CRU CORRESPONDENCE
##

256. 1011732147.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Stepan Shiyatov <stepan@ipae.uran.ru>
Subject: Re: INTAS final money
Date: Tue Jan 22 15:42:27 2002

Stepan
I have the form , but it is not clear . Where I think I sign (page 1 bottom. under
co-ordinator) it says I have to prove my identity in Brussels?
I will phone them to ask before sending the form back. Will Eugene need a similar signature?
Keith
At 12:15 PM 1/21/02 +0500, you wrote:

Dear Keith,
As I realized, our team must receive from INTAS the final sum of 737
EURO. I can get these money via Ekaterinburg Branch of VNESHTORGBANK,
as we did earlier. I am sending to you "Payment request" for
this sum, and you, as the coordinator, must sign it and send to
Brussels. In that case I can receive money in Ekaterinburg.
Last two months I was very bisy writing many reports for our activity
in 2001. From that days I will begin to work with material obtained
from the Polar Urals, mainly cartographic and photographic ones.
WE intent to take part at PAGES meeting which will be in May in
Moscow.
I wish you, your family and colleaques the best in New Year.
Best Regards,
Stepan stepan@ipae.uran.ru

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

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

References

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

257. 1014240346.txt
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From: Keith Briffa <k.briffa@uea.ac.uk>
To: tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: Re: SCIENCE review
Date: Wed Feb 20 16:25:46 2002

Date: Fri, 30 Nov 2001 13:48:13 +0000

To: "Jesse Smith" <hjsmith@aaas.org>
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: SCIENCE review
Dear Jesse
I am sorry for messing you about with this but I really am leading a complicated life at the moment. I am attaching my comments on The Esper et al manuscript . You will see that I think the work is genuinely interesting and potentially of wide significance. The bottom line is that you should publish this but the way the authors have chosen to present their results smacks of a lack of clarity of thought (and a lot of fudging!) . I believe that they are more concerned with trying to temper their ideas so as not to "offend" Mann et al. They choose to present their work as a generalised demonstration of how to process a tree-ring data set merely to argue against an unjustified remark made by Broecker about tree-ring reconstructions in general. This simply devalues the significance of their work as this refutation is out their in the literature already if only Broecker bothered to check. By trying to skate around the real questions that Broecker was implying - i.e. is the methodology removing the true low-frequency variance in the Mann et al curve and is the magnitude of the Medieval warmth understated ? - Esper et al are obscuring the real message of their results - namely that Mann et al do most likely loose the low frequency variance in their reconstruction and they may very well be underestimating the Medieval warmth . To get at this the authors need to be honest about what their data represent (probably summer and certainly not hemispheric wide coverage) and is this really that different from what Mann et al actually represent (even though they believe their's is a mean annual Hemispheric record).
I think the authors present a too-simplistic discussion of their curve and then gloss over these difficult but important issues.
So I really think they should be published , but they should think again about the interpretation and message .
At 09:25 AM 11/27/01 -0500, you wrote:

Dear Keith,
No, it is not too late, so please send your review. Thanks a million.
Sincerely,
Jesse
======================
Dr. Jesse Smith
Associate Editor

Science
1200 New York Avenue, NW
Washington, DC 20005
USA
----
(202) 326-6556

(202) 408-1256 (FAX)
hjsmith@aaas.org
=======================
>>> Keith Briffa <k.briffa@uea.ac.uk> 11/27/01 09:17AM >>>
Is it too late for this or should I send a review by tomorrow?
Keith

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

References

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

258. 1015388778.txt
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##########

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: questions
Date: Tue, 05 Mar 2002 23:26:18 -0500

<x-flowed>
I cut Hammer ref
I just thanked "ll those who provided data"
I was looking at Graybill & Shiyatov Fig 20.6, but you are right that the
warmest period was after 1160....though some argue the MWP extends into the
14th century....certainly it shows a cold 11th century. So I'lll cut that
reference, as requested...

I leave it to you to contact Dave Fisher as I don't know what he sent
you...so get back to me asap

Ray

>for the melt record (l) use .
>
>2. "Intercomparison of....techniques", Fisher and others.1996. Nato
>ASI Vol 141, "Climate variations and forcingmechanisms of the last
>2000 yrs", Springer Verlag etc. pp 297-328.
>Can not track down yet where the low re one came from (can you ask Dave

>directly)
>Other points are ok
>Did you track down the Hammer ref (some European conference) ?
>Do you need list of acknowledgements yet? Should include
>Mike Salmon for drawing the figure
>and Fisher, Black, Luterbacher, presumably Johnsson ,Bianchi,Kegwin,
>van Engelen,Keith Barber and Darrel.Maddy, for the data I used.
>I am really pushed , sorry about brief reponse- honest.
>Keith
>
>
>At 10:46 PM 3/4/02 -0500, you wrote:
>>yes--they do show a MWp in shiyatov and graybill 1992--but i added briffa
>>2000, too.
>>i still need a response to my last email
>>ray
>>
>>
>>Raymond S. Bradley
>>Distinguished Professor and Head of Department
>>Department of Geosciences
>>University of Massachusetts
>>Amherst, MA 01003-5820
>>
>>Tel: 413-545-2120
>>Fax: 413-545-1200
>>Climate System Research Center: 413-545-0659
>>Climate System Research Center Web Page:
>><http://www.geo.umass.edu/climate/climate.html>
>>Paleoclimatology Book Web Site (1999):
>>http://www.geo.umass.edu/climate/paleo/html
>>
>
>--
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>Climatic Research Unit
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>

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

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259. 1016746746.txt
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##########

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@ltrr.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) that 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/ alterior 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 FAX: (434) 982-2137
[1]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

260. 1016818778.txt
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##########

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, 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: Briffa & Osborn piece
Date: Fri, 22 Mar 2002 12:39:38 -0500

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

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.[2]shtml
Attachment Converted: "c:\eudora\attach\treerings-comments1.doc"

References

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

261. 1016831188.txt
##

From: "drdendro@ldeo.columbia.edu" <drdendro@ldeo.columbia.edu>
To: "mann@multiproxy.evsc.virginia.edu" <mann@multiproxy.evsc.virginia.edu>, "k.briffa@uea.ac.uk" <k.briffa@uea.ac.uk>, "t.osborn@uea.ac.uk" <t.osborn@uea.ac.uk>, "p.jones@uea.ac.uk" <p.jones@uea.ac.uk>, "tcrowley@duke.edu" <tcrowley@duke.edu>, "rbradley@geo.umass.edu" <rbradley@geo.umass.edu>, "mhughes@ltrr.arizona.edu" <mhughes@ltrr.arizona.edu>, "drdendro@ldeo.columbia.edu" <drdendro@ldeo.columbia.edu>, "rkerr@aaas.org" <rkerr@aaas.org>, "bhanson@aaas.org" <bhanson@aaas.org>
Subject: RE: Briffa & Osborn piece
Date: Fri, 22 Mar 2002 16:06:28 -0500
Reply-to: drdendro@ldeo.columbia.edu

Hi Mike and others,

I just read the AP release. As always, there is a bit of journalistic license that was applied to interpreting what I said. The opening statement in the release is utterly the words of the reporter. Some of the quotes are probably accurate, but of course do not include qualifiers, etc. I also talked with this journo before talking with you and would phrase things a bit more carefully now after hearing your concerns. So, I am not deceiving you in what I told you over the phone. I would not express things the same way as you in any case, because I do think that we have some legitimate differences of opinion on some issues, although I think we agree much more than we disagree. Be that as it may, talking over the phone to journalists in a rapid-fire manner is not the best way to convey ideas and information and I would have re-phrased or re-expressed some of what was written if I had seen it before it was released. This was not an option provided to me.

I think that it is a bit harse to say that the paper should not have been published. While I might wish to change some wording in the paper and express things a bit differently knowing what I know now, I don't think that the paper is fatally flawed, like you do. I should also point out that I have received a number of emails from respected scientists in global change research who do not appear to share your opinion. On the other hand, I have also received a couple of emails from certified nuts, which is what you are obviously most concerned about. I am not happy with such people, but I have also been savaged by similar nuts like John Daly in the past. So, I guess I can't win.

Finally, this whole global change debate totally sucks because it is so politicized. It reminds me too much of the ugly acid rain/forest decline debate that I was caught in the middle of years ago. I am quite happy to leave global change to others in the future.

Ed

Original Message:

From: Michael E. Mann mann@multiproxy.evsc.virginia.edu
Date: Fri, 22 Mar 2002 12:39:38 -0500
To: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, 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: Briffa & Osborn piece

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
____

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

http://mail2web.com/ .

262. 1016896740.txt
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From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: drdendro@ldeo.columbia.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
Subject: Op-Ed
Date: Sat, 23 Mar 2002 10:19:00 -0500

<x-flowed>
Ed:
I just waded through all the correspondence with Mike re the Science paper and Keef's commentary. I wish to disassociate myself with Mike's comments, or at least the tone of them. I do not consider myself the final arbiter of what Science should publish, nor do I consider what you did to signify the end of civilization as we know it. Life goes on--now we have another working hypothesis to examine. Great...one of these days we'll really know what happened....until then, I find all these efforts to be really interesting. That's not to say I agree with everything you said or did, but then I don't suppose you are too enamoured of what I've done in the past either. C'est la vie.
Ray

Raymond S. Bradley
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University of Massachusetts
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Climate System Research Center: 413-545-0659
Climate System Research Center Web Page:
<http://www.geo.umass.edu/climate/climate.html>
Paleoclimatology Book Web Site (1999):
http://www.geo.umass.edu/climate/paleo/html

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263. 1018045075.txt
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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 of all 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:

     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
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Charlottesville, VA 22903

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

   --

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

References

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

264. 1018539404.txt
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##########

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Edward Cook <drdendro@ldeo.columbia.edu>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>
Subject: Re: Your letter to Science
Date: Thu, 11 Apr 2002 11:36:44 -0400
Cc: esper@wsl.ch, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk,
tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu,
srutherford@virginia.edu, mann@virginia.edu

Ed,
It will take some time to digest these comments, but my initial response is one of some
disappointment. I will resist the temptation to make the letter to Science available to the
others on this list, because of my fears of violating the embargo policy (I know examples
of where doing so has led to Science retracting a piece form publication). So thanks for
also resisting the temptation to do so...
But I must point out that the piece by Malcolm and me is very similar in its content to
the letter of clarification that you and I originally crafted to send to Science some weeks
ago, before your co-author objected to your involvement! If there is no objection on your
part, I'd be happy to send that to everyone, because it is not under consideration in
Science (a quite unfortunate development, as far as I'm concerned). The only real change
from that version is the discussion of the use of RCS. That is in large part Malcolm's
contribution, but I stand behind what Malcolm says. I think there are some real sins of
omission with regard to the use of RCS too, and it would be an oversight on our part now to
comment on these.
Finally, with regard to the scaling issues, let me simply attach a plot which speaks more
loudly than several pages possibly could The plot takes Epser et al (not smoothed, but the
annual values) and scales it against the full Northern Hemisphere instrumental record
1856-1990 annual mean record, and compares against the entire 20th century

instrumental
   record (1856-1999), as well as with MBH99 and its uncertainties.
   Suppose that Esper et al is indeed representative of the full Northern Hemisphere annual
   mean, as MBH99 purports to be. To the extent that differences emerge between the two in
   assuming such a scaling, I interpret them as differences which exist due to the fact that
   the extratropical Northern Hemisphere series and full Northern Hemisphere series likely did
   not co-vary in the past the same way they co-vary in the 20th century (when both are driven
   predominantly, in a relative sense, by anthropogenic forcing, rather than natural forcing
   and internal variability). What the plot shows is quite remarkable. Scaled in this way,
   there is remarkably little difference between Esper et al and MBH99 in the first place (the
   two reconstructions are largely within the error estimates of MBH99!)!, but moreover, where
   they do differ, this could be explainable in terms of patterns of enhanced mid-latitude
   continental response that were discussed, for example, in Shindell et al (2001) in Science
   last December. So I think this plot says a lot. Its say that there are some statistically
   significant differences, but certainly no grounds to use Esper et al to contradict MBH99 or
   IPCC '2001 as, sadly, I believe at least one of the published pieces tacitly appears to
   want to do.
   It is shame that such a plot, which I think is a far more meaningful comparison of the two
   records, was not shown in either Esper et al or the Briffa & Osborn commentary. I've always
   given the group of you adequate opportunity for commentary on anything we're about to
   publish in Nature or Science. I am saddened that many of my colleagues (and, I have always
   liked to think friends) didn't affort me the same opportunity before this all erupted in
   our face. It could have been easily avoided. But that's water under the bridge.
   Finally, before any more back-and-forths on this, I want to make sure that everyone
   involved understands that none of this was in any way ever meant to be personal, at least
   not on my part (and if it ever has, at least on my part, seemed that way, than I offer my
   apologies--it was never intended that way). This is completely about the "science". To the
   extent that I (and/or others) feel that the science has been mis-represented in places,
   however, I personally will work very hard to make sure that a more balanced view is
   available to the community. Especially because the implications are so great in this case.
   This is what I sought to do w/ the NYT piece and my NPR interview, and that is what I've
   sought to do (and Malcolm to, as far as I'm concerned) with the letter to Science. Being a
   bit sloppy w/ wording, and omission, etc. is something we're all guilty of at times. But I
   do consider it somewhat unforgivable when it is obvious how that sloppiness can

be
exploited. And you all know exactly what I'm talking about!
So, in short, I think are some fundamental issues over which we're in disagreement, and
where those exist, I will not shy away from pointing them out. But I hope that is not
mis-interpreted as in any way personal.
I hope that suffices,
Mike
p.s. It seemed like an omission to not cc in Peck and Scott Rutherford on this exchange, so
I've done that. I hope nobody minds this addition...
At 10:57 AM 4/11/02 -0400, Edward Cook wrote:

