005-014.pdf> discussing the issue of replication in applied economics and referring favorably to our attempts in respect to MBH98.

Regards, Steve McIntyre

X-Mailer: QUALCOMM Windows Eudora Version 6.2.0.14
Date: Tue, 26 Apr 2005 13:28:53 +0100
To: Phil Jones <p.jones@uea.ac.uk>,"Keith Briffa" <k.briffa@uea.ac.uk>
From: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: CCNet: DEBUNKING THE "DANGEROUS CLIMATE CHANGE" SCARE
Keith and Phil,
you both feature in the latest issue of CCNet:

(4) GLOBAL WARMING AND DATA

Steve Verdon, Outside the Beltway, 25 April 2005

[2]<http://www.outsidethebeltway.com/archives/10200>

A new paper ([3]<http://research.stlouisfed.org/wp/2005/2005-014.pdf>) from the St. Luis Federal Reserve Bank has an interesting paper on how important it is to archive not only the data but the code for empirical papers. While the article looks mainly at economic research there is also a lesson to be drawn from this paper about the current state of research for global warming/climate change. One of the hallmarks of scientific research is that the results can be replicable. Without this, the results shouldn't be considered valid let alone used for making policy.

Ideally, investigators should be willing to share their data and programs so as to

PETITIONERS' EXEMPLARS (PE)

encourage other investigators to replicate and/or expand on their results.³ Such behavior allows science to move forward in a Kuhn-style linear fashion, with each generation seeing further from the shoulders of the previous generation.⁴ At a minimum, the results of an endeavor-if it is to be labeled "scientific"-should be replicable, i.e., another researcher using the same methods should be able to reach the same result. In the case of applied economics using econometric software, this means that another researcher using the same data and the same computer software should achieve the same results.

However, this is precisely the problem that Steven McIntyre and Ross McKittrick have run into since looking into the methodology used by Mann, Hughes and Bradely (1998) (MBH98), the paper that came up with the famous "hockey stick" for temperature reconstructions. For example, this post here shows that McIntyre was prevented from accessing Mann's FTP site. This is supposedly a public site where interested researchers can download not only the source code, but also the data. This kind of behavior by Mann et. al. is simply unscientific and also rather suspicious. Why lock out a researcher who is trying to verify your results...do you have something to hide professors Mann, Bradley and Huges? Not only has this been a problem has this been a problem for McIntyre with regards to MBH98, but other studies as well. This post at Climate Audit shows that this problem is actually quite serious.

Crowley and Lowery (2000)

After nearly a year and over 25 emails, Crowley said in mid-October that he has misplaced the original data and could only find transformed and smoothed versions. This makes proper data checking impossible, but I'm planning to do what I can with what he sent. Do I need to comment on my attitude to the original data being "misplaced"?

Briffa et al. (2001)

There is no listing of sites in the article or SI (despite JGR policies requiring citations be limited to publicly archived data). Briffa has refused to respond to any requests for data. None of these guys have the least interest in some one going through their data and seem to hoping that the demands wither away. I don't see how any policy reliance can be made on this paper with no available data.

Esper et al. (2002)

This paper is usually thought to show much more variation than the hockey stick. Esper has listed the sites used, but most of them are not archived. Esper has not responded to any requests for data. '

Jones and Mann (2003); Mann and Jones (2004)

Phil Jones sent me data for these studies in July 2004, but did not have the weights used in the calculations, which Mann had. Jones thought that the weights did not matter, but I have found differently. I've tried a few times to get the weights, but so far have been unsuccessful. My surmise is that the weighting in these papers is based on correlations to local temperature, as opposed to MBH98-MBH99 where the weightings are based on correlations to the temperature PC1 (but this is just speculation right now.)

The papers do not describe the methods in sufficient detail to permit replication.

Jacoby and d'Arrigo (northern treeline)

I've got something quite interesting in progress here. If you look at the original 1989 paper, you will see that Jacoby "cherry-picked" the 10 "most temperature-sensitive" sites from 36 studied. I've done simulations to emulate cherry-picking from persistent red noise and consistently get hockey stick shaped series, with the Jacoby northern treeline reconstruction being indistinguishable from simulated hockey sticks. The other 26 sites have not been archived. I've written to Climatic Change to get them to intervene in getting the data. Jacoby has refused to provide the data. He says that his research is "mission-oriented" and, as an ex-marine, he is only interested in a "few good" series.

Jacoby has also carried out updated studies on the Gaspé series, so essential to MBH98. I've seen a chronology using the new data, which looks completely different from the old data (which is a hockey stick). I've asked for the new data, but Jacoby-d'Arrigo have refused it saying that the old data is "better" for showing temperature increases. Need

PETITIONERS' EXEMPLARS (PE)

I comment? I've repeatedly asked for the exact location of the Gaspé site for nearly 9 months now (I was going to privately fund a re-sampling program, but Jacoby, Cook and others have refused to disclose the location.) Need I comment?

Jones et al (1998)

Phil Jones stands alone among paleoclimate authors, as a diligent correspondent. I have data and methods from Jones et al 1998. I have a couple of concerns here, which I'm working on. I remain concerned about the basis of series selection - there is an obvious risk of "cherrypicking" data and I'm very unclear what steps, if any, were taken to avoid this. The results for the middle ages don't look robust to me. I have particular concerns with Briffa's Polar Urals series, which takes the 11th century results down (Briffa arguing that 1032 was the coldest year of the millennium). It looks to me like the 11th century data for this series does not meet quality control criteria and Briffa was over-reaching. Without this series, Jones et al. 1998 is high in the 11th century. Note that none of this actually "disproves" the global warming hypothesis. However, it does raise very, very serious questions in my opinion. We are talking about enacting policies to curb global warming that could cost not billions, but trillions of dollars. Shouldn't we at least be allowed to see the source code, the data and ask for replication at a minimum? I think the answer is simple: YES!!

Dr Timothy J Osborn

Climatic Research Unit

School of Environmental Sciences, University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089 [begin_of_the_skype_highlighting](#) +44 1603 592089

[end_of_the_skype_highlighting](#)

fax: +44 1603 507784

web: [4]<http://www.cru.uea.ac.uk/~timo/>

suncllock: [5]<http://www.cru.uea.ac.uk/~timo/suncllock.htm>

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090 [begin_of_the_skype_highlighting](#)

+44 (0) 1603 592090 [end_of_the_skype_highlighting](#)

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

References

1. <http://research.stlouisfed.org/wp/2005/2005-014.pdf>
2. <http://www.outsidethebeltway.com/archives/10200>
3. <http://research.stlouisfed.org/wp/2005/2005-014.pdf>
4. <http://www.cru.uea.ac.uk/~timo/>
5. <http://www.cru.uea.ac.uk/~timo/suncllock.htm>

PETITIONERS' EXEMPLARS (PE)

PE-24

Date: Mon Feb 21 16:28:32 2005

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA
Date: Mon Feb 21 16:28:32 2005
Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

Mike, Ray and Malcolm,

The skeptics seem to be building up a head of steam here ! Maybe we can use this to our advantage to get the series updated !

Odd idea to update the proxies with satellite estimates of the lower troposphere rather than surface data !. Odder still that they don't realise that Moberg et al used the Jones and Moberg updated series !

Francis Zwiers is still inside. He said that PC1s produce hockey sticks. He stressed that the late 20th century is the warmest of the millennium, but Regaldo didn't bother with that. Also ignored Francis' comment about all the other series looking similar to MBH.

The IPCC comes in for a lot of stick.

Leave it to you to delete as appropriate !

Cheers

Phil

PS I'm getting hassled by a couple of people to release the CRU station temperature data.

Don't any of you three tell anybody that the UK has a Freedom of Information Act !

X-Sender: f023@pop.uea.ac.uk
X-Mailer: QUALCOMM Windows Eudora Version 6.1.0.6
Date: Mon, 21 Feb 2005 15:40:05 +0000
To: p.jones@uea.ac.uk
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

Subject: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

Date: Mon, 21 Feb 2005 15:02:37 -0000
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: pressure grows on climate modellers to relase secret data
Thread-Index: AcUXiV64e/f3li8uQSa0X88pndSQgQAI2O1w
From: "Peiser, Benny" <B.J.Peiser@livjm.ac.uk>
To: "cambridge-conference" <cambridge-conference@livjm.ac.uk>
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
CCNet 22/2005 - 21 February 2005
PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

This should have produced a healthy scientific debate. Instead, Mr. Mann tried to shut down debate by refusing to disclose the mathematical algorithm by which he arrived at his conclusions. All the same, Mr. Mann was forced to publish a retraction of some of his initial data, and doubts about his statistical methods have since grown.

--The Wall Street Journal, 18 February 2005

But maybe we are in that much trouble. The WSJ highlights what Regaldo and McIntyre

PETITIONERS' EXEMPLARS (PE)

says is Mann's resistance or outright refusal to provide to inquiring minds his data, all details of his statistical analysis, and his code. So this is what I say to Dr. Mann and others expressing deep concern over peer review: give up your data, methods and code freely and with a smile on your face.

--Kevin Vranes, Science Policy, 18 February 2005

Mann's work doesn't meet that definition [of science], and those who use Mann's curve in their arguments are not making a scientific argument. One of Pournelle's Laws states "You can prove anything if you can make up your data." I will now add another Pournelle's Law: "You can prove anything if you can keep your algorithms secret."

--Jerry Pournelle, 18 February 2005

The time has come to question the IPCC's status as the near-monopoly source of information and advice for its member governments. It is probably futile to propose reform of the present IPCC process. Like most bureaucracies, it has too much momentum and its institutional interests are too strong for anyone realistically to suppose that it can assimilate more diverse points of view, even if more scientists and economists were keen to join up. The rectitude and credibility of the IPCC could be best improved not through reform, but through competition.

--Steven F. Hayward, The American Enterprise Institute, 15 February 2005

(1) HOCKEY STICK ON ICE

The Wall Street Journal, 18 February 2005

(2) SCIENCE AND OPEN ALGORITHMS: "YOU CAN PROVE ANYTHING WITH SECRET DATA

AND

ALGORITHMS"

Jerry Pournell, 18 February 2005

(3) OPEN SEASON ON HOCKEY AND PEER REVIEW

Science Policy, 18 February 2005

(4) CLIMATE CHANGE SCIENCE: TIME FOR TEAM "B"?

The American Enterprise Institute, 15 February 2005

(5) BRING THE PROXIES UP TO DATE!

Climate Audit, 20 February 2005

(6) CARELESS SCIENCE COSTS LIVES

The Guardian, 18 February 2005

(7) RE: MORE TROUBLE FOR CLIMATE MODELS

Helen Krueger <hkrueger@sbcglobal.net>

(8) HOW TO HANDLE ASTEROID 2004 MN4

Jens Kieffer-Olsen <dstdba@post4.tele.dk>

(9) AND FINALLY: EUROPE FURTHER FALLING BEHIND IN TECHNOLOGY AND RESEARCH

EU Observer, 10 February 2005

=====

(1) HOCKEY STICK ON ICE

The Wall Street Journal, 18 February 2005

[1]http://online.wsj.com/article_email/0,,SB110869271828758608-ldjeoNmlah4n5yta4GHaqylm4,00.html

On Wednesday National Hockey League Commissioner Gary Bettman canceled the season, and we guess that's a loss. But this week also brought news of something else that's been put on ice. We're talking about the "hockey stick."

Just so we're clear, this hockey stick isn't a sports implement; it's a scientific graph. Back in the late 1990s, American geoscientist Michael Mann published a chart that purported to show average surface temperatures in the Northern Hemisphere over the past 1,000 years. The chart showed relatively minor fluctuations in temperature over the first 900 years, then a sharp and continuous rise over the past century, giving it a hockey-stick shape.

Mr. Mann's chart was both a scientific and political sensation. It contradicted a body of scientific work suggesting a warm period early in the second millennium, followed by a "Little Ice Age" starting in the 14th century. It also provided some visually

PETITIONERS' EXEMPLARS (PE)

arresting scientific support for the contention that fossil-fuel emissions were the cause of higher temperatures. Little wonder, then, that Mr. Mann's hockey stick appears five times in the Intergovernmental Panel on Climate Change's landmark 2001 report on global warming, which paved the way to this week's global ratification -- sans the U.S., Australia and China -- of the Kyoto Protocol.

Yet there were doubts about Mr. Mann's methods and analysis from the start. In 1998, Willie Soon and Sallie Baliunas of the Harvard-Smithsonian Center for Astrophysics published a paper in the journal *Climate Research*, arguing that there really had been a Medieval warm period. The result: Messrs. Soon and Baliunas were treated as heretics and six editors at *Climate Research* were made to resign.

Still, questions persisted. In 2003, Stephen McIntyre, a Toronto minerals consultant and amateur mathematician, and Ross McKittrick, an economist at Canada's University of Guelph, jointly published a critique of the hockey stick analysis. Their conclusion: Mr. Mann's work was riddled with "collation errors, unjustifiable truncations of extrapolation of source data, obsolete data, geographical location errors, incorrect calculations of principal components, and other quality control defects." Once these were corrected, the Medieval warm period showed up again in the data.

This should have produced a healthy scientific debate. Instead, as the *Journal's* Antonio Regalado reported Monday, Mr. Mann tried to shut down debate by refusing to disclose the mathematical algorithm by which he arrived at his conclusions. All the same, Mr. Mann was forced to publish a retraction of some of his initial data, and doubts about his statistical methods have since grown. Statistician Francis Zwiers of Environment Canada (a government agency) notes that Mr. Mann's method "preferentially produces hockey sticks when there are none in the data." Other reputable scientists such as Berkeley's Richard Muller and Hans von Storch of Germany's GKSS Center essentially agree. We realize this may all seem like so much academic nonsense. Yet if there really was a Medieval warm period (we draw no conclusions), it would cast some doubt on the contention that our SUVs and air conditioners, rather than natural causes, are to blame for apparent global warming.

There is also the not-so-small matter of the politicization of science: If climate scientists feel their careers might be put at risk by questioning some orthodoxy, the inevitable result will be bad science. It says something that it took two non-climate scientists to bring Mr. Mann's errors to light.

But the important point is this: The world is being lobbied to place a huge economic bet -- as much as \$150 billion a year -- on the notion that man-made global warming is real. Businesses are gearing up, at considerable cost, to deal with a new regulatory environment; complex carbon-trading schemes are in the making. Shouldn't everyone look very carefully, and honestly, at the science before we jump off this particular cliff?
Copyright 2005, The Wall Street Journal

=====

(2) SCIENCE AND OPEN ALGORITHMS: "YOU CAN PROVE ANYTHING WITH SECRET DATA AND ALGORITHMS"

Jerry Pournell, 18 February 2005

[2]<http://www.jerrypournelle.com/view/view349.html#hockeystick>

Science and Open Algorithms: You can prove anything with secret data and algorithms. There is a long piece on the global "hockey stick" in today's *Wall Street Journal* that explains something I didn't understand: Mann, who generated the "hockey stick" curve purporting to show that the last century was unique in all recorded history with its sharp climb in temperature, has released neither the algorithm that generated his curve nor the data on which it was based.

I had refrained from commenting on the "hockey stick" because I couldn't understand how it was derived. I've done statistical analysis and prediction from uncertainty much of my life. My first job in aerospace was as part of the Human Factors and Reliability Group at Boeing, where we were expected to deal with such matters as predicting component failures, and deriving maintenance schedules (replace it before it fails, but

PETITIONERS' EXEMPLARS (PE)

not so long before it fails that the costs including the cost of the maintenance crew and the costs of taking the airplane out of service are prohibitive) and other such matters. I used to live with Incomplete Gamma Functions and other complex integrals; and I could not for the life of me understand how Mann derived his famous curve. Now I know: he hasn't told anyone. He says that telling people how he generated it would be tantamount to giving in to his critics.

More on this after my walk, but the one thing we may conclude for sure is that this is not science. His curve has been distributed as part of the Canadian government's literature on why Canada supports Kyoto, and is said to have been influential in causing the "Kyoto Consensus" so it is certainly effective propaganda; but IT IS NOT SCIENCE. Science deals with repeatability and openness. When I took Philosophy of Science from Gustav Bergmann at the University of Iowa a very long time ago, our seminar came to a one-sentence "practical definition" of science: Science is what you can put in a letter to a colleague and he'll get the same results you did. Now I don't claim that as original for it wasn't even me who came up with it in the seminar; but I do claim Bergmann liked that formulation, and it certainly appealed to me, and I haven't seen a better one-sentence practical definition of science. Mann's work doesn't meet that definition, and those who use Mann's curve in their arguments are not making a scientific argument.

One of Pournelle's Laws states "You can prove anything if you can make up your data." I will now add another Pournelle's Law: "You can prove anything if you can keep your algorithms secret."

=====

(3) OPEN SEASON ON HOCKEY AND PEER REVIEW

Science Policy, 18 February 2005

[3]http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000355open_season_on_hocke.html

By Kevin Vranes

The recent 2/14 WSJ article ("Global Warring..." by Antonio Regaldo) addresses the debate that most readers of this site are well familiar with: the Mann et al. hockey stick. The WSJ is still asking - and trying to answer - the basic questions: hockey stick or no hockey stick? But the background premise of the article, stated explicitly and implicitly throughout, is that it was the hockey stick that led to Kyoto and other climate policy. Is it?

I think it's fair to say that to all of us in the field of climatology, the notion that Kyoto is based on the Mann curve is utter nonsense. If a climatologist, or a policy advisor charged with knowing the science well enough to make astute recommendations to his/her boss, relied solely on the Mann curve to prove definitively the existence of anthropogenic warming, then we're in deeper trouble than anybody realizes. (This is essentially what Stephan Ramstorf writes in a 1/27 RealClimate post.) And although it's easy to believe that national and international policy can hinge on single graphs, I hope we give policy makers more credit than that.

But maybe we are in that much trouble. The WSJ highlights what Regaldo and McIntyre says is Mann's resistance or outright refusal to provide to inquiring minds his data, all details of his statistical analysis, and his code. The WSJ's anecdotal treatment of the subject goes toward confirming what I've been hearing for years in climatology circles about not just Mann, but others collecting original climate data.

As concerns Mann himself, this is especially curious in light of the recent RealClimate posts ([link](#) and [link](#)) in which Mann and Gavin Schmidt warn us about peer review and the limits therein. Their point is essentially that peer review is limited and can be much less than thorough. One assumes that they are talking about their own work as well as McIntyre's, although they never state this. Mann and Schmidt go to great lengths in their post to single out Geophysical Research Letters. Their post then seems a bit ironic, as GRL is the journal in which the original Mann curve was published (1999, vol 26., issue 6, p. 759), an article which is now receiving much attention as being flawed and under-reviewed. (For that matter, why does Table 1 in Mann et al. (1999) list many

PETITIONERS' EXEMPLARS (PE)

chronologies in the Southern Hemisphere while the rest of the paper promotes a Northern Hemisphere reconstruction? Legit or not, it's a confusing aspect of the paper that should never have made it past peer review.)

Of their take on peer review, I couldn't agree more. In my experience, peer review is often cursory at best. So this is what I say to Dr. Mann and others expressing deep concern over peer review: give up your data, methods and code freely and with a smile on your face. That is real peer review. A 12 year-old hacker prodigy in her grandparents' basement should have as much opportunity to check your work as a "semi-retired Toronto minerals consultant." Those without three letters after their name can be every bit as intellectually qualified, and will likely have the time for careful review that typical academic reviewers find lacking.

Specious analysis of your work will be borne out by your colleagues, and will enter the debate with every other original work. Your job is not to prevent your critics from checking your work and potentially distorting it; your job is to continue to publish insightful, detailed analyses of the data and let the community decide. You can be part of the debate without seeming to hinder access to it.

=====

(4) CLIMATE CHANGE SCIENCE: TIME FOR TEAM "B"?

The American Enterprise Institute, 15 February 2005

[4]http://www.aei.org/publications/pubID.21974/pub_detail.asp

By Steven F. Hayward

The Intergovernmental Panel on Climate Change (IPCC) is currently working on its fourth assessment report. Despite the IPCC's noble intent to generate a scientific consensus, a number of factors have compromised the research and drafting process, assuring that its next assessment report will be just as controversial as previous reports in 1995 and 2001. Efforts to reform this large bureaucratic effort are unlikely to succeed. Perhaps the time has come to consider competition as the means of checking the IPCC's monopoly and generating more reliable climate science.

As the Intergovernmental Panel on Climate Change (IPCC) moves toward the release of its fourth assessment report (fourth AR) in 2007, the case of Chris Landsea offers in microcosm an example of why the IPCC's findings are going to have credibility problems. Last month Landsea, a climate change scientist with the U.S. National Oceanic and Atmospheric Administration (NOAA), resigned as a participant in the producing the report. Landsea had been a chapter author and reviewer for the IPCC's second assessment report in 1995 and the third in 2001, and he is a leading expert on hurricanes and related extreme weather phenomena. He had signed on with the IPCC to update the state of current knowledge on Atlantic hurricanes for the fourth report. In an open letter, Landsea wrote that he could no longer in good conscience participate in a process that is "being motivated by pre-conceived agendas" and is "scientifically unsound."^[1] Landsea's resignation was prompted by an all too familiar occurrence: The lead author of the fourth AR's chapter on climate observations, Kevin Trenberth, participated in a press conference that warned of increasing hurricane activity as a result of global warming.^[2] It is common to hear that man-made global warming represents the "consensus" of science, yet the use of hurricanes and cyclones as a marker of global warming represents a clear-cut case of the consensus being roundly ignored. Both the second and third IPCC assessments concluded that there was no global warming signal found in the hurricane record. Moreover, most climate models predict future warming will have only a small effect--if any--on hurricane strength. "It is beyond me," Landsea wrote, "why my colleagues would utilize the media to push an unsupported agenda that recent hurricane activity has been due to global warming."^[3] Landsea's critique goes beyond a fit of pique at the abuse of his area of expertise. The IPCC, he believes, has become thoroughly politicized, and is unresponsive to criticism. "When I have raised my concerns to the IPCC leadership," Landsea wrote, "their response was simply to dismiss my concerns."^[4]

Landsea's frustration is not an isolated experience. MIT physicist Richard Lindzen, another past IPCC author who is not participating in the fourth report, has written: "My

PETITIONERS' EXEMPLARS (PE)

experiences over the past 16 years have led me to the discouraging conclusion that we are dealing with the almost insoluble interaction of an iron triangle with an iron rice bowl." (Lindzen's "iron triangle" consists of activists misusing science to get the attention of the news media and politicians; the "iron rice bowl" is the parallel phenomenon where scientists exploit the activists' alarm to increase research funding and attention for the issue.[5]) And Dr. John Zillman, one of Australia's leading climate scientists, is another ex-IPCC participant who believes the IPCC has become "cast more in the model of supporting than informing policy development." [6] And when the IPCC is not ignoring its responsible critics like Landsea and Lindzen, it is demonizing them. Not long ago the IPCC's chairman, Dr. Rajendra Pachauri, compared eco-skeptic Bjorn Lomborg to Hitler. "What is the difference between Lomborg's view of humanity and Hitler's?" Pachauri asked in a Danish newspaper. "If you were to accept Lomborg's way of thinking, then maybe what Hitler did was the right thing." [7] Lomborg's sin was merely to follow the consensus practice of economists in applying a discount to present costs for future benefits, and comparing the range of outcomes with other world problems alongside climate change. It is hard to judge what is worse: Pachauri's appalling judgment in resorting to *reductio ad Hitlerum*, or his abysmal ignorance of basic economics. In either case, it is hard to have much confidence in the policy advice the IPCC might have. [...]

