

Response to additional Question regarding Keith Briffa's request to Eugene Wahl and his Response

Keith R. Briffa

Climatic Research Unit, School of Environmental Sciences, University of East Anglia

25 June 2010

The following explanation should be read in conjunction with the document "*Response to Questions raised by Professor Geoffrey Boulton in his letter of 6th May 2010 in his role as a member of the Muir-Russell Review Team*" dated 19th May 2010.

During the later stages of my work as a Lead Author of Chapter 6 of the Working Group 1 Fourth Assessment Report of the IPCC, I contacted Dr. Eugene Wahl specifically to ask his opinion of some of the Chapter 6 responses to points raised by referees concerning the Second Order (i.e. penultimate) draft (SOD). I had read the draft of the paper by Wahl and Ammann (later published in 2007) and other papers (e.g. Huybers, 2005; von Storch and Zorita, 2005) that influenced the drafting of some of these responses. Because Wahl and Ammann had undertaken the most recent and detailed re-examination of the issues raised by McIntyre and McKittrick (2005) related to the paper by Mann et al. (1998), I asked Wahl to review some of the responses with a view to ensuring that review comments by Steve McIntyre on this issue had been dealt with in a fair manner. As context for the responses I was asking him to review, I also provided Wahl with a copy of the relevant part of the SOD. I did not ask Wahl to write any text, either for the main chapter or for the responses. I considered, and still consider, that Wahl was an objective and informed judge of these matters and my decision to seek his opinion was in full accord with the spirit and letter of the procedures as outlined by the IPCC. I informed the Convening Lead Authors that I was doing this. As it turned out, I decided to use some of the text from Wahl's reply in two of the responses (which would eventually be publicly archived by the IPCC) because I believed that this text clarified some issues and expressed the relevant points well. Wahl also suggested minor edits to the SOD text. I do not have a copy of the exact suggested modifications but I considered his suggestion and made a change that was consistent with it and with the recent evidence provided in Wahl and Ammann (2007).

The SOD text read "*The latter may have some foundation, but it is unclear whether it has a marked impact on the final reconstruction (Von Storch et al.,2004; Huybers, 2005; McIntyre and McKittrick ,2005)*".

The Final text, updated and including further references to relevant literature (Jansen et al., 2007, p466) reads "*The latter may have some theoretical foundation, but Wahl and Ammann (2006) [a typographical error as it should have read 2007] also show that the impact on the amplitude of the final reconstruction is very small (~0.05°C; for further discussion of these issues see also Huybers,2005; McIntyre and McKittrick, 2005c,d; von Storch and Zorita,2005)*".

Wahl's contribution was minimal and in my opinion did not constitute sufficient justification for inviting his inclusion as a Contributing Author.

References

- Huybers, P. 2005. Comment on "Hockey sticks, principal components, and spurious significance" by S. McIntyre and R. McKittrick. *Geophysical Research Letters* **32**.
- Jansen, E., J. Overpeck, K. R. Briffa, J. C. Duplessy, F. Joos, V. Masson-Delmotte, D. Olago, B. Otto-Bliesner, W. R. Peltier, S. Rahmstorf, R. Ramesh, D. Raynaud, D. Rind, O. Solomina, R. Villalba, and D. E. Zhang. 2007. *Palaeoclimate*. in S. Solomon, D. Qin, M. Manning, Z. Chen, M. Marquis, K. B. Averyt, M. Tignor, and H. L. Miller, editors. *Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, UK.
- Mann, M. E., R. S. Bradley, and M. K. Hughes. 1998. Global-scale temperature patterns and climate forcing over the past six centuries. *Nature* **392**:779-787.
- McIntyre, S., and R. McKittrick. 2005. Hockey sticks, principal components, and spurious significance. *Geophysical Research Letters* **32**:art. no.-L03710.
- McIntyre, S., and R. McKittrick. 2005c. Reply to comment by von Storch and Zorita on "Hockey sticks, principal components, and spurious significance". *Geophysical Research Letters* **32**.
- McIntyre, S., and R. McKittrick. 2005d. Reply to comment by Huybers on "Hockey sticks, principal components, and spurious significance". *Geophysical Research Letters* **32**.
- von Storch, H., and E. Zorita. 2005. Comment on "Hockey sticks, principal components, and spurious significance" by S. McIntyre and R. McKittrick. *Geophysical Research Letters* **32**.
- von Storch, H., E. Zorita, J. M. Jones, Y. Dimitriev, F. Gonzalez-Rouco, and S. F. B. Tett. 2004. Reconstructing past climate from noisy data. *Science* **306**:679-682.
- Wahl, E. R., and C. M. Ammann. 2007. Robustness of the Mann, Bradley, Hughes reconstruction of Northern Hemisphere surface temperatures: Examination of criticisms based on the nature and processing of proxy climate evidence. *Climatic Change* **85**:33-69.

