

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

Mike,

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

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

Cheers
 Phil

Date: Fri, 16 Jan 2004 12:37:29 +0000

To: Christian Azar <christian.azar@fy.chalmers.se>, christian.pfister@hist.unibe.ch

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

Subject: Re: AW: CLIMATIC CHANGE needs your advice

Cc: "'David G. VICTOR'" <dgvector@stanford.edu>, 'Katarina Kivel' <kivel@stanford.edu>, N.W.Arnell@soton.ac.uk, frtca@fy.chalmers.se, d.camuffo@isac.cnr.it, scohen@sdri.ubc.ca, pmfearn@inpa.gov.br, jfoley@facstaff.wisc.edu, pgleick@pipeline.com, harvey@geog.utoronto.ca, ahs@ansto.gov.au, Thomas.R.Karl@noaa.gov, rwk@ucar.edu, rik.leemans@rivm.nl, diana.liverman@eci.ox.ac.uk, mccarl@tamu.edu, lindam@atd.ucar.edu, rmoss@usgcrp.gov, ogilvie@spot.colorado.edu, barrie.pittock@dar.csiro.au, pollard@essc.psu.edu, nj.rosenberg@pnl.gov, crosenzweig@giss.nasa.gov, j.salinger@niwa.co.nz, santer1@llnl.gov, h.j.schellnhuber@uea.ac.uk, F.I.Woodward@sheffield.ac.uk, gyohe@wesleyan.edu, leonid@atmosp.physics.utoronto.ca, shs@stanford.edu

Dear Steve et al,

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

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

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

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

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

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

Back to the question in hand:

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

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

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

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

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

each month. Groups at NCDC and NASA/GISS do this as well. We don't exchange codes

- we do occasionally though for the data. The code here is trivial as it is in the paleo work.

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

from 1400.

4. What has always intrigued me in this whole debate, is why the skeptics (for want of

a better term) always pick on Mike. There are several other series that I've produced, Keith Briffa has and Tom Crowley. Jan Esper's work has produced a slightly different series

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

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

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

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

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

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

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

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

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

I could go on and on

Cheers

Phil

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

Dear all,

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

Yours,

Christian

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References

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