Hi Mike and Malcolm,
I have received the letter that you sent to Science and will respond to it here first in
some detail and later in edited and condensed form in Science. Since much of what you
comment and criticize on has been disseminated to a number of people in your (Mike's)
somewhat inflammatory earlier emails, I am also sending this lengthy reply out to
everyone on that same email list, save those at Science. I hadn't responded in detail
before, but do so now because your criticisms will soon be in the public domain.
However, I am not attaching your letter to Science to this email since that is not yet
in the public domain. It is up to you to send out your submitted letter to everyone if
you wish.
I must say at the beginning that some parts of your letter to Science are as "flawed" as
your claims about Esper et al. (hereafter ECS). The Briffa/Osborn perspectives piece
points out an important scaling issue that indeed needs further examination. However, to
claim as you do that they show that the ECS 40-year low-pass temperature reconstruction
is "flawed" begs the question: "flawed" by how much? It is not at all clear that
scaling the annually resolved RCS chronology to annually resolved instrumental
temperatures first before smoothing is the correct way to do it. The ECS series was
never created to examine annual, or even decadal, time-scale temperature variability.
Rather, as was clearly indicated in the paper, it was created to show how one can
preserve multi-centennial climate variability in certain long tree-ring records, as a
refutation of Broecker's truly "flawed" essay. As ECS showed in their paper (Table 1),
the high-frequency correlations with NH mean annual temperatures after 20-year high-pass
filtering is only 0.15. That result was expected and it makes no meaningful difference
if one uses only extra-tropical NH temperature data. So, while the amplitude of the
temperature-scaled 40-year low-pass ECS series might be on the high end (but still
plausible given the gridded borehole temperature record shown in Briffa/Osborn), scaling

on the annually resolved data first would probably have the opposite effect of
excessively reducing the amplitude. I am willing to accept an intermediate
value, but
probably not low enough to satisfy you. Really, the more important result from
ECS is
the enhanced pattern of multi-centennial variability in the NH extra-tropics
over the
past 1100 years. We can argue about the amplitude later, but the enhanced
multi-centennial variability can not be easily dismissed. I should also point
out,
again, that you saw Fig. 3 in ECS BEFORE it was even submitted to Science and
never
pointed out the putative scaling "flaw" to me at that time.
With regards to the issue of the late 20th century warming, the fact that I did
not
include some reference to or plot of the up-to-date instrumental temperature
data (cf.
Briffa/Osborn) is what I regard as a "sin of omission". What I said was that
the
estimated temperatures during the MWP in ECS "approached" those in the 20th
century
portion of that record up to 1990. I don't consider the use of "approached" as
an
egregious overstatement. But I do agree with you that I should have been a bit
more
careful in my wording there. As you know, I have publicly stated that I never
intended
to imply that the MWP was as warm as the late 20th century (e.g., my New York
Times
interview). However, it is a bit of overkill to state twice in the closing
sentences of
the first two paragraphs of your letter that the ECS results do not refute the
unprecedented late 20th century warming. I would suggest that once is enough.
ECS were also very clear about the extra-tropical nature of their data. So,
what you
say in your letter about the reduced amplitude in your series coming from the
tropics,
while perhaps worth pointing out again, is beating a dead horse. However, I
must say
that the "sin of omission" in the Briffa/Osborn piece concerning the series
shown in
their plot is a bit worrying. As they say in the data file of series used in
their plot
(and in Keith's April 5 email response to you), Briffa/Osborn only used your
land
temperature estimates north of 20 degrees and recalibrated the mean of those
estimates
to the same domain of land-only instrumental temperatures using the same
calibration
period for all of the other non-borehole series in the same way. I would have
preferred
it if they had used your data north of 30N to make the comparisons a bit more
one-to-one. However, I still think that their results are interesting. In
particular,
they reproduce much of the reduced multi-centennial temperature variability
seen in your
complete NH reconstruction. So, if the amplitude of scaled ECS
multi-centennial
variability is far too high (as you would apparently suggest), it appears that
it is
also too low in your estimates for the NH extra-tropics north of 20N. I think
that we
have to stop being so aggressive in defending our series and try to understand

the
     strengths and weaknesses of each in order to improve them. That is the way
that science
     is supposed to work.
     I must admit to being really irritated over the criticism of the ECS tree-ring
data
     standardized using the RCS method. First of all, ECS acknowledged up front the
     declining available data prior to 1200 and its possible effect on interpreting
an MWP in
     the mean record. ECS also showed bootstrap confidence intervals for the mean
of the RCS
     chronologies and showed where the chronologies drop out. Even allowing for the
reduction
     in the number of represented sites before 1400 (ECS Fig. 2d), and the reduction
in
     overall sample size (ECS Fig. 2b), there is still some evidence for
significantly above
     average growth during two intervals that can be plausibly assigned to the MWP.
Of course
     we would like to have had all 14 series cover the past 1000-1200 years. This
doesn't
     mean that we can't usefully examine the data in the more weakly replicated
intervals.
     In any case, the replication in the MWP of the ECS chronology is at least as
good as in
     other published tree-ring estimates of large-scale temperatures (e.g., NH
     extra-tropical) covering the past 1000+ years. It also includes more long
tree-ring
     records from the NH temperate latitudes than ever before. So to state that
"this is a
     perilous basis for an estimate of temperature on such a large geographic scale"
is
     disingenuous, especially when it is unclear how many millennia-long series are
     contributing the majority of the temperature information in the
Mann/Bradley/Hughes
     (MBH) reconstruction prior to AD 1400. Let's be balanced here.
     I basically agree with the closing paragraph of your letter. The ECS record
was NEVER
     intended to refute MBH. It was intended, first and foremost, to refute
Broecker's essay
     in Science that unfairly attacked tree rings. To this extent, ECS succeeded
very well.
     The comparison of ECS with MBH was a logical thing to do given that it has been
accepted
     by the IPCC as the benchmark reconstruction of NH annual temperature
variability and
     change over the past millennium. Several other papers have made similar
comparisons
     between MBH and other even more geographically restricted estimates of past
     temperature. So, I don't apologize in the slightest for doing so in ECS. The
     correlations in Table 2 between ECS and MBH were primarily intended to
demonstrate the
     probable large-scale, low-frequency temperature signal in ECS independent of
explicitly
     calibrating the individual RCS chronologies before aggregating them. The
results should
     actually have pleased you because, for the 20-200 year band, ECS and MBH have
     correlations of 0.60 to 0.68, depending on the period used. Given that ECS is
based on
     a great deal of new data not used in MBH, this result validates to a reasonable
degree
     the temperature signal in MBH in the 20-200 year band over the past 1000 years.
     Given the incendiary and sometimes quite rude emails that came out at the time

when ECS
and Briffa/Osborn were published, I could also go into the whole complaint about how the
review process at Science was "flawed". I will only say that this is a very dangerous
game to get into and complaints of this kind can easily cut both ways. I will submit an
appropriately edited and condensed version of this reply to Science.
Regards,
Ed
--

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____
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[1]http://www.evsc.virginia.edu/faculty/people/mann.[2]shtml
Attachment Converted: "c:\eudora\attach\esper-scaledcompare1980.jpg"

References

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

265. 1018623296.txt
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From: Tom Crowley <tcrowley@duke.edu>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: peace
Date: Fri, 12 Apr 2002 10:54:56 -0400
Cc: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

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Dear friends,

I am concerned about the the stressed tone of some of the words being circulated lately. Such difficulties not only hamper collegiality (which I value greatly) but also the actual progress in our field.

I think you are all fine fellows and very good scientists and that it is time to smoke the peace pipe on all this and put a temporary moratorium on more email messages until tempers cool down a bit.

After this maybe we can discuss things somewhere where each party
comes to the meeting beforehand with a commitment to even-handed
discussion and give and take.

I hope I have not offended anyone in this message -- it is of course
a personal opinion. Maybe it is an illusion or prejudice on my part,
but somehow I am not convinced that the "truth" is always worth
reaching if it is at the cost of damaged personal relationships....

Best wishes, Tom

--
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##########

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

References

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

267. 1018647333.txt
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Your letter to Science
Date: Fri, 12 Apr 2002 17:35:33 -0400
Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

Dear Ed, Tom, Keith, etc.

In keeping w/ the spirit of Tom's and Keith's emails, I wanted to stress, before we all
break for the weekend, that this is ultimately about the science, its not personal. If my
comments seemed to assail e.g. Keith's motives or integrity, etc. I believe that they were
misunderstood (as I tried to clarify that in my previous message), but I can see that
there was a potential for misunderstanding of my message (precision in wording is very
important) given the high levels of sensitivity in this debate. So I wanted to leave no
uncertainty about that. And of course, I very much apologize to Keith (and Tim) if they
took them my comments that way. They, again, were most decidedly not intended that way.
I hope we can resolve the scientific issues objectively, and w/out injecting or any
personal feelings into any of this. There are some substantial scientific differences here,
lets let them play out the way they are supposed to, objectively, and in the peer reviewed
literature.
Enjoy the weekend all.
cheers,
Mike
At 01:35 PM 4/12/02 -0400, Ed Cook wrote:

Hi Mike, Tom, etc,
Okay, I am quite happy to give this debate a rest, although I am sure that the issues
brought up will still be grounds for scientific debate. I admit to getting a bit riled
when I saw the ECS results on the MWP described as "perilous" because I regard that as
being an unfair characterization of the work presented. Be that as it may, my reply to
Science will be very carefully worded so as not to inflame the issues. Nuff said. Have a
good weekend. I certainly intend to do so.
Ed

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
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
At 10:21 AM 4/12/02 -0400, Ed Cook wrote:

Just a few comments here and then I'm done.

Dear Ed and Mike and others,
All of our attempts, so far, to estimate hemisphere-scale
temperatures for the period around 1000 years ago are
based on far fewer data than any of us would like. None
of the datasets used so far has anything like the
geographical distribution that experience with recent
centuries indicates we need, and no-one has yet found a
convincing way of validating the lower-frequency
components of them against independent data. As Ed
wrote, in the tree-ring records that form the backbone of
most of the published estimates, the problem of poor
replication near the beginnings of records is particularly
acute, and ubiquitous. I would suggest that this problem
probably cuts in closer to 1600 than 1400 in the several

published series. Therefore, I accept that everything we
are doing is preliminary, and should be treated with
considerable caution.

Therefore, I would guess that you would apply the word "perilous" to everyones'
large-scale NH reconstructions covering the past 500-1000 years including those
that you
have been involved in. Why the sudden increase in caution now? It sounds very
self-serving to me for you to call ECS "perilous" and not describe every other
large-scale reconstruction in that way as well.

I differ from Ed, and his co-authors,
in believing that these problems have a special
significance for the particular implementation of RCS
they used, in the light of one of their conclusions that
depends heavily on that implementation.
As I understand what Ed, Keith and Hal Fritts have
written at various times about RCS, and from my own
limited experience with the method, it is extremely
important to have strong replication, and I don't see 50-70
samples probably from 25-35 trees as a big sample. For
reference, most chronologies used in dendroclimatology
are based on 10-40 trees, that is 20-80 samples at 2 cores
per tree for a single "site", usually a few hectares.
Here are two passages from Briffa et al., 1992:
page 114, column 1, last paragraph, "For a chronology
composed of the same number of samples, one would
therefore expect a larger statistical uncertainty using this
approach than in a chronology produced using
standardization curves fitted to the data from individual
trees..............The RCS method therefore requires greater
chronology depth (i.e. greater sample replication) to
provide the same level of confidence in its representation
of the hypothetical "true" chronology." ECS mention this
issue.

As I said in my previous email, we hid nothing in terms of the uncertainty
concerning
the pre-1200 interval. Are you suggesting that we should not have even shown
those
results? If so, that is ridiculous.

page 114, column 1, third paragraph, there is a discussion
of the problems arising from applying RCS when pith age
is not known, "In the ring-width data, the final
standardization curve probably slightly underestimates
the width of young trees and could therefore impart a
small positive bias to the standardized ring-width indices
for young rings in a number of series. However, this
effect will be insignificant when the biased indices are
realigned according to calendar growth years and
averaged with many other series." The problem here is
that this latter condition is not met (in my view), and the
"small positive bias" that may be retained could turn out
to be important to the most controversial conclusion of
ECS (the Medieval question).

I can't speak for Jan here, but most of the data he used came from
Schweingruber's lab.
I believe that pains were taken to estimate the pith offset and that Jan used
this
information in his RCS analyses. Jan would be best to comment here. In any
case, Jan has

done a number of experiments in which he has artificially added large pith offset errors
into the RCS analysis and the resulting bias is small. So, I do not believe that your
"view" is correct.

I also suspect that Keith
and colleagues underestimated both the size and
variability of the loss of years at the beginning of records,
but the point stands even if this is not so. So far as I can
see, ECS do not mention this issue, at least in the context
of a possible positive bias.

Are you claiming that the only possible bias is positive? I can show you examples of a
probable negative bias using RCS.

The discussion of RCS in the
supplementary materials seems to assume good
replication.

It was a generic description of the method. The replication is clearly shown in the
supplementary materials section as well as in the main paper. If you don't like the
replication, that is your opinion. I would love to have more replication as well. Who
wouldn't. But we did show the uncertainties, which you seem to ignore in your criticism.
Ironically, the ECS estimates of warmth in the MWP are not that dissimilar to those seen
in MBH, as ECS Fig. 3 shows. Are the MBH estimates of MWP warmth also similarly biased?

ECS, as Ed rightly points out, clearly indicate, in both
words and diagrams at several points in their paper and in
the supplementary materials, that the number of sites and
number of samples they used decreases sharply before
1200. Even so, ECS gives prominence (second sentence
of the abstract, for example) to the reconstruction in that
very period, and makes a comparison with the magnitude
of 20th-century warming. All the methods, and their
realizations so far, have significant problems. In our letter
(Mike and I) we draw attention to a specific problem with
this implementation of RCS that has a special bearing on
the reconstruction of a period to which ECS have drawn
attention. Hence the strong note of caution about the ECS
conclusion on the comparison between the 10th/11th and
late 20th centuries.
I hope it's clear from this that I don't disagree with the
general proposition that all existing reconstructions of
hemipsphere-scale temperatures 1000 years ago (or even
for all the first half of the second millennium AD) should
be viewed as very preliminary. If anyone is interested I
attach a short note on the replication in the year AD 1000
of records used in MBH99 to give an idea of what we are
up against.

There is obviously a lot more we can debate about here. I will simply stop here by
saying that I stand by the results shown in ECS and will say so in my reply to your
letter, pointing out that the use of the word "perilous" could be just as

easily be
applied to MBH.

