

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 underestimate 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 guarantee, 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 first 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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