The following copies of emails are provided here in support of this account.

Date: 18 July 2006

To: "Wahl, Eugene R" <wahl@alfred.edu>
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: confidential
Cc:
Bcc:
X-Eudora-Signature: <Standard>

Gene

I am taking the liberty (confidentially) to send you a copy of the reviewers comments (please keep these to yourself) of the last IPCC draft chapter. I am concerned that I am not as objective as perhaps I should be and would appreciate your take on the comments from number 6-737 onwards , that relate to your reassessment of the Mann et al work. I have to consider whether the current text is fair or whether I should change things in the light of the sceptic comments. In practise this brief version has evolved and there is little scope for additional text , but I must put on record responses to these comments - any confidential help , opinions are appreciated . I have only days now to complete this revision and response.

note that the sub heading 6.6 the last 2000 years

is page 27 line35 on the original (commented) draft.

Cheers

Keith

--

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Date: 21 July 2006

To: "Wahl, Eugene R" <wahl@alfred.edu>

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

Subject: RE: confidential

Cc:

Bcc:

X-Eudora-Signature: <Standard>

Gene

thanks a lot for this - I need to digest and I will come back to you.

thanks again

Keith

At 09:23 21/07/2006, you wrote:

Hi Keith:

I hope you are well in all this!!

I have done my best this evening to digest the issues you asked me to look at, and to give perspective on them. Here is what I can offer at this point.

1) Thoughts and perspective concerning the reviewer's comments per se. These are coded in blue and are in the "Notes" column between pages 103 and 122 inclusive. It got to the point that I could not be exhaustive, given the very lengthy set of review thoughts, so I am also attaching a review article Caspar and I plan to submit to Climatic Change in the next few days. [The idea is that this would accompany the Wahl-Ammann article, to summarize and amplify on it -- given all the proper and non-proper interpretation WA has received and the need for subsequent analysis that WA only lightly touches on. Steve Schneider is aware that

it is coming.] I think a read through this, especially the part on PCs and Bristlecones, can say about all I might offer additionally. It is not lengthy.

Please note that this Ammann-Wahl text is sent strictly confidentially -- it should not be cited or mentioned in any form, and MUST not be transmitted without permission. However, I am more than happy to send it for your use, because it succinctly summarizes what we have found on all the issues that have come up re: MBH. As you can see, we agree at some level with some of the criticisms raised by MM and others, but we do not find that they invalidate MBH in any substantial way.

2) I have added a brief suggested alteration to page 6-3 of the draft text you sent, to take into account the fact Wahl-Ammann decidedly settles the issue concerning how proxy PC calculations impact the MBH style reconstruction. These changes are encoded using WORD's "Track Changes" feature.

I did not get into suggesting how that paragraph might otherwise be rewritten. You can see more generally where Caspar and I have gone in the attached text, and how our work relates generally to the MM, von Storch, etc. "examinations" of MBH. Thinking further, the "Validation Thresholds and Measures of Merit" and "Amplitude Issues" sections might also be well worth a look. The former will help you see how over-strong and one-sided are the arguments Steven McIntyre puts forth in this area. (Cf. Wahl-Ammann Appendix 1 also on this topic -- McIntyre strongly avoids, or simply chastizes as ad hoc, the false negative issues at lower frequencies that we raise concerning the use of r^2 .) He has done with the IPCC just what he did in reviewing the Wahl-Ammann paper--and indeed in all his efforts--write volumes of very strongly worded, one-sided critiques, which can take a lot of time to see through and then respond to. I hope what we have written can help you in this way. I note that Mike Mann, Richard Alley, and others have written response comments, which would be useful for getting perspective also.

Finally, note also that I corrected the reference to Wahl, Ritson, Ammann (Wahl et al., 2006) on page 6-6, and put the correct publication information in the reference section.

I hope this all helps. I would be happy to do my best to answer any further questions you might have.