Time for "Team B"?

The time has come to question the IPCC's status as the near-monopoly source of information and advice for its member governments. It is probably futile to propose reform of the present IPCC process. Like most bureaucracies, it has too much momentum and its institutional interests are too strong for anyone realistically to suppose that it can assimilate more diverse points of view, even if more scientists and economists were keen to join up. The rectitude and credibility of the IPCC could be best improved not through reform, but through competition....

FULL PAPER at [5]http://www.aei.org/publications/pubID.21974/pub_detail.asp

=====

(5) BRING THE PROXIES UP TO DATE!

Climate Audit, 20 February 2005

[6]<http://www.climateaudit.org/index.php?p=89#more-89>

Steve McIntyre

I will make here a very simple suggestion: if IPCC or others want to use "multiproxy" reconstructions of world temperature for policy purposes, stop using data ending in 1980 and bring the proxies up-to-date. Let's see how they perform in the warm 1990s - which should be an ideal period to show the merit of the proxies. I do not believe that any responsible policy-maker can base policy, even in part, on the continued use of obsolete data ending in 1980, when the costs of bringing the data up-to-date is inconsequential compared to Kyoto costs.

I would appreciate comments on this note as I think that I will pursue the matter with policymakers.

For example, in Mann's famous hockey stick graph, as presented to policymakers and to the public, the graph used Mann's reconstruction from proxies up to 1980 and instrumental temperatures (here, as in other similar studies, using Jones' more lurid CRU surface history rather than the more moderate increases shown by satellite measurements). Usually (but not always), a different color is used for the instrumental portion, but, from a promotional point of view, the juxtaposition of the two series achieves the desired promotional effect. (In mining promotions, where there is considerable community experience with promotional graphics and statistics, securities commission prohibit the adding together of proven ore reserves and inferred ore reserves - a policy which deserves a little reflection in the context of IPCC studies).

Last week, a brand new multiproxy study by European scientists [Moberg et al., 2005] was published in *Nature*. On the very day of publication, I received an email from a prominent scientist telling me that Mann's hockey stick was yesterday's news, that the "community" had now "moved on" and so should I. That the "community" had had no

PETITIONERS' EXEMPLARS (PE)

opportunity to verify Moberg's results, however meritorious they may finally appear, seemed to matter not at all.

If you look at the proxy portion of the new Moberg graphic, you see nothing that would be problematic for opponents of the hockey stick: it shows a striking Medieval Warm Period (MWP), a cold Little Ice Age and 20th century warming not quite reaching MWP levels by 1979, when the proxy portion of the study ends. (I'm in the process of examining the individual proxies and the Moberg reconstruction is not without its own imperfections.) In the presentation to the public - see the figure in the Nature article itself, once again, there is the infamous splice between reconstruction by proxy (up to 1980) and the instrumental record thereafter (once again Jones' CRU record, rather than the satellite record).

One of the first question that occurs to any civilian becoming familiar with these studies (and it was one of my first questions) is: what happens to the proxies after 1980? Given the presumed warmth of the 1990s, and especially 1998 (the "warmest year in the millennium"), you'd think that the proxy values would be off the chart. In effect, the last 25 years have provided an ideal opportunity to validate the usefulness of proxies and, especially the opportunity to test the confidence intervals of these studies, put forward with such assurance by the multiproxy proponents. What happens to the proxies used in MBH99 or Moberg et al [2005] or Crowley and Lowery [2000] in the 1990s and, especially, 1998?

This question about proxies after 1980 was posed by a civilian to Mann in December at realclimate. Mann replied:

Most reconstructions only extend through about 1980 because the vast majority of tree-ring, coral, and ice core records currently available in the public domain do not extend into the most recent decades. While paleoclimatologists are attempting to update many important proxy records to the present, this is a costly, and labor-intensive activity, often requiring expensive field campaigns that involve traveling with heavy equipment to difficult-to-reach locations (such as high-elevation or remote polar sites). For historical reasons, many of the important records were obtained in the 1970s and 1980s and have yet to be updated. [my bold]

Pause and think about this response. Think about the costs of Kyoto and then think again about this answer. Think about the billions spent on climate research and then try to explain to me why we need to rely on "important records" obtained in the 1970s. Far more money has been spent on climate research in the last decade than in the 1970s. Why are we still relying on obsolete proxy data?

As someone with actual experience in the mineral exploration business, which also involves "expensive field campaigns that involve traveling with heavy equipment to difficult-to-reach locations", I can assure readers that Mann's response cannot be justified and is an embarrassment to the paleoclimate community. The more that I think about it, the more outrageous is both the comment itself and the fact that no one seems to have picked up on it.

It is even more outrageous when you look in detail at what is actually involved in collecting the proxy data used in the medieval period in the key multiproxy studies. The number of proxies used in MBH99 is from fewer than 40 sites (28 tree ring sites being U.S. tree ring sites represented in 3 principal component series).

As to the time needed to update some of these tree ring sites, here is an excerpt from Lamarche et al. [1984] on the collection of key tree ring cores from Sheep Mountain and Campito Mountain, which are the most important indicators in the MBH reconstruction: "D.A.G. [Graybill] and M.R.R. [Rose] collected tree ring samples at 3325 m on Mount Jefferson, Toquima Range, Nevada and 11 August 1981. D.A.G. and M.R.R. collected samples from 13 trees at Campito Mountain (3400 m) and from 15 trees at Sheep Mountain (3500 m) on 31 October 1983."

Now to get to Campito Mountain and Sheep Mountain, they had to get to Bishop, California, which is hardly "remote" even by Paris Hilton standards, and then proceed by road to within a few hundred meters of the site, perhaps proceeding for some portion of the journey on unpaved roads.

PETITIONERS' EXEMPLARS (PE)

The picture below illustrates the taking of a tree ring core. While the equipment may seem "heavy" to someone used only to desk work using computers, people in the mineral exploration business would not regard this drill as being especially "heavy" and I believe that people capable of operating such heavy equipment can be found, even in out-of-the way places like Bishop, California. I apologize for the tone here, but it is impossible for me not to be facetious.

There is only one relatively remote site in the entire MBH99 roster - the Quelccaya glacier in Peru. Here, fortunately, the work is already done (although, needless to say, it is not published.) This information was updated in 2003 by Lonnie Thompson and should be adequate to update these series. With sufficient pressure from the U.S. National Science Foundation, the data should be available expeditiously. (Given that Thompson has not archived data from Dundee drilled in 1987, the need for pressure should not be under-estimated.)

I realize that the rings need to be measured and that the field work is only a portion of the effort involved. But updating 28 tree ring sites in the United States is not a monumental enterprise nor would updating any of the other sites.

I've looked through lists of the proxies used in Jones et al. [1998], MBH99, Crowley and Lowery [2000], Mann and Jones [2003], Moberg et al [2005] and see no obstacles to bringing all these proxies up to date. The only sites that might take a little extra time would be updating the Himalayan ice cores. Even here, it's possible that taking very short cores or even pits would prove adequate for an update and this might prove easier than one might be think. Be that as it may, any delays in updating the most complicated location should not deter updating all the other locations.

As far as I'm concerned, this should be the first order of business for multiproxy studies.

Whose responsibility is this? While the costs are trivial in the scheme of Kyoto, they would still be a significant line item in the budget of a university department. I think that the responsibility here lies with the U.S. National Science Foundation and its equivalents in Canada and Europe. The responsibilities for collecting the proxy updates could be divided up in a couple of emails and budgets established.

One other important aspect: right now the funding agencies fund academics to do the work and are completely ineffective in ensuring prompt reporting. At best, academic practice will tie up reporting of results until the publication of articles in an academic journals, creating a delay right at the start. Even then, in cases like Thompson or Jacoby, to whom I've referred elsewhere, the data may never be archived or only after decades in the hands of the originator.

So here I would propose something more like what happens in a mineral exploration program. When a company has drill results, it has to publish them through a press release. It can't wait for academic reports or for its geologists to spin the results. There's lots of time to spin afterwards. Good or bad - the results have to be made public. The company has a little discretion so that it can release drill holes in bunches and not every single drill hole, but the discretion can't build up too much during an important program. Here I would insist that the proxy results be archived as soon as they are produced - the academic reports and spin can come later. Since all these sites have already been published, people are used to the proxies and the updates will to a considerable extent speak for themselves.

What would I expect from such studies? Drill programs are usually a surprise and maybe there's one here. My hunch is that the classic proxies will not show anywhere near as "loud" a signal in the 1990s as is needed to make statements comparing the 1990s to the Medieval Warm Period with any confidence at all. I've not surveyed proxies in the 1990s (nor to my knowledge has anyone else), but I've started to look and many do not show the expected "loud" signal e.g. some of the proxies posted up on this site such as Alaskan tree rings, TTHH ring widths, and theories are starting to develop. But the discussions so far do not explicit point out the effect of signal failure on the multiproxy reconstruction project.

But this is only a hunch and the evidence could be otherwise. The point is this: there's

PETITIONERS' EXEMPLARS (PE)

no need to speculate any further. It's time to bring the classic proxies up to date.

=====

(6) CARELESS SCIENCE COSTS LIVES

The Guardian, 18 February 2005

[7]<http://www.guardian.co.uk/comment/story/0,3604,1417224,00.html>

Dick Taverne

In science, as in much of life, it is believed that you get what you pay for. According to opinion polls, people do not trust scientists who work for industry because they only care about profits, or government scientists because they suspect them of trying to cover up the truth. Scientists who work for environmental NGOs are more highly regarded. Because they are trying to save the planet, people are ready to believe that what they say must be true. A House of Lords report, Science and Society, published in 2000, agreed that motives matter. It argued that science and scientists are not value-free, and therefore that scientists would command more trust "if they openly declare the values that underpin their work".

It all sounds very plausible, but mostly it is wrong. Scientists with the best of motives can produce bad science, just as scientists whose motives may be considered suspect can produce good science. An obvious example of the first was Rachel Carson, who, if not the patron saint, was at least the founding mother of modern environmentalism. Her book *The Silent Spring* was an inspiring account of the damage caused to our natural environment by the reckless spraying of pesticides, especially DDT.

However, Carson also claimed that DDT caused cancer and liver damage, claims for which there is no evidence but which led to an effective worldwide ban on the use of DDT that is proving disastrous. Her motives were pure; the science was wrong. DDT is the most effective agent ever invented for preventing insect-borne disease, which, according to the US National Academy of Sciences and the WHO, prevented over 50 million human deaths from malaria in about two decades. Although there is no evidence that DDT harms human health, some NGOs still demand a worldwide ban for that reason. Careless science cost lives.

Contrast the benefits that have resulted from the profit motive, a motive that is held to be suspect by the public. Multinationals, chief villains in the demonology of contemporary anti-capitalists, have developed antibiotics, vaccines that have eradicated many diseases like smallpox and polio, genetically modified insulin for diabetics, and plants such as GM insect-resistant cotton that have reduced the need for pesticides and so increased the income and improved the health of millions of small cotton farmers. The fact is that self-interest can benefit the public as effectively as philanthropy.

Motives are not irrelevant, and unselfish motives are rightly admired more than selfish ones. There are numerous examples of misconduct by big companies, and we should examine their claims critically and provide effective regulation to control abuses of power and ensure the safety of their products. Equally, we should not uncritically accept the claims of those who act from idealistic motives. NGOs inspired by the noble cause of protecting our environment often become careless about evidence and exaggerate risks to attract attention (and funds). Although every leading scientific academy has concluded that GM crops are at least as safe as conventional foods, this does not stop Greenpeace reiterating claims about the dangers of "Frankenfoods". Stephen Schneider, a climatologist, publicly justified distortion of evidence: "Because we are not just scientists but human beings as well ... we need to ... capture the public imagination ... So we have to offer up scary scenarios, make simplified dramatic statements, and make little mention of any doubts we have."

But in the end motives are irrelevant to the validity of science. It does not matter if a scientist wants to help mankind, get a new grant, win a Nobel prize or increase the profits of her company. It does not matter whether a researcher works for Monsanto or for Greenpeace. Results are no more to be trusted if the researcher declares his values and confesses that he beats his wife, believes in God, or is an Arsenal supporter. What matters is that the work has been peer-reviewed, that the findings are reproducible and

PETITIONERS' EXEMPLARS (PE)

that they last. If they do, they are good science. If not, not. Science itself is value-free. There are objective truths in science. We can now regard it as a fact that the Earth goes rounds the sun and that Darwinism explains the evolution of species. A look at the history of science makes it evident how irrelevant the values of scientists are. Newton's passion for alchemy did not invalidate his discovery of the laws of gravitation. To quote Professor Fox of Rutger's University: "How was it relevant to Mendel's findings about peas that he was a white, European monk? They would have been just as valid if Mendel had been a Spanish-speaking, lesbian atheist."

· Lord Taverne is chair of Sense About Science and author of The March of Unreason, to be published next month

Copyright 2005, The Guardian

===== LETTERS =====

(7) RE: MORE TROUBLE FOR CLIMATE MODELS

Helen Krueger <hkrueger@sbcglobal.net>

Dear Dr. Peiser,

I just want to let you know how much I am enjoying being included in your list so that I can benefit from your astute handling of alarmist information personally and with my students.

Thank you so much!

Regards,

Helen A. Krueger

Educational Consultant

Phone: 203-426-8043 begin_of_the_skype_highlighting

203-426-8043

end_of_the_skype_highlighting

FAX: 203-426-3541

=====

(8) HOW TO HANDLE ASTEROID 2004 MN4

Jens Kieffer-Olsen <dstdba@post4.tele.dk>

Dear Benny Peiser,

In CCNet 18/2005 - 11 February 2005 you brought an interesting article on the possible breakup of NEA 2004 MN4 in the year 2029:

> But there's another reason for concern. According to Dan
> Durda, another SWRI astronomer, 2004 MN4 is likely to be
> a "rubble-pile" asteroid, consisting of material only
> loosely held together by gravity. Because the asteroid
> will pass us at just 2.5 times Earth's diameter, tidal
> forces could tear it apart. The result would be a trail
> of rocks drifting slowly apart with the passage of time.
> One or more of these might hit Earth in the more distant
> future, creating a spectacular fireball as it burns up
> in the atmosphere.

> --Bill Cooke, Astronomy Magazine, 10 February 2005
First of all, a 300m asteroid could break into 100 pieces each larger than the Tunguska impactor. Secondly, the years for which a TS rating of 1 already exist for the object are NOT in the distant future, but 6, 7, and 8 years later. That reminds us that neither the Torino nor the Palermo scale takes into account the possibility of such a MIRV'ed approach. Furthermore, the Palermo scale is designed to take into account the lead time. Even if 2004 MN4 were not to break up, the lead time to virtual impact in 2029 would be down to one sixth of the time to-day. In other words, if the post-2029 orbit is not being resolved before then, we may as well up the PS rating accordingly. If my math is correct, we should add 0.78 to its Palermo Scale rating,

PETITIONERS' EXEMPLARS (PE)

ie. $\log_{10}(6)$, for a total of -0.65.

Yours sincerely

Jens Kieffer-Olsen, M.Sc.(Elec.Eng.)

Slagelse, Denmark

=====

(9) AND FINALLY: EUROPE FURTHER FALLING BEHIND IN TECHNOLOGY AND RESEARCH
EU Observer, 10 February 2005

[8]<http://www.euobserver.com/?aid=18382&print=1>

By Lucia Kubosova

BRUSSELS / EUOBSERVER - Europeans are still failing to show world leadership in technology and research, a new report shows.

The paper, published on Thursday (10 February) has evaluated the EU research and development programmes and their impact on Europe's knowledge-base and potential for innovation.

While it argues that EU funds for the programmes make a "major contribution", it suggests that more resources, industry participation and simplified administration are needed for them to have a greater effect in future.

"We have somehow lost momentum", said Erkki Ormala, chair of the panel issuing the report.

"The EU is falling behind. And we are now under pressure not only compared to our traditional rivals like the US or Japan, but also China, India or Brazil. We are facing a much tougher competition in talent and knowledge than we are used to".

Research Commissioner Janez Potocnik considers the paper's results as a reason for doubling the funds in his portfolio within the next budgetary period of 2007-2013.

"We don't want to achieve our economic growth by lowering the social or environmental standards. So to compete globally, we need to focus on knowledge", Mr Potocnik said to journalists, adding that the EU programmes should "make a bridge between practical innovation and research".

The report has listed several possible solutions for tackling outlined setbacks.

It argues that the EU must attract and reward the best talent, mobilise resources for innovation and boost cooperation between governments, businesses and universities in research.

It supports the idea of setting up a European Research Council to promote excellence and encourages more industry involvement, mainly on the part of small and medium-sized enterprises (SMEs).

However, SME representatives complain that their ideas about EU research and innovation funding are not taken into consideration.

"It's not about how big the budget is for SMEs and their involvement in such projects.

It is rather about the allocation of the funds. Most of them are granted for huge long-term projects which cost millions of euro and they can hardly attract smaller companies", according to Ullrich Schroeder, from UEAPME, the main umbrella organisation. He argues that while several reports have already pointed out that SMEs must be more involved if the "Lisbon agenda" goal of 3 percent of GDP to be invested in research and development in the EU by 2010 is to be achieved, in reality they are not as well supported as huge transnational companies.

"It is not that the EU member states invest much less in universities than the US, but the greatest difference is that European SMEs are only investing 8% of the US amount, and it is simply not enough".

Mr Schroeder also said that while "there is a lot of rhetoric from politicians, that the SMEs should get involved, innovate and compete, when they come up with good projects, they are not sufficiently supported".

"The European Commission is more concerned about big companies and hightech areas, while innovation is needed also in more down-to earth sectors", Mr Schroeder told the EUobserver.

© EUobserver.com 2005

PETITIONERS' EXEMPLARS (PE)

CCNet is a scholarly electronic network. To subscribe/unsubscribe, please contact the editor Benny Peiser <b.j.peiser@livjm.ac.uk>. Information circulated on this network is for scholarly and educational use only. The attached information may not be copied or reproduced for any other purposes without prior permission of the copyright holders. DISCLAIMER: The opinions, beliefs and viewpoints expressed in the articles and texts and in other CCNet contributions do not necessarily reflect the opinions, beliefs and viewpoints of the editor.

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909 begin_of_the_skype_highlighting +44-1603-593909
end_of_the_skype_highlighting
Fax: +44-1603-507784
[9]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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

References

1. http://online.wsj.com/article_email/0,,SB110869271828758608-ldjeoNmlah4n5yta4GHaqylm4,00.html
2. <http://www.jerrypournelle.com/view/view349.html#hockeystick>
3. http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000355open_season_on_hocke.html
4. http://www.aei.org/publications/pubID.21974/pub_detail.asp
5. http://www.aei.org/publications/pubID.21974/pub_detail.asp
6. <http://www.climateaudit.org/index.php?p=89#more-89>
7. <http://www.guardian.co.uk/comment/story/0,3604,1417224,00.html>
8. <http://www.euobserver.com/?aid=18382&print=1>
9. <http://www.cru.uea.ac.uk/cru/people/briffa/>

PETITIONERS' EXEMPLARS (PE)

PE-25

Date: Fri Jan 16 13:25:59 2004

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: CLIMATIC CHANGE needs your advice - YOUR EYES ONLY !!!!!
Date: Fri Jan 16 13:25:59 2004

Mike,

This is for YOURS EYES ONLY. Delete after reading - please ! I'm trying to redress the balance. One reply from Pfister said you should make all available !! Pot calling the kettle black - Christian doesn't make his methods available. I replied to the wrong Christian message so you don't get to see what he said. Probably best. Told Steve separately and to get more advice from a few others as well as Kluwer and legal.

PLEASE DELETE - just for you, not even Ray and Malcolm

Cheers
Phil

Date: Fri, 16 Jan 2004 12:37:29 +0000
To: Christian Azar <christian.azar@fy.chalmers.se>, christian.pfister@hist.unibe.ch
From: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: AW: CLIMATIC CHANGE needs your advice
Cc: "David G. VICTOR" <dgvector@stanford.edu>, 'Katarina Kivel' <kivel@stanford.edu>, N.W.Arnell@soton.ac.uk, frtca@fy.chalmers.se, d.camuffo@isac.cnr.it, scohen@sdri.ubc.ca, pmfearn@inpa.gov.br, jfoley@facstaff.wisc.edu, pgleick@pipeline.com, harvey@geog.utoronto.ca, ahs@ansto.gov.au, Thomas.R.Karl@noaa.gov, rwk@ucar.edu, rik.leemans@rivm.nl, diana.liverman@eci.ox.ac.uk, mccarl@tamu.edu, lindam@atd.ucar.edu, rmoss@usgcrp.gov, ogilvie@spot.colorado.edu, barrie.pittock@dar.csiro.au, pollard@essc.psu.edu, nj.rosenberg@pnl.gov, crosenzweig@giss.nasa.gov, j.salinger@niwa.co.nz, santer1@lnl.gov, h.j.schellnhuber@uea.ac.uk, F.I.Woodward@sheffield.ac.uk, gyohe@wesleyan.edu, leonid@atmosph.physics.utoronto.ca, shs@stanford.edu
Dear Steve et al,

I've been away this week until today. Although the responses so far all make valid points, I

will add my thoughts. I should say I have been more involved in all the exchanges between

Mike and MM so I'm probably biased in Mike's favour. I will try and be impartial, though, but

I did write a paper with Mike (which came out in GRL in Aug 2003) and we currently have a long paper tentatively accepted by Reviews of Geophysics. With the latter all 4 reviewers

think the paper is fine, but the sections referring to MM and papers by Soon and Baliunas

are not and our language is strong. We need to work on this.

Back to the question in hand:

1. The papers that MM refer came out in Nature in 1998 and to a lesser extent in GRL in

1999. These reviewers did not request the data (all the proxy series) and the code. So, acceding to the request for this to do the review is setting a VERY dangerous precedent.

Mike has made all the data series and this is all anyone should need. Making model code available is something else.

PETITIONERS' EXEMPLARS (PE)

2. The code is basically irrelevant in this whole issue. In the GRL paper (in 2003 Mann and Jones), we simply average all the series we use together. The result is pretty much the same as MBH in 1998, Nature and MBH in 1999 in GRL.

3. As many of you know I calculate gridded and global/hemispheric temperature time series

each month. Groups at NCDC and NASA/GISS do this as well. We don't exchange codes - we do occasionally though for the data. The code here is trivial as it is in the paleo work.