We all have a lot to do. I see four important tasks - 1) more investigation of the strengths and limitations of methods like RCS and age-banding - for example, how many samples would have been enough in this case, does the RC change through time? and so on; 2) use of tree-ring records where the loss of low-frequency information is least - those with long segments from open stands; 3) the search for tree-ring parameters without age/size related trend; 4) the development of completely independent proxies with intrinsically better low-frequency fidelity.
Cheers, Malcolm
The Briffa et al reference is to the 1992 paper, Climate Dynamics, 7:111-119

Hi Ed,
OK--thanks for your response. I'll let Malcolm respond to the technical issues regarding RC. I'm not really qualified to do so myself anyway. Your other points are well taken...
Cheers,
Mike
At 12:09 PM 4/11/02 -0400, Edward Cook wrote:
Hi Mike,
Thanks for the reply. I too do not want to see anything personal in our disagreements. It would be a shame if it got to that and it shouldn't. I don't think that the science we are talking about is sufficiently known yet to claim the "truth", which is why we are having some of our disagreements. I mainly wanted to clarify some issues relating to some criticisms of the ECS results that I thought were not totally fair. My biggest complaint is with Malcolm's contribution to your letter because it really isn't fair to use such words as "perilous". ECS did not hide anything and the uncertainties are clearly indicated in EGS

> Figs. 2 and 3. So, you can make your own judgement. However,

Malcolm's opinion does not invalidate the ECS record. If Malcolm's statement is correct, than ALL previous estimates of NH temperature over the past 1000 years are "perilous", especially before AD 1400 when the number of series available declines significantly in most records.
Ed
Ed,
It will take some time to digest these comments, but my initial response is one of some disappointment. I will resist the temptation to make the letter to Science available to the others on this list, because of my fears of violating the embargo policy (I know examples of where doing so has led to Science retracting a piece form publication). So thanks for also resisting the temptation to do so...
But I must point out that the piece by Malcolm and me is very similar in its content to the letter of clarification that you and I originally crafted to send to Science some weeks ago, before your co-author objected to your involvement! If there is no objection on your part, I'd be happy to send that to everyone, because it is not under consideration in Science (a quite unfortunate development, as far as I'm concerned). The only real change from that version is the discussion of the use of RCS. That is in large part Malcolm's contribution, but I stand behind what

> Malcolm says. I think there are some real sins of omission with

regard to the use of RCS too, and it would be an oversight on our part now to comment on these.
Finally, with regard to the scaling issues, let me simply attach a plot which speaks more loudly than several pages possibly could The plot takes Epser et al (not smoothed, but the annual values) and scales it against the full Northern Hemisphere instrumental record 1856-1990 annual mean record, and compares against the entire 20th century instrumental record (1856-1999), as well as with MBH99 and its uncertainties.
Suppose that Esper et al is indeed representative of the fullNorthern Hemisphere annual mean, as MBH99 purports to be. To the extent that differences emerge between the two in assuming such a scaling, I interpret them as differences which exist due to the fact that the extratropical Northern Hemisphere series and full Northern Hemisphere series likely did not co-vary in the past the same way they co-vary in the 20th century (when both are driven predominantly, in a relative sense, by anthropogenic forcing, rather than natural forcing and internal variability). What the plot shows is quite remarkable. Scaled in this way, there is remarkably little difference between Esper et al and MBH99 in the first place (the two reconstructions are largely within the error estimates of MBH99!)!, but moreover, where they do differ, this could be explainable in terms of patterns of enhanced mid-latitude continental response that were discussed, for example, in Shindell et al (2001) in Science last December. So I think this plot says a lot. Its say that there are some statistically significant differences, but certainly no grounds to use Esper et al to contradict MBH99 or IPCC '2001 as, sadly, I believe at least one of the published pieces tacitly appears to want to do.
It is shame that such a plot, which I think is a far more meaningful comparison of the two records, was not shown in either Esper et al or the Briffa & Osborn commentary. I've always given the group of you adequate opportunity for commentary on anything we're about to publish in Nature or Science. I am saddened that many of my colleagues (and, I have always liked to think friends) didn't affort me the same opportunity before this all erupted in our face. It could have been easily avoided. But that's water under the bridge.

>

Finally, before any more back-and-forths on this, I want to make sure that everyone involved understands that none of this was in any way ever meant to be personal, at least not on my part (and if it ever has, at least on my part, seemed that way, than I offer my apologies--it was never intended that way). This is completely about the "science". To the extent that I (and/or others) feel that the science has been mis-represented in places, however, I personally will work very hard to make sure that a more balanced view is available to the community. Especially because the implications are so great in this case. This is what I sought to do w/ the NYT piece and my NPR interview, and that is what I've sought to do (and Malcolm to, as far as I'm concerned) with the letter to Science. Being a bit sloppy w/ wording, and omission, etc. is

something we're all guilty of at times. But I do consider it
somewhat unforgivable when it is obvious how that sloppiness can
be exploited. And you all know exactly what I'm talking about!
So, in short, I think are some fundamental issues over
which we're in disagreement, and where those exist, I will
not shy away from pointing them out. But I hope that is
not mis-interpreted as in any way personal.
I hope that suffices,

>

Mike
p.s. It seemed like an omission to not cc in Peck and
Scott Rutherford on this exchange, so I've done that. I
hope nobody minds this addition...
At 10:57 AM 4/11/02 -0400, Edward Cook wrote:
Hi Mike and Malcolm,
I have received the letter that you sent to Science
and will respond to it here first in some detail and
later in edited and condensed form in Science.
Since much of what you comment and criticize on
has been disseminated to a number of people in
your (Mike's) somewhat inflammatory earlier
emails, I am also sending this lengthy reply out to
everyone on that same email list, save those at
Science. I hadn't responded in detail before, but
do so now because your criticisms will soon be in
the public domain. However, I am not attaching
your letter to Science to this email since that is
not yet in the public domain. It is up to you to
send out your submitted letter to everyone if you
wish.
I must say at the beginning that some parts of
your letter to Science are as "flawed" as your
claims about Esper et al. (hereafter ECS). The
Briffa/Osborn perspectives piece points out an
important scaling issue that indeed needs further
examination. However, to claim as you do that
they show that the ECS 40-year low-pass
temperature reconstruction is "flawed" begs the
question: "flawed" by how much? It is not at all
clear that scaling the annually resolved RCS
chronology to annually resolved instrumental
temperatures first before smoothing is the correct
way to do it. The ECS series was never created to
examine annual, or even decadal, time-scale
temperature variability. Rather, as was clearly
indicated in the paper, it was created to show how
one can preserve multi-centennial climate
variability in certain long tree-ring records, as a
refutation of Broecker's truly "flawed" essay. As
ECS showed in their paper (Table 1), the high-
frequency correlations with NH mean annual
temperatures after 20-year high-pass filtering is
only 0.15. That result was expected and it makes
no meaningful difference if one uses only extra-
tropical NH temperature data. So, while the
amplitude of the temperature-scaled 40-year low-
pass ECS series might be on the high end (but
still plausible given the gridded borehole
temperature record shown in Briffa/Osborn),
scaling on the annually resolved data first would
probably have the opposite effect of excessively

> reducing the amplitude. I am willing to accept an

intermediate value, but probably not low enough
to satisfy you. Really, the more important result
from ECS is the enhanced pattern of multi-
centennial variability in the NH extra-tropics over
the past 1100 years. We can argue about the
amplitude later, but the enhanced multi-centennial
variability can not be easily dismissed. I should
also point out, again, that you saw Fig. 3 in ECS
BEFORE it was even submitted to Science and
never pointed out the putative scaling "flaw" to
me at that time.
With regards to the issue of the late 20th century
warming, the fact that I did not include some
reference to or plot of the up-to-date instrumental
temperature data (cf. Briffa/Osborn) is what I
regard as a "sin of omission". What I said was
that the estimated temperatures during the MWP
in ECS "approached" those in the 20th century
portion of that record up to 1990. I don't consider
the use of "approached" as an egregious
overstatement. But I do agree with you that I
should have been a bit more careful in my
wording there. As you know, I have publicly
stated that I never intended to imply that the
MWP was as warm as the late 20th century (e.g.,

> my New York Times interview). However, it is a

bit of overkill to state twice in the closing
sentences of the first two paragraphs of your
letter that the ECS results do not refute the
unprecedented late 20th century warming. I
would suggest that once is enough.
ECS were also very clear about the extra-tropical
nature of their data. So, what you say in your
letter about the reduced amplitude in your series
coming from the tropics, while perhaps worth
pointing out again, is beating a dead horse.
However, I must say that the "sin of omission" in
the Briffa/Osborn piece concerning the series
shown in their plot is a bit worrying. As they say
in the data file of series used in their plot (and in
Keith's April 5 email response to you),
Briffa/Osborn only used your land temperature
estimates north of 20 degrees and recalibrated the
mean of those estimates to the same domain of
land-only instrumental temperatures using the
same calibration period for all of the other non-
borehole series in the same way. I would have
preferred it if they had used your data north of
30N to make the comparisons a bit more one-to-
one. However, I still think that their results are
interesting. In particular, they reproduce much of
the reduced multi-centennial temperature
variability seen in your complete NH
reconstruction. So, if the amplitude of scaled
ECS multi-centennial variability is far too high
(as you would apparently suggest), it appears that
it is also too low in your estimates for the NH
extra-tropics north of 20N. I think that we have

to stop being so aggressive in defending our
series and try to understand the strengths and
weaknesses of each in order to improve them.
That is the way that science is supposed to work.
I must admit to being really irritated over the
criticism of the ECS tree-ring data standardized
using the RCS method. First of all, ECS
acknowledged up front the declining available
data prior to 1200 and its possible effect on
interpreting an MWP in the mean record. ECS
also showed bootstrap confidence intervals for
the mean of the RCS chronologies and showed
where the chronologies drop out. Even allowing
for the reduction in the number of represented
sites before 1400 (ECS Fig. 2d), and the
reduction in overall sample size (ECS Fig. 2b),
there is still some evidence for significantly
above average growth during two intervals that
can be plausibly assigned to the MWP. Of course

> we would like to have had all 14 series cover the

past 1000-1200 years. This doesn't mean that we
can't usefully examine the data in the more
weakly replicated intervals. In any case, the
replication in the MWP of the ECS chronology is
at least as good as in other published tree-ring
estimates of large-scale temperatures (e.g., NH
extra-tropical) covering the past 1000+ years. It
also includes more long tree-ring records from the
NH temperate latitudes than ever before. So to
state that "this is a perilous basis for an estimate
of temperature on such a large geographic scale"
is disingenuous, especially when it is unclear how
many millennia-long series are contributing the
majority of the temperature information in the
Mann/Bradley/Hughes (MBH) reconstruction
prior to AD 1400. Let's be balanced here.
I basically agree with the closing paragraph of
your letter. The ECS record was NEVER
intended to refute MBH. It was intended, first
and foremost, to refute Broecker's essay in
Science that unfairly attacked tree rings. To this
extent, ECS succeeded very well. The
comparison of ECS with MBH was a logical
thing to do given that it has been accepted by the

> IPCC as the benchmark reconstruction of NH

annual temperature variability and change over
the past millennium. Several other papers have
made similar comparisons between MBH and
other even more geographically restricted
estimates of past temperature. So, I don't
apologize in the slightest for doing so in ECS.
The correlations in Table 2 between ECS and
MBH were primarily intended to demonstrate the
probable large-scale, low-frequency temperature
signal in ECS independent of explicitly
calibrating the individual RCS chronologies
before aggregating them. The results should
actually have pleased you because, for the 20-200
year band, ECS and MBH have correlations of

0.60 to 0.68, depending on the period used.
Given that ECS is based on a great deal of new
data not used in MBH, this result validates to a
reasonable degree the temperature signal in MBH
in the 20-200 year band over the past 1000 years.
Given the incendiary and sometimes quite rude
emails that came out at the time when ECS and
Briffa/Osborn were published, I could also go
into the whole complaint about how the review
process at Science was "flawed". I will only say
that this is a very dangerous game to get into and
complaints of this kind can easily cut both ways.
I will submit an appropriately edited and
condensed version of this reply to Science.
Regards,
Ed
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[4]http://www.evsc.virginia.edu/faculty/people/mann.[5]shtml

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Your letter to Science
Date: Mon, 15 Apr 2002 12:44:53 -0400
Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk,
p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu,
srutherford@virginia.edu, mann@virginia.edu

   HI Tim,
   Thanks for your message. Yes, you guys have us beat on the early monday end of things!
   Your points are all taken. I think we all agree there is much work left to be done, more
   than enough for all of us to continue to be involved in constructive collaboration, etc.
   Scott and I, for example, are almost done writing up the work based on your visit w/ us
   last year, and will send the initial draft on to you, Keith, and the others involved in the
   near future. It will be a good chance to try to address a lot of these questions in an
   article of adequate length to discuss the nuances that unfortunately cannot be addressed in
   a shorter piece.
   I also appreciate your more detailed comments about the comparisons, etc. Your points are
   all reasonable ones. We can maintain an honest difference about how well those points were
   conveyed in the Science piece (for example, you can imagine how the statement in your piece
   "This record has a smaller amplitude of century-to-century variability, and is consistently
   at or near the upper limit of alternate records produced by other researchers" might indeed
   have been interpreted as setting MBH99 apart as, in your words, an "outlier").
   We have good reason to believe that our reconstruction *will* in fact nderestimate
   extratropical temperature means but far less so full globe/hemisphere-means prior to the
   18th century because the basis functions that primarily set the extratropics apart from the
   full hemispheric patterns (e.g., NAO type patterns and other anomaly patterns largely
   carried by EOFs #2 and #3) start to drop out from our basis set prior to the 18th century,
   while the pattern that best resolves the full global and/or hemispheric mean (with note
   from MBH98, particularly large loadings primarily in the tropics and subtropics) still
   remains. That is why we have never published an *extratropical* temperature reconstruction
   prior to the 18th century. I would be happy to discuss this point with you and Keith and
   others in more detail. Thus, I have compared Esper et al w/ our records in the manner
   described in my previous email, which I think allows us to diagnose the extent to which
   differing high-latitude and full-hemispheric patterns may, at times, explain the somewhat
   modest differences between the records when similarly scaled to the full hemispheric
   1856-1990 mean, and always, within the context of the diagnosed uncertainties. There is no
   guarentee, as you say, that the uncertainties are correct, but I personally believe they'll
   stand up over time. You can call me on this 10 years from now, and somebody will owe
   somebody a beer...
   In any case, I hope and fully expect we can all continue to all be engaged in constructive

interaction & hopefully continued collaboration. It will require some sensitivity on all
our part to the larger issues surrounding our work, and the way it gets presented to the
broader community, but I don't think that should be all that difficult.
I look forward to these more constructive interactions. I'll do my best to foster them,
Mike
At 01:57 PM 4/15/02 +0100, Tim Osborn wrote:

Dear all,
well, the time zone may let you have the last word before the weekend, but we can get
the first word in on a Monday morning!
At 22:35 12/04/02, Michael E. Mann wrote:

In keeping w/ the spirit of Tom's and Keith's emails, I wanted to stress, before we all
break for the weekend, that this is ultimately about the science, its not personal. If
my comments seemed to assail e.g. Keith's motives or integrity, etc. I believe that they
were misunderstood (as I tried to clarify that in my previous message), but I can see
that there was a potential for misunderstanding of my message (precision in wording is
very important) given the high levels of sensitivity in this debate. So I wanted to
leave no uncertainty about that. And of course, I very much apologize to Keith (and Tim)
if they took them my comments that way. They, again, were most decidedly not intended
that way.

Thanks for clarifying that, Mike. I think that both Keith and I interpreted your
earlier e-mail as being more critical of us than you actually meant it to be.
Most issues surrounding the recent Esper et al. and Briffa & Osborn pieces seem to have
been covered adequately already. There are just a couple of issues on which I'd like to
add a few comments, hopefully clarifying the situation rather than opening up more
avenues for debate.
The first relates to the purpose and style of the Briffa & Osborn piece. Perspectives
are brief, non-technical and not peer-reviewed. Our instructions were: "The Perspective
should provide an overview of recent research in the field and explain to the general
reader why the work is particularly exciting." Is it any surprise then that we should
focus on the new insights provided by the Esper et al. work, and that it suggests a
different climate history than earlier work? And that the constraints of the
perspectives format (in terms of length, audience and style) prevented us from listing
ALL the caveats and uncertainties related to this and earlier reconstructions and that
might be of relevance to their intercomparison? I don't think it is surprising, nor do
I think we should be criticised for it.
Moreover, despite the constraints of the perspectives format, I think we were

very
careful with our wording to avoid misleading the reader. The reference to the IPCC, for
example, was not at all sloppy - the opposite, in fact, since it was very carefully
worded: the IPCC Synthesis Report is referred to, rather than the full TAR, and it is
quite true that there is a focus on the reconstruction of Mann et al. in the former. As
Mike says, IPCC conclusions were based on other work too. But I'd guess that many of
the readers of our perspective won't have read the full IPCC report, so we thought it
valid to focus on the difference between the new work and that shown in the Synthesis
Report (which more will have seen). To do this is certainly not unfair to the IPCC. It
would only have been unfair if we had implied that the IPCC had ignored this new work -
but of course we weren't doing that, because how could one expect the TAR to consider
work that is published a year after the TAR itself? We were similarly careful with our
wording in our brief mention of the MWP, by saying it is "more pronounced" in Esper et
al. - this doesn't mean it is warmer than the others (and thus has no implications for
the IPCC conclusion of recent unusual warmth), rather it is pronounced because it is
followed by stronger cooling.