All the best, and Peace, Gene
Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607-871-2604
1 Saxon Drive
Alfred, NY 14802

From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
Sent: Tue 7/18/2006 10:20 AM

To: Wahl, Eugene R
Subject: confidential

Gene

I am taking the liberty (confidentially) to send you a copy of the reviewers comments (please keep these to yourself) of the last IPCC draft chapter. I am concerned that I am not as objective as perhaps I should be and would appreciate your take on the comments from number 6-737 onwards , that relate to your reassessment of the Mann et al work. I have to consider whether the current text is fair or whether I should change things in the light of the sceptic comments. In practise this brief version has evolved and there is little scope for additional text , but I must put on record responses to these comments - any confidential help , opinions are appreciated . I have only days now to complete this revision and response.
note that the sub heading 6.6 the last 2000 years

is page 27 line35 on the original (commented) draft.

Cheers
Keith

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Date: 21 July 2006
To: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>,
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject:
Cc: Fortunat Joos <joos@climate.unibe.ch>
Bcc:
X-Eudora-Signature: <Standard>

Peck and Eystein
OK I am still struggling . I will not be able to get stuff to you til tuesday I reckon - masses of typing and having to re-read and consult with others (Henry will get back to me early next week) on the borehole stuff. Discussing stuff with Eugene Wahl (confidentially) and still need to check corrections and balance text. Tim still working on Figures. We are doing best to get stuff back asap - but if I have to incorporate Ricardo's stuff and put into version by Fortunat , it is getting more complicated. Fortunat should do edits relating to the rationalising of the forcing text (as per Gavin comment - or has he already?) . Best if Oyvind puts the lot together then.
Keith

--
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=====
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Date: 31 July 2006
To: "Wahl, Eugene R" <wahl@alfred.edu>
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: RE: confidential
Cc:
Bcc:
X-Eudora-Signature: <Standard>

First Gene - let me say that I never intended that you should spend so much time on this - though I really appreciate your take on these points. The one you highlight here - correctly warns me that in succumbing to the temptation to be lazy in the sense of the brief answer that I have provided - I do give an implied endorsement of the sense of the whole comment. This is not, of course what I intended. I simply meant to agree that some reference to the "divergence" issue was necessitated . I will revise the reply to say briefly that I do not agree with the interpretation of the reviewer. I am attaching what I have done (see blue highlighting) to the section in response to comments (including the addition of the needed extra section on the "tree-ring issues" called for by several people). I have had no feedback yet on this as it has not been generally circulated , but thought you might like to see it. PLEASE REMEMBER that this is "for your eyes only " . Please do NOT feel that I am asking /expecting you to go through this in any detail - but given the trouble you have taken, I thought it reasonable to give you a private look. Cheers
Keith

At 07:16 27/07/2006, you wrote:

Hi Keith:

Here is the text with my comments. I will go over the "stolen" parts (highlighted in blue outline) for a final time tomorrow morning, but I wanted to get this to you ASAP. The main new point I have to make is added in bold/blue font on pp. 101-103. I question the way the response to the comment there is currently worded, as it seems to imply that the divergence issue really does invalidate any dendro-based reconstructions before about 1850--which I imagine is not what you would like to say. I give a series of arguments against this as a general conclusion. Maybe I got over-bold in doing so, as in my point (1) I'm examining issues that are at the very core of your expertise! Excuse me that one, but I decided to jump in anyway. Let me know if I got it wrong in any way!

There are other quite minor suggestions (mostly focused on referencing other responses in a few places) that are also in bold/blue. These go on into the "120's" in terms of page numbers.

This is really a lot of work you've taken on, and I REALLY appreciate what you and the others are doing!

[I've also been a lot involved with helping to get a person from the Pew Center for Global Climate Change ready to testify in front of the House Energy and Environment Committee tomorrow. That is why I couldn't get this done and sent to you earlier today. Send Mike Mann and Jay Gulledge (Pew Center) all good thoughts for strength and clarity.]

NB -- "r" towards the end of the filename stands for my middle initial.

Peace, Gene
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1 Saxon Drive
Alfred, NY 14802

From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
Sent: Mon 7/24/2006 3:16 PM
To: Wahl, Eugene R
Subject: RE: confidential

Gene

here is where I am up to now with my responses (still a load to do) - you can see that I have "borrowed (stolen)" from 2 of your responses in a significant degree - please assure me that this OK (and will not later be obvious) hopefully.

You will get the whole text(confidentially again) soon. You could also see that I hope to be fair to Mike - but he can be a little unbalanced in his remarks sometime - and I have had to disagree with his interpretations of some issues also.