MBH get spatial patterns but the bottom line (the 1000 year series of global temps) is almost the same if you simply average. The patterns give more, though, when it comes to trying to understand what has caused the changes - eg by comparison with models. MM are only interested in the NH/Global 1000-year time series - in fact only in the MBH work from 1400.

4. What has always intrigued me in this whole debate, is why the skeptics (for want of a better term) always pick on Mike. There are several other series that I've produced, Keith Briffa has and Tom Crowley. Jan Esper's work has produced a slightly different series

but we don't get bombarded by MM. Mike's paper wasn't the first. It was in Nature and is well-used by IPCC. I suspect the skeptics wish to concentrate their effort onto one person as they did with Ben Santer after the second IPCC report.

5. Mike may respond too strongly to MM, but don't we all decide not to work with or co-operate with people we do not get on with or do not like their views. Mike will say that MM are disingenuous, but I'm not sure how many of you realise how vicious the attack on him has been. I will give you an example.

When MM came out, we had several press calls (I don't normally get press calls about my papers unless I really work at it - I very rarely do). This was about a paper in E&E, which when we eventually got it several days later was appalling. I found out later that the authors were in contact with the reviewers up to a week before the article

appeared. So there is peer review and peer review !! Here the peer review was done by like-minded colleagues. Anyway, I'm straying from the point. Tim Osborn, Keith Briffa and I felt we should put something on our web site about the paper and directs people to Mike's site and also to E&E and the MM's site. MM have hounded us about this for the last four months. In the MM article, they have a diagram which says 'corrected version' when comparing with MBH. We have seen people refer to this paper (MM) as an alternative reconstruction - yet when we said this is our paragraph MM claim they are not putting forward a new reconstruction but criticizing MBH 1998 !! We have decided to remove the sentence on our web page just to stop these emails. But if a corrected version isn't a new or alternative reconstruction I don't know what is.

So, in conclusion, I would side with Mike in this regard. In trying to be scrupulously

fair, Steve, you've opened up a whole can of worms. If you do decide to put the Mann response into CC then I suspect you will need an editorial. MM will want to respond also.

I know you've had open and frank exchanges in CC before, but your email clearly shows that you think this is in a different league. MM and E&E didn't give Mann the chance to

respond when they put their paper in, but this is a too simplistic. It needs to be pointed

out in an editorial though - I'm not offering by the way.

I could go on and on

Cheers

Phil

At 10:36 15/01/2004 +0100, Christian Azar wrote:

PETITIONERS' EXEMPLARS (PE)

Dear all,
I agree with most of what has been said so far. Reproducibility is the key word. If the Mann et al material (to be) posted on the website is sufficient to ensure reproducibility, then there is no compelling need to force them to hand it out. If not, then the source code is warranted. Also, even if there is no compelling need to make the source code public, doing it anyway would clearly be beneficial for the entire debate.

Yours,
Christian

Christian Azar
Professor
Department of physical resource theory
Chalmers University of Technology
Göteborg University
412 96 Göteborg
Sweden
ph: ++46 31 772 31 32
[1]www.frt.fy.chalmers.se
[2]www.miljo.chalmers.se/cei

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

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

References

1. <http://www.frt.fy.chalmers.se/>
2. <http://www.miljo.chalmers.se/cei>

PETITIONERS' EXEMPLARS (PE)

PE-26

Wed, 16 Feb 2005 17:56:01 -0500

From: "Michael E. Mann" <mann@virginia.edu>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>, Stephen H Schneider <shs@stanford.edu>, Tom Wigley <wigley@ucar.edu>, Ben Santer <santer1@llnl.gov>, mann@virginia.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, omichael@Princeton.edu, jmahman@ucar.edu, k.briffa@uea.ac.uk, p.jones@uea.ac.uk
Subject: Fwd: RE: WSJ article
Date: Wed, 16 Feb 2005 17:56:01 -0500

Interesting that Antonio R. doesn't (or at least claims not to) recognize a lack of balance in the article.

Please treat this email as confidential. I don't believe that sending a letter to the editor myself would be the best avenue. But perhaps someone else is interested in pursuing this?

Mike

Subject: RE: WSJ article
Date: Wed, 16 Feb 2005 17:43:10 -0500
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: WSJ article
Thread-Index: AcUUalg6ON4Ck5ANQ2OfoGmU0QNsvAAAEqMA
From: "Regalado, Antonio" <Antonio.Regalado@wsj.com>
To: "Michael E. Mann" <mann@virginia.edu>
X-OriginalArrivalTime: 16 Feb 2005 22:43:10.0610 (UTC) FILETIME=[E423A720:01C51478]
X-UVA-Virus-Scanned: by amavisd-new at fork11.mail.virginia.edu

Hi Mike,

On the personal stuff, I'd go with your first impressions, rather than the perceptions of others. This isn't a one-sided story. Anyway, I certainly want to find out who is right here and so I am open to writing more as the papers come out and the facts become clearer, just as I have written in the past about the Soon and Ballunias business (p. A3 not bad) and about paleo-climate (p. 1 story in 2002 about Gary Comer's funding, feature story on Lonnie Thompson's melting glaciers), etc. Would it surprise you to hear that anytime I write a story which seems to favor global warming I am also deluged by accusations of bias and demands for corrections etc.?

Regarding Moberg, I think the issue you are raising is a question of emphasis and not a matter for a correction. The specific sentences you're thinking of (Indeed, new research from Stockholm University on historical temperatures suggests past fluctuations were nearly twice as great as the hockey stick shows. That could mean the 20th-century jump isn't quite so anomalous.) seem to me to be not only factual but precisely to the point of what the mainstream of science is discussing vis a vis MBH, which was the topic of that part of my story. For instance, in the Anderson/Woodhouse commentary that accompanied Moberg in the same issue of Nature, they too stress the increased variability just as I did and they make no mention of the late 1990s. And as per my email Monday, my article does also say that other reconstructions also indicate that the 20th Century was unusually warm and that the punch line is the same.

I'm sure you're fully sick of writing letters, but this may be a right opportunity for a letter to the editor from you or someone who you can second. The person to send a letter to is [1]Karen.Pensiero@wsj.com. If you want, CC: me and my editor, [2]Elyse.tanouye@wsj.com. Or even an editorial on the broader topic of where the science is at. I can give you the name for who to send an editorial to if you want it.

PETITIONERS' EXEMPLARS (PE)

It is probably worth pointing out that no amount of debate can change the facts buried in those tree rings, etc..

Yes, I will continue to write about climate. The next topic is impacts. What do you think is the best story there? I'd like to write about current impacts rather than only projected ones as these will be more tangible for the reader. Also, since the Arctic has been well covered I'd be interested in impacts at lower latitudes.

Antonio

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

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

References

1. <mailto:Karen.Pensiero@wsj.com>
2. <mailto:Elyse.tanouye@wsj.com>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

PETITIONERS' EXEMPLARS (PE)

PE-27 **Date: Fri, 04 Feb 2005 15:52:53 -0500**

From: Keith Briffa <k.briffa@uea.ac.uk>
To: chris.folland@metoffice.gov.uk
Subject: Fwd: Re: FW: "hockey stock" methodology misleading
Date: Tue Feb 8 16:44:17 2005

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 6.1.1.1

Date: Fri, 04 Feb 2005 16:04:57 -0500

To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu,
tom crowley <tom@ocean.tamu.edu>, tom crowley <tom@ocean.tamu.edu>,
mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu,
Keith Briffa <k.briffa@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Fwd: Re: FW: "hockey stock" methodology misleading

X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
X-UEA-MailScanner-SpamScore: s
sorry, forgot to attach the paper...
mike

Date: Fri, 04 Feb 2005 15:54:15 -0500

To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Tom Crowley, Tom Crowley,
mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Fwd: Re: FW: "hockey stock" methodology misleading

Date: Fri, 04 Feb 2005 15:52:53 -0500

To: Andy Revkin <anrevk@nytimes.com>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: FW: "hockey stock" methodology misleading

Hi Andy,

The McIntyre and McKittrick paper is pure scientific fraud. I think you'll find this reinforced by just about any legitimate scientist in our field you discuss this with.

Please see the RealClimate response:

[1]<http://www.realclimate.org/index.php?p=111>

and also:

[2]<http://www.realclimate.org/index.php?p=114>

The Moberg et al paper is at least real science. But there are some real problems with it (you'll want to followup w/ people like Phil Jones for a 2nd opinion).

While the paper actually reinforces the main conclusion of previous studies (it also finds the late 20th century to be the warmest period of the past two millennia), it challenges various reconstructions

using tree-ring information (which includes us, but several others such as Jones et al, Crowley, etc). I'm pretty sure, by the way, that a very similar version of the paper was rejected previously by Science. A number of us are therefore very surprised that Nature is publishing it, given a number of serious problems:

Their method for combining frequencies is problematic and untested:

A. they only use a handful of records, so there is a potentially large sampling bias.

B. worse, they use different records for high-frequencies and low-frequencies, so the bias isn't even the same--the reconstruction is apples and oranges.

C. The wavelet method is problematic. We have found in our own work that you cannot simply combine the content in different at like frequencies, because different proxies have different signal vs. noise characteristics at different frequencies--for some records, there century-scale variability is likely to be pure noise. They end up

PETITIONERS' EXEMPLARS (PE)

therefore weighting noise as much as signal. For some of the records used, there are real age model problems. The timescale isn't known to better than +/- a couple hundred years in several cases. So when they average these records together, the century-scale variability is likely to be nonsense.

D. They didn't do statistical verification. This is absolutely essential for such reconstructions (see e.g. the recent Cook et al and Luterbacher et al papers in Science). They should have validated their reconstruction against long-instrumental records, as we and many others have. Without having done so, there is no reason to believe the reconstruction has any reliability. This is a major problem w/ the paper. It is complicated by the fact that they don't produce a pattern, but just a hemispheric mean--that makes it difficult to do a long-term verification. But they don't attempt any sort of verification at all! There are some decades known to be warm from the available instrumental records (1730s, some in the 16th century) which the Moberg reconstruction completely misses--the reconstruction gives the impression that all years are cold between 1500 and 1750. The reconstruction would almost certainly fail cross-validation against long instrumental records. If so, it is an unreliable estimate of past changes. We're surprised the Nature Reviewers didn't catch this.

E. They also didn't validate their method against a model (where I believe it would likely fail). We have done so w/ our own "hybrid frequency-domain" method that combines information separately at low and high-frequencies, but taking into account the problem mentioned above. This is described in:

Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., [3]Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal of Climate, in press (2005).

In work that is provisionally accepted in "Journal of Climate" (draft attached), we show that our method gives the correct history using noisy "pseudoproxy" records derived from a climate model simulation with large past changes in radiative forcing. Moberg et al have not tested their method in such a manner.

F. They argue selectively for favorable comparison w/ other work:

(1) Esper et al: when authors rescaled the reconstruction using the full instrumental record (Cook et al, 2004), they found it to be far more similar to Mann et al, Crowley and Lowery, Jones et al, and the roughly dozen or so other empirical and model estimates consistent w/ it. Several studies, moreover [see e.g.: Shindell, D.T., Schmidt, G.A., Mann, M.E., Faluvegi, G., [4]Dynamic winter climate response to large tropical volcanic eruptions since 1600, Journal of Geophysical Research, 109, D05104, doi: 10.1029/2003JD004151, 2004.] show that extratropical, land-only summer temperatures, which Esper et al emphasises, are likely to be biased towards greater variability--so it's an apples and oranges comparison anyway.

(2) von Storch et al: There are some well known problems here: (a) their forcing is way too large (Foukal et al in Science a couple months back indicates maybe 5 times too large), DKMI uses same model, more conventional forcings, and get half the amplitude and another paper submitted recently by the Belgium modeling group suggests that some severe spin-up/initialization problems give the large century-scale swings in the model--these are not reproducible.

(3) Boreholes: They argue that Boreholes are "physical measurements" but many papers in the published literature have detailed the various biases in using continental ground surface temperature to estimate past surface air temperature changes--changing snow cover gives rise to a potentially huge bias (see e.g. : Mann, M.E., Schmidt, G.A., [5]Ground vs. Surface Air Temperature Trends: Implications for Borehole Surface Temperature Reconstructions, Geophysical Research Letters, 30 (12), 1607, doi: 10.1029/2003GL017170, 2003).

Methods that try to correct for this give smaller amplitude changes from borehole temperatures:

Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., [6]Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of

PETITIONERS' EXEMPLARS (PE)

Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003]

[[7]Correction(Rutherford and Mann, 2004)]

Most reconstructions and model estimates still "sandwich" the Mann et al reconstruction.

See e.g. figure 5 in: Jones, P.D., Mann, M.E., [8]Climate Over Past Millennia, Reviews of Geophysics, 42, RG2002, doi: 10.1029/2003RG000143, 2004.

Ironically, MM say our 15th century is too cold, while Moberg et al say its too warm.

Hmmm....

To recap, I hope you don't mention MM at all. It really doesn't deserve any additional publicity. Moberg et al is more deserving of discussion, but, as outlined above, there are some real problems w/ it. I have reason to believe that Nature's own commentary by Schiermeier will actually be somewhat critical of it.

I'm travelling and largely unavailable until monday. If you need to talk, you can

possibly reach me at 434-227-6969 begin_of_the_skype_highlighting 434-227-6969
end_of_the_skype_highlighting over the weekend.

I hope this is of some help. Literally got to run now...

mike

At 02:14 PM 2/4/2005, Andy Revkin wrote:

Hi all,

There is a fascinating paper coming in Nature next week (Moberg of Stockholm Univ., et al) that uses mix of sediment and tree ring data to get a new view of last 2,000 years.

Very warped hockeystick shaft (centuries-scale variability very large) but still pronounced 'unusual' 1990's blade.

i'd like your reaction/thoughts for story i'll write for next thursday's Times.

also, is there anything about the GRL paper forthcoming from Mc & Mc that warrants a response?

I can send you the Nature paper as pdf if you agree not to redistribute it (you know the embargo rules).

that ok?

thanks for getting in touch!

andy

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

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

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

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

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

PETITIONERS' EXEMPLARS (PE)

Charlottesville, VA 22903

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

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909 begin_of_the_skype_highlighting +44-1603-593909
end_of_the_skype_highlighting
Fax: +44-1603-507784
[12]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.realclimate.org/index.php?p=111>
2. <http://www.realclimate.org/index.php?p=114>
3. <http://www.realclimate.org/RuthetalJClim2004.pdf>
4. <ftp://holocene.evsc.virginia.edu/pub/mann/Shindellletal-jgr04.pdf>
5. <ftp://holocene.evsc.virginia.edu/pub/mann/gissgst03.pdf>
6. <ftp://holocene.evsc.virginia.edu/pub/mann/borehole-jgr03.pdf>
7. <http://holocene.evsc.virginia.edu/shared/articles/JGRBoreholeCorrection04.pdf>
8. <ftp://holocene.evsc.virginia.edu/pub/mann/JonesMannROG04.pdf>
9. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
10. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
11. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
12. <http://www.cru.uea.ac.uk/cru/people/briffa/>

PETITIONERS' EXEMPLARS (PE)

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

References

1. <http://personal.inet.fi/koti/hameranta/climate.htm>
2. <http://groups.yahoo.com/group/climatesceptics>

PETITIONERS' EXEMPLARS (PE)

PE-29

Date: Mon, 26 Feb 2001 12:04:32 +0000

From: Phil Jones <p.jones@uea.ac.uk>
To: mhughes@ltrr.arizona.edu,"Michael E. Mann" <mann@virginia.edu>
Subject: Re: Wally
Date: Mon, 26 Feb 2001 12:04:32 +0000
Cc: <mhughes@ltrr.arizona.edu>,"Michael E. Mann" <mann@virginia.edu>, tom crowley <tom@ocean.tamu.edu>, "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, <rbradley@geo.umass.edu>,<tom@ocean.tamu.edu>, <mhughes@ltrr.arizona.edu>,k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

<x-flowed>

Dear All,

I was away over the weekend at Bowdoin College in Maine, giving a talk about the last 1000 years. There were three others as well on other paleo aspects, Richard Alley, Gary Clow and Wally Broecker ! The latter briefly mentioned to me that he had had something in last Friday's Science, which was getting at the Mann et al. series. He didn't have a copy so we've not seen it here yet. I tried to get a copy of Science on the bookstand at Logan airport last night - I guess it's not sold that way !

Wally was going on about this 1500 yr cycle of Bond's, which seemed pretty flimsy. I was showing all the various series in a general talk - and I used some of the overheads from the upcoming Science paper. This is due to appear in the issue for the last week of April. It is all accepted now. I will forward if you'll all abide by the Science rules. Both Wally and Alley seem convinced that the climate of Greenland changed by 10 C in the space of 2-3 years at times in the past (Y Dryas etc). I had long talks with both and they don't seem to have got their heads around spatial scales (local changes and hemispheric). Also they don't seem to realise where we are coming from. He has a downer on trees (believes all the multiproxy series depend exclusively on trees) but he thinks Ed Cook is a great scientist. The latter is true, but he might just think that because he's at Lamont. I did tell him that Keith's paper on the age banding is out in JGR. I should send him a reprint and maybe ask that great scientist to go and explain it to him ! Ed's in NZ at the moment. Also Wally believes much more in glacier advances/retreats. I'll get Keith to send him Sarah's paper where the long Tornetrask reconstruction is shown to agree with Storglaciaren advance/retreat dates from moraine evidence. Also Sarah's been working on similar glaciers in the Swiss Alps with long tree-ring reconstructions. One interesting thing was he didn't seem to realise that a lot of the tree-ring reconstructions use density. Seemed to think they were all ring widths and there had to be moisture changes we were not accounting for.

It is easy to respond to a Perspectives piece. Some of you did it with respect to one of mine. I'm not sure it will achieve much - it won't come out before the paper in the last week of April. I need to wait to see what he says. Our paper (me, Tim and Keith) clearly says that the MWP couldn't have been warmer (for the NH average) than the late 20th century.

Another possible reason for not doing anything is that the IPCC report will be out soon. The summary is written in pretty clear language.

The above is my first thoughts, not having read the piece and just got off the flight back.

Best to ignore Wojciek. All he seems to want to do is deflect us into responding.

Cheers
Phil

At 11:47 25/02/01 -0700, mhughes@ltrr.arizona.edu wrote:
>Dear all,

PETITIONERS' EXEMPLARS (PE)

>What mechanism does "Science" have for responding to Perspective pieces? Most
>of the answer to Wally is contained within his own piece - he comments on the
>ambiguity of the record, which, in various ways, we have all done. What he
>doesn't offer, however, is anything other than an anecdotal alternative. As
>always, he seeks to damn (in this case with faint praise) the records or
>work
>that don't serve his purpose , and to elevate any scrap of evidence that does
>serve it. I think it will be important for us to stick closely to what we
>have
>written in published papers. Cheers, Malcolm
>
>Quoting "Michael E. Mann" <mann@virginia.edu>:
>
>> Dear Phil, Ray,
>>
>> What do you guys think. If we're all on board, than an appropriately
>> toned,
>> "high road" response here might be appropriate. We don't want to engage
>> Wally in a personal battle, but simply should correct the record where
>> Wally has muddied it. Again, Phil et al do have a Science article in
>> press
>> that serves this purpose to some extent, so I'm especially interested in
>> what
>> Phil thinks (Phil?)...
>>
>> mike
>>
>> At 02:52 PM 2/24/01 -0700, mhughes@lrr.arizona.edu wrote:
>> >Dear Mike et al., I think we should definitely let Wojick stew in his
>> own
>> >juice - as Mike pointed out to me the other day he, and his like, have
>> a
>> >specific agenda, and anything we write will be pressed into the service
>> of
>> that
>> >agenda. I'm not so sure about Wally. I share Tom's disinclination to
>> get
>> into a
>> >street fight with Wally - generally I take the view that life's too
>> short and
>> >uncertain for such activities. On the other hand, would we let such a
>> shoddy
>> >piece of work(and editing) go by if it were from another author? There
>> are so
>> >many holes in Wally's argument, and such a selective choice of evidence
>> that it
>> >should beggar belief. One of the more obvious holes is that he writes
>> of the
>> >Great Basin droughts of the 10th through 14th centuries as proof of
>> warmer
>> >conditions then, but doesn't explain why we don't have such conditions
>> now.
>> >Interestingly, Larry Benson, Dave Meko and others have good evidence
>> that
>> these
>> >same multidecadal periods were marked by a great excess of

PETITIONERS' EXEMPLARS (PE)

> > precipitation
> > just a
> > >few hundred miles north in northern Nevada and California and southern
> > Oregon.
> > >He just hasn't grasped that the methods that are appropriate for
> > tracking the
> > >consequences of major changes in boundary conditions don't work in the
> > late
> > >Holocene. I've been trying to figure out the issue of "Was there a
> > Medieval
> > >Warm Period, and if so where and when" for a decade or so, and still
> > have the
> > >impression that the records for the 9th through 14th centuries are
> > extremely
> > >mixed. But then, I didn't come to the investigation with a certain
> > knowledge of
> > >the absolute truth, and have had to 'misfortune' to work with people
> > who let
> > >careful analysis get in the way - Henry Diaz, Ray and Mike, and others.
> >
> > >Anyway, the point of this rant is that I think we should give careful
> > >consideration to making a measured response to Wally. Cheers, Malcolm
> > >
> > >
> > >
> > >
> > >Quoting "Michael E. Mann" <mann@virginia.edu>:
> > >
> > >> Hi Tom,
> > >>
> > >> Thanks for your quick reply. I agree with you entirely. I think its
> > very
> > >> unfortunate he's chosen to disinform the community rather than engage
> > in
> > >> a
> > >> constructive dialogue (we tried the latter w/ him in a series of
> > emails
> > >> last
> > >> year, but clearly to no avail).
> > >>
> > >> On the other hand, think that a war of words w/ Broecker would be
> > >> exploited
> > >> by the skeptics, and perhaps we should just try to let this thing
> > die...
> > >>
> > >> I'm not sure. I'd appreciate knowing what others think?
> > >>
> > >> mike
> > >>
> > >> At 10:25 AM 2/24/01 -0600, tom crowley wrote:
> > >> >Mike,
> > >> >
> > >> >I was not aware of the Broecker piece - I am dismayed but not
> > >> surprised. I
> > >> >do not know what to do - I personally cannot stand the combative
> > >> personal

PETITIONERS' EXEMPLARS (PE)