The second issue is our re-calibration of the reconstructions. While it hasn't been
explicitly stated, I get the impression that this is considered by some to be a poor
thing to do. The particular re-calibration we do has a number of effects, including
making the Mann et al. reconstruction appear more consistently at the top of the range
of alternatives. But please let me assure you (Mike, Ray and Malcolm) that the reason
for re-calibrating the records is definitely *not* to make your record appear as an
outlier, and I hope you believe me. Indeed, in Jones, Osborn & Briffa (2001: Science
292, 662-667) we showed various NH records *without* applying our re-calibration.

We produced our first comparison of records for an earlier Science perspectives piece in
1999 (Briffa & Osborn, 1999) and thought it would be useful to do a re-calibration to
remove some of the reasons for inter-reconstruction differences (which can be due to:
different proxy data, different statistical methods, different calibration target and
different calibration period). The latter two reasons were removed by re-calibrating
against a common target series and over a common period. We updated this in Briffa et
al. (2001) and acknowledged that the target series (in terms of its spatial and seasonal
definition) may not be optimal in all cases. Indeed, it may be especially sub-optimal
for Mann et al., because their reconstruction approach combines the proxy

records to
optimally reconstruct full NH, annual mean T (whereas we have selected land
north of
20N, warm-season T as our target for the recalibration). Despite this, we felt
justified in doing the recalibration because the Mann et al. series still
outperformed
the others in terms of its correlation with the instrumental record over the
calibration
period! In our latest piece, we have updated the intercomparison in two ways
(as well
as including new series): (i) we took the spatially-resolved gridded
reconstructions of
Mann et al. and extracted only land boxes north of 20N; and (ii) we used
annual, not
warm-season, temperature as the target. The first of these (as explained by
Keith and I
in an earlier e-mail, which is repeated below because it didn't get sent to all
of you
firs time round) deals with all the points raised by Mike about tropical versus
extratropical differences. I would again argue that we were not sloppy,
because these
changes to our intercomparison were carefully thought out.
So that explains what we have done and why. There is some sensitivity,
clearly, to
calibration choices, which implies to me that the true uncertainty ranges are
probably
larger than those estimated solely from the statistical properties of
calibration
residuals (as used by Briffa et al., and [I think] by Mann et al.). There is
clearly
more progress to be made!
Best regards to you all
Tim

Date: Fri, 05 Apr 2002 17:17:55 +0100
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>,p.jones@uea.ac.uk,
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From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Briffa & Osborn piece
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 of all 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.
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the text, so we can see why you may not have appreciated this, but we think you will
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than you realised. Blame *Science* for being so mean with their space allocation if you
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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
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"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)

Dr Timothy J Osborn | phone: +44 1603 592089
Senior Research Associate | fax: +44 1603 507784
Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
School of Environmental Sciences | web-site:
University of East Anglia __________| [1]http://www.cru.uea.ac.uk/~timo/

Norwich NR4 7TJ | sunclock:
UK | [2]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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

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

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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.evsc.virginia.edu/faculty/people/mann.shtml

269. 1018893474.txt
##

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Your letter to Science
Date: Mon Apr 15 13:57:54 2002
Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

Dear all,
well, the time zone may let you have the last word before the weekend, but we can get the first word in on a Monday morning!
At 22:35 12/04/02, Michael E. Mann wrote:

In keeping w/ the spirit of Tom's and Keith's emails, I wanted to stress, before we all break for the weekend, that this is ultimately about the science, its not personal. If my comments seemed to assail e.g. Keith's motives or integrity, etc. I believe that they were misunderstood (as I tried to clarify that in my previous message), but I can see that there was a potential for misunderstanding of my message (precision in wording is very important) given the high levels of sensitivity in this debate. So I wanted to leave no uncertainty about that. And of course, I very much apologize to Keith (and Tim) if they took them my comments that way. They, again, were most decidedly not intended that way.

Thanks for clarifying that, Mike. I think that both Keith and I interpreted your earlier e-mail as being more critical of us than you actually meant it to be.
Most issues surrounding the recent Esper et al. and Briffa & Osborn pieces seem to have been covered adequately already. There are just a couple of issues on which I'd

like to
   add a few comments, hopefully clarifying the situation rather than opening up more avenues
   for debate.
   The first relates to the purpose and style of the Briffa & Osborn piece. Perspectives are
   brief, non-technical and not peer-reviewed. Our instructions were: "The Perspective should
   provide an overview of recent research in the field and explain to the general reader why
   the work is particularly exciting." Is it any surprise then that we should focus on the
   new insights provided by the Esper et al. work, and that it suggests a different climate
   history than earlier work? And that the constraints of the perspectives format (in terms
   of length, audience and style) prevented us from listing ALL the caveats and uncertainties
   related to this and earlier reconstructions and that might be of relevance to their
   intercomparison? I don't think it is surprising, nor do I think we should be criticised
   for it.
   Moreover, despite the constraints of the perspectives format, I think we were very careful
   with our wording to avoid misleading the reader. The reference to the IPCC, for example,
   was not at all sloppy - the opposite, in fact, since it was very carefully worded: the IPCC
   Synthesis Report is referred to, rather than the full TAR, and it is quite true that there
   is a focus on the reconstruction of Mann et al. in the former. As Mike says, IPCC
   conclusions were based on other work too. But I'd guess that many of the readers of our
   perspective won't have read the full IPCC report, so we thought it valid to focus on the
   difference between the new work and that shown in the Synthesis Report (which more will
   have seen). To do this is certainly not unfair to the IPCC. It would only have been
   unfair if we had implied that the IPCC had ignored this new work - but of course we weren't
   doing that, because how could one expect the TAR to consider work that is published a year
   after the TAR itself? We were similarly careful with our wording in our brief mention of
   the MWP, by saying it is "more pronounced" in Esper et al. - this doesn't mean it is warmer
   than the others (and thus has no implications for the IPCC conclusion of recent unusual
   warmth), rather it is pronounced because it is followed by stronger cooling.
   The second issue is our re-calibration of the reconstructions. While it hasn't been
   explicitly stated, I get the impression that this is considered by some to be a poor thing
   to do. The particular re-calibration we do has a number of effects, including making the
   Mann et al. reconstruction appear more consistently at the top of the range of
   alternatives. But please let me assure you (Mike, Ray and Malcolm) that the reason for
   re-calibrating the records is definitely *not* to make your record appear as an outlier,

and I hope you believe me. Indeed, in Jones, Osborn & Briffa (2001: Science 292, 662-667)
we showed various NH records *without* applying our re-calibration.
We produced our first comparison of records for an earlier Science perspectives piece in
1999 (Briffa & Osborn, 1999) and thought it would be useful to do a re-calibration to
remove some of the reasons for inter-reconstruction differences (which can be due to:
different proxy data, different statistical methods, different calibration target and
different calibration period). The latter two reasons were removed by re-calibrating
against a common target series and over a common period. We updated this in Briffa et al.
(2001) and acknowledged that the target series (in terms of its spatial and seasonal
definition) may not be optimal in all cases. Indeed, it may be especially sub-optimal for
Mann et al., because their reconstruction approach combines the proxy records to optimally
reconstruct full NH, annual mean T (whereas we have selected land north of 20N, warm-season
T as our target for the recalibration). Despite this, we felt justified in doing the
recalibration because the Mann et al. series still outperformed the others in terms of its
correlation with the instrumental record over the calibration period! In our latest piece,
we have updated the intercomparison in two ways (as well as including new series): (i) we
took the spatially-resolved gridded reconstructions of Mann et al. and extracted only land
boxes north of 20N; and (ii) we used annual, not warm-season, temperature as the target.
The first of these (as explained by Keith and I in an earlier e-mail, which is repeated
below because it didn't get sent to all of you firs time round) deals with all the points
raised by Mike about tropical versus extratropical differences. I would again argue that
we were not sloppy, because these changes to our intercomparison were carefully thought
out.
So that explains what we have done and why. There is some sensitivity, clearly, to
calibration choices, which implies to me that the true uncertainty ranges are probably
larger than those estimated solely from the statistical properties of calibration residuals
(as used by Briffa et al., and [I think] by Mann et al.). There is clearly more progress
to be made!
Best regards to you all
Tim

Date: Fri, 05 Apr 2002 17:17:55 +0100
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
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Briffa & Osborn piece

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 of all 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)

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270. 1019513684.txt
##

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: SSI Alert: IPCC Chair Vote]
Date: Mon Apr 22 18:14:44 2002
Cc: s.raper

Phil,
I can't quite see what all the fuss is about Watson - why should he be re-nominated
anyway? Why should not an Indian scientist chair IPCC? One could argue the CC issue is
more important for the South than for the North. Watson has perhaps thrown his weight
about too much in the past. The science is well covered by Susan Solomon in WGI, so why
not get an engineer/economist since many of the issues now raised by CC are more to do with
energy and money, than natural science.
If the issue is that Exxon have lobbied and pressured Bush, then OK, this is regrettable
but to be honest is anyone really surprised? All these decisions about IPCC chairs and
co-chairs are deeply political (witness DEFRA's support of Martin Parry for getting the
WGII nomination).
Mike
At 07:17 20/04/02 +0100, you wrote:

There is more on the BBC Sci/Tech web site.
Phil

Date: Fri, 19 Apr 2002 18:24:58 -0600
From: Tom Wigley <wigley@ucar.edu>
X-Mailer: Mozilla 4.76 [en] (Windows NT 5.0; U)
X-Accept-Language: en
To: Phil Jones <p.jones@uea.ac.uk>, Sarah Raper <s.raper@uea.ac.uk>,
Mike Hulme <m.hulme@uea.ac.uk>
Subject: [Fwd: SSI Alert: IPCC Chair Vote]
You may not have seen this latest piece of politicization by the
Bushies.

Tom.
****
-------- Original Message --------
Subject: SSI Alert: IPCC Chair Vote
Date: Fri, 19 Apr 2002 18:00:59 -0400
From: "SSI Mailbox" <ssi@ucsusa.org>
******************* EXECUTIVE SUMMARY *******************
ISSUE: Today - April 19, 2002, the Intergovernmental Panel on Climate Change (IPCC) plenary voted for Dr. Rajendra Pachauri as the sole chair of the IPCC. Dr. Pachauri, an economist and engineer, will replace Dr. Robert Watson, an atmospheric chemist, as chair of the IPCC. This outcome was actively sought by the Bush Administration at the behest of the most conservative elements of the fossil fuel industry. This development threatens to undermine the scientific credibility and integrity of the IPCC and may weaken the job this extraordinary body has done to bring the world's attention to one of the most pressing environmental problems.
ACTION: Monitor your local paper and respond to news stories with a letter-to-the-editor.
MAIN MESSAGE: Given the Bush Administration's consistent opposition to climate change mitigation, it is especially imperative at this time that the scientific community and Dr. Pachauri work together to ensure that the IPCC remains a strong and credible scientific process.
DEADLINE: As soon as possible after the story runs in your paper -- preferably the same day but no later than a day or two after.
****
*** THE ISSUE ***
According to a report by Associated Press today (appended below), Dr. Rajendra Pachauri was elected as Chair of the IPCC at a plenary meeting in Geneva. As you would be aware from our earlier SSI alerts of the past several weeks, this follows on from intense lobbying of the US government by the fossil fuel industry to remove Dr. Robert Watson as Chair. Although reports from Geneva are still sketchy, our sources on the ground tell us that there was intense behind-the-scenes lobbying by Saudi Arabia, with assistance from Don Pearlman -- a well known oil and gas lobbyist with strong connections to industry-backed organizations opposed to climate change mitigation. Through their maneuvering, the co-chair compromise approach -- comprised of former chair Dr. Robert Watson and Dr. Pachauri -- was not considered.
As a result of this election, there is considerable concern in the climate science and environmental communities -- reinforced by the intensive lobbying from fossil fuel interests on this decision -- that the Bush Administration's lack of support for former IPCC Chair Dr. Robert Watson signals a more general lack of support for the IPCC as a credible international scientific assessment process that provides governments with sound information on climate science, impacts, and solutions.
By supporting Dr. Pachauri for primarily political purposes, the Bush Administration has seriously threatened the scientific credibility of the IPCC process. The conservative fossil fuel interests should be exposed for their role in influencing the US government's stance on this issue, and the IPCC process must remain a scientifically credible and non-politicized process.
The next IPCC Climate Change Assessment is due out in five years, and it is the chair's role to oversee this complex

process. The scientific community's voice is important in
this issue to ensure that the IPCC process remains strong
under the leadership of Dr. Pachauri and that the Bush
Administration does not erode the effectiveness of this
important international body.
*** THE ACTION ***
-- Monitor your local paper and respond to news stories with
a letter-to-the-editor.
Information on how and to whom to submit a LTE is usually
found right on the Letters Page in your paper. Many papers
now accept letters via email. If you can't find the
information you need, simply call the paper and ask how to
go about submitting a letter in response to a recently
published article.
To increase the chances that your letter will be published,
do the following:
- keep it under 200 words and stay focused on one or two
main points you'd like to make;
- focus on a local angle, if possible, that adds something
new to the story that appeared in your paper;
- be sure to include your name, address, and daytime phone
number; the paper will contact you before printing your
letter; and
- submit the letter on the same day the story appears, if
possible.
[For additional help with writing an effective letter to the
editor, you may turn to the reference guide on the SSI
member page at <[1]http://www.ucsusa.org/ssimembers/index.html >.]
-- MAIN MESSAGE: Given the Bush Administration's consistent
opposition to climate change mitigation, it is especially
imperative at this time that the scientific community and
Dr. Pachauri work together to ensure that the IPCC remains a
strong and credible scientific process.
-- TIMING: Your letter to the editor should reach your paper
within a few days of the publication of the story to
increase the chances of it being published.
-- SPECIAL NOTE: If your paper did not carry the story at
all yet, send an LTE describing the story and emphasizing
that this issue is of great interest to the paper's
subscribers.
*** SUPPORTING MESSAGES ***
-- [Be sure to include a description of your scientific
expertise, your involvement with the IPCC process, or the
importance of the climate issue to your community.]
-- For the past 10 years, the IPCC's science has been the
foundation for sound policymaking on the climate issue. The
IPCC's unique intergovernmental approach to scientific
consensus has worked amazingly well but is now threatened.
-- It is disturbing that the Bush Administration sought and
received advice from the fossil fuel industry on the
leadership of an important scientific body such as the IPCC.
A politicized IPCC threatens the integrity and credibility
of the scientific process.
-- There are fears that it will now be easier for the US to
distance itself from the IPCC process. You may point out
that the US already rejected the Kyoto protocol last year.
-- It is vital that the scientific process for the next
Assessment Report (due out in another five years) not be
compromised so that the IPCC continues to produce sound
science on climate change.
-- The credibility of the IPCC's Third Assessment Report
(TAR) findings were strongly affirmed by the US National
Academy of Sciences (NAS), which published its supportive