Please do not pass these on to anyone at all.
Keith

Will pass all comments to you before they are fixed in stone- nothing from review article will be mentioned.
Really grateful to you - thanks
Keith

At 05:08 22/07/2006, you wrote:

>Hi Keith:

>

>Glad to help. (!)

>

>If I could get a chance to look over the sections of my text you

>would post the comments before you do, I would appreciate it. If

>this is a burden/problem let me know and we'll work it out.

>

>If it is anything from the Wahl-Ammann paper, of course that is fine

>to use at once since it is publicly available. There will only be

>exceedingly minor/few changes in the galleys, including a footnote

>pointing to the extended RE benchmarking analysis contained in the

>Ammann-Wahl review article.

>

>What I am concerned about for the time being is that nothing in the

>review article shows up anywhere. It is just going in, and
>confidentiality is important. The only exception to this are the
>points I make in my blue comments in the big review file on page
>104, concerning the MM way of benchmarking the RE statistic. Those
>comments are fine to repeat at this point. [Please excuse my
>hesitance in this way.]

>

>Actually, all the other blue comments I made in the big review file
>are also fine to use at once.

>

>

>Again, if this request is in any way a problem, let me know and
>we'll figure out something.

>

>

>Peace, Gene

>Dr. Eugene R. Wahl

>Asst. Professor of Environmental Studies

>Alfred University

>

>_____

>

>From: Keith Briffa [mailto:k.briffa@uea.ac.uk]

>Sent: Fri 7/21/2006 2:00 PM

>To: Wahl, Eugene R

>Subject: RE: confidential

>

>Gene

>your comments have been really useful and reassuring that I am not
>doing MM a disservice. I will use some sections of your text in my
>comments that will be eventually archived so hope this is ok with
>you. I will keep the section in the chapter very brief - but will
>cite all the papers to avoid claims of bias. I really would like to
>discuss the whole issue of the reconstruction differences at a later
>, less stressful time. I completely accept the arguments about the
>limitation in the r2 and the value of capturing longer-term variance
>. I think I will have to stop now as the temp and humidity are killing here.

>

>Thanks a lot again

>

>Keith

>

>At 18:39 21/07/2006, you wrote:

>>Hi Keith:

>>

>>I'm sorry that there is a bit to digest...although I know it is just
>>a result of the nature of things.

>>

>>By the way, copied below is a synopsis that I sent this morning to a
>>person in DC who is working on all this with regard to the House of
>>Representative hearings. Evidently, there is to be at least one

>>more hearing next week, and Mike Mann will go. The person I sent
>>this to is trying to understand the importance of the proxy PC
>>issues --especially how, no matter what way the PC extraction is
>>done, the reconstructions converge if the structures actually
>>present in the data are not tossed out by truncating the number
>>retained PCs at a too low level. What I've copied is this
>>synopsis. I think it is straightforward -- maybe a bit dense, but
>>at least brief.

>>

>>Also, let me know if I can help on the issue of RE vs r^2 . I could
>>write a few brief sentences as something for you to look at if you
>>would like. Wahl-Ammann show very clearly that there is objectively
>>demonstrated skill at the low-frequency level of the verification
>>period mean for all the MBH segments, although the earlier MBH
>>segments do have really low r^2 values (indicating very little skill
>>at the interannual level). Our argument that to throw out the
>>reconstruction completely based on the fastest varying frequency,
>>when it has objectively demonstrable meaning at lower frequencies,
>>is to me quite reasonable. That it is some how entirely ad hoc, as
>>McIntyre claims in one (more?) of his comments, is neither logical
>>nor factual in my perspective. The idea of frequency dependent
>>skill/non-skill is not new to the literature, and the independent
>>re-reviewer that Steve Schneider had look over Wahl-Ammann said s/he
>>had experienced this issue in his/her work. G.

>>

>>

>>***** COPIED TEXT *****

>>

>>What it boils down to in the end is as follows:

>>

>>1) The different reference periods used to calculate proxy PCs from
>>N. America (calibration only for MBH, full period for MM) only have
>>the effect of re-arranging how the hockey stick shape appears across
>>the rank ordering of PCs. In MBH it is concentrated in PC1. In the
>>full-period method, it is spread over PCs 1 and 2. If one adds PCs
>>1 and 2 (either arithmetically or as vectors) from either
>>convention, you get an essentially IDENTICAL time series, only the
>>amplitudes are a bit different. [Note that the input data were
>>centered AND standardized before being put into the PC calculation
>>algorithm. This is important, as shown below.]