> > > >approach Broecker relishes but it does seem as if some rebuttal is
> > > called
> > > >for. Maybe you Ray Phil I and Malcolm could pen a response - we are
> > > >heading to Germany in a week, for a month, so I am not sure how much
> > I
> > > can
> > > >keep up on this but it seems as if some response is called for.
> > > >
> > > >What think ye?
> > > >
> > > >Tom
> > > >
> > > >
> > > >>Dear Mike,
> > > >>
> > > >>Thanks for passing this along.
> > > >>
> > > >>Wojick of course completely misrepresents Broecker, and puts his
> > > >>conventional intellectually dishonest spin on this.
> > > >>
> > > >>That having been said, it is a bit disappointing that Wally
> > continues
> > > to
> > > >>cling to some of his flawed beliefs which aren't supported from
> > either
> > > our
> > > >>best current understanding of the observations or of the results of
> > > careful
> > > >>modeling experiments. My own perception is that the climate
> > community,
> > > >>modelers as well as observationalists, simply don't take seriously
> > > anymore
> > > >>the idea that the history of climate change over the past 1000
> > years
> > > is
> > > >>part of an internal oscillation. The sediment core evidence oft
> > cited
> > > by
> > > >>Broecker (e.g. Bond et al) for this is tremendously weak, and I, as
> > > well as
> > > >>the vast majority of my colleagues, simply don't buy it for even a
> > > >>second.
> > > >>But people don't like to challenge Broecker publically. He can and
> > > will
> > > >>play hardball.
> > > >>
> > > >>There is an odd irony. Broecker refused to accept the modeling
> > > evidence
> > > >>that the 100 kyr ice age Pleistocene variations were part of an
> > > internal
> > > >>oscillation paced by insolation variations, favoring instead the
> > > >>discredited notion that they were a direct response to (too weak)
> > > >>eccentricity forcing, until the evidence became insurmountable
> > (from
> > > my
> > > >>adviser, Barry Saltzman, may he rest in piece, and people like Dick

PETITIONERS' EXEMPLARS (PE)

> > >>Peltier). Ironically, Broecker then took credit for the very
> > >> proposition he
> > >>had fought w/ tooth and nail.
> > >>
> > >>Broecker is even more wrong, and unfortunately equally stubborn, in
> > >> this case.
> > >>And, again, the reason: because his pet theory, that climate
> > >> variability is
> > >>a simple millennial oscillation, is finally being challenged w/
> > hard
> > >> data
> > >>and hard facts.
> > >>
> > >>Broecker misrepresents the nature of that data that we and others
> > have
> > >>used, and misunderstands the source of the muted hemispheric trends
> > >> (there
> > >>*is* a hemispheric "medieval warm period" and "little ice age",
> > just
> > >> not of
> > >>the magnitude or the distinctiveness that Broecker imagines).
> > >> Individual
> > >>regions in our reconstructions, and Phils, and others, vary by
> > several
> > >>degrees C, ie, the proxies we use have no problem whatsoever in
> > >> resolving
> > >>high-amplitude temperature variations in the past. The problem is
> > that
> > >> when
> > >>we look at the different regions we find that periods of cold and
> > >> warm
> > >>often occur at very different times in different regions, and so in
> > a
> > >>hemispheric or global average, a lot of purely regional variability
> > >> cancels
> > >>out. The resulting trends are somewhat smaller. I remained
> > befuddled
> > >> as to
> > >>why Wally doesn't understand this point. Its been explained to him
> > >> time and
> > >>time again. Maybe he's just not listening, or doesn't want to
> > >> listen...
> > >>
> > >>In fact, Tom Crowley has clearly shown that the observed millennial
> > >>temperature reconstruction is precisely consistent w/ our
> > >> understanding of
> > >>*forced* climate change over the past 1000 years (solar changes,
> > >> volcanic
> > >>output, and recent greenhouse gas concentrations). There is, simply
> > >> put, no
> > >>room for a global millennial internal oscillation. Regionally, such
> > >> types
> > >>of climate phenomena, associated for example with changes in the
> > North
> > >>Atlantic ocean circulation, are supported by the observations. This
> > >>explains why, for example, European temperature variations are

PETITIONERS' EXEMPLARS (PE)

> > > somewhat
> > > >larger than those in other regions not effected so strongly by such
> > > climate
> > > >processes.
> > > >
> > > >Other recent perspectives, by Ray Bradley and myself provide a far
> > > more
> > > >balanced and nuanced (and less dogmatic or defensive) viewpoint.
> > I'm
> > > not
> > > >sure a written response to Broecker is worthwhile (this is,
> > afterall,
> > > a
> > > >"perspective" and everyone understands that a scientist may have a
> > > flawed
> > > >perspective). If Wally wants this to be his legacy, so be it...
> > > >
> > > >Phil and others have a review article coming out in the near future
> > > which
> > > >also provides a much more balanced perspective on the climate
> > changes
> > > of
> > > >the past millennium, and will set the record straight once again
> > (good
> > > >timing Phil!). Science's embargo policy prevents me from saying
> > much
> > > more
> > > >at this time, but if Phil or anyone else wishes to comment further,
> > > I'd
> > > >encourage it.
> > > >
> > > >Well, I've still got some snow to shovel here in Charlottesville!
> > > Happy
> > > >weekend to all,
> > > >
> > > >mike
> > > >
> > > >p.s. For those with electronic subscriptions, Broecker's latest
> > piece
> > > can
> > > >be found here:
> > > >
> > > > PALEOCLIMATE:
> > > > Was the Medieval Warm Period Global?
> > > > Wallace S. Broecker
> > > > Science Feb 23 2001: 1497-1499. [Summary] [Full Text]
> > > >
> > > ><http://www.sciencemag.org/cgi/content/full/291/5508/1497>
> > > >
> > > >While my previous perspective piece is here:
> > > > CLIMATE CHANGE:
> > > > Lessons for a New Millennium
> > > > Michael E. Mann
> > > > Science 2000 July 14; 289: 253-254. (in Perspectives) [Summary]
> > > > [Full
> > > >Text]

PETITIONERS' EXEMPLARS (PE)

> > > >>URL:
> > >
> > >>http://www.sciencemag.org/cgi/content/full/289/5477/253?maxtoshow=&HITS=10&hits=10&RESULTFORMAT=&author1=Mann&searchid=QID_NOT_SET&stored_search=&FIRSTINDEX=&fdate=10/1/1995&tdate=2/28/2001
> > >>
> > >>and Bradley's is here:
> > >>
> > >>> PALEOCLIMATE: Enhanced: 1000 Years of Climate Change
> > >>> Ray Bradley
> > >>> Science 2000 May 26; 288: 1353-1355. (in Perspectives) [Summary]
> > >> [Full
> > >>>Text]
> > >>>
> > >>>URL:
> > >>
> > >>>http://www.sciencemag.org/cgi/content/full/288/5470/1353?maxtoshow=&HITS=10&hits=10&RESULTFORMAT=&author1=Bradley&searchid=QID_NOT_SET&stored_search=&FIRSTINDEX=&fdate=10/1/1995&tdate=2/28/2001
> > >>>
> > >>>>Dear Michael--The third point below has comments on the
> > >>> controversy
> > >>>>betweenyou and Broecker--I'd be interested in your response (did
> > >>> Wally not
> > >>>>just understand what your data show?).
> > >>>>
> > >>>>Mike
> > >>>>
> > >>>>>Three Wojick Pieces on Climate Change.
> > >>>>>I've been busy busy.
> > >>>>>
> > >>>>>David
> > >>>>>
> > >>>>>>FIRST, the latest issue of Insight Magazine includes a
> > >>>>>> point-counterpoint
> > >>>>>>>between measly old me and the great Robert Watson. Boy has he got
> > >>>>>>>credentials! Too bad he's wrong.
> > >>>>>>>
> > >>>>>>><<http://www.insightmag.com/archive/200103143.shtml>>
> > >>>>>>>
> > >>>>>>>>Symposium: Do scientists have compelling evidence of global
> > >>>>>>>>warming?
> > >>>>>>>>
> > >>>>>>>>>Yes: Rising sea levels worldwide and retreating Arctic glaciers
> > >>>>>>>>>are
> > >>>>>>>>>ominous
> > >>>>>>>>>>signs.
> > >>>>>>>>>>
> > >>>>>>>>>>>By Robert T. Watson -- chairman of the UN Intergovernmental Panel
> > >>>>>>>>>>>on

PETITIONERS' EXEMPLARS (PE)

> > >>>Climate Change, chief scientist at the World Bank and former chief
> > >> science
> > >>>advisor to the Clinton White House.
> > >>>
> > >>>No: Despite the overheated rhetoric, there is no new evidence of
> > >> warming
> > >>>
> > >>>By David E. Wojick -- covers climate policy for Electricity Daily
> > and
> > >> is a
> > >>>science adviser to the Greening Earth Society
> > >>><<http://www.greeningearthsociety.org>>, as well as Undereditor of
> > the
> > >>>Washington Pest <<http://www.WashingtonPest.com>>
> > >>>
> > >>>SECOND, the February 15 Eco-logic on-line has published "The Black
> > >> Hole of
> > >>>Global Climate Government" by David Wojick, my detailed attack on
> > the
> > >>>Framework Convention on Climate Change. It includes a lot of the
> > >> actual
> > >>>treaty language.
> > >>>
> > >>><<http://www.eco.freedom.org/el/20010202/wojick.shtml>>
> > >>>
> > >>>THIRD, here is a draft Electricity Daily article of mine. Seems
> > I'm
> > >> not the
> > >>>only one who thinks the IPCC is nuts.
> > >>>
> > >>>Climate Guru Kicks The Hockey Stick
> > >>>by David Wojick (dwojick@shentel.net)
> > >>>
> > >>>Global warming is natural and the recent warming is probably no
> > >> exception.
> > >>>This is the controversial argument made by prominent climatologist
> > >> Wallace
> > >>>S. Broecker in today's issue of Science.
> > >>>
> > >>>Broecker's bombshell bears the seemingly innocent title "Was the
> > >> Medieval
> > >>>Warm Period Global?" It may seem esoteric, but whether the
> > apparent
> > >> warmth
> > >>>reported in Europe about 1000 years ago was global or simply local
> > is
> > >>>becoming a central issue in climate science. What makes it
> > >> contentious is
> > >>>the recent claims by the United Nations Intergovernmental Panel on
> > >> Climate
> > >>>Change that the earth is warmer now than it has been for
> > millennia,
> > >> and
> > >>>that therefore human carbon dioxide emissions are to blame.
> > Broecker,
> > >> a

PETITIONERS' EXEMPLARS (PE)

> > > >>>leading figure at Lamont-Doherty Earth Observatory, Columbia
> > > University,
> > > >>>questions both IPCC claims.
> > > >>>
> > > >>>The focus of the debate is a 1000-year temperature reconstruction
> > > known in
> > > >>>climate circles as the "hockey stick". Produced in 1999 by M. E.
> > > Mann, R.
> > > >>>S. Bradley, M. K. Hughes, the long handle of the hockey stick
> > shows
> > > global
> > > >>>temperatures for the first 8 centuries as basically unchanging,
> > > followed by
> > > >>>the sharply up-tilting blade of the last 150 years or so. The Mann
> > et
> > > al
> > > >>>hockey stick is the central feature of the recently released IPCC
> > > working
> > > >>>group one Summary for Policy makers, which claims to embody the
> > best
> > > of
> > > >>>climate science.
> > > >>>
> > > >>>Broecker does not like the hockey stick, nor the conclusions the
> > IPCC
> > > draws
> > > >>>from it. He says " A recent, widely cited reconstruction (Mann's)
> > > leaves
> > > >>>the impression that the 20th century warming was unique during the
> > > last
> > > >>>millennium. It shows no hint of the Medieval Warm Period (from
> > around
> > > 800
> > > >>>to 1200 A.D.) during which the Vikings colonized Greenland,
> > > suggesting that
> > > >>>this warm event was regional rather than global. It also remains
> > > unclear
> > > >>>why just at the dawn of the Industrial Revolution and before the
> > > emission
> > > >>>of substantial amounts of anthropogenic greenhouse gases, Earth's
> > > >>>temperature began to rise steeply. Was it a coincidence? I do not
> > > think so.
> > > >>>Rather, I suspect that the post-1860 natural warming was the most
> > > recent in
> > > >>>a series of similar warmings spaced at roughly 1500-year intervals
> > > >>>throughout the present inter-glacial, the Holocene."
> > > >>>
> > > >>>Broecker presents the evidence for a global Medieval Warm Period,
> > as
> > > well
> > > >>>as for a Little Ice Age from around 1300 to 1860, when the present
> > > >>>temperature rise begins. He also argues that the "proxy" evidence
> > > used by
> > > >>>Mann et al, such as tree ring data, is ill suited to the time
> > period
> > > and

PETITIONERS' EXEMPLARS (PE)

> > > > e-mail: mann@virginia.edu Phone: (804) 924-7770 begin_of_the_skype_highlighting
(804) 924-7770 end_of_the_skype_highlighting FAX: (804)
> > > 982-2137

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

> > > >
> > > >
> > > >
> > > >
> > > >

> > > > Thomas J. Crowley
> > > > Dept. of Oceanography
> > > > Texas A&M University
> > > > College Station, TX 77843-3146
> > > > 979-845-0795 begin_of_the_skype_highlighting 979-845-0795
end_of_the_skype_highlighting
> > > > 979-847-8879 (fax)
> > > > 979-845-6331 (alternate fax)

> > > >
> > > >
> > > >
> > > >
> > > >

> > > > _____
> > > > 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 begin_of_the_skype_highlighting (804)
924-7770 end_of_the_skype_highlighting FAX: (804)
> > 982-2137

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

> > > >
> > > >
> > > >
> > > >
> > > >
> > > >
> > > >
> > > >
> > > >

> > > > _____
> > > > 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 begin_of_the_skype_highlighting (804)
924-7770 end_of_the_skype_highlighting FAX: (804) 982-2137
> > <http://www.evsc.virginia.edu/faculty/people/mann.html>

> > > >
> > > >
> > > >

Prof. Phil Jones

PETITIONERS' EXEMPLARS (PE)

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

</x-flowed>

PETITIONERS' EXEMPLARS (PE)

PE-30

Date: Tue May 20 14:57:55 2003

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

Mike

Lennart has managed to confuse me with his latest message. At one point he mentioned that you and I would do a joint overview paper . Now he suggests we choose 5-10 co-authors but also refers to "other people in our section" who he has apparently already informed , need "to consult with you (ie us) as required" (my emphasis).

As for my opinion of the theme or content of our section , I suggest it be "quantifying Natural and Anthropogenic influences on the course of Global climate during recent millennia" or some such . This allows for the review , redefinition of Global climate history (Southern as well as Northern , and moisture as well as Temperature). Importantly , it also incorporates the issue of forcing history(ies) and work quantifying the influence of these histories - using simple empirical techniques or using them in conjunction with models of different complexity to attribute causes of this change.

I am happy to go with the "usual suspects" in the overview paper , but would be happy if we considered others who are also running controlled model/data comparisons (examples are Von Storch , Simon Tett , Caspar Ammann). We need first to clarify whether we will present one large , multi-author presentation/paper or whether it is just me and you and the others divided into other papers/presentations/posters. Should we copy this message to Lennart or contact him directly with specific questions?

Keith

At 09:49 PM 5/18/03 -0400, you wrote:

Hi Keith,

I hope all is well.

Apparently, we're supposed to choose 5-10 additional "co-authors"? I guess the obvious ones would be Phil, Tim, Ray, Malcolm, perhaps Ed Cook, Scott Rutherford,...any other suggestions?

As I understand it, the co-authors would be invited to attend and present in the poster session; I assume they are listed separately from you and I who will jointly present the oral overview. As for the theme, I'm assuming "climate changes of the past couple/few millennia" or something like that. As we have 45 minutes total between the two of us, I would suggest we each take about 20 minutes, and then we'll have 5 minutes left for questions.

Any suggestions, thoughts would be greatly appreciated.

thanks,

mike

X-Sender: m214001@regen.dkrz.de
Date: Sun, 18 May 2003 22:53:58 +0200
To: k.briffa@uea.ac.uk, mann@virginia.edu
From: "Prof. Dr. Lennart Bengtsson" <bengtsson@dkrz.de>
Subject: Clivar Conference 2004
Cc: bengtsson@dkrz.de, kornelia.mueller@dkrz.de

--

Dear Dr. Mann,

Dear Dr. Briffa,

The preparation of the Clivar conference is progressing well and all invited speakers have now agreed (See attached draft program). As I have informed you previously Journal of Climate will have a special issue devoted to the Conference and I expect you would be willing to prepare a paper to be ready at the time of the conference. I have made

PETITIONERS' EXEMPLARS (PE)

arrangements with the chief editor to make a flexible interpretation of the content of the papers so to agree with the objective of the conference and the draft program. We would now like you to come up with a suitable theme for your presentation at the conference as well a list of names which you have selected as co-authors. As we anticipate a broad and forward-looking contribution I believe some 5-10 people seems appropriate. It was our intention that the first person listed should be the lead author but you can arrange this otherwise if you prefer to do so. I have informed the other speakers in your section to consult with you as required.

For the conference I expect a rather wide audience in addition to a broad scientific community including representatives from different agencies such as the meteorological services, as well as media representatives. For the media we intend to provide a special set of information. In view of the societal importance of the CLIVAR program and the considerable progress in extended range forecasts and climate change assessment and prediction I believe there will be an excellent opportunity to bring the scientific progress and associated applications of CLIVAR to the participants of the conference. It would be very helpful if you could to let me know the status of your arrangements not later than June 15. If you see any particular difficulties please let me know as soon as possible.

As you can see from the attached program each part of the conference will have poster sessions. The poster sessions will be an important part of the conference and I anticipate that some of your co-authors will prepare such posters. We also plan to have the poster contents on a CD ROM prior to the conference.

The practical planning of the conference as a whole is proceeding well. The arrangements in Baltimore are quite excellent with the nearby Baltimore inner harbor as a particular attractive focal point. There are all reasons that the conference will be a success both scientifically and socially. See further the Clivar Conference website:

[1]<http://www.clivar2004.org>.

We are presently exploring the possibilities for financial support of selected participants. However, any support you may manage to obtain from national funds would be most helpful.

With my very best regards

Lennart Bengtsson

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

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

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909 [begin_of_the_skype_highlighting](#) +44-1603-593909
[end_of_the_skype_highlighting](#)
Fax: +44-1603-507784
[3][http://www.cru.uea.ac.uk/cru/people/briffa\[4\]/](http://www.cru.uea.ac.uk/cru/people/briffa[4]/)

References

PETITIONERS' EXEMPLARS (PE)

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

PETITIONERS' EXEMPLARS (PE)

PE-31

Date: Tue Aug 10 15:47:04 2004

From: Phil Jones <p.jones@uea.ac.uk>
To: Gabi Hegerl <hegerl@duke.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Mann and Jones (2003)
Date: Tue Aug 10 15:47:04 2004
Cc: Tom Crowley <tcrowley@duke.edu>

Gabi,

No second attempt - don't know what the first was? We'll be doing a new instrumental data

set (surprisingly called HadCRUT3), but that's it at the moment.

Attached is a good review of corals - just out.

Cheers

Phil

At 10:36 10/08/2004 -0400, Gabi Hegerl wrote:

Hi Mike and Phil,

Thanks! Yes, factor 1.29 will get me closer to my best guess scaling (factor 1.6 to same-size signals).

The scaling is a tough issue, and I think there are lots of possibilities to do it depending on what one wants

to do. For comparing underlying forced signals, I think t1s is best. To get a conservative size paleo reconstruction

(like what part of instrumental do we reconstruct with paleo), the traditional scaling is best.

I'll write up what Myles and I have been thinking and send it.

Phil, if there is a second attempt at that with the Hadley Centre, let me know, I don't like racing anybody!

Gabi

Michael E. Mann wrote:

Dear Phil and Gabi,

I've attached a cleaned-up and commented version of the matlab code that I wrote for doing the Mann and Jones (2003) composites. I did this knowing that Phil and I are likely to have to respond to more crap criticisms from the idiots in the near future, so best to clean up the code and provide to some of my close colleagues in case they want to test it, etc. Please feel free to use this code for your own internal purposes, but don't pass it along where it may get into the hands of the wrong people.

In the process of trying to clean it up, I realized I had something a bit odd, not necessarily wrong, but it makes a small difference. It seems that I used the 'long' NH instrumental series back to 1753 that we calculated in the following paper:

* Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., [1]Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.

(based on the sparse available long instrumental records) to set the scale for the decadal standard deviation of the proxy composite. Not sure why I used this, rather than using the CRU NH record back to 1856 for this purpose. It looks like I had two similarly named series floating around in the code, and used perhaps the less preferable one for setting the scale.

Turns it, this has the net effect of decreasing the amplitude of the NH reconstruction by a factor of $0.11/0.14 = 1.29$.

This may explain part of what perplexed Gabi when she was comparing w/ the instrumental series. I've attached the version of the reconstruction where the NH is scaled by the CRU NH record instead, as well as the Matlab code which you're welcome to try to use

PETITIONERS' EXEMPLARS (PE)

yourself and play around with. Basically, this increases the amplitude of the reconstruction everywhere by the factor 1.29. Perhaps this is more in line w/ what Gabi was estimating (Gabi?)

Anyway, doesn't make a major difference, but you might want to take this into account in any further use of the Mann and Jones series...

Phil: is this worth a followup note to GRL, w/ a link to the Matlab code?

Mike

p.s. Gabi: when do you and Tom plan to publish your NH reconstruction that now goes back about 1500 years or so? It would be nice to have more independent reconstructions published in the near future! Maybe I missed this? Thanks...