report in response to President Bush's request for an
independent assessment on the state of climate science.
*** SUPPLEMENTAL INFORMATION ***
-- Dr Rajendra K. Pachauri is an Indian engineer and
economist. Pachauri, formerly one of the five vice chairs of
the IPCC, is highly regarded but will be the first non-
atmospheric chemist as chair of the IPCC.
-- For more information on the ExxonMobil memo urging the
Bush Administration to remove Dr. Watson from his position
as IPCC Chair, please see
< [2]http://www.nrdc.org/media/docs/020403.pdf >.
-- For information on the Saudi/Pearlman connection, see the
summary by Jeremy Leggett, author of "The Carbon War", at
< [3]http://www.carbonwar.com/ccchrono.html >.
-- IPCC - Intergovernmental Panel on Climate Change: The
Intergovernmental Panel on Climate Change (IPCC) was
established in 1988 under the auspices of the United Nations
Environment Programme and the World Meteorological
Organization for the purpose of assessing "the scientific,
technical and socioeconomic information relevant for the
understanding of the risk of human-induced climate change."
To date, the IPCC has issued three comprehensive
assessments. The first assessment report (FAR) was released
in 1990, the second assessment report (SAR) was released in
1996, and the third assessment report (TAR) was released in
2001. These assessments are based on "published and peer
reviewed scientific technical literature"
For more information see < [4]http://www.ipcc.ch >
****
NOTE: Please send us an email message that tells us what
action you took. If you actually send a letter, please send
us a "blind copy." (A blind copy simply means that you do
not indicate anywhere on your letter that you are sending a
copy to us.) Send to: ssi@ucsusa.org or UCS, 2 Brattle
Square, Cambridge, MA 02238-9105 (attn. Jason Mathers).
CHANGE OF EMAIL ADDRESS: Help us keep you posted! If your
email address will soon change, or if you'd like us to use a
different address, please let us know by sending a message
to ssi@ucsusa.org with your new address. Thanks!
****
Associated Press
Fri Apr 19, 1:18 PM ET
U.S. scientist voted off international climate panel
By JONATHAN FOWLER, Associated Press Writer
GENEVA - A U.S. scientist was voted off an international
climate panel Friday following what campaigners claimed was
pressure from the oil industry and Washington.
Atmospheric scientist Robert Watson was seeking re-election
as head of the Intergovernmental Panel on Climate Change.
World Meteorological Organization (news - web sites)
spokeswoman Mo Lagarde said Watson was defeated by Indian
challenger Rajendra Pachauri. Some 76 countries supported
Pachauri, while 49 voted for Watson in the secret ballot,
she said.
Seven nations voted for Jose Goldemberg, a Brazilian (news -
web sites) who entered the race this week.
The WMO and the U.N. Environment Program jointly host the
IPCC's offices and organized the Geneva meeting.
Environmental groups have accused the administration of
President George W. Bush (news - web sites) of caving in to
a request from Exxon Mobil that it try to remove Watson, a
leading expert on global warming (news - web sites), because
he had consistently warned governments of the dangers of

climate change.
"The fossil fuel industry and the U.S. government will be celebrating their success in kicking out Bob Watson, an experienced scientist who understood that urgent action is needed to tackle global climate change," said Kate Hampton, international climate co-ordinator for British-based Friends of the Earth (news - web sites). "The Bush administration and its friends would rather shoot the messenger than listen to the message," Hampton said in a statement.
The Swiss-based Worldwide Fund for Nature said it was worried by the "apparent politicization" of the IPCC.
"WWF is concerned that oil and gas interests had too much to say in the removal of Dr. Watson as chairman of what should be an impartial, scientific body," said Jennifer Morgan, Director of WWF's Climate Program.
But, Morgan said, the "IPCC is a vibrant group of scientists and WWF looks forward to working closely with Dr. Pachauri to protect the integrity of the IPCC and ensure that it continues to produce sound science on climate change."
The U.S. State Department said earlier this month that it would support Pachauri, who was the Indian government's nominee, to become the next chair.
Two weeks ago, the Natural Resources Defense Council, a Washington, D.C.-based environmental group, said the White House's Council on Environmental Quality received a memo from Exxon Mobil in February 2001 that asked, "Can Watson be replaced now at the request of the U.S.?"
The memo, which the group said it obtained through the Freedom of Information Act, also recommended that the administration "restructure the U.S. attendance at upcoming IPCC meetings to assure none of the Clinton/Gore proponents are involved in any decisional activities."
U.S. officials were unavailable for comment.
Watson has been an outspoken proponent of the idea that fossil fuel emissions contribute to rising global temperatures. He has led the panel since 1996 and is also the chief scientist of the World Bank (news - web sites).
Pachauri is an engineer and an economist and is the director of the Tata Energy Research Institute in New Delhi, India.

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

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2. http://www.nrdc.org/media/docs/020403.pdf
3. http://www.carbonwar.com/ccchrono.html%A0
4. http://www.ipcc.ch/

271. 1021757151.txt
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From: Mike Hulme <m.hulme@uea.ac.uk>
To: s.torok

Subject: In Tyndall
Date: Sat May 18 17:25:51 2002

Simon,
A version of this for In Tyndall please - you should add the relevant EPSRC web site if you
can track it down.
Mike
____

EPSRC invests in adventurous ideas
EPSRC is to establish an adventurous research fund. A total of £4.5 million has been
earmarked for research projects that include a mixture of disciplines and as such may face
barriers to selection under EPSRCs core research programmes. The pilot initiative will be
launched with a call for outline proposals at the end of May. The closing date will be at
end
of July. Those successful at the outline stage will be asked to submit full proposals by
December. The new funds principal novelty is an emphasis on funding people to work in
other disciplines or between disciplines. EPSRC will fund any research project that falls
within its centre of gravity. We are happy for it to be 49 per cent in another research
council remit, so long as the majority is in the EPSRC remit, says Hylton. Equally, EPSRC
has not capped how much money people can apply for. Another key difference is the way in
which the proposals will be evaluated. It will be a two-stage process with outline
proposals
followed by full proposals. The outline stages of applying to the adventure fund are to be
assessed anonymously. In addition, the initiative will have its own bespoke outline
application form, proposal form and referees assessment form. EPSRC also hopes the
initiative will go some way to changing UK research culture.

272. 1024334440.txt
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From: Ed Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Esper et al. and Mike Mann
Date: Mon, 17 Jun 2002 13:20:40 -0400

<x-flowed>
Hi Keith,

Of course, I agree with you. We both know the probable flaws in
Mike's recon, particularly as it relates to the tropical stuff. Your
response is also why I chose not to read the published version of his
letter. It would be too aggravating. The only way to deal with this
whole issue is to show in a detailed study that his estimates are
clearly deficient in multi-centennial power, something that you
actually did in your Perspectives piece, even if it was not clearly
stated because of editorial cuts. It is puzzling to me that a guy as

bright as Mike would be so unwilling to evaluate his own work a bit
more objectively.

Ed

>I have just read this lettter - and I think it is crap. I am sick to
>death of Mann stating his reconstruction represents the tropical
>area just because it contains a few (poorly temperature
>representative) tropical series. He is just as capable of
>regressing these data again any other "target" series , such as the
>increasing trend of self-opinionated verbage he has produced over
>the last few years , and ... (better say no more)
>Keith
>

--
==================================
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
Fax: 845-365-8152
==================================
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>,rbradley@geo.umass.edu,
k.briffa@uea.ac.uk,mhughes@lttr.arizona.edu,t.osborn@uea.ac.uk,
srutherford@virginia.edu,mann@virginia.edu
Subject: Re: AGU abstract
Date: Tue, 13 Aug 2002 10:16:42 +0100

Mike,
Checked with Keith and Tim. The abstract is like one we would write - leaves
all
options
open as to what will be presented. At least AGU and EGS don't charge to get
abstracts
printed. AMS have so many missing now with their charges that the book of
abstracts is
ridiculous. Fine for all three of us to be there and we look forward to seeing
some
results
in the autumn. This will be when the real action begins.
The CCDD meeting in early Nov. might be at a good time to discuss some
results.
Add an 'of' between choice and actual on the third line.
Cheers
Phil
At 19:56 12/08/02 -0400, Michael E. Mann wrote:

Dear all,
The following is an abstract for a talk I've been invited to give at the winter
AGU
meeting in a session on "Climate of the Past 2000 Years". I would like to

summarize the
collaborative work that was begun by Scott, Tim and myself a couple summers ago during
Tim's visit here. Scott is working on finalizing the results of our analyses now, and a
draft should be available for review shortly that compares reconstructions based on our
covariance-based reconstruction method, using (i) multiproxy, (ii) MXD, and (ii)
combined multiproxy+MXD datasets for different (cold, warm, annual) target seasonal
windows.   I'd like to invite everyone listed below to be authors on both this abstract,
and the paper that we're in the process of drafting, describing the results. I've kept
the abstract intentionally vague, so that we can work out an interpretation of the
results that we're all comfortable with in the months ahead, prior to the talk, and
submission of the paper.

I look forward to confirmation of your interest in being a co-author, and any feedback
you have. I'd like to submit this by the end of the week, which will be my last
opportunity to do so prior to the AGU abstract deadline, owing to my travel schedule.

thanks in advance for getting back to me ASAP.

best regards,

Mike

Progress in Proxy-Based Reconstruction of Surface Temperature Variations in Past
Centuries

Michael E. Mann
Raymond Bradley
Keith Briffa
Malcolm Hughes
Philip Jones
Timothy Osborn
Scott Rutherford

Results are presented from a set experiments designed to control for the various factors
that may influence reconstructions of large-scale temperature patterns in past
centuries, including (a) the choice actual proxy data used, (b) the reconstruction
methodology, (c) the spatial domain of the reconstruction and (d) the seasonal window
targeted. These experiments compare results based both on the global multiproxy data set
used by Mann and coworkers and the extratropical Northern Hemisphere maximum latewood
tree-ring density set used by Briffa and coworkers. Estimates of hemispheric mean
temperature trends are formed both through averaging of   large-scale patterns
reconstructed from full proxy data network, and through simple compositing of regional
temperature reconstructions. Northern hemisphere mean estimates are compared for the
full Northern hemisphere (tropics and extratropics, land and ocean), and extratropical
continents only, and using various (cold-season half year, warm-season half year, and
annual mean) seasonal targets for the reconstructions. Implications of these experiments

for the robustness of proxy-based reconstructions of past large-scale temperature trends
are discussed.

____
Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903
____
e-mail: mann@virginia.edu   Phone: (434) 924-7770   FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

Prof. Phil Jones
Climatic Research Unit   Telephone +44 (0) 1603 592090
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Norwich   Email   p.jones@uea.ac.uk
NR4 7TJ
UK

References

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

274. 1029966978.txt
##

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Yamal paper for The Holocene special issue
Date: Wed, 21 Aug 2002 17:56:18 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,
thank you very much for editing our paper.
It's a pity you strike your name off the list of authors, you make an important contribution to writing paper. Your corrections and additions surely improve paper.

I would only notice the next sentence (page 8):

'The low interannual variability and the minimum occurrence of cold extremes during the 20th century, argue that the most recent decades of this long summer record represent the most favourable climate conditions for tree growth within the last four millennia.'

I'm not sure that this statement follows unambiguous from results presented in this paper. Because mean temperatures during last decades, according presented reconstruction, are not exceptional. Besides, e.g. period about 1700 BC, according this reconstruction, represent probably the same conditions taking into account low variability, low occurrence of extremes and high mean temperature.
May be to soften this statement and replace 'the most favourable' with something like 'highly favourably' or 'probably the most favourable'?

Thank you once more for invaluable assistance.

Best regards,
Rashit M. Hantemirov

(I'm sorry for the late answer, I just come back from the trip to
the north.)

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e-mail: rashit@ipae.uran.ru
Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92
http://ipae.uran.ru/8personalies/dendro.html#3

275. 1031762366.txt
##

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Iain Brown (UKCIP)" <iain.brown@ukcip.org.uk>
Subject: Re: temporal interpolation for UKCIP scenarios
Date: Wed Sep 11 12:39:26 2002
Cc: geoff.jenkins@metoffice.com,x.lu,j.turnpenny

Iain (and Geoff),
Definitive explanations are always dangerous! The reasoning behind this is as follows:
- the report only analysed and pictured seasonal and annual data (DJF,MAM, etc.) [in fact,
nearly all published maps of climate model outputs show changes in seasonal - 3-month -
averages]. This applying a uniform filter over 90 or 360 days.
- the requested datasets are at monthly time-steps. The default option for this is in
effect applying a uniform 30-day filter. [one might also conceive of weekly or daily
time-step files - e.g. changes in Week 13 for the 2050s for precip. for Medium-High or
changes for Julian day number 256 for the 2080s for Tmin for Low].
- these are all arbitrary choices of course, dictated by convention. But the important
point it seems to me is again a signal to noise issue - the shorter the time-averaging
period, the weaker the S/N ratio [i.e., we have more confidence that averaged over a year,
Tmin in the UK will increase by, say, 2.7degC for certain scenario, than that for the same
scenarios Tmin on 13 June will increase - on average - by 2.6degC and on 14 June only by
2.3degC - is this difference between 2.6 on 13 June and 2.3 on 14 June really meaningful?
No - it is most likely due to noise - natural variability].
- this reasoning suggests that as the time-averaging period decreases, one should pay less
attention to small differences between adjacent time-averaged periods, e.g. if June precip.
goes down by 10%, is the fact that July precip. goes down by 20% and August by 5% really
meaningful?

-
At 10:13 11/09/02 +0100, Iain Brown (UKCIP) wrote:

Mike,
For the UKCIP Scenarios datasets - both 98 and 02 - temporal interpolation was applied to the raw model data in the form of a 1-2-1 filter. This had the effect of smoothing out monthly values so that there are not as abrupt transitions between adjacent months.
Can you provide us with the definitive explanation for the interpolation?
Some users (eg. in the recent London study) have noted that there are differences between the maps they have derived from the data and the maps in the UKCIP02 report.
best wishes,
Iain

Dr. Iain Brown
UK Climate Impacts Programme
12 St. Michael's St.
Oxford
OX1 2DU

276. 1031923640.txt
##
##########

From: Tom Wigley <wigley@ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: Hadley Centre request for MAGICC
Date: Fri, 13 Sep 2002 09:27:20 -0600
Cc: Gareth Jones <gareth.s.jones@metoffice.com>, s.raper@uea.ucar.edu, wigley@ncar.ucar.edu, Ben Santer <santer1@llnl.gov>

Gareth,

It seems to me, from reading your email, that you do not realize that this is precisely what MAGICC/SCENGEN already does -- i.e., it uses the scaling method that Ben Santer and I 'invented' in the late 1980s to get time dependent patterns of future climate change. I am attaching a description of the method as we employ it.

The current CDROM version uses only a SAR version of the UD-EBM. Of course, there is a TAR version that Sarah used for the TAR, developed by me and Sarah -- but mainly Sarah. This has not yet been put into MAGICC/SCENGEN, although I am in the process of doing so (along with making a number of other changes to the software). We do not normally give the code for TAR/MAGICC to others unless it is as part of a collaborative project. As Mike Hulme noted, what we can do for/with you will have to be a joint decision with me and Sarah.

