

## ***Interactive comment on “Millennial temperature reconstruction intercomparison and evaluation” by M. N. Juckes et al.***

**S. McIntyre**

stephen.mcintyre@utoronto.ca

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I am dividing this review in two parts- one part dealing with section 3 in which our work is criticized and one part dealing with section 4, the Union Reconstruction.

Juckes et al allege that our analyses contain a variety of errors, but do not cite or consider the following relevant literature: the reports of the U.S. National Research Council panel on Surface Temperature Reconstructions [North et al 2006 or the “NRC Panel”] and of the Chairman of the U.S. National Academy of Sciences Committee on Theoretical and Applied Statistics and associates [Wegman et al 2006], or the exchange in GRL between Huybers and ourselves (Huybers 2005; McIntyre and McKittrick 2005d - Reply to Huybers).

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Wegman et al concluded that our criticisms were “valid and compelling”. The NRC Panel specifically endorsed our key criticisms: of the MBH principal components method (p.85, 106), of reliance on bristlecone pines as an essential proxy (50,106, 107); of inappropriate estimation of confidence intervals (107); of the failure of MBH verification  $r^2$  statistic (91,105). It is really quite amazing that Juckes et al have ventured into this controversy without any consideration or rebuttal of these relevant authorities.

In MM2005a-b, we illustrated the difference between the MBH PC1 and the PC1 from a principal components analysis using covariance matrices, but also discussed results using correlation matrices - a procedure which is exactly equivalent to dividing by the standard deviation. In our Reply to Huybers, not discussed by Juckes et al, we gave a comprehensive discussion of standardization issues in the context of the North American tree ring network, illustrating PC series under a variety of standardization methods, including the method said by Juckes et al to have been “omitted”. See recent online discussion at <http://www.climateaudit.org/?p=929>. <http://www.climateaudit.org/?p=928> <http://www.climateaudit.org/?p=893>

In our Reply to Huybers, we observed that tree ring networks were in common dimensionless units and that statistical authorities (see references therein) recommended PC analysis using a covariance matrix in such cases. We are unaware of any general-purpose statistical text recommending use of a correlation matrix in such circumstances and Juckes et al did not cite any. We have never assumed that any PC methodology could extract a temperature index from the grab-bag assortment of North American tree ring chronologies and stated that the onus was on the proponent of any methodology to establish the validity of the resulting series as a temperature proxy. The NRC Panel (p. 87) considered this issue and stated that, “in this case, argument can be made for using the variables without further normalization” and, in effect, endorsed our position that the methodology needed to be proved from “scientific” (rather than a priori statistical) considerations. Obviously, this discussion should have been

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considered by Juckes et al.

Furthermore, even before the discussion in Reply to Huybers, we had previously discussed the impact of dividing tree ring chronologies by their standard deviation in MM2005b as follows:

“If the data are transformed as in MBH98, but the principal components are calculated on the covariance matrix, rather than directly on the de-centered data, the results move about halfway from MBH to MM. If the data are not transformed (MM), but the principal components are calculated on the correlation matrix rather than the covariance matrix, the results move part way from MM to MBH, with bristlecone pine data moving up from the PC4 to influence the PC2.”

If a centered PC calculation on the North American network is carried out, MBH-type results occur if the NOAMER network is expanded to 5 PCs in the AD1400 segment (as proposed in Mann et al., 2004b, 2004d). Specifically, MBH-type results occur as long as the PC4 is retained, while MM-type results occur in any combination which excludes the PC4.”

In total, these disprove the Juckes et al claim that we had “omitted” consideration of the case in which tree ring proxies had been “centred [and] normalised to unit variance (standardised)” (i.e. correlation PCs) or that we had committed “another apparent error: the omission of the normalization of proxies prior to the calculation of proxy principal components”, as asserted in their SI.

In a recent online discussion <http://www.climateaudit.org/?p=928> see comment #21, I presented these paragraphs to Juckes and challenged him to justify the above allegations. In comment #28, Juckes replied: “Re 21: Sorry I missed the fact that you had given an answer to some points on a later page”

Juckes et al have already withdrawn a false allegation that we had failed to archive our source code and, after the above admission, should also have withdrawn these further

false allegations concerning supposed “errors”.