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

e-mail: [2]mann@virginia.edu Phone: (434) 924-7770 begin_of_the_skype_highlighting (434)
924-7770 end_of_the_skype_highlighting FAX: (434) 982-2137
[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

```
% COMPOSITENH"  
%  
% (c) 2003, M.E. Mann  
%  
% THIS ROUTINE PERFORMS A RECONSTRUCTION OF NORTHERN HEMISPHERE  
% MEAN ANNUAL TEMPERATURE BASED ON A WEIGHTED COMPOSITE OF LONG-TERM  
% TEMPERATURE  
% PROXY RECORDS SCALED AGAINST THE INSTRUMENTAL HEMISPHERIC MEAN  
% TEMPERATURE  
% SERIES, AS USED IN THE FOLLOWING TWO PUBLICATIONS:  
%  
%  
% Jones, P.D., Mann, M.E., Climate Over Past Millennia, Reviews of Geophysics,  
% 42, RG2002, doi:10.1029/2003RG000143, 2004  
%  
% Mann, M.E., Jones, P.D., Global Surface Temperatures over the Past two Millennia,  
% Geophysical Research Letters,  
% 30 (15), 1820, doi: 10.1029/2003GL017814, 2003  
%  
%  
% 1. READ IN INSTRUMENTAL RECORD  
%  
% Read in CRU instrumental NH mean temperature record (1856-2003)  
load nh.dat;  
yearinstr=nh(:,1);  
% calculate both warm-season and annual means  
warmseason=(nh(:,5)+nh(:,6)+nh(:,7)+nh(:,8)+nh(:,9)+nh(:,10))/6;  
annualmean=nh(:,14);  
% use annual mean record in this analysis  
nhmean=annualmean;  
%  
% 2. READ IN PREVIOUSLY PUBLISHED PROXY-RECONSTRUCTIONS OF NH ANNUAL MEAN  
% RECONSTRUCTIONS AND FORM APPROPRIATELY SCALED COMPOSITE  
%  
% Read in Mann et al (1998), Crowley and Lowery (2000), and Jones et al (1998)
```

PETITIONERS' EXEMPLARS (PE)

```
% NH temperature reconstructions
load nhem-millennium.dat;
load crowleylowery.dat;
load joneshemisrecons.dat;
nhmbh=nhem_millennium(1:981,2);
nhjones=joneshemisrecons(1:981,2);
nhcl=crowleylowery(1:981,2);
yearmillen=nhem_millennium(1:981,1);
% since some reconstructions are only decadal resolved, smooth each on
% decadal timescales through use of a lowpass filter with cutoff at
% f=0.1 cycle/year. Based on use of the filtering routine described in:
%
% Mann, M.E., On Smoothing Potentially Non-Stationary Climate Time Series,
% Geophysical Research Letters, 31, L07214, doi: 10.1029/2004GL019569, 2004.
%
% using 'minimum norm' constraint at both boundaries for all time series
nhsmooth=lowpass(nhmean,0.10,0,0);
nhmbhsmooth=lowpass(nhmbh,0.10,0,0);
nhjonessmooth=lowpass(nhjones,0.10,0,0);
nhclsmooth=lowpass(nhcl,0.10,0,0);
% Mann et al (1998) already calibrated in terms of hemispheric annual mean temperature, but
% reference mean has to be adjusted to equal that of the instrumental series
% over the 1856-1980 overlap period (which uses a 1961-1990 reference period)
admbh=mean(nhsmooth(1:125))-mean(nhmbhsmooth(857:981));
newmbh=nhmbhsmooth+admbh;
% need to adjust and scale Jones et al (1998) and Crowley and Lowery (2000)
% reconstructions to match mean and trend of smoothed instrumental series
% over 1856-1980
t1=1856;
t2=1980;
x=(t1:t2)';
nhlong=nhmean(1:125);
smoothlong=lowpass(nhlong,0.10,0,0);
amean0=mean(smoothlong);
y=smoothlong;
[yc,t,trend0,detrend0,xm,ym] = lintrend(x, y);
%
y=nhclsmooth(t1-999:t2-999);
[yc,t,trendcl,detrendcl,xm,ym] = lintrend(x, y);
%
y=nhjonessmooth(t1-999:t2-999);
[yc,t,trendjones,detrendjones,xm,ym] = lintrend(x, y);
%
multjones=norm(trend0)/norm(trendjones);
adjustedjones=nhjonessmooth*multjones;
offsetjones=amean0-mean(adjustedjones(t1-999:t2-999));
newjones=adjustedjones+offsetjones;
newjones=newjones';
%
multcl=norm(trend0)/norm(trendcl);
adjustedcl=nhclsmooth*multcl;
offsetcl=amean0-mean(adjustedcl(t1-999:t2-999));
newcl=adjustedcl+offsetcl;
newcl=newcl';
%
nhlongcompose=0.3333*(newmbh+newjones'+newcl)';
```

PETITIONERS' EXEMPLARS (PE)

```
%
% 3. READ IN AND PROCESS PROXY TEMPERATURE RECORDS
%
M=8;
load 'china-series1.dat'
load 'itrdb-long-fixed.dat'
load 'westgreen-o18.dat'
load 'torny.dat'
load 'chesapeake.dat'
load 'mongolia-darrigo.dat'
load 'dahl-jensen-gripbh1yrinterp.txt'
load 'dahl-jensen-dye3bh1yrinterp.txt'
% read in years
x1=china_series1(:,1);
x2=itrdb_long_fixed(:,1);
x3=westgreen_o18(:,1);
x4=torny(:,1);
x5=chesapeake(:,1);
x6=mongolia_darrigo(:,1);
x7=dahl_jensen_gripbh1yrinterp(:,1);
x8=dahl_jensen_dye3bh1yrinterp(:,1);
% read in proxy values
y1=china_series1(:,2);
y2=itrdb_long_fixed(:,2);
y3=westgreen_o18(:,2);
y4=torny(:,2);
y5=chesapeake(:,2);
y6=mongolia_darrigo(:,2);
y7=dahl_jensen_gripbh1yrinterp(:,2);
y8=dahl_jensen_dye3bh1yrinterp(:,2);
% Store decadal correlation of each proxy record with local available
% overlapping CRU gridpoint surface temperature record (see Mann and Jones, 2003)
corr(1)=0.22;
corr(2)=0.52;
corr(3)=0.75;
corr(4)=0.32;
corr(5)=0.31;
corr(6)=0.40;
corr(7)=0.53;
corr(8)=0.52;
% Estimate Area represented by each proxy record based on latitude of
% record and estimated number of temperature gridpoints represented by record
pi=3.14159;
factor=pi/180.0;
lat(1)=32.5;
dof(1)=4;
lat(2)=37.5;
dof(2)=2;
lat(3)=77;
dof(3)=0.667;
lat(4)=68;
dof(4)=3.5;
lat(5)=37.0;
dof(5)=1.0;
lat(6)=47;
dof(6)=1;
```

PETITIONERS' EXEMPLARS (PE)

```
lat(7)=73;
dof(7)=0.667;
lat(8)=65;
dof(8)=0.667;
for j=1:M
    area(j)=dof(j)*cos(lat(j)*factor);
end
% determine min and max available years over all proxy records
%
minarray=[min(x1) min(x2) min(x3) min(x4) min(x5) min(x6) min(x7) min(x8)];
maxarray=[max(x1) max(x2) max(x3) max(x4) max(x5) max(x6) max(x7) max(x8)];
tbegin=max(minarray);
tend1=min(maxarray);
tend=max(maxarray);
% initialize proxy data matrix
notnumber = -9999;
for j=1:M
for i=1:minarray(j)-1
    time(i)=i;
    mat(i,j)=notnumber;
end
for i=minarray(j):tend
    time(i)=i;
end
for i=minarray(j):maxarray(j)
    if (j==1) mat(i,j)=y1(i-minarray(j)+1);
    end
    if (j==2) mat(i,j)=y2(i-minarray(j)+1);
    end
    if (j==3) mat(i,j)=y3(i-minarray(j)+1);
    end
    if (j==4) mat(i,j)=y4(i-minarray(j)+1);
    end
    if (j==5) mat(i,j)=y5(i-minarray(j)+1);
    end
    if (j==6) mat(i,j)=y6(i-minarray(j)+1);
    end
    if (j==7) mat(i,j)=y7(i-minarray(j)+1);
    end
    if (j==8) mat(i,j)=y8(i-minarray(j)+1);
    end
end
% added in Jones and Mann (2004), extend series ending between
% 1980 calibration period end and 2001 boundary by persistence of
% last available value through 2001
for i=maxarray(j)+1:tend
    if (j==1) mat(i,j)=y1(maxarray(j)-minarray(j)+1);
    end
    if (j==2) mat(i,j)=y2(maxarray(j)-minarray(j)+1);
    end
    if (j==3) mat(i,j)=y3(maxarray(j)-minarray(j)+1);
    end
    if (j==4) mat(i,j)=y4(maxarray(j)-minarray(j)+1);
    end
    if (j==5) mat(i,j)=y5(maxarray(j)-minarray(j)+1);
    end
end
```

PETITIONERS' EXEMPLARS (PE)

```
if (j==6) mat(i,j)=y6(maxarray(j)-minarray(j)+1);
end
if (j==7) mat(i,j)=y7(maxarray(j)-minarray(j)+1);
end
if (j==8) mat(i,j)=y8(maxarray(j)-minarray(j)+1);
end
end
end
time=time';
data=[time mat];
% decadally lowpass of proxy series at f=0.1 cycle/year as described earlier
for j=1:M
    unfiltered=mat(minarray(j):tend,j);
    filt=lowpass(unfiltered,0.1,0,0);
    for i=1:minarray(j)-1
        filtered(i,j)=mat(i,j);
    end
    for i=minarray(j):tend
        filtered(i,j)=filt(i-minarray(j)+1);
    end
end
% standardize data
% first remove mean from each series
for j=1:M
    icount=0;
    amean(j)=0;
    for i=1:tend
        if (filtered(i,j)>notnumber)
            icount=icount+1;
            amean(j)=amean(j)+filtered(i,j);
        end
    end
    amean(j)=amean(j)/icount;
end
% now divide through by standard deviation
for j=1:M
    icount=0;
    asum=0;
    for i=1:tend
        if (filtered(i,j)>notnumber)
            asum=asum+(filtered(i,j)-amean(j))^2;
            icount=icount+1;
        end
    end
    sd(j)=sqrt(asum/icount);
    for i=1:tend
        standardized(i,j)=filtered(i,j);
        if (mat(i,j)>notnumber)
            standardized(i,j)=(filtered(i,j)-amean(j))/sd(j);
        end
    end
end
end
%
% 4. Calculate NH mean temperature reconstruction through weighted (and
% unweighted) composites of the decadally-smoothed proxy indicators
%
```

PETITIONERS' EXEMPLARS (PE)

```
% impose weighting scheme for NH mean composite
for j=1:M
% weighting method 1: weight each proxy series by approximate area
% weighting method 2: weight each proxy series by correlation between
% predictor and local gridpoint series over available overlap period
% during calibration interval
% weighting method 3: weight each proxy series by correlation between
% predictor and NH mean series over calibration interval:
% weightlong(j)=lincor(nhlong,standardized(1856:1980,j));
% weighting method 4: combine 1 and 3
% weighting method 5: combine 1 and 2 (this is the 'standard' weighting
% scheme chosen by Mann and Jones (2003)
% use standard weighting scheme
    weight(j)=corr(j)*area(j);
end
% perform reconstructions based on:
% (1) the 6 proxy temperature records available over interval AD 200-1980
% (2) all 8 proxy temperature records available over interval AD 553-1980
istart0=200;
istart1=200;
istart2=553;
nseries1=0;
nseries2=0;
weightsum1=0;
weightsum2=0;
for j=1:M
    if (istart1>=minarray(j))
        nseries1=nseries1+1;
        weightsum1=weightsum1+weight(j);
    end
    if (istart2>=minarray(j))
        nseries2=nseries2+1;
        weightsum2=weightsum2+weight(j);
    end
end
% calculate composites through 1995 (too few series available after that date)
% As discussed above, persistence is used to extend any series ending
% between 1980 and 1995 as described by Jones and Mann (2004).
tend=1995;
for i=istart1:tend
    unweighted1(i)=0;
    unweighted2(i)=0;
    weighted1(i)=0;
    weighted2(i)=0;
    for j=1:M
        if (istart1>=minarray(j))
            unweighted1(i)=unweighted1(i)+standardized(i,j);
            weighted1(i)=weighted1(i)+weight(j)*standardized(i,j);
        end
        if (istart2>=minarray(j))
            unweighted2(i)=unweighted2(i)+standardized(i,j);
            weighted2(i)=weighted2(i)+weight(j)*standardized(i,j);
        end
    end
end
end
unweighted1=unweighted1/nseries1;
```

PETITIONERS' EXEMPLARS (PE)

```
unweighted2=unweighted2/nseries2;
weighted1=weighted1/weightsum1;
weighted2=weighted2/weightsum2;
unweighted1(1:istart1-1)=0;
unweighted2(1:istart2-1)=0;
weighted1(1:istart1-1)=0;
weighted2(1:istart2-1)=0;
% scale composite to have same variance as decadal-smoothed instrumental
% NH series

% Mann and Jones (2003) and Jones and Mann (2004) used for this purpose
% the extended (1753-1980) NH series used in:
% Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T.,
% Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data,
% Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.
% That series has a decadal standard deviation sd=0.1123
% If instead, the 1856-2003 CRU instrumental NH mean record is used, with
% a decadal standard deviation of sd=0.1446, the amplitude of the reconstruction
% increases by a factor 1.29 (this scaling yields slightly lower verification
% scores)
load nhem-long.dat
nhemlong=nhem_long(:,2);
longsmooth=lowpass(nhemlong,0.10,0,0);
sd0=std(longsmooth);
% use weighted (rather than unweighted) composite in this case
series1=weighted1;
% center composites on 1856-1980 calibration period
y=series1(t1:t2)';
amean1=mean(series1(t1:t2));
compseries1=series1(t1:t2)-amean1;
mult1=sd0/std(compseries1);
% scale composite to standard deviation of instrumental series and re-center
% to have same (1961-1990) zero reference period as CRU NH instrumental
% temperature record
adjusted1=series1*mult1;
offset1=amean0-mean(adjusted1(t1:t2));
compose1=adjusted1+offset1;
compose1=compose1';
series2=weighted2;
y=series2(t1:t2)';
amean2=mean(series2(t1:t2));
compseries2=series2(t1:t2)-amean2;
mult2=sd0/std(compseries2);
adjusted2=series2*mult2;
offset2=amean0-mean(adjusted2(t1:t2));
compose2=adjusted2+offset2;
compose2=compose2';
%
% 5. UNCERTAINTY ESTIMATION, AND STATISTICAL VERIFICATION
%
% estimate uncertainty in reconstruction
% nominal (white noise) unresolved calibration period variance
calibvar=lincor(smoothlong,compose1(t1:t2))^2;
uncalib=1-calibvar;
sdunc=sd0*sqrt(uncalib);
% note: this is the *nominal* white noise uncertainty in the reconstruction
```

PETITIONERS' EXEMPLARS (PE)

```
% a spectral analysis of the calibration residuals [as discussed briefly in
% Mann and Jones, 2003] indicates that a peak at the multidecadal timescale
% that exceeds the white noise average residual variance by a factor of
% approximately 6. A conservative estimate of the standard error in the
% reconstruction thus inflates the nominal white noise estimate "sdunc" by a
% factor of sqrt(6)
sdlow = sdunc*sqrt(6)
% calculate long-term verification statistics for reconstruction
% use composite of Mann et al (1998)/Crowley and Lowery (2000)/Jones et al (1998)
% and AD 1600-1855 interval
overlapcomp=nhlongcompose(1:981);
% work with longer reconstruction (back to AD 200)
overlaprecon=compose1(1000:1980)';
%overlaprecon=compose2(1000:1980)';
%calculate verification R^2
series11=overlaprecon(601:856);
series22=overlapcomp(601:856);
verifrsq=lincor(series11,series22)^2
% calculate verification RE
var1=0.0;
var2=0.0;
var3=0.0;
var4=0.0;
var5=0.0;
am0=0.0;
% insure convention of zero mean over calibration interval
for i=857:981
    am0=am0+overlapcomp(i);
end
am0=am0/125;
for i=601:856
    var1=var1+(overlapcomp(i)-am0)^2;
    var2=var2+(overlapcomp(i)-overlaprecon(i))^2;
end
verifRE=1-var2/var1
```