>> WHEN ACTUALLY USED IN THE RECONSTRUCTION, THE DIFFERENCE
>> IS MINISCULE -- MBH is colder over 1400-1449 by 0.05 degrees!

>>

>>2) IF the data are centered but NOT standardized and are input into
>>in a PCA algorithm using the variance-covariance matrix and not the
>>correlation matrix (the way MM did it), then the hockey stick shape
>>shows up in PC4. MM in fact reported this first in their 2005
>>Energy and Environment article. In effect, the first two PCs are
>>ARE ACTING TO DO THE STANDARDIZING OF THE DATA not done as a
>>pre-processing step. [When the correlation matrix is used instead
>>in the PCA algorithm, then the standardization is in effect done by

>>the algorithm, because all the correlations are "standardized" by
>>construction--they all range between 0 and 1.]
>> When 4 PCs from this calculation method are used rather
>> than 2 PCs calculated as above, then the RECONSTRUCTION CONVERGES
>> TO THE SAME AS ABOVE.
>>
>>3) Thus, all the different "flavors" for PC extraction have
>>essentially no effect on reconstruction when one does the exercise
>>of adding PCs sequentially from 2 to 5 for any flavor. In the case
>>of (1), the reconstructions converge by the second PC. In the case
>>of (2), they converge by PC4. They don't change with higher order
> PCs added.
>> THIS SHOULD BE EXPECTED FROM FIRST PRINCIPLES. That is,
>> the same underlying information is there in all cases, it is only
>> how the structures present in these data are spread across the rank
>> order of PCs, as explained. The simple exercise of taking the
>> reconstructions to convergence across the number of PCs used shows
>> this clearly.
>>
>>4) In fact, MM essentially say all this in the 2005 EE
>>article--INCLUDING ABOUT THE RECONSTRUCTION RESULTS -- but they
>>strongly claim that the movement of the hockey stick shape to the
>>4th PC shows it is not a leading pattern of variance as MBH claim,
>>and thus should not be used. This might be logical if their
>>analysis was an apples-apples comparison, but it is not, due to the
>>PCA method they use and applying it on NON-standardized data.
>> THESE TWO DIFFERENCES (which one can only fully get
>> from their actual code, not in the articles published) DRIVE THEIR
>> ENTIRE ARGUMENT ON THIS PARTICULAR ISSUE. What they do not say is
>> that convergence to something like the MBH result is expectable,
>> and indeed MUST happen given the data used, because the hockey
>> stick shape is actually IN the data, it is NOT an artifact of PC
>> calculation procedure.
>>
>>
>>5) FINALLY, note that all of this rests on the foundation that
>>keeping the bristlecone pine records in the data is appropriate,
>>which Caspar and I find can be reasonable presumption. If one
>>believes that the bristlecone data should be removed, then the
>>1400-1449 reconstruction does not pass verification testing with the
>>RE statistic, and the MBH reconstruction should commence from 1450 on out.
>>
>>Although there are a number of reasons to keep the bristlecone data
>>in, maybe the most compelling reason they are a NON-ISSUE is that,
>>over the common period of overlap (1450-1980), the reconstruction
>>based on using them from 1400-1980 is very close to the
>>reconstruction based on omitting them from 1450-1980. Since the
>>issues about the bristlecone response to climate are primarily about
>>1850 onwards, especially 1900 onwards [KEITH -- PLEASE LET ME KNOW
>>IF I AM NOT ACCURATE IN THIS], there is no reason to expect that
>>their behavior during 1400-1449 is in any way anomalous to their

>>behavior from 1450-1850. Thus, THERE IS NO REASON TO THINK THAT THE
>>BRISTLECONES ARE SOMEHOW MAKING THE 1400-1449 SEGMENT OF THE MBH
>>RECONSTRUCTION BE INAPPROPRIATELY SKEWED.

>>

>>

>>***** END OF COPIED TEXT *****

>>

>>Peace, Gene

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>>

>>_____

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>>From: Keith Briffa [mailto:k.briffa@uea.ac.uk]

>>Sent: Fri 7/21/2006 4:51 AM

>>To: Wahl, Eugene R

>>Subject: RE: confidential

>>

>>

>>

>>Gene

>>thanks a lot for this - I need to digest and I will come back to you.

>>

>>thanks again

>>Keith

>

>--

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