The issue of how well scaling works compared with a full AOGCM is both important and of considerable interest to me (and Ben Santer). It is something we have looked at in the past, cursorily, and which we were planning to investigate more fully with the suite of PCM runs that we have here. There are some tricky issues that need to be addressed.

So, perhaps we should pool our intellectual, modelling and data resources?

Anyhow, check out the attached and get back to me with your views.

The 'new and improved version' of MAGICC/SCENGEN should be available in beta-test form in about a month. It will have around 30 models in its

data base, and it does a lot of new things that I can tell you about
later.

Tom.

++++++++++++++++++++++++++++++++++

Mike Hulme wrote:
>
> Gareth,
>
> Thank you for endowing me with the grand title of co-ordinator of magic!!
>
> Such a position does not really exist here. The model developers are Sarah
> Raper and Tom Wigley, to whom I am copying this reply, and it is the two of
> them that really need to grant your request.
>
> My role is more specifically in relation to the availability and
> distribution of the public domain version of MAGICC/SCENGEN Version 2.4 on
> CD-ROM and the accompanying manual. However, your request is really for
> the TAR version of MAGICC and even the source code and that request I
> cannot grant.
>
> I would hope that either/or Sarah and Tom will reply to you directly.
>
> Best wishes,
>
> Mike
>
> At 11:54 13/09/02 +0100, you wrote:
> >Dear Dr Hulme,
> >   I believe that you are the MAGICC co-ordinator in the Climatic
> > Research
> >Unit. I hope you can assist me with the following request.
> >
> >   I would like to obtain a version of the Magicc model that would allow
> >the input of climate forcings (rather than emission scenerios).
> >
> >I am in the detection and attribution group within the Hadley Centre, Met
> >Office. I am working with Dr Peter Stott and Dr John Mitchell on a project
> >that
> >requires an EBM.
> >
> >What we want to use the EBM for is to simulate global mean temperatures for
> >different forcings which we can then multiply with equilibrium temperature
> >spatial patterns for the same forcings to create surrogate transient time
> >varying climate patterns. If the surrogate patterns compare favourably
> >with our
> >HadCM3 simulations, we will then want to investigate how the detection and
> >attribution of climate change (for the detection schemes we use) will be
> >affected by uncertainties in the forcings we use. We would like to use
> >Magicc
> >as it has been tuned already to the HadCM3 anthropogenic emissions scenerios,
> >and as a model used extensively in the recent IPCC TAR would be most
> >appropriate
> >for our work.
> >
> >Would it be possible to obtain a copy of MAGICC or can you tell me how I
> >could
> >go about obtaining the model?
> >
> >Thanks in advance
> >Gareth

> >
> >--
> >Dr Gareth S. Jones   Climate Research Scientist
> >Met Office, Hadley Centre for Climate Prediction and Research,
> >London Road, Bracknell, RG12 2SY, UK   http://www.metoffice.com
> >Tel/Fax: +44(0)1344 85 6903/4898 email:gareth.s.jones@metoffice.comContent-Type: x-msword;
 name="MAG-SG.doc"
Content-Disposition: inline;
 filename="MAG-SG.doc"

Attachment Converted: "c:\eudora\attach\MAG-SG1.doc"
Content-Type: x-msword;
 name="SGFlowchart.doc"
Content-Disposition: inline;
 filename="SGFlowchart.doc"

Attachment Converted: "c:\eudora\attach\SGFlowchart1.doc"

277. 1033599602.txt
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From: Martin Welp <martin.welp@pik-potsdam.de>
To: gberz@munichre.com, ccarraro@unive.it, baldur.eliasson@ch.abb.com, juergen.engelhard@rheinbraun.de, bhare@ams.greenpeace.org, klaus.hasselmann@dkrz.de, hourcade@centre-cired.fr, m.hulme@uea.ac.uk, SSinger@wwfepo.org, carlo.jaeger@pik-potsdam.de, martin.welp@pik-potsdam.de
Subject: ECF: Monthly telephone conference (7 October)
Date: Wed, 02 Oct 2002 19:00:02 +0200
Cc: tloster@munichre.com, anders.h.nordstrom@se.abb.com, e.l.jones@uea.ac.uk, Ottmar.Edenhofer@pik-potsdam.de

Dear member of the extended board

The next ECF telephone conference takes place on Monday, 7 October 2002 at
17-18 CET (Central European Time). The participants are:

Gerhard Berz 089-3891 5290
Carlo Carraro +39-335-6170 775
Baldur Eliasson +41-58-586-8031
Jürgen Engelhard 0221-480 1460
Bill Hare 0331-288 2412
Klaus Hasselmann 04121-508 849
Jean-Charles Hourcade +33-1-43 94 73 63
Mike Hulme +44-1603-593162
Stephan Singer +32-2-74 38817
Carlo Jaeger 0331-288 2601
Martin Welp 0331-288 2619

Please check that your number is correct. If you want to be called at
another number please inform me by the end of this week. In case there are
technical problems at the beginning or during the conference please call
the Deutsche Telekom at +49-(0)69-90922723.

The agenda is as follows (it may be modified at the beginning of the meeting):

1 Minutes of the previous telephone conference (5 Min.)
2 Working groups (10 Min.)
3 Meetings & Events (15 Min.)
        - Report of the meeting with IEA (International Energy Agency)
        - Report of the meeting with Vivendi Environnement Institute

        - ECF general assembly (13 November)
        - ECF conference in Berlin (14-15 November)
        - Workshop of the Technology Group in Oldenburg (12-13 December)
4 Next steps (15 Min.)
5 Varia (15 Min.)

Best regards,
Martin Welp

--
NOTE NEW FAX NUMBER

Dr. Martin Welp
Potsdam Institute for Climate Impact Research (PIK)
Dept. Global Change and Social Systems
P.O. Box 601203, 14412 Potsdam, Germany
Tel. +49 331 288 2619
Fax +49 331 288 2640
E-mail: martin.welp@pik-potsdam.de
Internet: http://www.pik-potsdam.de
http://www.pik-potsdam.de/~welp/index.html
http://www.European-Climate-Forum.net/

278. 1034341705.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Mike Salmon <m.salmon@uea.ac.uk>
Subject: Fwd: Re: Polar Urals data
Date: Fri Oct 11 09:08:25 2002

I am forwarding this to stimulate you (no it's not one of those emails!) to hassle me to check and update the tree-ring and my stuff on the web. Cheers Keith

Date: Thu, 10 Oct 2002 11:22:37 -0400
From: Leonid Polyak <polyak.1@osu.edu>
Subject: Re: Polar Urals data
X-Sender: lpolyak@pop.service.ohio-state.edu
To: Keith Briffa <k.briffa@uea.ac.uk>
X-Mailer: QUALCOMM Windows Eudora Pro Version 3.0.3 (32)
Got it! Note that there appears to be an error in the explanation for the data file: Polar Ural data are f2, not f1 (as far as I can judge).
Thank you,
Leonid
>
>Leonid
>see [1]http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
>The data (and other possibly interesting data are available there) .
>Best wishes
>Keith

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

Phone: +44-1603-593909
Fax: +44-1603-507784

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

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

279. 1035838207.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: T data
Date: Mon, 28 Oct 2002 15:50:07 +0000
Cc: Ben Santer <santer1@llnl.gov>,t.osborn@uea.ac.uk

<x-flowed>

Tom,
Talked to Tim re the SD field. Can you read the following (J. Climate 10, 2548-2568)
before you come so you know how Tim infilled the SD field ? HadCM2 data was used.
This would seem to bias any model validation to this model. Also it would seem odd to
validate any model in a region where there is no data - in a region that had to be infilled.
I can see that global fields make things simpler, but they will need to constructed in
the best possible way. In 1997 we thought the best way was to use a model, but our aim
then was different from yours.

Cheers
Phil

At 06:04 28/10/02 -0700, Tom Wigley wrote:
>Phil,
>
>Thanx. I need to see if CMIP has the height fields for models ---
>Ben????
>
>Tom.
>
>______________________________
>
>Phil Jones wrote:
> >
> > Tom,
> > Here's the file that you should have got back in September. It is
> > 1981-2000 where this
> > could be calculated and 1961-90 elsewhere. The other fields (already
> > sent) enable you to
> > know where the 1961-90 field has been used.
> > All you need to overcome the problem of this being surface
> > temperatures is to get a
> > 5 by 5 degree average height field. I have emailed Mark New to see if he
> > has a 1 by 1 degree

> >   height field, which could then be averaged. Mark must have had this at
> > some stage - he
> >   has a 10 minute height field for the world, which I'm sure he has
> > degraded to 1 degree. I
> >   have a land/sea mask at 1 by 1 degree, so am hoping Mark has the heights.
> > With this
> >   all you will need is the model height fields.
> >   As for the SD's it would be possible to produce this for a period
> > like 1981-2000 or 1961-90
> >   but both would have gaps - probably exactly the same as in the
> > climatology. The options
> >   to consider here are:
> >
> >   1. Period 1981-2000 or 1961-90?
> >   2. How many years in each needed to get an SD?
> >   3. How to infill the gaps?
> >
> >   Tim Osborn must have infilled the gaps for the errors paper in 1997 as we
> > needed a complete
> >   field of variances. He did this by blending some model data
> > (HadCM2/ECHAM3 probably)
> >   with the real observations. Most areas get infilled easily - big problem
> > is the Southern Oceans
> >   and the Antarctic (also central Arctic). I will talk to Tim.
> >
> >   We can discuss this more when you come.
> >
> >   Cheers
> >   Phil
> >
> >   PS I should have some results from Anders by the time you come. He is
> > comparing means/
> >   SDs and extremes etc of HadRM3 with real world data from 200 sites across
> > Europe. Only
> >   temperature variables in the first part. Clearly shows that for
> > islands/coasts comparisons
> >   must be with land points in the model. We've had to 'move' some stations
> > to be on model
> >   land to get better comparisons. Islands that are not in the model have
> > poor comparisons.
> >   It is possible to see country outlines in some comparisons with either
> > max or min
> >   temperatures. Corrections for elevation are needed to get over large
> > elevational differences
> >   between stations and the model, but the Alps are still visible. Lapse
> > rates work best only
> >   in some seasons - not very good in summer. Max temps produce consistent
> > difference maps
> >   (model-obs) over Europe, but mins are more erratic/random. Min error is
> > overall small but
> >   with a large variability while max has a larger error but low
> > variability. Due to mins being more
> >   affected by local environment.
> >
> > At 09:13 27/10/02 -0700, Tom Wigley wrote:
> > >Phil,
> > >
> > >Re my last email ....
> > >
> > >I have looked at the data you sent. It would be very nice to have a
> > >gapless 1981-2000 T climatology to match the Xie/Arkin precip
> > >climatology. However, this means somehow filling in the gaps in the
> > >61-90 minus 81-00 differences, a nontrivial task. So my choice in the

> > >absence of this is either a gappy 81-00, or a full 61-90. I have chosen
> > >the latter -- perhaps we can discuss how to produce a gapless 81-00
> > >climatology when I am at CRU?
> > >
> > >A problem with the 61-90 is that it is surface, and that observed
> > >surface is not equal to model surface. I'm sure you have thought about
> > >this (in the model validation context) already, so this is another item
> > >to discuss.
> > >
> > >For precip, I also have the inter-annual S.D. climatology, so I can
> > >validate both the mean climate and the variability. Very interesting. It
> > >would be nice to be able to do this with temperature (especially since
> > >the mean climate for temperature in the models is pretty darn good --
> > >but how good is variability?) Is there an S.D. climatology for
> > >temperature that you can send me?
> > >
> > >Cheers, Tom.
> >
> > Prof. Phil Jones
> > Climatic Research Unit   Telephone +44 (0) 1603 592090
> > School of Environmental Sciences   Fax +44 (0) 1603 507784
> > University of East Anglia
> > Norwich   Email   p.jones@uea.ac.uk
> > NR4 7TJ
> > UK
> >
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> >   --
> >   Name: newabsref8100.out
> >   newabsref8100.out   Type: Plain Text (text/plain)
> >   Encoding: base64

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

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280. 1036182485.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: Fwd: Re: paleo data
Date: Fri, 01 Nov 2002 15:28:05 +0000

X-Sender: hegerl@mail-he.acpub.duke.edu
Date: Fri, 1 Nov 2002 09:56:45 -0500
To: Phil Jones <p.jones@uea.ac.uk>
From: Gabi Hegerl <hegerl@duke.edu>
Subject: Re: paleo data
No worries, I can wait till next week!
It would be great to hear from you next week particularly if you
feel I have overlooked something, I am planning to submit a little

GRL paper on the detection results based on paleodata soon, and so a warning
if I am doing something wrong would be great.
Its not surprising that the detection results are stable, since other than
volcanic
forcing is mainly driven by the low-f component anyway.
But it looks to me like the volcanic response is not smaller or even a bit
larger in the
annual JGR data (except for one real real big peak in
the 1998 data).
Greetings, have a good weekend and good luck for Keith's back
Gabi

Gabi,
I have printed the files, but I do not know the answer. Keith is off today
with a
bad back -
seeing a chiropractor. I need to talk to him before we can reply. I will be
away
Mon/Tues
next week, so we will not be able to reply until later next week.
Cheers
Phil
At 11:27 31/10/02 -0500, Gabi Hegerl wrote:

Dear Keith and Phil,
I checked and found that we did indeed use the JGR 2001 data (by reloading them
from your JGR data file). I also got the
1998 data from the volcano paper, and did some checking. My detection results
appear quite unimpressed by if I filter the 2001 data to focus on lower
frequencies or not (the estimated amplitudes of solar, volcanic and ghg signals
are virtually identical, volcanism gets a bit tougher to detect if you remove
the high-frequency component).
Then I redid the Epoch analysis comparing the
response of your data old and new to volcanism, and find somewhat bigger
volcanic
signals on average (using 50 eruptions between 1400 and 1940) in the
JGR paper record. I high-passed both datasets and get somewhat more variability
in the JGR record, not the 1998 record.
I am wondering is there something I am overlooking?
I append a figure of the high-passed (var > ca 10 yrs removed) records,
and the volcanic response in both datasets (averaging years 1-20 after the
eruption,
and removing the best-estimate solar and ghg signal before the analysis).
The analysis omits years with another volcanic eruption within the 20 yrs.
I also append one version of the figure where the upper 95%ile of the ghg
signal (which
appears underestimated in Briffa 98 data) is removed rather than the
best estimate, in that case, the volcanic signals in both data appear nearly
identical.
Greetings, and please let me know if I am doing something wrong with your data!
Also, what is the best reference to a discussion on the difference between both
datasets?
Thanks in advance
Gabi