--

~~~~~  
Gabriele Hegerl  
Division of Earth and Ocean Sciences,  
Nicholas School for the Environment and Earth Sciences,  
Box 90227  
Duke University, Durham NC 27708  
Ph: 919 684 6167 begin\_of\_the\_skype\_highlighting 919 684 6167  
end\_of\_the\_skype\_highlighting, fax 684 5833  
email: [4]hegerl@duke.edu, [5]http://www.env.duke.edu/faculty/bios/hegerl.html

## PETITIONERS' EXEMPLARS (PE)

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

### References

1. <ftp://holocene.evsc.virginia.edu/pub/mann/borehole-jgr03.pdf>
2. <mailto:mann@virginia.edu>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <mailto:hegerl@duke.edu>
5. <http://www.env.duke.edu/faculty/bios/hegerl.html>

## PETITIONERS' EXEMPLARS (PE)

PE-32

Date: Wed Sep 22 16:19:06 1999

---

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>  
Subject: RE: IPCC revisions  
Date: Wed Sep 22 16:19:06 1999  
Cc: tkarl@ncdc.noaa.gov

Hi everyone

Let me say that I don't mind what you put in the policy makers summary if there is a general consensus. However some general discussion would be valuable. First, like Phil, I think that the supposed separation of the tree-ring reconstruction from the others on the grounds that it is not a true "multi-proxy" series is hard to justify. What is true is that these particular tree-ring data best represent SUMMER temperatures mostly at the northern boreal forest regions. By virtue of this, they also definitely share significant variance with Northern Hemisphere land and land and marine ANNUAL temperatures - but at decadal and multidecadal timescales - simply by virtue of the fact that these series correlated with the former at these timescales. The multi proxy series (Mann et al. Jones et al) supposedly represent annual and summer seasons respectively, and both contain large proportions of tree-ring input. The latest tree-ring density curve (i.e. our data that have been processed to retain low frequency information) shows more similarity to the other two series- as do a number of other lower resolution data (Bradley et al, Peck et al., and new Crowley series - see our recent Science piece) whether this represents 'TRUTH' however is a difficult problem. I know Mike thinks his series is the 'best' and he might be right - but he may also be too dismissive of other data and possibly over confident in his (or should I say his use of other's). After all, the early (pre-instrumental) data are much less reliable as indicators of global temperature than is apparent in modern calibrations that include them and when we don't know the precise role of particular proxies in the earlier portions of reconstruction it remains problematic to assign genuine confidence limits at multidecadal and longer timescales. I still contend that multiple regression against the recent very trendy global mean series is potentially dangerous. You could calibrate the proxies to any number of seasons, regardless of their true optimum response. Not for a moment am I saying that the tree-ring, or any other proxy data, are better than Mike's series - indeed I am saying that the various reconstructions are not independent but that they likely contribute more information about reality together than they do alone. I do believe, that it should not be taken as read that Mike's series (or Jones et al. for that matter) is THE CORRECT ONE. I prefer a Figure that shows a multitude of reconstructions (e.g similar to that in my Science piece). Incidentally, arguing that any particular series is probably better on the basis of what we now know about glaciers or solar output is flaky indeed. Glacier mass balance is driven by the difference mainly in winter accumulation and summer ablation, filtered in a complex non-linear way to give variously lagged tongue advance/retreat. Simple inference on the precedence of modern day snout positions does not translate easily into absolute (or relative) temperature levels now or in the past. Similarly, I don't see that we are able to substantiate the veracity of different temperature reconstructions through reference to Solar forcing theories without making assumptions on the effectiveness of (seasonally specific) long-term insolation changes in different parts of the globe and the contribution of solar forcing to the observed 20th century warming.

There is still a potential problem with non-linear responses in the very recent period of some biological proxies (or perhaps a fertilisation through high CO2 or nitrate input). I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple. We don't have a lot of proxies that come right up to date and those that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.

For the record, I do believe that the proxy data do show unusually warm conditions in recent decades. I am not sure that this unusual warming is so clear in the summer responsive data. I believe that the recent warmth was probably matched about 1000 years ago. I do not believe that global mean annual temperatures have simply cooled progressively over thousands of years as Mike appears to and I contend that there is strong evidence for major changes in climate over the Holocene (not

## PETITIONERS' EXEMPLARS (PE)

Milankovich) that require explanation and that could represent part of the current or future background variability of our climate. I think the Venice meeting will be a good place to air these issues.

Finally I apologise for this rather self-indulgent ramble, but I thought I may as well voice these points to you. I too would be happy to go through the recent draft of the chapter when it becomes available.

cheers to all  
Keith

At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:

>Dear All

>

>A proxy diagram of temperature change is a clear favourite for the Policy  
>Makers summary. But the current diagram with the tree ring only data  
>somewhat contradicts the multiproxy curve and dilutes the message rather  
>significantly. We want the truth. Mike thinks it lies nearer his result  
>(which seems in accord with what we know about worldwide mountain glaciers  
>and, less clearly, suspect about solar variations). The tree ring results  
>may still suffer from lack of multicentury time scale variance. This is  
>probably the most important issue to resolve in Chapter 2 at present.

>

>Chris

>

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

>> From: Phil Jones [SMTP:p.jones@uea.ac.uk]

>> Sent: 22 September 1999 12:58

>> To: Michael E. Mann; k.briffa@uea.ac.uk

>> Cc: ckfolland@meto.gov.uk; tkarl@ncdc.noaa.gov

>> Subject: Re: IPCC revisions

>>

>>

>> Mike,

>> Been away in Japan the last week or so. Malcolm was there in a  
>> wheelchair

>> because of his ruptured achilles. We both mentioned the lack of evidence  
>> for global scale change related to the MWE and LIA, but all the later  
>> Japanese speakers kept saying the same old things.

>>

>> As for the TAR Chap 2 it seems somewhat arbitrary division to exclude  
>> the

>> tree-ring only reconstructions. Keith's reconstruction is of a different  
>> character to other tree-ring work as it is as 'hemispheric in scale' as  
>> possible so is unlike any other tree-ring related work that is reported  
>> upon.

>> If we go as is suggested then there would be two diagrams - one simpler  
>> one with just Mann et al and Jones et al and in another section Briffa et  
>> al. This might make it somewhat awkward for the reader trying to put them  
>> into context.

>> The most important bit of the proxy section is the general discussion  
>> of

>> 'Was there an MWE and a LIA' drawing all the strands together. Keith and  
>> I

>> would be happy to look through any revisions of the section if there is  
>> time.

>>

>> One other thing, did you bring up the possibility of having a  
>> proxy-only

## PETITIONERS' EXEMPLARS (PE)

>> chapter ( albeit short) for the next assessment ?  
>>  
>> On Venice I suggested to Peck that you and Keith give talks on the  
>> reconstructions - frank and honest etc emphasising issues and I lead a  
>> discussion with you both and the rest of those there where the issues  
>> can be addressed ( ie I would like to get the views of other proxy types  
>> and  
>> the modellers/detectors there). I suggested to Peck that this was early  
>> in the week as I have to leave on the Thursday to go to the last day of  
>> a Working Group meeting of the Climate Change Detection group in Geneva  
>> ( a joint WMO Commission for Climatology/CLIVAR). I hope to report on the  
>> main findings of the Venice meeting.  
>>  
>> Another issue I would like to raise is availability of all the series  
>> you use in your reconstructions. That old chestnut again !  
>>  
>> How is life in Charlottesville ? Do you ever bump into Michaels or is  
>> always off giving skeptical talks ?  
>>  
>> Tim Osborn is making great progress with his NERC grant and will be  
>> looking  
>> into dates soon for coming to see you.  
>>  
>> Cheers  
>> Phil  
>>  
>>  
>> Prof. Phil Jones  
>> Climatic Research Unit Telephone +44 (0) 1603 592090 begin\_of\_the\_skype\_highlighting  
>> +44 (0) 1603 592090 end\_of\_the\_skype\_highlighting  
>> School of Environmental Sciences Fax +44 (0) 1603 507784  
>> University of East Anglia  
>> Norwich Email p.jones@uea.ac.uk  
>> NR4 7TJ  
>> UK  
>>  
>> -----  
>> --  
>>  
>>  
>

## PETITIONERS' EXEMPLARS (PE)

PE-33                      Date: Wed, 26 Mar 2008 12:45:42 +0000

---

From: David Parker <david.parker@metoffice.gov.uk>  
To: "Mann, Michael" <mann@virginia.edu>  
Subject: Heads up  
Date: Wed, 26 Mar 2008 12:45:42 +0000  
Cc: "Folland, Chris" <chris.folland@metoffice.gov.uk>, "Kennedy, John" <john.kennedy@metoffice.gov.uk>, "Jones, Phil" <p.jones@uea.ac.uk>, "Karl, Tom" <Thomas.R.Karl@noaa.gov>

Mike

Yes it was based on only Jan+Feb 2008 and padding with that final value but John Kennedy has changed / shortly will change this misleading plot!

Regards

David

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

From: Michael Mann [mailto:mann@meteo.psu.edu]  
Sent: 26 March 2008 11:19  
To: Folland, Chris  
Cc: Phil Jones; Thomas R Karl  
Subject: heads up

Hi Chris (and Tom and Phil),

I hope you're all doing well. Just wanted to give you a heads up on something. Have you seen this?  
[http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual\\_s21.png](http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21.png)  
apparently the contrarians are having a field day w/ this graph. My understanding that it is based on using only Jan+Feb 08 and padding w/ that final value.

Surely this can't be?? Is Fred Singer now running the UK Met Office website?

Would appreciate any info you can provide,

mike

--

Michael E. Mann  
Associate Professor  
Director, Earth System Science Center (ESSC)

Department of Meteorology                      Phone: (814) 863-4075 begin\_of\_the\_skype\_highlighting  
(814) 863-4075                      end\_of\_the\_skype\_highlighting  
503 Walker Building                      FAX: (814) 865-3663  
The Pennsylvania State University                      email: mann@psu.edu

## **PETITIONERS' EXEMPLARS (PE)**

--

David Parker Met Office Hadley Centre FitzRoy Road EXETER EX1 3PB UK  
E-mail: david.parker@metoffice.gov.uk  
Tel: +44-1392-886649 begin\_of\_the\_skype\_highlighting +44-1392-886649  
end\_of\_the\_skype\_highlighting Fax: +44-1392-885681 http:www.metoffice.gov.uk

## PETITIONERS' EXEMPLARS (PE)

PE-34

Date: Wed, 22 Sep 1999 12:35:24 -0400

---

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>  
Subject: RE: IPCC revisions  
Date: Wed, 22 Sep 1999 12:35:24 -0400  
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks for your response Keith,

For all:

Walked into this hornet's nest this morning! Keith and Phil have both raised some very good points. And I should point out that Chris, through no fault of his own, but probably through ME not conveying my thoughts very clearly to the others, definitely overstates any singular confidence I have in my own (Mann et al) series. I believe strongly that the strength in our discussion will be the fact that certain key features of past climate estimates are robust among a number of quasi-independent and truly independent estimates, each of which is not without its own limitations and potential biases. And I certainly don't want to abuse my lead authorship by advocating my own work.

I am perfectly amenable to keeping Keith's series in the plot, and can ask Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody liked my own color/plotting conventions so I've given up doing this myself). The key thing is making sure the series are vertically aligned in a reasonable way. I had been using the entire 20th century, but in the case of Keith's, we need to align the first half of the 20th century w/ the corresponding mean values of the other series, due to the late 20th century decline.

So if Chris and Tom (?) are ok with this, I would be happy to add Keith's series. That having been said, it does raise a conundrum: We demonstrate (through comparing an extratropical averaging of our northern hemisphere patterns with Phil's more extratropical series) that the major discrepancies between Phil's and our series can be explained in terms of spatial sampling/latitudinal emphasis (seasonality seems to be secondary here, but probably explains much of the residual differences). But that explanation certainly can't rectify why Keith's series, which has similar seasonality \*and\* latitudinal emphasis to Phil's series, differs in large part in exactly the opposite direction that Phil's does from ours. This is the problem we all picked up on (everyone in the room at IPCC was in agreement that this was a problem and a potential distraction/detraction from the reasonably consensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.

So, if we show Keith's series in this plot, we have to comment that "something else" is responsible for the discrepancies in this case. Perhaps Keith can help us out a bit by explaining the processing that went into the series and the potential factors that might lead to it being "warmer" than the Jones et al and Mann et al series?? We would need to put in a few words in this

## PETITIONERS' EXEMPLARS (PE)

regard. Otherwise, the skeptics have an field day casting doubt on our ability to understand the factors that influence these estimates and, thus, can undermine faith in the paleoestimates. I don't think that doubt is scientifically justified, and I'd hate to be the one to have to give it fodder!

The recent Crowley and Lowery multiproxy estimate is an important additional piece of information which I have indeed incorporated into the revised draft.

Tom actually estimates the same mean warming since the 17th century in his reconstruction, that we estimate in ours, so it is an added piece of information that Phil and I are probably in the ballpark (Tom has used a somewhat independent set of high and low-resolution proxy data and a very basic compositing methodology, similar to Bradley and Jones, so there is some independent new information in this estimate.

One other key result with respect to our own work is from a paper in the press in "Earth Interactions". An unofficial version is available here:

[http://www.ngdc.noaa.gov/paleo/ei/ei\\_cover.html](http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html)

The key point we emphasize in this paper is that the low-frequency variability in our hemispheric temperature reconstruction is basically the same if we don't use any dendroclimatic indicators at all (though we certainly resolve less variance, can't get a skillful reconstruction as far back, and there are notable discrepancies at the decadal and interannual timescales). I believe I need to add a sentence to the current discussion on this point,

since there is an unsubstantiated knee-jerk belief that our low-frequency variability is suppressed by the use of tree ring data.

We have shown that this is not the case: (see here:

[http://www.ngdc.noaa.gov/paleo/ei/ei\\_datarev.html](http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html)

and specifically, the plot and discussion here:

[http://www.ngdc.noaa.gov/paleo/ei/ei\\_nodendro.html](http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html)

Ironically, you'll note that there is more low-frequency variability when the tree ring data \*are\* used, then when only other proxy and historical/instrumental data are used!

SO I think we're in the position to say/resolve somewhat more than, frankly, than Keith does, about the temperature history of the past millennium. And the issues I've spelled out all have to be dealt with in the chapter.

One last point: We will (like it or not) have SUBSTANTIAL opportunity/requirement to revise much of this discussion after review, so we don't have to resolve everything now. Just the big picture and the important details...

I'm sure we can can up with an arrangement that is amenable to all, and I'm looking forward to hearing back from Keith, Phil, and Chris in particular about the above, so we can quickly move towards finalizing a first draft.

Looking forward to hearing back w/ comments,

## PETITIONERS' EXEMPLARS (PE)

mike

At 04:19 PM 9/22/99 +0100, Keith Briffa wrote:

>

>Hi everyone

> Let me say that I don't mind what you put in the policy makers  
>summary if there is a general consensus. However some general discussion  
>would be valuable . First , like Phil , I think that the supposed  
>separation of the tree-ring reconstruction from the others on the grounds  
>that it is not a true "multi-proxy" series is hard to justify. What is true  
>is that these particular tree-ring data best represent SUMMER temperatures  
>mostly at the northern boreal forest regions. By virtue of this , they also  
>definitely share significant variance with Northern Hemisphere land and  
>land and marine ANNUAL temperatures - but at decadal and multidecadal  
>timescales - simply by virtue of the fact that these series correlated with  
>the former at these timescales. The multi proxy series (Mann et al . Jones  
>et al) supposedly represent annual and summer seasons respectively, and  
>both contain large proportions of tree-ring input. The latest tree-ring  
>density curve ( i.e. our data that have been processed to retain low  
>frequency information) shows more similarity to the other two series- as do  
>a number of other lower resolution data ( Bradley et al, Peck et al ., and  
>new Crowley series - see our recent Science piece) whether this represents  
>'TRUTH' however is a difficult problem. I know Mike thinks his series is  
>the 'best' and he might be right - but he may also be too dismissive of  
>other data and possibly over confident in his (or should I say his use of  
>other's). After all, the early ( pre-instrumental) data are much less  
>reliable as indicators of global temperature than is apparent in modern  
>calibrations that include them and when we don't know the precise role of  
>particular proxies in the earlier portions of reconstruction it remains  
>problematic to assign genuine confidence limits at multidecadal and longer  
>timescales. I still contend that multiple regression against the recent  
>very trendy global mean series is potentially dangerous. You could  
>calibrate the proxies to any number of seasons , regardless of their true  
>optimum response . Not for a moment am I saying that the tree-ring , or any  
>other proxy data, are better than Mike's series - indeed I am saying that  
>the various reconstructions are not independent but that they likely  
>contribute more information about reality together than they do alone. I do  
>believe , that it should not be taken as read that Mike's series (or  
>Jones's et al. for that matter) is THE CORRECT ONE. I prefer a Figure that  
>shows a multitude of reconstructions (e.g similar to that in my Science  
>piece). Incidentally, arguing that any particular series is probably better  
>on the basis of what we now know about glaciers or solar output is flaky indeed.  
>Glacier mass balance is driven by the difference mainly in winter  
>accumulation and summer ablation , filtered in a complex non-linear way to  
>give variously lagged tongue advance/retreat .Simple inference on the  
>prevalence of modern day snout positions does not translate easily into  
>absolute (or relative) temperature levels now or in the past. Similarly, I  
>don't see that we are able to substantiate the veracity of different  
>temperature reconstructions through reference to Solar forcing theories  
>without making assumptions on the effectiveness of (seasonally specific )  
>long-term insolation changes in different parts of the globe and the  
>contribution of solar forcing to the observed 20th century warming .  
> There is still a potential problem with non-linear responses in the  
>very recent period of some biological proxies ( or perhaps a fertilisation  
>through high CO2 or nitrate input) . I know there is pressure to present a  
>nice tidy story as regards 'apparent unprecedented warming in a thousand

## PETITIONERS' EXEMPLARS (PE)

>years or more in the proxy data' but in reality the situation is not quite  
>so simple. We don't have a lot of proxies that come right up to date and  
>those that do (at least a significant number of tree proxies ) some  
>unexpected changes in response that do not match the recent warming. I do  
>not think it wise that this issue be ignored in the chapter.

> For the record, I do believe that the proxy data do show unusually  
>warm conditions in recent decades. I am not sure that this unusual warming  
>is so clear in the summer responsive data. I believe that the recent warmth  
>was probably matched about 1000 years ago. I do not believe that global  
>mean annual temperatures have simply cooled progressively over thousands of  
>years as Mike appears to and I contend that that there is strong evidence  
>for major changes in climate over the Holocene (not Milankovich) that  
>require explanation and that could represent part of the current or future  
>background variability of our climate. I think the Venice meeting will be  
>a good place to air these issues.

> Finally I apologise for this rather self-indulgent ramble, but I  
>thought I may as well voice these points to you . I too would be happy to  
>go through the recent draft of the chapter when it becomes available.

>  
> cheers to all  
> Keith  
>

>At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:  
>>Dear All  
>>  
>>A proxy diagram of temperature change is a clear favourite for the Policy  
>>Makers summary. But the current diagram with the tree ring only data  
>>somewhat contradicts the multiproxy curve and dilutes the message rather  
>>significantly. We want the truth. Mike thinks it lies nearer his result  
>>(which seems in accord with what we know about worldwide mountain glaciers  
>>and, less clearly, suspect about solar variations). The tree ring results  
>>may still suffer from lack of multicentury time scale variance. This is  
>>probably the most important issue to resolve in Chapter 2 at present.

>>  
>>Chris  
>>  
>>> -----Original Message-----  
>>> From: Phil Jones [SMTP:p.jones@uea.ac.uk]  
>>> Sent: 22 September 1999 12:58  
>>> To: Michael E. Mann; k.briffa@uea.ac.uk  
>>> Cc: ckfolland@meto.gov.uk; tkarl@ncdc.noaa.gov  
>>> Subject: Re: IPCC revisions  
>>>  
>>>  
>>> Mike,  
>>> Been away in Japan the last week or so. Malcolm was there in a  
>>> wheelchair  
>>> because of his ruptured achilles. We both mentioned the lack of evidence  
>>> for global scale change related to the MWE and LIA, but all the later  
>>> Japanese speakers kept saying the same old things.  
>>>  
>>> As for the TAR Chap 2 it seems somewhat arbitrary division to exclude  
>>> the  
>>> tree-ring only reconstructions. Keith's reconstruction is of a different  
>>> character to other tree-ring work as it is as 'hemispheric in scale' as  
>>> possible so is unlike any other tree-ring related work that is reported

## PETITIONERS' EXEMPLARS (PE)

>>> upon.  
>>> If we go as is suggested then there would be two diagrams - one simpler  
>>> one with just Mann et al and Jones et al and in another section Briffa et  
>>> al. This might make it somewhat awkward for the reader trying to put them  
>>> into context.  
>>> The most important bit of the proxy section is the general discussion  
>>> of  
>>> 'Was there an MWE and a LIA' drawing all the strands together. Keith and  
>>> I  
>>> would be happy to look through any revisions of the section if there is  
>>> time.  
>>>  
>>> One other thing, did you bring up the possibility of having a  
>>> proxy-only  
>>> chapter ( albeit short) for the next assessment ?  
>>>  
>>> On Venice I suggested to Peck that you and Keith give talks on the  
>>> reconstructions - frank and honest etc emphasising issues and I lead a  
>>> discussion with you both and the rest of those there where the issues  
>>> can be addressed ( ie I would like to get the views of other proxy types  
>>> and  
>>> the modellers/detectors there). I suggested to Peck that this was early  
>>> in the week as I have to leave on the Thursday to go to the last day of  
>>> a Working Group meeting of the Climate Change Detection group in Geneva  
>>> ( a joint WMO Commission for Climatology/CLIVAR). I hope to report on the  
>>> main findings of the Venice meeting.  
>>>  
>>> Another issue I would like to raise is availability of all the series  
>>> you use in your reconstructions. That old chestnut again !  
>>>  
>>> How is life in Charlottesville ? Do you ever bump into Michaels or is  
>>> always off giving skeptical talks ?  
>>>  
>>> Tim Osborn is making great progress with his NERC grant and will be  
>>> looking  
>>> into dates soon for coming to see you.  
>>>  
>>> Cheers  
>>> Phil  
>>>  
>>>  
>>> Prof. Phil Jones  
>>> Climatic Research Unit Telephone +44 (0) 1603 592090 begin\_of\_the\_skype\_highlighting  
>>> +44 (0) 1603 592090 end\_of\_the\_skype\_highlighting  
>>> School of Environmental Sciences Fax +44 (0) 1603 507784  
>>> University of East Anglia  
>>> Norwich Email p.jones@uea.ac.uk  
>>> NR4 7TJ  
>>> UK  
>>>  
>>> -----  
>>> --  
>>>  
>>>  
>>  
>--

**PETITIONERS' EXEMPLARS (PE)**

>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,  
>Norwich, NR4 7TJ, United Kingdom  
>Phone: +44-1603-592090 begin\_of\_the\_skype\_highlighting +44-1603-592090  
end\_of\_the\_skype\_highlighting Fax: +44-1603-507784  
>  
>  
>

---

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 begin\_of\_the\_skype\_highlighting (804) 924-  
7770 end\_of\_the\_skype\_highlighting FAX: (804) 982-2137  
<http://www.evsc.virginia.edu/faculty/people/mann.html>

## PETITIONERS' EXEMPLARS (PE)

PE-35

Date: Thu, 24 May 2001 11:35:06 -0600 (MDT)

---

From: Kevin Trenberth <trenbert@cgd.ucar.edu>  
To: "Michael E. Mann" <mann@virginia.edu>  
Subject: Re: Fwd: Recent Paper from the Competitive Enterprise Institute  
Date: Thu, 24 May 2001 11:35:06 -0600 (MDT)  
Reply-to: <trenbert@ucar.edu>  
Cc: <rbradley@geo.umass.edu>, <tkarl@ncdc.noaa.gov>, tom crowley <tom@ocean.tamu.edu>, <mhughes@ltrr.arizona.edu>, <jto@u.arizona.edu>, <rbradley@geo.umass.edu>, <p.jones@uea.ac.uk>, <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>

Mike:

You are right: this is a disinformation campaign.  
Some remarks

1) On the Christy et al grl paper, I sent the following to John following the IPCC Shanghai mtg.:

Date: Mon, 22 Jan 2001 15:39:20 -0700 (MST)  