In making these allegations, Juckes et al also perpetuated prior “academic check-kiting” by Wahl and Ammann. As support for the above allegations, Juckes et al cited statements on this topic in Wahl and Ammann (Climatic Change 2006). However this article did not itself demonstrate any of the alleged errors; it merely re-stated allegations from Ammann and Wahl (submitted to GRL). However, the Ammann and Wahl submission to GRL was rejected, in part, because, like Juckes et al, it failed to consider, let alone advance beyond, the prior exchange with Huybers.

This is not the only incident of academic check-kiting in Juckes et al. Juckes et al also cite Jones and Mann 2004 in connection with an alleged error in MM2003. Jones and Mann 2004 merely re-stated an allegation from a then unpublished submission by Mann et al to Climatic Change. The submission by Mann et al to Climatic Change was subsequently rejected.

Juckes et al claimed that an alleged “misunderstanding” of a then unreported “stepwise” principal components method was a “major factor” in the MM2003 conclusion that MBH principal components had been incorrectly calculated. I deny that this MM2003 conclusion was incorrect. Our claim - that MBH principal components were incorrectly calculated - has been endorsed by both Wegman et al and the NRC Panel.

I also deny that any alleged “misunderstanding” of the then unreported MBH “stepwise” PC method was a “major” or even a minor factor in our conclusion that the MBH principal components were incorrectly calculated. (In passing, “stepwise” principal components is not a method that we have seen used outside the MBH corpus and the validity of the method should be established before its “correctness” is asserted.)

There is more than one discrepancy between the methodology actually used in MBH98 and the methodology said to have been used. In MM2003, we had not fully disentangled the multiple problems in MBH98 PC methodology. In addition to the de-centering problem and unreported stepwise methodology, the data then available at Mann’s FTP

site - the url being specifically provided by Mann's associate, Scott Rutherford - contained spliced PCs from different steps, which, in addition, had been incorrectly collated, so that some networks contained identical 1980 values to 8 decimal places for as many as 7 different PCs. We specifically and intentionally avoided using networks that obviously had been incorrectly collated - which included the NOAMER network - and illustrated the defective MBH PC calculations with a short network (the AUSTRAL network, which was not affected by the collation problems. By doing so, we used a network which was unaffected by the "stepwise" methodology. Thus, while there were various additional problems related to the incorrect splicing of stepwise PC series in the MBH98 data archive then online, these were not a "major factor" or even a minor factor in the example that we presented. Instead of considering our example, Juckes et al (see Figure 2) switched the example, substituting another network (the NOAMER network) which was affected by stepwise issues - but one which we intentionally did not use in MM2003 as an illustration.

Juckes et al discuss and illustrate results using a variation of the incorrect MBH principal components methodology (mbhx) in which the short-segment standardization is carried out on a segment of 150 years, rather than 79 years. Since the short-segment standardization method has itself been found wanting by both Wegman et al and North et al, I see little purpose of introducing the mbhx variation into peer-reviewed literature.

Similarly, Juckes et al discuss and illustrate results in which North American tree ring series ending prior to 1980 are excluded from the network, resulting in a diminished network of 56 series. Juckes et al say that this analysis is responding to an issue raised in MM2005, but this claim is incorrect. In MM2003, we noted that many 1980 values were obtained from extrapolations. However, in subsequent exchanges between MBH and ourselves, it became clear that this was not a major issue in terms of yielding variant results and was not carried forward into our 2005 articles as a key issue. There are many issues which are in play (e.g. the impact of bristlecones). Given the already crowded controversy in this field, I see little purpose in reviving an issue

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in peer-reviewed literature that is not actually in controversy and which has negligible impact on any result.

Juckes et al misrepresented our discussion of MBH99. In MM2005b, we explicitly stated that the key issue in MBH99 was the validity of bristlecones as a proxy, not principal components methodology (which did affect the 15th century networks). We observed that bristlecones in MBH99 received heavy weighting merely through longevity and not through the erroneous MBH98 principal components method. Here Juckes et al have distorted our analysis and constructed a straw man - see discussion at <http://www.climateaudit.org/?p=926>

There has been extensive discussion of various aspects of Juckes et al at [www.climateaudit.org](http://www.climateaudit.org) - see <http://www.climateaudit.org/?cat=36>.

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Interactive comment on Clim. Past Discuss., 2, 1001, 2006.

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2, S697–S702, 2006

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