Dear Tom
after a little detective work we have deduced that the data sent to you
constitute a
version of Northern Hemisphere Land temperatures (april- sept) produced by PCA
regression using regional average density chronologies (ie the JGR paper you
refereed I
believe). It is true that high frequency component is not in my opinion optimal
in

describing the relative magnitude of extreme inter-annual extremes. This is to do with
the unpredictable weighting ascribed to certain areas (tree-density series) in the
averaging of the original raw data (this is boring and I won't go into it unless you
really want me to). Te relative differences in year-to-year values are likely better
represented in the N.Hemisphere series produced by averaging regional series produced
using a different approach in which the initial data are high-pass filtered and then
merged in a more straight forward way. This is more equivalent to the series on volcanic
signals described in our Nature paper, though the low-frequency component in this series
is definitely not represented. There is another series , that one could consider a good
compromise . That is a composite of the Age-Banding approach (JGR) low-frequency
variance added to the earlier (Nature) high-frequency component. We did this for Figure
6 in the JGR paper , but did not provide the data on our web site I now realize. However
this composite series is VERY highly correlated with the "better" high frequency data -
see the correlations (Table 1 and related text in
[1]http://www.cru.uea.ac.uk/cru/people/briffa/jgr2001/Briffa2001.pdf
There are many possible ways of producing a "Northern Hemisphere" average , involving
different prior regionalisation and secondary weighting (in space and through time) of
the constituent series) . Non can be considered "correct". If you would like us to dig
out the composite series or discuss specific aspects of the logic or uncertainties
associated with the different large averages let me know. Perhaps it would be better to
discuss this on the phone? As for longer series , we can provide the 2000 year
N.Eurasian data (a composite of ring width chronologies in N.Sweden, The Yamal
peninsula, and Taimyr) . I will soon be able to provide a 4000-year version , that is
now being worked on.
or a similar Northern tree-ring chronology incorporating more data eg see
[2]http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
We do not have the bristlecone data - but they are available I presume from the
International Tree-Ring Data bank , part of the NGDC holdings?
At 02:29 PM 10/1/02 +0100, Phil Jones wrote:

Tom,
Been away and going again tomorrow. Had a chat with Keith and Tim and one of them
will send a reply and data later this week.
Cheers
Phil
At 11:28 26/09/02 -0400, Tom Crowley wrote:

Hi Phil,
thanks for all your help on the bams paper
DOE is being exceedingly slow in processing the paperwork for our new round - I will
keep you posted.
I am also wondering whether we can get some data from you:

Gabi is comparing our 2d ebm run with the briffa et al 2001 jgr time series in order to
compare the model prediction of - I think you mentioned at one point something to the
effect that, although this series is good for estimating low resolution temperature
variability, it may dampen high frequency variability. if my memory is correct in this
case, would you please send gabi the record you consider best for comparing with the
model predicted interannual response to volcanic eruptions?
on another matter we are extending our runs back in time - I have now compiled a record
of global volcanism back to 4000 BP for both hemispheres - extended back to 8000 BP for
30-90N. we are therefore trying to compile paleo records older than AD 1000 to at least
get some reconstruction we can compare with.
I seem to recall that Keith or you may have published some longer reconstructionn but
cannot recall where it is? if so, would you be so kind as to send it to me? also I am
trying to find a long record from the eastern California for the bristlecone pine - for
some reason I am having difficulty finding one. if you have a long record - even going
back beyond 2000 BP, it would be very much appreciated.
thanks for any help you can give us on this and best wishes, Tom
--
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NR4 7TJ
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--
Professor Keith Briffa,
Climatic Research Unit
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Norwich, NR4 7TJ, U.K.

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

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Gabriele Hegerl - NOTE CHANGE IN ADDRESS FORMAT
Department of Earth and Ocean Sciences,
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Box 90227
Duke University, Durham NC 27708
Ph: 919 684 6167, fax 684 5833
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UK

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/jgr2001/Briffa2001.pdf
2. http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
3. http://www.cru.uea.ac.uk/cru/people/briffa/
4. http://www.env.duke.edu/faculty/bios/hegerl.html
5. http://www.env.duke.edu/faculty/bios/hegerl.html

281. 1036591086.txt
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##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Leonid Polyak <polyak.1@osu.edu>
Subject: Re: Polar Urals data
Date: Wed Nov 6 08:58:06 2002

The delay again is simply because I was away for 2 days. Attached are the data you want.
First number is number of years of record, followed by
(in first column) year A.D. and (in second column) the numbers you want . Ignore other
columns. Cheers
Keith
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

At 02:58 PM 11/5/02 -0500, you wrote:

Keith,
To keep you informed about the use of your Salekhard data, I attach the MS
which I'm submitting to The Holocene. I've referred to your papers of 1995
and 2000. If you'd like me to add more acknowledgement of your data, let me
know and I'll gladly do that.
Sincerely,
Leonid
Leonid Polyak

Byrd Polar Research Center
Ohio State University
1090 Carmack Rd., Columbus, OH 43210
614-292-2602, fax 614-292-4697
[1]http://polarmet.mps.ohio-state.edu/GeologyGroup/polyak.htm
>Leonid
>see [2]http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
>The data (and other possibly interesting data are available there) .
>Best wishes
>Keith

--
Professor Keith Briffa,
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References

1. http://polarmet.mps.ohio-state.edu/GeologyGroup/polyak.htm
2. http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/
3. http://www.cru.uea.ac.uk/cru/people/briffa/
4. http://www.cru.uea.ac.uk/cru/people/briffa/

282. 1037241376.txt
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From: "Ronald M. Lanner" <pinetree30@EARTHLINK.NET>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: The Great Controversy
Date: Wed, 13 Nov 2002 21:36:16 -00
Reply-to: grissino@UTKUX.UTCC.UTK.EDU

Dear Forumites -- Since I am neither a dendrochronologist nor a tree
physiologist, I have a
different take on this little brushfire we have going. Ideally, tree phys people
should be
producing information (among other things) that dendrochronologists find useful.
And
dendrochronologists should use the information within its limits and with enough
understanding to get it right. I don't think either of those things is occurring
with as
much frequency as we would all like. I can understand Rod's annoyance at the
massaging of
numerical data that dendrochronologists do. I am basically a non-mathematical
biologist

mystified by such stuff, and I prefer handling measurements to deriving indices, or
whatever. When I run up against such derived data, I generally turn skeptical, because I
cannot verify the results from my own experience or intuition. On the other hand, when I
read papers by cambial physiologists like Rod I also get annoyed. That's because my biology
wants to integrate upwards, and all I get from cambial labs is biochemistry. So I'm in the
middle, where it gets lonely. I try not to get mad at anybody, though I do wish I didn't
find myself alone on the margins.

I find it frustrating that some dendrochronologists stubbornly see tree ring
characteristics as being affected by climate. They are not. They are affected by cambial
activity. Cambial activity is affected by internalities of tree behavior, mainly hormonal
and nutrient fluxes in the crown. Those things are largely influenced by climatic factors.
So there is quite a bit of slack between the climatic factor and the ring characteristic.
Is this just negligible static? I doubt it. I see this as an oversight by
dendrochronologists that weakens their credibility a tad among those knowledgable about
tree growth. I also have a quarrel with the dogma of dendrochology that the cambium changes
as the tree becomes senescent. I know of no data that trees senesce -- that is, that they
undergo changes due solely to aging. This started as forestry dogma, and was accepted by
tree-ringers, who then corrected for it. I'm practically the only one who has
systematically looked for evidence of senescence (with a Ph.D. student), and we could not
find any in young to ancient bristlecones. But tree physiologists do not generally look at
such issues because they have become progressively more reductionist. Nor do they try to
produce a theory of tree growth based, as it must be, on evolutionary theory. Such a theory
would be simple and general, and it would allow tree-ringers to approach rings with more
sympathy and understanding. That might not get you further, but it would improve your
character, I'm certain. And it would put all that assorted mishmash of tree phys data that
have accumulated since 19th century Germany into a context at last, and maybe liberate the
minds of all those tense physiologists out there with their ever-increasing inventories of
electronic sensors and analyzers. The world would be a better place with more people having
fun in the woods. ---Ronald M. Lanner

--- [1]pinetree30@earthlink.net

--- EarthLink: It's your Internet.

References

1. mailto:pinetree30@earthlink.net

283. 1037394925.txt
###
##########

From: John Ogden <j.ogden@AUCKLAND.AC.NZ>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: Fwd: History and trees
Date: Fri, 15 Nov 2002 16:15:25 +1300
Reply-to: grissino@UTKUX.UTCC.UTK.EDU

Dear Professor Savidge, Hal Fritts's comments were, as always,
to the point and gracious. I
have much less patience with your ignorance and arrogance. The
sampling and statistical procedures involved in the production of a
cross-dated chronology are of course quite different to those used in a
randomised experiment, but they are none-the-less logical,
rigorous, science. We have been through all those arguments so many
times - you are wasting everyone's time.
John Ogden.

On Wed, 13 Nov 2002 13:16:20 -0700 "Harold C. Fritts"
<hfritts@LTRR.ARIZONA.EDU> wrote:

> Dear Ron,
> I respectfully disagree with you. We have reached out to you many times
> and find little but judgmental response. I have worked with this group
> for many years now and they are just as exact scientists as you. They
> are interested in what the tree tells us about the earth and its history
> and not as interested and experienced as you in how the tree works. I
> agree with you to the extent that we must understand how the tree works
> but I fear you have "created the reality that dendrochronologists are
> stupid and beneath your greatness" and that it will not ever change.
>
> People like you in the past such as Waldo Glock and Sampson at Berkley,
> CA made similar statements. When I was a young man, I set out trying to
> examine their criticism objectively with both physiological
> investigations and statistical analysis. I found that these criticisms
> could be met with data from solid physiological tests and even though
> those practicing the science at that time were astronomers, not
> physiologists. There are talented and insightful people in other
> sciences outside of plant physiology.
>
> I am sorry for all of our sakes. as the future holds many possibilities
> with many experts contributing to the future of science. If you could
> only get outside the judgmental ideas that you hold about us, I think
> you might be very surprised and pleased.
>
> Yes, I think many in this group oversimplify the response of the tree,
> but in the same way you oversimplify the practice of dendrochronology.
> We all have much to learn from each other, but calling each other names
> doesn't further anyone's science.
>
> I believe science is embarking on a course of greater cooperation among
> different disciplines. This implies respect and cooperation in both
> directions. We welcome your interest in dendrochronology but are
> saddened that you have so little respect for our integrity and honesty.
> It would be more appreciated if we could together work for a better
> future, not just quarrel, call each other names and delve on what is
> wrong with the past.
>
> Sincerely, Regretfully and Lovingly,
> Hal Fritts