From: Kevin Trenberth <trenbert@cgd.ucar.edu>  
To: John Christy <christy@atmos.uah.edu>  
Subject: your grl paper

John:

Just back from IPCC. One surprise was the strong Saudi delegation distributed your recent grl paper and wanted it inserted into the SPM! In spite of the fact that you are a lead author on Chapter 2, the paper is referenced, etc. In fact Simon Brown was there.

Chris Folland made a comment about his hypothesis for this: related to changes/growth in ships. My hypothesis focusses on the buoy data. See our recent paper submitted to jgr:

<http://www.cgd.ucar.edu/cas/papers/jgr2001b/jgr2.html> also

[http://www.cgd.ucar.edu/cas/papers/jgr2001a/jgr\\_interann.html](http://www.cgd.ucar.edu/cas/papers/jgr2001a/jgr_interann.html)

This shows that during and following El Nino there is an anomalous flux of heat out of ocean into atmosphere in the east Pacific of order 50 W m<sup>-2</sup> over many months: so ocean T warms relative to air. During La Lina flux goes other way. i.e. air warms relative to ocean.

So your results must be affected by 1997-98 event at end of series and that may explain trend differential.

Hope this helps  
Regards  
Kevin

i.e. the result is not as advertized.

=====

2) wrt Lindzen's paper

## PETITIONERS' EXEMPLARS (PE)

Here is the text from my recent Senate testimony

The determination of the climatic response to the changes in heating and cooling is complicated by feedbacks. Some of these can amplify the original warming (positive feedback) while others serve to reduce it (negative feedback). If, for instance, the amount of carbon dioxide in the atmosphere were suddenly doubled, but with other things remaining the same, the outgoing long-wave radiation would be reduced and instead trapped in the atmosphere. To restore the radiative balance, the atmosphere must warm up and, in the absence of other changes, the warming at the surface and throughout the troposphere would be about 1.2\deg C. In reality, many other factors will change, and various feedbacks come into play, so that the best IPCC estimate of the average global warming for doubled carbon dioxide is 2.5\deg C. In other words, the net effect of the feedbacks is positive and roughly doubles the response otherwise expected. The main positive feedback comes from increases in water vapor with warming.

In 2001, the IPCC gave special attention to this topic. The many issues with water vapor and clouds were addressed at some length in Chapter 7 (of which I was a lead author, along with Professor Richard Lindzen (M.I.T.), and others). Recent possibilities that might nullify global warming (Lindzen 2001) were considered but not accepted because they run counter to the prevailing evidence, and the IPCC (Stocker et al., 2001) concluded that "the balance of evidence favours a positive clear sky water vapour feedback of the magnitude comparable to that found in the simulations."

===

Here is a more complete rebuttal, written March 23 to MacCracken.

Subject: Re: Recent Lindzen paper

Kevin Trenberth

1) The paper is based on very simple conceptual ideas that do not mesh with reality. Fig. 2 is simply not correct. For a more correct view of the overturning see:

Trenberth, K. E., D. P. Stepaniak and J. M. Caron, 2000: The global monsoon as seen through the divergent atmospheric circulation. {J. Climate}, 13, 3969--3993.

This paper also shows that the flow in the tropics is dominated by transients (and thus mixing) of all kinds. The mean overturning is only about a third of the daily mean variance for a month and much less if the intra diurnal variations and interannual variations are included.

2) The "observations" analysis makes absolutely no sense to me at all. There is a totally inadequate description of what is done and no way to decipher what a dot in Fig 5 or Fig 6 is. Given 20 months, and daily values (how was that done?) why are there only about 330 points? Why isn't Fig 6 part of Fig. 5?

In any event the results are totally at odds with other evidence. Here I refer to the Goes Precipitation Index which uses 3 hourly data on OLR, and

## PETITIONERS' EXEMPLARS (PE)

thus on high cloud, as an index of rainfall, and it is clear from many studies that OLR generally decreases (convection and high cloud increase) with SST, the reverse of the relationship in Fig. 5.

Moreover the whole conceptual basis for anything here is surely flawed. As stated, on short time scales SST is not changing. But clouds are NOT caused by local SST, rather they arise from either transients, like the MJO, or for the ITCZ and SPCZ (which are major operators in this region), they come from moisture convergence ( $P \gg E$ ) and so it is the patterns of SST (gradients) as well as where the warmest water is that determines where the convergence and clouds occur. Now in the warm pool, the convergence is focussed more on the edges, as that is where the pressure gradients are greater, and so the convergence is not where SST is necessarily highest.

In any case, moisture is not equal to cloudy air. Many analyses show that moisture is much more extensive, see for example Trenberth, K. E., and C. J. Guillemot, 1998: Evaluation of the atmospheric moisture and hydrological cycle in the NCEP/NCAR reanalyses. {Climate Dyn.}, {14}, 213--231.

Even with such results, other factors need to be considered.

One process might be

High SST => convergence => rainfall and cloud

OR

Less cloud => more solar radiation => higher SST

Those give opposite relations and both operate. The latter is more important in the Indian Ocean where subsidence (from the Pacific) dominates. However, it also operates over the oceans in the region in question in northern summer, because that is the monsoon season, and the main convection is over land, meaning subsidence over the ocean.

None of this is sorted out in any way in this paper.

In fact it is so bad in this regard I do not know how it got published.

In Fig 5 etc, no correlations are given, nor are their significance levels.

My rough estimate is that the correlation is about 0.2 to 0.3 and that is significant if the 330 or so points are independent. But why should I have to guess at that.

Again I would question the editorial and review process.

3) Finally, I refer you to chapter 7 of IPCC which is a more balanced assessment. Lindzen was a coauthor of that with me and others. Lindzen wrote 7.2.1 and the same figure 1 in the BAMS article was included as 7.1 in chapter 7 along with similar ones from models, showing that these things are fully simulated in good models, although better with higher resolution. Anyway, his arguments were fully considered in chapter 7 and you can read it to see the result. The whole of 7.2.1, including 7.2.1.1. 7.2.1.2 and 7.2.1.3 was put together originally by Lindzen, Pierrehumbert and Le Treut, but basically the final version was rewritten by me to provide better balance. Pierrehumbert is an agnostic of sorts: disbelieves everything including models but seems to have faith in simple theories. Le Treut was sound on the modeling. I did not change the substance of what they prepared, I did reshape it and polish and it ended up in a form they accepted.

## PETITIONERS' EXEMPLARS (PE)

Note at the end it clearly states:

"the balance of evidence favours a positive clear sky water vapour feedback of the magnitude comparable to that found in the simulations."

The 4 subsections together are quite long and thoroughly air the issue, much moreso than any previous IPCC report. For those of you who do not have it: 7.2.1 "Physics of the water vapour and cloud feedbacks" (draft written by Lindzen) is 1.3 pages, 7.2.1.1 (I think Pierrehumbert) "Water vapour feedback", is 1 page, 7.2.1.2 "Representation of watre vapour in models" is 1.5 pages (Le Treut) and 7.2.1.3 "Summary on water vapour feedbacks" is half a page or so.

-----  
Kevin E. Trenberth e-mail: trenbert@ucar.edu  
Climate Analysis Section, NCAR, ML www.cgd.ucar.edu/cas/  
P. O. Box 3000, [1850 Table Mesa Drive] (303) 497 1318 begin\_of\_the\_skype\_highlighting  
(303) 497 1318 end\_of\_the\_skype\_highlighting  
Boulder, CO 80307 [80305] (303) 497 1333 (fax)  
\*\*\*\*\*

On Thu, 24 May 2001, Michael E. Mann wrote:

> FYI. I received this from a colleague. This gives you some idea of who is  
> behind this latest disinformation push.  
>  
> A note to all regarding the Broecker piece, which has been heavily referred  
> to in this and other similar recent pieces (though it is an opinion piece,  
> and not peer-reviewed).  
> A response by Bradley, Briffa, Crowley, Hughes, Jones, and Mann appears in  
> tomorrows issue of "Science". This response simply points out that old  
> fallacies that are simply reiterated in Broecker's piece...

>  
> mike

>  
>  
>  
>  
>  
>

>> COMPETITIVE ENTERPRISE INSTITUTE

>>  
>>  
>>  
>>  
>>  
>>  
>>

>> Advancing the principles of free enterprise and  
>> limited government

>>  
>>

>> 5/16/01

>>  
>>

>> Latest Global Warming Report Already Obsolete

>>  
>>

>> By Paul J. Georgia

>>

## PETITIONERS' EXEMPLARS (PE)

>>  
>>  
>> The United Nations Intergovernmental Panel on Climate Change  
>>(IPCC) is  
>> conducting a campaign of fear to convince us that energy  
>>suppression is  
>> our only salvation. The "Summary for Policymakers" of the  
>>group's latest  
>> report ? the report itself has not been officially released ?  
>>paints a horrific  
>> picture of a climate system gone mad.  
>>  
>> The new report, known as the "Third Assessment Report" (TAR),  
>>is  
>> expected to be the focal point for policymakers for the next  
>>five years as  
>> they decide what to do about global warming, just as the 1995  
>>Second  
>> Assessment Report has guided policymakers for the last five  
>>years.  
>> Indeed, the bureaucrats driving the global warming process  
>>are using the  
>> IPCC to justify their anti-energy policies. Klaus Toepfer,  
>>executive  
>> director of the United Nations Environment Programme, said,  
>>"The  
>> scientific consensus presented in this comprehensive report  
>>about  
>> human induced climate change should sound alarm bells in  
>>every  
>> national capital and in every local community."[1]  
>>  
>> In the midst of this campaign, however, the science continues  
>>to move  
>> apace, leaving many of the IPCC's underlying assumptions and  
>> subsequent conclusions in shambles. A sampling of scientific  
>>studies  
>> published after the completion of the final drafts of the TAR  
>>is presented  
>> here to give the reader a taste of the constant flux of  
>>scientific inquiry and  
>> our rapidly changing understanding of the climate system.  
>>Indeed, if  
>> recent studies are correct there would be little  
>>justification for Kyoto-style  
>> policies that would ultimately impede humanity's ability to  
>>provide itself  
>> with the wealth- and health-enhancing benefits of modern  
>>civilization.  
>>  
>> Water Vapor Feedback. The biggest uncertainty in climate  
>>science  
>> remains "feedback" effects on the climate. The conventional  
>>explanation  
>> by proponents of global warming theory always assumes that  
>> human-induced increases in atmospheric concentrations of

## PETITIONERS' EXEMPLARS (PE)

>>greenhouse  
>> gases, primarily carbon dioxide, could lead to catastrophic  
>>warming of  
>> the planet. Man-made greenhouse gas emissions, however, are  
>>only an  
>> indirect cause of the forecasted warming. A doubling of  
>>carbon dioxide  
>> concentrations alone would lead to slight warming of about  
>>one degree  
>> Celsius (1.8 degrees Fahrenheit) over the next 100 years.  
>>This small  
>> amount of warming, according to standard global warming  
>>theory, speeds  
>> up evaporation, thereby increasing the amount of water vapor  
>>(a major  
>> greenhouse gas) in the atmosphere. This "positive water  
>>vapor feedback"  
>> effect is where most of the predicted warming comes from.  
>>This  
>> assumption has never been tested.  
>>  
>> A recent study in the Bulletin of the American Meteorological  
>>Society  
>> suggests that the reverse is true.[2] The authors find a  
>>negative water  
>> vapor feedback effect that is powerful enough to offset all  
>>other positive  
>> feedbacks. Using detailed daily observations of cloud cover  
>>from  
>> satellites in the tropics and comparing them to sea surface  
>>temperatures,  
>> the researchers found that there is an "iris effect" in which  
>>higher  
>> temperatures reduce the warming effect of clouds.  
>>  
>> According to a NASA statement about the study, "Clouds play a  
>>critical  
>> and complicated role in regulating the temperature of the  
>>Earth. Thick,  
>> bright, watery clouds like cumulus shield the atmosphere from  
>>incoming  
>> solar radiation by reflecting much of it back into space.  
>>Thin, icy cirrus  
>> clouds are poor sunshields but very efficient insulators that  
>>trap energy  
>> rising from the Earth's warmed surface. A decrease in cirrus  
>>cloud area  
>> would have a cooling effect by allowing more heat energy, or  
>>infrared  
>> radiation, to leave the planet." [3]  
>>  
>> The researchers found that a one degree Celsius rise in ocean  
>>surface  
>> temperature decreased the ratio of cirrus cloud area to  
>>cumulus cloud  
>> area by 17 to 27 percent, allowing more heat to escape.

## PETITIONERS' EXEMPLARS (PE)

>>  
>> In an interview, lead author Dr. Richard S. Lindzen said the  
>>climate  
>> models used in the IPCC have the cloud physics wrong. "We  
>>found that  
>> there were terrible errors about clouds in all the models,  
>>and that that will  
>> make it impossible to predict the climate sensitivity because  
>>the  
>> sensitivity of the models depends primarily on water vapor  
>>and clouds.  
>> Moreover, if clouds are wrong, there's no way you can get  
>>water vapor  
>> right. They're both intimately tied to each other." Lindzen  
>>argues that  
>> due to this new finding he doesn't expect "much more than a  
>>degree  
>> warming and probably a lot less by 2100." [4]  
>>  
>> The study is the best empirical confirmation to date of the  
>>negative  
>> feedback hypothesis proposed by Lindzen early on in the  
>>global warming  
>> debate. It builds on earlier empirical work by Drs. Roy  
>>Spencer of NASA  
>> and William Braswell of Nichols Research Corporation. Their  
>>1997 study  
>> also cast doubt on the assumption of a positive water vapor  
>>feedback  
>> effect.[5] They found that the tropical troposphere, the  
>>layer of air  
>> between 25,000 and 50,000 feet, is much dryer than climate  
>>modelers  
>> previously thought. Further empirical work will no doubt  
>>confirm whether  
>> this phenomenon is common throughout the tropics, which act  
>>as the  
>> Earth's exhaust vents for escaping heat.  
>>  
>>  
>> Black Carbon. In 1995, the IPCC had to explain in its Second  
>>Assessment Report why its previous predictions of global  
>>temperature  
>> change were nearly three times larger than observed in the  
>>actual  
>> temperature record. The SAR concluded that emissions of  
>>sulfate  
>> aerosols from burning coal were offsetting the warming that  
>>should be  
>> caused by carbon dioxide levels in the atmosphere. Sulfate  
>>aerosols,  
>> according to this explanation, reflect incoming solar  
>>radiation back to  
>> space, thereby cooling the planet.  
>>  
>>  
>>

## PETITIONERS' EXEMPLARS (PE)

>> The TAR takes the sulfate aerosol idea even further. The SAR  
>>had  
>> predicted a temperature rise of 1 to 3.5 degrees C (1.8 to  
>>6.3 degrees F)  
>> over the next 100 years. The TAR goes even further,  
>>anticipating a 1.4 to  
>> 5.8 degrees C (2.52 to 10.44 degrees F) rise in temperature.  
>>The  
>> extreme case scenario of a 5.8 degrees C of warming, for  
>>instance, is  
>> based partly on assumptions that the whole world will raise  
>>its level of  
>> economic activity to that of the U.S., will equal U.S. per  
>>capita energy  
>> use, and energy use will be carbon intensive. The primary  
>>assumption  
>> behind the new scenario, however, is that sulfate aerosol  
>>emissions will  
>> be eliminated by government regulation, giving carbon dioxide  
>>free  
>> reign.[6]  
>>  
>> Sulfate aerosols, then, are a key component of catastrophic  
>>global  
>> warming scenarios. Without them, the IPCC cannot explain why  
>>the  
>> earth is not warming according to their forecasts, nor can  
>>they  
>> reasonably claim that global warming will lead to  
>>catastrophes of biblical  
>> proportions.  
>>  
>> A new study in Nature eliminates sulfate aerosols as a  
>>corrective for the  
>> models. [7] The author, Mark Jacobson, a professor with the  
>>Department  
>> of Civil & Environmental Engineering at Stanford University,  
>>examines  
>> how black carbon aerosols affect the Earth's climate. Unlike  
>>other  
>> aerosols that reflect solar radiation back into space, black  
>>carbon (soot)  
>> absorbs solar radiation, thereby raising atmospheric  
>>temperatures.  
>>  
>> Until now the warming influence of black carbon was thought  
>>to be minor,  
>> leading researchers to ignore it. James Hansen, with the  
>>Goddard  
>> Institute for Space Studies, in a paper published in August  
>>2000, first  
>> suggested that black carbon plays an important role in global  
>>warming.[8] Jacobson found "a higher positive forcing from  
>>black carbon  
>> than previously thought, suggesting that the warming effect  
>>from black

## PETITIONERS' EXEMPLARS (PE)

>> carbon may nearly balance the net cooling effect of other  
>>anthropogenic  
>> aerosol constituents."  
>>  
>> There you have it. Soot offsets the cooling effect of other  
>>aerosols,  
>> meaning we are back at square one. Scientists still do not  
>>have a  
>> plausible explanation for why the Earth has failed to warm in  
>>line with  
>> climate model results. Indeed, all the prognostications of  
>>the IPCC are  
>> wrong if the Nature study is right.  
>>  
>>  
>> Natural Cycles. The main propaganda device of the TAR is the  
>>"hockey  
>> stick graph." The graph is a temperature record derived from  
>>tree rings  
>> dating back to 1000 AD and running through 1900, with the  
>>20th century  
>> thermometer-based temperature data attached at the end.[9]  
>>It claims to  
>> show that global temperatures have remained steady or even  
>>decreased  
>> during the last millennium until the industrial age, when  
>>there was an  
>> anomalous warming represented by the blade of the hockey  
>>stick. The  
>> hockey stick is largely bogus, however. The margin of error  
>>is so large  
>> that nearly any temperature trend could be drawn to fit  
>>within it.  
>>  
>>  
>>  
>> The hockey stick features prominently in all of IPCC Chairman  
>>Robert  
>> Watson's speeches, and to the uninitiated it is very  
>>persuasive. Senator  
>> John McCain (R-AZ), for example, expressed alarm when he saw  
>>the  
>> graph at Commerce Committee hearings last May.  
>>  
>>  
>> Watson uses the hockey stick to claim that current warming is  
>>greater  
>> than at any other time in the last 1,000 years. The Medieval  
>>Warm  
>> Period (MWP) and the Little Ice Age (LIA) were two naturally  
>>occurring  
>> events during the last millennium where the range of global  
>>temperature  
>> change exceeded that of the 20th century. During the MWP,  
>>global  
>> temperatures were higher than they are today. The MWP,

## PETITIONERS' EXEMPLARS (PE)

>>however, does  
>> not show up in the hockey stick graph.  
>>  
>> The hockey stick has effectively been dismantled in a recent  
>>study in  
>> Science, however.[10] Wallace Broecker, of the  
>>Lamont-Doherty Earth  
>> Observatory, argues that the MWP and the LIA were indeed  
>>global  
>> phenomena. Referring to the hockey stick, Broecker notes, "A  
>>recent,  
>> widely cited reconstruction leaves the impression that the  
>>20th century  
>> warming was unique during the last millennium. It shows no  
>>hint of the  
>> Medieval Warm Period (from around 800 to 1200 A.D.) during  
>>which the  
>> Vikings colonized Greenland, suggesting that this warm event  
>>was  
>> regional rather than global. It also remains unclear why just  
>>at the dawn  
>> of the Industrial Revolution and before the emission of  
>>substantial  
>> amounts of anthropogenic [manmade] greenhouse gases, Earth's  
>> temperature began to rise steeply."  
>>  
>>  
>> Broecker reviewed several scientific studies which  
>>reconstruct the Earth's  
>> temperature history into the distant past using various  
>>proxies. He  
>> concludes, "The post-1860 natural warming was the most recent  
>>in a  
>> series of similar warmings spaced at roughly 1500-year  
>>intervals  
>> throughout the present interglacial, the Holocene."[11] In  
>>other words,  
>> the current warm period may just be attributable to natural  
>>cycles.  
>>  
>>  
>> Flawed Temperature Data. The National Oceanic and  
>>Atmospheric  
>> Administration (NOAA) claimed that the year 2000 was the  
>>sixth  
>> warmest since 1880. Other temperature records find less  
>>warming.[12]  
>> Last year was only the 14th warmest, or 9th coolest, year  
>>since 1979  
>> according to the satellite temperature record,[13] and only  
>>the 9th  
>> warmest, according to records that include only measurements  
>>from  
>> meteorological stations.[14]  
>>  
>> The NOAA data, which is cited by government officials and the

## PETITIONERS' EXEMPLARS (PE)

> > news  
> > media, may be the least accurate, according to a study that  
> > recently  
> > appeared in Geophysical Research Letters.[15] The NOAA  
> > datasets "are  
> > a mixture of near-surface air temperatures over land and sea  
> > water  
> > temperatures over oceans," according to lead author Dr. John  
> > Christy,  
> > professor of atmospheric science and director of the Earth  
> > System  
> > Science Center at the University of Alabama in Huntsville.  
> >  
> > Since actual air temperature data over many large ocean areas  
> > are  
> > nonexistent, the NOAA uses sea surface temperatures as a  
> > "proxy,"  
> > assuming that sea surface temperatures and air temperatures  
> > move in  
> > lock step. This is not the case, according to the data  
> > compiled by  
> > Christy and his colleagues at the Hadley Centre of the United  
> > Kingdom's  
> > Meteorological Office, who worked on the study. The  
> > researchers used  
> > buoy data in the tropical Pacific Ocean to compare "long-term  
> > (8-20 year)  
> > trends for temperatures recorded one meter below the sea  
> > surface and  
> > three meters above it."  
> >  
> > What they found was a significant discrepancy. "For each  
> > buoy in the  
> > Eastern Pacific, the air temperatures measured at the three  
> > meter height  
> > showed less of a warming trend than did the same buoy's water  
> > temperatures at one meter depth," the study said. The  
> > difference is a  
> > near-surface seawater warming trend of 0.37 degrees C per  
> > decade and  
> > an air temperature trend of only 0.25 degrees C per decade  
> > during the  
> > 20-year period tested. Replacing the sea surface  
> > temperatures with the  
> > air temperature data reduces the Earth's global warming trend  
> > by a third,  
> > from 0.19 to 0.13 degree C per decade.  
> >  
> > This is significant due to difficulties with reconciling the  
> > various global  
> > temperature data sets, particularly the discrepancy between  
> > tropospheric  
> > temperatures measured by satellites that show little to no  
> > warming, and  
> > the surface-based temperature data that show slightly more  
> > warming.

## PETITIONERS' EXEMPLARS (PE)

>> Last year, the National Research Council stated that both  
>>temperature  
>> records are correct and speculated about an explanation.[16]  
>>  
>> This brings up another problem, however. The standard  
>>explanation of  
>> the greenhouse effect suggests warming occurs first five  
>>kilometers  
>> above the earth's surface in the atmospheric layer known as  
>>the  
>> troposphere. How events at the surface are connected to what  
>>happens  
>> high in the atmosphere is not clear, but it is believed that  
>>surface  
>> warming would follow tropospheric warming through climatic  
>>processes  
>> such as air circulation.[17] If both temperature records are  
>>correct, then  
>> this explanation of the greenhouse effect is wrong. Christy  
>>et al. brings  
>> the surface temperature data into closer agreement with the  
>>satellite  
>> data, suggesting that a better explanation for the  
>>discrepancy is flawed  
>> surface data.  
>>  
>> Progressive Science. At a press conference at the National  
>>Press  
>> Club on April 18, Mr. Jan Pronk, chairman of the Sixth  
>>Conference of the  
>> Parties of the United Nations Framework Convention on Climate  
>>Change  
>> said most issues were still on the table in the ongoing Kyoto  
>>negotiations  
>> but the scientific basis of catastrophic global warming could  
>>not be  
>> questioned. That would be like going back ten years, he  
>>said. This is a  
>> myopic and erroneous view of science. Science is not static  
>>but  
>> dynamic. It reaches tentative conclusions at best, and those  
>> conclusions constantly give way to new data. The IPCC is a  
>>static  
>> process, however. The Third Assessment Report is already  
>>obsolete and  
>> it has not even been released yet. With these four recent  
>>studies, it may  
>> be time to bid catastrophic global warming theory a warm  
>>farewell.  
>>  
>>  
>>  
>>  
>>  
>> [1] "Evidence of Rapid Global Warming Accepted by 99 Nations,"  
>>Environment News Service, January 22,

## PETITIONERS' EXEMPLARS (PE)

>> 2001.  
>> [2] Richard S. Lindzen, Ming-Dah Chou, and Arthur Y. Hou, "Does the  
>>Earth Have an Adaptive Infrared Iris?,  
>> Bulletin of the American Meteorological Society, 82:417-32, March  
>>2001.  
>> [3] <ftp://www.gsfc.nasa.gov/pub/PAO/Releases/2001/01-18.htm>  
>> [4] "Is Globe Warming? Sure, But Far Less than Alarmists Say,"  
>>Tech Central Station  
>> (<http://www.techcentralstation.com/BigShotFriday.asp>), March 5,  
>>2001.  
>> [5] Roy W. Spencer and William D. Braswell, "How Dry is the  
>>Tropical Free Troposphere? Implications for  
>> Global Warming Theory," Bulletin of the American Meteorological  
>>Society, 78:1097-1106.  
>> [6] In correspondence with Nature magazine, one of the IPCC's  
>>coordinating lead authors, Thomas Stocker of  
>> the Physics Institute at the University of Bern in Switzerland,  
>>wrote, "First, although climate modeling has  
>> advanced during the past five years, this is not the main reason  
>>for the revised range of temperature  
>> projections. The higher estimates of maximum warming by the year  
>>2100 stem from a more realistic view of  
>> sulphate aerosol emissions. The new scenarios assume emissions  
>>will be reduced substantially in the coming  
>> decades, as this becomes technically and economically feasible, to  
>>avoid acid rain. Sulphate emissions have  
>> a cooling effect, so reducing them leads to higher estimates of  
>>warming." See "Climate panel looked at all  
>> the evidence," Nature, 410: 299, March 15, 2001.  
>> [7] Mark Z. Jacobson, "Strong radiative heating due to the mixing  
>>state of black carbon in atmospheric  
>> aerosols." Nature, 409: 695-72, February 8, 2001.  
>> [8] James D. Hansen, Makiko Sato, Reto Ruedy, Andrew Lacis, and  
>>Valdir Oinas, "Global Warming in the  
