

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

**M. N. Juckes et al.**

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**S. McIntyre:** ([cpd-2-s697\\_p](#))

Para 1: We allege there are serious flaws in McIntyre and McKittrick (2003, EE) and McIntyre and McKittrick (2005, EE). We do not say that every statement in these papers is false and the fact that some statements in those papers are indeed true does not have any bearing on the the assertion that there are serious errors.

Para 2: We are reviewing peer reviewed literature and are primarily interested in estimating the temperature of the past millenium. Prof. Wegman’s views on who Prof. Mann might have talked and his survey of who has written papers with whom are very interesting, but not on the topic of our review.

Para 3: We are concerned with the temperature reconstruction, not with the principal

components themselves. Now that the code used in MM2005 has been made available some aspects of the calculation are clearer.

Para 4: The units are dimensionless, but not common.

Para 5,6,7,8,9, 10: See comment on para 3.

Para 11: We never suggested that the code was not archived. Since publication McIntyre has revealed the location of the archived code (an edited version of the code originally used, which does not appear to have been archived), provided an updated version correcting for the omission of the function which carried out the reconstruction, and added configuration files.

Para 12: We repeat those aspects of Wahl and Ammann's calculations which are essential to our discussion. These are placed in the Appendix. There are variations between our approach and that of Wahl and Ammann, which are referred to in the manuscript.

Para 13, 14, 15, 16: The major finding claimed by MM2003 concerns the temperature of the reconstruction. We are concerned here with the temperature of the reconstruction. The reconstruction in MM2003 cannot be defended.

Para 17: These comments are included to point out that certain claims which have been made in the published literature and which are known to be false (including by the author of those claims, it appears) have not been withdrawn.

Para 18: See comment on para 3.

### **S. McIntyre, (cpd-2-s708\_p)**

Para 1: There does not appear to be a corresponding review in the NAS report.

Para 2: I believe, on the basis of discussion elsewhere, that the "elementary statistical test" referred to here is the Durbin-Watson test, which relates to the correlations of the residual. This test is not relevant to the composite technique. The NRC panel

are concerned primarily with multiple regression techniques which are not used in the majority of reconstructions.

Para 3, 4, 5: The idea that data can only be used once is going to need a little more justification before it gains wide acceptance. The issue is not how many times the data was used, but how it was selected. We are not claiming independence from past studies.

Para 6: The Durbin-Watson test does not test for spurious correlations.

Para 7: We use all the available data for calibration. Again, the recommendations of the NRC panel relate to the use of the Mann et al. technique which we do not employ, except in order to comment on past work.

Para 8: The significance given is, as stated, the significance of the correlation between the composite and the instrumental temperature in the calibration period.

Para 9: The appendices are elementary and are provided to clarify the formulae used.

Para 10: Our main results do not use the Mann et al. PCs. We used them in order to evaluate and comment on past work. The flip in sign of the Chesapeake series was an error. This is corrected in the revision.

Para 11: The source code is the code used to carry out the calculations. It is provided to ensure full transparency.

Para 12: This will be changed.

Para 13: The Sargasso Sea series finishes well before the end of our calibration period, so cannot be used in our reconstruction. It has been used in one peer reviewed study and cited on at least two web sites with its dating erroneously shifted 50 years forward, so that the last data point, which represents the 1900 to 1950 mean is instead presented as the 1950 to 2000 mean. The data file stored at the WDCP is ambiguous in this respect, but the data was clearly collected at a time when it could not represent

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the 1950 to 2000 mean. We have put all the data used in our study online: the Indigirka series is not available for publication in this way.

Para 14: The geolocation information does not affect the results: it will be corrected in the revised version.

Para 15: There is some confusion here between the requirements of different analytic approaches. The revised version seeks to make our modelling assumptions clearer. In particular, we do not assume that the signal to noise ratio in individual proxies is greater than unity. A simple estimate suggests that is not. In this situation selecting proxies on the basis of their individual correlations with temperature is inappropriate. The peer reviewed literature does not have clear evidence of a substantial CO<sub>2</sub> fertilization effect. We note that all the proxies are influenced by factors other than temperature.

Para 16: Extensive and ill-informed.

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

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