>
> P.S.
> One other comment to my fellow scientists. I agree with Frank that I
> have made only a start at understanding the basis for tree ring
> formation. It will take much more work in physiology and modeling. In
> current discussions and debates on the importance of physiology and
> process modeling in dendrochronology, understanding plant processes
> often takes secondary impotence in the eyes of many
> dendrochronologists. I think this will change because I believe in the
> integrity of my colleagues, but I sometimes wonder how long this will
> take. I had at one time hoped that I might see it happen. We can
> answer such criticism, but not until we investigate further how the tree
> responds to its environment and how the tree lays down layers of cells
> we call the tree ring. Physiologists outside dendrochronology have
> little inclination to do it for us as this message reveals. We can and
> must do it ourselves by including, welcoming and funding physiological
> investigation in tree-ring research.
> HCF
>
>
> Rod Savidge wrote:
> >
> > To the Editor, New York Times
> >
>   Indeed, its activities
> > include subjective interpretations of what does and what does not
> > constitute an annual ring, statistical manipulation of data to fulfill
> > subjective expectations, and discarding of perfectly good data sets when
> > they contradict other data sets that have already been accepted. Such
> > massaging of data cannot by any stretch of the imagination be considered
> > science; it merely demonstrates a total lack of rigor attending so-called
> > dendrochronology "research".
> >
> > I would add that it is the exceptionally rare dendrochronologist who has
> > ever shown any inclination to understand the fundamental biology of wood
> > formation, either as regulated intrinsically or influenced by extrinsic
> > factors. The science of tree physiology will readily admit that our
> > understanding of how trees make wood remains at quite a rudimentary state
> > (despite several centuries of research). On the other hand, there are many
> > hundreds, if not thousands, of publications by dendrochronologists
> > implicitly claiming that they do understand the biology of wood formation,
> > as they have used their data to imagine when past regimes of water,
> > temperature, pollutants, CO2, soil nutrients, and so forth existed.   Note
> > that all of the counts and measurements on tree rings in the world cannot
> > substantiate anything unequivocally; they are merely observations. It
> > would be a major step forward if dendrochronology could embrace the
> > scientific method.
> >
> > sincerely,
> > RA Savidge, PhD
> > Professor, Tree Physiology/Biochemistry
> > Forestry & Environmental Management
> > University of New Brunswick
> > Fredericton, NB E3B 6C2
> >
> > >X-Sieve: cmu-sieve 2.0
> > >X-Mailer: Microsoft Outlook, Build 10.0.4024
> > >Importance: Normal
> > >Date: Tue, 12 Nov 2002 23:24:03 -0500
> > >Reply-To: grissino@UTKUX.UTCC.UTK.EDU
> > >Sender: ITRDB Dendrochronology Forum <ITRDBFOR@LISTSERV.ARIZONA.EDU>
> > >From: "David M. Lawrence" <dave@FUZZO.COM>
> > >Subject: History and trees

```
> > >Comments: To: scitimes@nytimes.com
> > >To: ITRDBFOR@LISTSERV.ARIZONA.EDU
> > >
> > >I was rather horrified by the inaccurate statements about tree-ring
> > >dating that you allowed to slip into print in the interview with Thomas
> > >Pakenham today.  Tree-ring science is an exact science -- none of the
> > >data obtained from tree rings would be useful if the dates were
> > >inaccurate.  Dendrochronologists don't say much these days about how old
> > >trees are because they are interested in more important questions --
> > >such as "What can the tree rings tell us about our planet's past?"
> > >
> > >You at The New York Times should know something about tree rings.  A
> > >check on Lexis-Nexis shows that since 1980 you have run more than 100
> > >stories in which the words "tree rings" appear in full text.  Some of
> > >the stories are irrelevant.  But most are not, such as the July 13,
> > >2002, story in which you misspell the name of Neil Pederson at
> > >Lamont-Doherty Earth Observatory, or the March 26, 2002, story about a
> > >medieval climate warming detected in tree-ring data.  I do not remember
> > >tree-ring dating being labeled an "inexact" science in stories like
> > >that.
> > >
> > >Did Walter Sullivan, who wrote a story about tree rings and drought on
> > >September 2, 1980, ever question the "exact" nature of tree-ring dating?
> > >He didn't seem to question it on June 7, 1994, when he wrote a story
> > >about ash from Santorini and said that the ash cloud may have "persisted
> > >long enough to stunt the growth of oak trees in Irish bogs and of
> > >bristlecone pines in the White Mountains of California, producing
> > >tightly packed tree rings."  You really do have to know when those rings
> > >were laid down before you can associate them with a specific volcanic
> > >eruption.
> > >
> > >I tell you what.  I am a member of the National Association of Science
> > >Writers as well as a working dendrochronologist and occasionally paid-up
> > >member of the Tree-Ring Society.  If you feel the need for a refresher
> > >course on tree-ring dating, I'll be more than happy to try to introduce
> > >you to knowledgeable practioners in you neighborhood, such as Neil
> > >Pederson (not Peterson) at Lamont-Doherty Earth Observatory.  (It's
> > >actually a local phone call for youse guys.)
> > >
> > >Sincerely,
> > >
> > >Dave Lawrence
> > >
> > >-----------------------------------------------------
> > >  David M. Lawrence        | Home:  (804) 559-9786
> > >  7471 Brook Way Court     | Fax:   (804) 559-9787
> > >  Mechanicsville, VA 23111 | Email: dave@fuzzo.com
> > >  USA                      | http:  http://fuzzo.com
> > >-----------------------------------------------------
> > >
> > >"We have met the enemy and he is us."  -- Pogo
> > >
> > >"No trespassing
> > >  4/17 of a haiku"  --  Richard Brautigan
>
> --
> Harold C. Fritts, Professor Emeritus, Lab. of Tree-Ring Research
> University of Arizona/ Owner of DendroPower
> 5703 N. Lady Lane, Tucson, AZ 85704-3905
> Ph Voice: (520) 887 7291
> http://www.ltrr.arizona.edu/~hal

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```

John Ogden
j.ogden@auckland.ac.nz

284. 1037719165.txt
##

From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: CRU strategic review
Date: Tue, 19 Nov 2002 10:19:25 -0800

Dear Tim,

I'm really sorry I've been so slow in responding to your request for input to the CRU strategic review. Life has been rather hectic over the past few months. I hope to send you my response to your questionnaire by no later than the end of this month. Would that still be o.k?

Cheers,

Ben
===
Tim Osborn wrote:
>
> Dear Ben,
>
> I've not had time to speak with Phil recently, so I don't know how things
> are with you at the moment, work-wise and home-wise. But I hope all is
> well. The (rather formal, sorry) message below is a follow-up to a
> letter/questionnaire that I sent in the summer. It would certainly be good
> to obtain your input, so if you have time...!
>
> Cheers
>
> Tim
>
> --------------------------------
> Dear Dr. Santer
>
> I wrote to you in the summer in my role as leader of the Climatic Research
> Unit's (CRU) strategic review team, as part of an exercise to obtain
> external input to our review process. This exercise was reasonably
> successful, with a 45% response rate. Despite this response rate, there
> are still some gaps in the "categories" that we hoped to obtain input
> from. We have analysed the responses, together with our own internal
> assessments, and are now looking to fill in some of the remaining gaps.
>
> I am contacting you again in the hope that you might be able to assist us
> in our review process, via the attached questionnaire. As stated in my
> original letter, we are aware that this process is primarily for our
> benefit, rather than yours, so we greatly appreciate any time that you
> could spend in assisting our review.
>
> Some respondents said that they would prefer to have received an electronic
> version of the questionnaire, and so I have decided to attach a Microsoft
> Word document containing the questionnaire that I sent to you in the summer.
>
> If you have any questions about the review process, or would prefer to
> provide your opinions over the telephone, then please phone me on 01603
> 592089. We will be grateful for whatever level of input you feel able to
> provide.

>
> Best regards
>
> Tim
>
> [Dr. Tim Osborn, Chair of Strategic Review Team]
>
> --
> Name: questions for Santer.doc
> questions for Santer.doc Type: Microsoft Word Document (application/msword)
> Encoding: base64
>
> Part 1.3Type: Plain Text (text/plain)

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

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-7638
FAX: (925) 422-7675
email: santer1@llnl.gov
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285. 1038027690.txt
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From: "L.B. Klyashtorin" <klyashtorin@mtu-net.ru>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Re: Fw: Fw: Reconstruction etc.
Date: Sat, 23 Nov 2002 00:01:30 +0300

Dear Keith,

Do not be embarassed. This situation is very humorous and I am very

glad to smile. It happens.

Thank you very much for your time series.

I would like to analyse specta characteristics of summer temperatures (your

series) and winter temperature series using Dansgaard's time series for

the same period (since 550s). It seems to me the temperature data of Arctic
basin is
the

most pronounced indices illustrating of long term climate oscillations.

Best wishes

Leonid

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

From: [1]Keith Briffa

To: [2]L.B. Klyashtorin

Sent: Monday, November 18, 2002 11:01 PM

Subject: Re: Fw: Fw: Reconstruction etc.

I am very embarrassed as I have just realized I sent the data (a couple of weeks ago at
least !) to the wrong person (someone called Leonid Polyak) by mistake. He wanted
polar Urals data. I now attach the file with the Nature temperature reconstruction.
First number is the number of values , then subsequent lines contain the date in the
first column (years AD) and the anomalies in the second (as described in the paper).
Sorry!!!!!!!!!!!!!!!!!!!
Keith
At 10:45 PM 11/18/02 +0300, you wrote:

Dear Keith,

I apologise for persistens but I really need in the time series I requested from you
and I will very grateful to you for these materials which you so kind promised send to
me .
I hope receive it from you yet, although I have not reply from you to my two last
messages.

Yours sincerely

Leonid Klyashtorin

----- Original Message -----
From: [3]L.B. Klyashtorin
To: [4]Keith Briffa
Sent: Sunday, October 27, 2002 1:45 PM
Subject: Re: Fw: Reconstruction etc.
Dear Keith,

I apologize for disturbing you but I did not received the data you promised to send
me yet.
I would be very grateful to you for these time series.

Using your kind permission (from October 22) to remind you if these date do not arrive
I hope to receive it from you....

Sorry for inconveniences and thank you in advance

Leonid

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

From: [5]Keith Briffa
To: [6]L.B. Klyashtorin
Sent: Tuesday, October 22, 2002 5:08 PM
Subject: Re: Fw: Reconstruction etc.
Leonid
Sorry not to respond
I will search out the tree-ring series (ring width and density) and the numbers for the
reconstruction and send them as soon as I can get to it. Remind me in a couple of days
if they do not arrive. Cheers
Keith
At 02:17 PM 10/22/02 +0400, you wrote:

Dear Dr Briffa,

Unfortunately I did not receive reply on my first message sent to your address
by October 8.
I apologize for disturbing you again but I will be very grateful to you
for sending me the address of web site where I can find the data of tree ring
reconstruction of the summer temperature.

I also very interested in receiving data published in one of your et al. old paper:
"A 1400 year tree ring record of summer temperature in Fennoscandia,1990, Nature.vol
346, 2 August 1990."
The time series of Pinus silvestris published at Fig 2 a is very interesting for my
work on the dynamics
climate-linked fisheries of Northern Hemisphere.

I would be very grateful to you for your reply.

Best regards
Leonid Klyashtorin

----- Original Message -----
From: [7]L.B. Klyashtorin
To: [8]Briffa Keith R.
Sent: Tuesday, October 08, 2002 4:58 PM
Subject: Fw: Reconstruction etc.

I am Leonid Klyashtorin from Federal Institute for Fisheries and
Oceanography (VNIRO),Moscow,Russia.

The last 6 monthes I was National Research Council Senior
Associate and worked as Visiting Scientist in the
Pacific Fisheries Environmental Laboratory (PFEL),
NOAA, National Marine Fisheries Service,
Monterey , CA on the item "Climate and Fisheries".
My paper "Climate change and long -term fluctuations of commercial
catches:the possibility of forecasting" published recently as a separate
broshure, FAO Fisheries Technical Paper No 410,
pp 86, 2001, and is rather popular among fisheries specialists.
It gives insight of world major fisheries dynamics and contains
forecast to the next 10-20 years. (the Abstract is attached, PDF file of

all paper also is available)

I have read of your and T. Osborn very interesting and so useful paper "Blowing Hot asnd Cold.." in Science, v.295.,2002.
Your results clearly shows that main conception of IPCC experts about unicity of Global Warming events in 20-century is erroneous and now the additional data appear on the natural long term cyclic climate change at least for the last 2000 years .
My work on the "Climate - Fisheries" connected with questions of Climate Change and ,naturely, touches of Global Warming Problem.

Me and my colleague from Institute of Physics of the Earth of Russian Academie of Science recently submitted our paper "On the coherence between dynamics of the world fuel consumption and global temperature anomaly". in the International Journal " Natrural Hazards" .
The paper is now under reviewing. (The Abstract is attached.)

Now me and a few my collegues from US are in process of writiing book dedicated of Climate- Fisheries problem and we would like use the data on the tree -rings anlysis showing cyclic character of long-term climate changes.

I will be very grateful to you for receiving from you (if possible) the time series of annual reconstructed temperature anomaly from Figure (Esper02) and address of website, where these data are available.

Thank you in
advance

Best regards
Leonid Klyashtorin

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

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References

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From: Clare Goodess <C.Goodess@uea.ac.uk>
To: j.palutikof@uea.ac.uk,p.jones@uea.ac.uk,d.viner@uea.ac.uk, k.briffa@uea.ac.uk
Subject: UK Research Office - FP6 Proposal Writing for Researchers
Date: Tue, 26 Nov 2002 18:34:49 +0000
Cc: j.darch@uea.ac.uk

Dear all
I went to this meeting in London yesterday - which was useful. Julie will photocopy my notes/the overheads for you some time this week (if she doesnt have time, I'll do it when I get back next week). In the meantime, here are my main impressions/thoughts from the meeting. (Incidentally, Alex Haxeltine was due to go from UEA, but didnt turn up. Not sure who the other UEA people were! There was no list of participants.) Maybe we should get together (next week some time?) once you've had chance to look at some of this.
The Commission (EC) seems to be favouring smaller projects, e.g., typically 10 million Euro. Though it is up to proposers to define the necessary 'critical mass'.
UKRO seem quite wary of Networks of Excellence (NoE), e.g., warning of potential conflicts of interest with institutions. As with projects, smaller size seems to be in favour. An UKRO analysis suggests an NoE of 150-400 researchers would maximise the amount of money received per researcher.
Research activities can now be funded in NoE (the EC has changed its mind on this in the last month), but only if focused on integration.
The EC wont be proposing indicators of integration for NoE - the proposals should explain how this will be 'measured'.
Consortium quality seems to be an important concern for the EC, i.e., having the right people for the job and ensuring everyone has a clear role. In our rush to get a 'critical mass', I'm concerned that the GENIE consortium may appear too much as 'all our friends'.
One possible strategy which UKRO seemed to think quite good for people, would be

to put in
   a proposal from 6-8 key partners, indicating for which activities additional partners will
   be brought in at appropriate points. The EC will be providing formal procedures for these
   'internal project' calls.
   It is unlikely that the new online proposal preparation tool will be ready for the first
   call, but electronic submission (on CD) should be possible. Any paper submissions will be
   scanned.
   Evaluation will be by electronic means initially, with possibility of proposers (and
   evaluators?) being invited to hearings in Brussels prior to panel meetings.
   No signatures are required for the proposals (though a password/username will be required
   by co-ordinators to access the online system). Some institutions/consortia are apparently
   drawing up pre-consortia agreements or letters of intent/memorandum of understanding.
   The guide for proposers is currently only in very rough draft.
   There will be a second 'EOI' type exercise at the end of 2003/early 2004. This could lead
   to changes in the indicative themes for 2004.
   UKRO is not keen on UK institutions using consultants for project management - we should be
   building our own capacity.
   Proposals should be written for the informed lay person. It is best if they are not
   obviously written by one person - better to show joint effort/co-ordination at an early
   stage.
   Redundancy costs (i.e., costs of implementing the new fixed-term regulations) can be
   included for research staff.
   The EC aims to audit all FP6 projects (because there will be fewer of them).
   Recognition of the ERA and policy links will be important for the EC. (The ERA includes
   references to developing long-term careers for research staff and increasing the
   involvement of women - so maybe we should be thinking of some activities to address these
   issues.)
   IPR will be an important issue in FP6 - need to get expert advice (e.g., what happens if
   consortium changes over course of project).
   Consortium agreements will be compulsory.
   The proposal forms (for IPs anyway) are relatively simple, e.g., only need to cost four
   different types of activity.
   Clare

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   Southern Africa crisis appeal: [3]http://dec.londonweb.net/appeal/

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3. http://dec.londonweb.net/appeal/

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To: Laurent Labeyrie <Laurent.Labeyrie@lsce.cnrs-gif.fr>, Keith Alverson <keith.alverson@pages.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Rick Battarbee <r.battarbee@geog.ucl.ac.uk>, didier.paillard@lsce.cnrs-gif.fr, Dominique Raynaud <domraynaud@glaciog.obs.ujf-grenoble.fr>, jean jouzel <jouzel@lsce.saclay.cea.fr>, Chappellaz Jerome <jerome@glaciog.obs.ujf-grenoble.fr>, Gerald Ganssen <gang@geo.vu.nl>, Jean Marc Barnola <barnola@glaciog.obs.ujf-grenoble.fr>, Ralph Schneider <rschneid@uni-bremen.de>
Subject: FP6 - NoE Dynamics of Climate Changes (DOCC)
Date: Mon, 2 Dec 2002 10:17:31 +0100
Cc: martin.miles@geol.uib.no, b.balino@uib.no

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Dear friends,

I assume many of you have followed the development of the work programme for FP6, which have been quite dramatic at times for our field. The end result is not particularly good, and the whole area of Global Change has been cut by comparuison with FP5. I talked with Anver Ghazi last week, and what I know stems from this and from the Nov. 18 version of the work programme.
The will be no opening for climate dynamics in the first call (Dec. 17).
The second call due in June /July with a deadline in October 2003 will include some paleoclimate openings:
- STREPS for novel paleoreconstructions methods (i.e. a few of the normal projects of previous FPs) - but remember: 75% of funding goes to New Instruments: Integrated Projects and NoEs).
- Hot spots in the climate system, including the thermohaline circulation and the Arctic.

Brussels will not issue anything now about the thrird call, but according to Ghazi they plan to invite for either an NoE or an IP in climate dynamics with emphasis on past climate change at that point.
Call will be in 2004. But things can change with this call.
Thus we have quite some time to discuss if we shall go forward with DOCC or go for IP. The overall size of the IPs have been substantially reduced, so if we try an IP or an NoE either will need to be more focussed in terms of science and in terms of partnership than our Expression of interest.

Ceers,

Eystein
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The Bjerknes Training site offers 3-12 months fellowships to PhD students
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From: "Andy McLeod" <Andy.McLeod@ed.ac.uk>
To: "Mike Hulme" <m.hulme@uea.ac.uk>, <H.J.Schellnhuber@uea.ac.uk>
Subject: Climate Change Funding in Scotland
Date: Mon, 2 Dec 2002 15:09:24 -0000

Dear John and Mike

It was over two years ago that we first briefly discussed the opportunity to develop climate change research funding in Scotland using a grant to HEI's from the Scottish Higher Education Funding Council (SHEFC). My Centre, CECS, has been successful with such grants in the past. Last year there were no such grants but the opportunity has now arisen again. The funding is quite large (0.5 - 1.5 million over up to 4 years). With support from the three main agencies in Scotland I am keen to develop such a research proposal and will be entering the internal competition (within the University) shortly.

I am keen to develop a strong link/cooperation with the Tyndall Centre and I would like to explore ways in which this might be achieved. Last week I believe that you were busy with your Advisory Board. I would be very keen to talk with you on the phone about this as soon as possible. Please let me know if there is a suitable time when I might phone or feel free to contact me.

Best wishes

Andy

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