>> twenty-first century: An alternative scenario," Proceedings of the  
>>National Academy of Sciences,  
>> 97:9875-9880.  
>> [9] The tree ring data originated with Michael E. Mann, Raymond S.  
>>Bradley and Malcolm K. Hughes,  
>> "Northern Hemisphere Temperatures During the Past Millennium:  
>>Inferences, Uncertainties, and Limitations,"  
>> Geophysical Research Letters, 26: 759, March 15, 1999.  
>> [10] Wallace S. Broecker, "Was the Medieval Warm Period Global?"  
>>Science, 291: 1497-99, February 23,  
>> 2001.  
>> [11] Also see H.H. Lamb, Climate History and the Modern World, (New  
>>York: Routledge, 1985), and Brian  
>> Fagan, The Little Ice Age: How Climate Made History, 1300-1850,  
>>(New York: Basic Books, 2000).  
>> [12] <http://www.ncdc.noaa.gov/ol/climate/research/2000/ann/ann.html>  
>> [13] <http://www.ghcc.msfc.nasa.gov/MSU/msusci.html>  
>> [14] <http://www.john-daly.com/press/press-01.htm#Phil>  
>> [15] John R. Christy, David E. Parker, Simon J. Brown, Ian Macadam,  
>>Martin Stendal, and William B. Norris,  
>> "Differential Trends in Tropical Sea Surface and Atmospheric  
>>Temperatures since 1979," Geophysical

**PETITIONERS' EXEMPLARS (PE)**

> > Research Letters, 28:183.  
> > [16] Reconciling Observations of Global Temperature Change,  
> > National Academy Press: Washington, D.C.,  
> > 2000.  
> > [17] Richard S. Lindzen, "Climate Forecasting: When Models are  
> > Qualitatively Wrong," George C. Marshall  
> > Institute, Washington, D.C., 2000.  
> >  
> >  
> >  
> > ©5/16/01 Competitive Enterprise Institute  
>  
>  
> \_\_\_\_\_  
> 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 begin\_of\_the\_skype\_highlighting (804) 924-  
> 7770 end\_of\_the\_skype\_highlighting FAX: (804) 982-2137  
> http://www.evsc.virginia.edu/faculty/people/mann.shtml  
>  
>  
>

## PETITIONERS' EXEMPLARS (PE)

PE-36

Date: Thu Jul 07 16:18 2003

---

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

Thanks very much for helping me out with this Mike. Rest assured that the data won't be passed on to anyone else. I'll let you know if I use them to compute uncertainties at different time scales.

Cheers

Tim

At 16:18 31/07/2003, you wrote:

Tim,

Attached are the calibration residual series for experiments based on available networks back to:

AD 1000

AD 1400

AD 1600

I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...

You only want to look at the first column (year) and second column (residual) of the files. I can't even remember what the other columns are!

Let me know if that helps. Thanks,

mike

p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

## PETITIONERS' EXEMPLARS (PE)

PE-37

Date: Sat, 10 Feb 2001 21:47:57 +1100

---

From: "John L. Daly" <daly@microtech.com.au>  
To: Chick Keller <ckeller@igpp.ucsd.edu>  
Subject: Re: Hockey Sticks again  
Date: Sat, 10 Feb 2001 21:47:57 +1100  
Reply-to: daly@microtech.com.au  
Cc: "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, daly@vision.net.au, onar@netpower.no, jarl.ahlbeck@abo.fi, richard@courtney01.compulink.co.uk, McKitrick <rmckit@css.uoguelph.ca>, Bjarnason <agust@rt.is>, Harry Priem <priem@dds.nl>, vinmary.gray@paradise.net.nz, balberts@nas.edu, Martin Manning <m.manning@niwa.cri.nz>, Albert Arking <arking@jhu.edu>, Sallie Baliunas <baliunas@cfa.harvard.edu>, Jack Barrett <100436.3604@compuserve.com>, Sonja Boehmer-Cristianse <sonja.b-c@geo.hull.ac.uk>, Nigel Calder <nc@windstream.demon.co.uk>, John Christy <christy@atmos.uah.edu>, cpaynter@greeningearthsociety.org, driessen@global-commpartners.net, dwojick@shentel.net, Myron Ebell <mebell@cei.org>, Ellsaesser <hughel@home.com>, John Emsley <j.emsley@ic.ac.uk>, Jim Goodridge <jdg@mcn.org>, gsharp@montereybay.com, Peter Holle <cog@escape.ca>, Douglas V Hoyt <d Hoyt1@erols.com>, "W. S. Hughes" <wsh@unite.com.au>, Wibjörn Karlén <wibjorn.karlen@natgeo.su.se>, kidso@hotmail.com, Klirill Kondratyev <kirill.kondratyev@niersc.spb.ru>, "Dr. Theodor Landscheidt" <theodor.landscheidt@ns.sympatico.ca>, Ross McKitrick <rmckitri@uoguelph.ca>, omcshane <omcshane@wk.planet.gen.nz>, Pat Michaels <pmichael@cato.org>, pbrekke@esa.nascom.nasa.gov, "David M. Ritson" <dmr@SLAC.Stanford.EDU>, robert.balling@asu.edu, Tom Segalstad <t.v.segalstad@toyen.uio.no>, Fred Singer <singer@sepp.org>, Roy Spencer <roy.spencer@msfc.nasa.gov>, Hartwig Volz <Hartwig.Volz@rwedea.de>, Gerd-Rainer Weber <gerd-rainer.weber@gvst.de>, tlowery@ocean.tamu.edu, Rosanne D'Arrigo <druidrd@Ideo.columbia.edu>, k.briffa@uea.ac.uk

Dear Chick & all

> the first is Keith Briffa's rather comprehensive treatment of getting  
> climate variations from tree rings: Annual climate variability in  
> the Holocene: "interpreting the message of ancient trees", Quaternary  
> Science Reviews, 19 (2000) 87-105 begin\_of\_the\_skype\_highlighting 19 (2000) 87-105  
end\_of\_the\_skype\_highlighting. It should deal with many of the  
> questions people raise about using them to determine temperatures.

Take this from first principles.

A tree only grows on land. That excludes 70% of the earth covered by water. A tree does not grow on ice. A tree does not grow in a desert. A tree does not grow on grassland-savannahs. A tree does not grow in alpine areas. A tree does not grow in the tundra

We are left with perhaps 15% of the planet upon which forests grow/grew. That does not make any studies from tree rings global, or even hemispheric.

The width and density of tree rings is dependent upon the following variables which cannot be reliably separated from each other.

sunlight - if the sun varies, the ring will vary. But not at night of course.  
cloudiness - more clouds, less sun, less ring.  
pests/disease - a caterpillar or locust plague will reduce photosynthesis  
access to sunlight - competition within a forest can disadvantage or advantage some trees.  
moisture/rainfall - a key variable. Trees do not prosper in a drought even if there's a heat wave.  
snow packing in spring around the base of the trees retards growth

## PETITIONERS' EXEMPLARS (PE)

temperature - finally!

The tree ring is a composite of all these variables, not merely of temperature. Therefore on the 15% of the planet covered by trees, their rings do not and cannot accurately record temperature in isolation from the other environmental variables.

In my article on Greening Earth Society on the Hockey Stick, I point to other evidence which contradicts Mann's theory. The Idso's have produced more of that evidence, and a new article on Greening Earth has 'unearthed' even more.

Mann's theory simply does not stack up. But that was not the key issue. Anyone can put up a dud theory from time to time. What is at issue is the uncritical zeal with which the industry siezed on the theory before its scientific value had been properly tested. In one go, they tossed aside dozens of studies which confirmed the existence of the MWE and LIA as global events, and all on the basis of tree rings - a proxy which has all the deficiencies I have stated above.

The worst thing I can say about any paper such as his is that it is 'bad science'. Legal restraint prevents me going further. But in his case, only those restraints prevent me going \*much\* further.

Cheers

John Daly

--

John L. Daly  
'Still Waiting For Greenhouse'  
<http://www.microtech.com.au/daly>

replies to: [daly@microtech.com.au](mailto:daly@microtech.com.au)

PLEASE NOTE:

WEBSITE URL HAS BEEN CHANGED TO <http://www.microtech.com.au/daly>  
EMAIL ADDRESS HAS BEEN CHANGED TO [daly@microtech.com.au](mailto:daly@microtech.com.au)  
BOOKMARKS AND ADDRESS ENTRIES, IF ANY, SHOULD BE AMENDED ACCORDINGLY.

## PETITIONERS' EXEMPLARS (PE)

PE-38

Date: Sun, 26 Oct 2003 13:47:44 -0500

---

From: "Michael E. Mann" <mann@virginia.edu>  
To: Ray Bradley <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Steve Schneider <shs@stanford.edu>, tom crowley <tom@ocean.tamu.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>, Jonathan Overpeck <jto@u.arizona.edu>, asocci@cox.net, Michael Oppenheimer <omichael@Princeton.EDU>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tim\_Profeta@lieberman.senate.gov, Ben Santer <santer1@llnl.gov>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "Lonnie G. Thompson" <thompson.3@osu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>  
Subject: CONFIDENTIAL Fwd:  
Date: Sun, 26 Oct 2003 13:47:44 -0500  
Cc: mann@virginia.edu

Dear All,

This has been passed along to me by someone whose identity will remain in confidence. Who knows what trickery has been pulled or selective use of data made. Its clear that "Energy and Environment" is being run by the baddies--only a shill for industry would have republished the original Soon and Baliunas paper as submitted to "Climate Research" without even editing it. Now apparently they're at it again...

My suggested response is:

1) to dismiss this as stunt, appearing in a so-called "journal" which is already known to have defied standard practices of peer-review. It is clear, for example, that nobody we know has been asked to "review" this so-called paper

2) to point out the claim is nonsense since the same basic result has been obtained by numerous other researchers, using different data, elementary compositing techniques, etc. Who knows what sleight of hand the authors of this thing have pulled. Of course, the usual suspects are going to try to peddle this crap. The important thing is to deny that this has any intellectual credibility whatsoever and, if contacted by any media, to dismiss this for the stunt that it is..

Thanks for your help,  
mike

two people have a forthcoming 'Energy & Environment' paper that's being unveiled tomoro (monday) that -- in the words of one Cato / Marshall/ CEI type -- "will claim that Mann arbitrarily ignored paleo data within his own record and substituted other data for missing values that dramatically affected his results.

When his exact analysis is rerun with all the data and with no data substitutions, two very large warming spikes will appear that are greater than the 20th century.

Personally, I'd offer that this was known by most people who understand Mann's methodology: it can be quite sensitive to the input data in the early centuries. Anyway, there's going to be a lot of noise on this one, and knowing Mann's very thin skin I am afraid he will react strongly, unless he has learned (as I hope he has) from the past...."

---

Professor Michael E. Mann  
Department of Environmental Sciences, Clark Hall

## PETITIONERS' EXEMPLARS (PE)

University of Virginia  
Charlottesville, VA 22903

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 begin\_of\_the\_skype\_highlighting (434) 924-  
7770 end\_of\_the\_skype\_highlighting FAX: (434) 982-2137  
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

### References

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

## PETITIONERS' EXEMPLARS (PE)

PE-39

Date: Tue, 28 Jun 2005 07:21:55 -0400

---

From: "Michael E. Mann" <mann@virginia.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Re: Fwd: Re: NEED HELP!  
Date: Tue, 28 Jun 2005 07:21:55 -0400

Hi Keith,

Thanks--yes, we seem to back in the days of McCarthyism in the States. Fortunately, we have some good people who will represent us legally pro bono, and in the best case scenario, this backfires on these thugs...

The response of the wording is likely to change dramatically after consultation w/ lawyers, etc. but any feedback on the substance would nonetheless be very helpful...

thanks for both your help and your support,

mike

At 05:48 AM 6/28/2005, you wrote:

Mike

just in and seeing this for time - will digest - but do not like look or implications of this at all

Keith

At 17:00 25/06/2005, you wrote:

Tim/Keith/Phil,

Please see attached letter from the U.S. House republicans. As Tom has mentioned below, it would be very helpful if I can get feedback from you all as I proceed w/ drafting a formal response.

Thanks in advance for any help,

mike

Date: Sat, 25 Jun 2005 09:36:49 -0600

From: Tom Wigley <wigley@cgd.ucar.edu>

Organization: NCAR/CGD

User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.4) Gecko/20030624

Netscape/7.1 (ax)

X-Accept-Language: en-us, en

To: Michael Oppenheimer <omichael@princeton.edu>

Cc: "Michael E. Mann" <mann@virginia.edu>, shs@stanford.edu, dlashof@nrdc.org, jhansen@giss.nasa.gov, mmaccrac@comcast.net, santer1@llnl.gov, wigley@ucar.edu, Caspar Ammann <ammann@ucar.edu>

Subject: Re: NEED HELP!

X-UVA-Virus-Scanned: by amavisd-new at fork9.mail.virginia.edu

Mike,

There are broader implications of this, so it is important to respond well. It is a pity you have to be the guinea pig after what you have gone through already, but you have many supporters.

I would not advise a legal route. I think you need to consider this as just another set of referees' comments and respond simply, clearly and directly. On the science side the key point is that the M&M criticisms are unfounded.

Although this may be difficult, remember that this is not really a criticism of you personally, but one aspect of a criticism of the foundations of global warming science by people both inside and outside of Congress who have ulterior motives. There may, in fact, be an opportunity here. As you know, we suspect that there has been an abuse of the scientific review process at the journal editor level.

## PETITIONERS' EXEMPLARS (PE)

The method is to choose reviewers who are sympathetic to the anti-greenhouse view. Recent papers in GRL (including the M&M paper) have clearly not been reviewed by appropriate people. We have a strong suspicion that this is the case, but, of course, no proof because we do not know \*who\* the reviewers of these papers have been. Perhaps now is the time to make this a direct accusation and request (or demand) that this information be made available. In order to properly defend the good science it is essential that the reasons for bad science appearing in the literature be investigated.

The lever here is that the Subcommittee on Oversight and Investigations of the House Committee on Energy and Commerce is suggesting that your papers are bad science and asking (their point 8e) for the identity of people who reviewed your work. In response, it is completely fair and justifiable to point out that it is the papers that criticize your and related work that are bad science, and that, through the Subcommittee you can request the identities of the reviewers of all of these critical papers -- starting with M&M.

When you respond, there are a number of items that require a direct response from you alone. There are also a number of scientific points where you could give a multi-authored response. There are many people who have expertise in this area and familiarity with the scientific issues who I am sure would be willing to join you (I would be happy to do so).

At this stage, however, I would keep the group small. A few others could be added to the original email list nevertheless. I took the liberty of copying your plea and the Subcommittee's letter to Caspar Ammann, primarily because I think he can help with the scientific aspects better than most people. After all, he has been able to follow your method and reproduce your results, he has shown the flaws in M&M's work, he has investigated the bristlecone pine issue, and he has made all his software available on the web.

The others who could be added at this early stage are Ray Bradley and Malcolm Hughes, your 'co-conspirators' -- and perhaps Phil Jones, Keith Briffa and Tim Osborn. I do not know how 'powerful' these alien opinions may be in the present parochial context, but I note that the instigators of all this are Canadians and that the science has no national boundaries. Phil, Keith and Tim are useful because they have demonstrated the flaws in the von Storch work -- which is, I assume, the Science paper that the Subcommittee's letter refers to.

A word of warning. I would be careful about using other, independent paleo reconstruction work as supporting the MBH reconstructions. I am attaching my version of a comparison of the bulk of these other reconstructions. Although these all show the hockey stick shape, the differences between them prior to 1850 make me very nervous. If I were on the greenhouse deniers' side, I would be inclined to focus on the wide range of paleo results and the differences between them as an argument for dismissing them all.

I attach also a run with MAGICC using central-estimate climate model parameters (DT2x = 2.6 degC, etc. -- see the TAR), and forcings used by Caspar in the runs with paleo-CSM. I have another Figure somewhere that compares MAGICC with paleo-CSM. The agreement is nearly perfect (given that CSM has internally generated noise while MAGICC is pure signal). The support for the hockey stick is not just the paleo reconstructions, but also the model results. If one takes the best estimates of past forcing off the shelf, then the model results show the hockey stick shape. No tuning or fudging here; this is a totally independent analysis, and critics of the paleo data, if they disbelieve these data, have to explain why models

## PETITIONERS' EXEMPLARS (PE)

get the same result.

Of course, von Storch's model results do not show such good century timescale agreement, but this is because he uses silly forcing and has failed to account for the fact that his model was not in equilibrium at the start of the run (the subject of Tim Osborn et al.'s submitted paper).

This is a pain in the but, but it will all work out well in the end (unintentional pun

--

sorry). Good science will prevail.

Best wishes,

Tom.

-----  
Michael Oppenheimer wrote:

Michael:

This is outrageous. I'll contact some people who may be able to help right away.

-----

From: Michael E. Mann [<[1]mailto:mann@virginia.edu>[2]mailto:mann@virginia.edu]

Sent: Friday, June 24, 2005 4:27 PM

To: <[3]mailto:shs@stanford.edu>shs@stanford.edu;

<[4]mailto:omichael@Princeton.EDU>omichael@Princeton.EDU;

<[5]mailto:dslashof@nrdc.org>dslashof@nrdc.org;

<[6]mailto:jhansen@giss.nasa.gov>jhansen@giss.nasa.gov;

<[7]mailto:mmaccrac@comcast.net>mmaccrac@comcast.net;

<[8]mailto:santer1@llnl.gov>santer1@llnl.gov; <[9]mailto:wigley@ucar.edu>wigley@ucar.edu

Subject: NEED HELP!

Importance: High

dear all,

this was predicted--they're of course trying to make things impossible for me. I need immediate help regarding recourse for free legal advice, etc.

mike

---

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

---

e-mail: <[10]mailto:mann@virginia.edu>mann@virginia.edu Phone: (434) 924-7770  
begin\_of\_the\_skype\_highlighting (434) 924-7770 end\_of\_the\_skype\_highlighting FAX:  
(434) 982-2137  
[11]http://www.evsc.virginia.edu/faculty/people/mann.shtml

---

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

---

e-mail: mann@virginia.edu Phone: (434) 924-7770 begin\_of\_the\_skype\_highlighting (434)  
924-7770 end\_of\_the\_skype\_highlighting FAX: (434) 982-2137  
[12]http://www.evsc.virginia.edu/faculty/people/mann.shtml

--

Professor Keith Briffa,  
Climatic Research Unit  
University of East Anglia

## PETITIONERS' EXEMPLARS (PE)

Norwich, NR4 7TJ, U.K.  
Phone: +44-1603-593909 begin\_of\_the\_skype\_highlighting +44-1603-593909  
end\_of\_the\_skype\_highlighting  
Fax: +44-1603-507784  
[13]<http://www.cru.uea.ac.uk/cru/people/briffa/>

---

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

---

e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 begin\_of\_the\_skype\_highlighting (434) 924-  
7770 end\_of\_the\_skype\_highlighting FAX: (434) 982-2137  
[14]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

### References

1. <mailto:mann@virginia.edu>
2. <mailto:mann@virginia.edu>
3. <mailto:shs@stanford.edu>
4. <mailto:omichael@Princeton.EDU%3Eomichael@Princeton.EDU>
5. <mailto:dlashof@nrdc.org>
6. <mailto:jhansen@giss.nasa.gov%3Ejhansen@giss.nasa.gov>
7. <mailto:mmaccrac@comcast.net>
8. <mailto:santer1@llnl.gov%3Esanter1@llnl.gov>
9. <mailto:wigley@ucar.edu>
10. <mailto:mann@virginia.edu>
11. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
12. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
13. <http://www.cru.uea.ac.uk/cru/people/briffa/>
14. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

## PETITIONERS' EXEMPLARS (PE)

PE-40

Date: Tue, 11 Nov 2003 23:39:46 -0500

---

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

<x-flowed>  
Keith and Phil,

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

In the meantime, here is an email (copied below) to Mike Mann from McIntyre, requesting data and programs (and making other criticisms). I do wish Mike had not rushed around sending out preliminary and incorrect early responses - the waters are really muddled now. He would have done better to have taken things slowly and worked out a final response before publicising this stuff. Excel files, other files being created early or now deleted is really confusing things!

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

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

Cheers

Tim

>From: "Steve McIntyre" <smcintyre@cgxenergy.com>  
>To: "Michael E. Mann" <mann@virginia.edu>  
>Cc: "Tim Osborn" <t.osborn@uea.ac.uk>,  
> "Ross McKittrick" <rmckitri@uoguelph.ca>  
>Subject: MBH98  
>Date: Tue, 11 Nov 2003 23:39:46 -0500  
>  
>November 11, 2003  
>  
>  
>Professor Michael E. Mann  
>  
>School of Earth Sciences  
>  
>University of Virginia  
>  
>Dear Professor Mann,  
>  
>We apologize for not sending you a copy of our recent paper ("MM") in  
>Energy and Environment for comment, as we understood from your email of  
>September 25, 2003 that time constraints prevented you from considering

## PETITIONERS' EXEMPLARS (PE)

>our material. We notice that you seem to have subsequently changed your  
>mind and hope that you will both be able to clarify some points for us and  
>to rectify the public record on other points.

>

>

>1) You have claimed that we used the wrong data and the wrong  
>computational methodology. We would like to reconcile our results to  
>actual data and methodology used in MBH98. We would therefore appreciate  
>copies of the computer programs you actually used to read in data (the 159  
>data series referred to in your recent comments) and construct the  
>temperature index shown in Nature (1998) ("MBH98"), either through email  
>or, preferably through public FTP or web posting.

>

>

>2) In some recent comments, you are reported as stating that we requested  
>an Excel file and that you instead directed us to an FTP site for the  
>MBH98 data. You are also reported as saying that despite having pointed us  
>to the FTP site, you and your colleague took trouble to prepare an Excel  
>spreadsheet, but inadvertently introduced some collation errors at that  
>time. In fact, as you no doubt recall, we did not request an Excel  
>spreadsheet, but specifically asked for an FTP location, which you were  
>unable or unwilling to provide. Nor was an Excel spreadsheet ever supplied  
>to us; instead we were given a text file, pcproxy.txt. Nor was this file  
>created in April 2003. After we learned on October 29, 2003 that the  
>pertinent data was reported to be located on your FTP site  
><ftp://holocene.evsc.virginia.edu/pub>ftp://holocene.evsc.virginia.edu/pub  
>(and that we were being faulted for not getting it from there), we  
>examined this site and found it contains the exact same file (pcproxy.txt)  
>as the one we received, bearing a date of creation of August 8, 2002. On  
>October 29, 2003, your FTP site also contained the file pcproxy.mat, a  
>Matlab file, the header to which read: "MATLAB 5.0 MAT-file, Platform:  
>SOL2, Created on: Thu Aug 8 10:18:19 2002." Both files contain identical  
>data to the file pcproxy.txt emailed to one of us (McIntyre) in April  
>2003, including all collation errors, fills and other problems identified  
>in MM. It is therefore clear that the file pcproxy.txt as sent to us was  
>not prepared in April 2003 in response to our requests, nor was it  
>prepared as an Excel spreadsheet, but in fact it was prepared many months  
>earlier with Matlab. It is also clear that, had we gone to your FTP site  
>earlier, we would simply have found the same data collation as we received  
>from Scott Rutherford. Would you please forthwith issue a statement  
>withdrawing and correcting your earlier comments.

>

>

>3) In reported comments, you also claimed that we overlooked the collation  
>errors in pcproxy.txt and "slid" the incorrect data into our calculations,  
>a statement which is untrue and made without a reasonable basis. In MM, we  
>described numerous errors including, but not limited to, the collation  
>errors, indicating quite obviously that we noticed the data problems. We  
>then describe how we "firewalled" our data from the errors contained in  
>the data you provided us, by re-collating tree ring proxy data from  
>original sources and carrying out fresh principal component calculations.  
>We request that you forthwith withdraw the claim that we deliberately used  
>data we knew to be in error.

>

>

>4) On November 8, 2003, when we re-visited your FTP site, we noticed the

## PETITIONERS' EXEMPLARS (PE)

>following changes since October 29, 2003: (1) the file pcproxy.mat had  
>been deleted from your FTP site; (2) the file pcproxy.txt no longer was  
>displayed under the /sdr directory, where it had previously been located,  
>although it could still be retrieved through an exact call if one  
>previously knew the exact file name; (3) without any notice, a new file  
>named "mbhfilled.mat" prepared on November 4, 2003 had been inserted into  
>the directory. Obviously, the files pcproxy.mat and pcproxy.txt are  
>pertinent to the comments referred to above and we view the deletion of  
>pcproxy.mat from the archival record under the current circumstances as  
>unjustifiable. Would you please restore these files to your FTP site,  
>together with an annotated text file documenting the dates of their  
>deletion and restoration.

>

>

>5) We note that the new file mbhfilled.mat is an array of dimension  
>381x2016. Could you state whether this file has any connection to MBH98,  
>and, if so, please explain the purpose of this file, why it has been  
>posted now and why it was not previously available at the FTP site.

>

>

>6) Can you advise us whether the directory MBH98 has been a subdirectory  
>within the folder "pub" since July 30, 2002 or whether it was transferred  
>from another (possibly private) directory at a date after July 30, 2002?  
>If the latter, could you advise on the date of such transfer.

>

>

>We have prepared a 3-part response to your reply to MM. The first, which  
>we have released publicly, goes over some of the matters raised in points  
>#2-#5 above. The second is undergoing review. It deals with additional  
>issues of data quality and disclosure, resulting from inspection of your  
>FTP site since October 29, 2003. The third part will consider the points  
>made in your response, both in terms of data and methodology, and will  
>attempt a careful reconciliation of our calculation methods, hence the  
>necessity of our request in point #1. Thank you for your attention.

>

>

>Yours truly,

>

>Stephen McIntyre

Ross McKittrick

>

>

>cc: Timothy Osborn

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

e-mail: [t.osborn@uea.ac.uk](mailto:t.osborn@uea.ac.uk)

phone: +44 1603 592089

fax: +44 1603 507784

web: <http://www.cru.uea.ac.uk/~timo/>

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