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Climate of the Past Discussions

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

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This paper deals with the intercomparison and evaluation of millennial temperature reconstruction over the Northern Hemisphere.

There are a few major points I would like to highlight

1) First of all, in my opinion this paper does not provide much new results on different aspects of past temperature change compared to previous reviews of Jones and Mann (2004, Rev. Geophys) as well as Mann (2007; Annual Review of Earth and Planetary Sciences). The main findings of that study have already been reported previously by other papers. It uses already published proxies from different places from the NH that have recently been incorporated for long temperature reconstructions. It describes two specific methods applied to 18 proxies and shows the similarities and

differences. 2) Actually the introduction until page 20 is quite interesting and gives a nice overview, however, also here there is not much new. It does possibly does not need to be so as it should be a review. However, in my opinion, it could be much shortened as much is already mentioned in the two reviews of Jones and Mann as well as Mann (2007). There are papers in press by Wahl and Ammann and Ammann and Wahl that cover more or less also section 3 that discusses the critics of the IPCC consensus on millennial temperature. I would therefore suggest, to really point out the new topics of the whole discussion and the relevant points related to the question of the methodology and its influence on past temperature change. 3) The author selected two reconstruction techniques and compared them with each other. That choice is arbitrary and the results and interpretations are not convincing. What are the arguments that one method should be used in favor of the other? Is this choice dependent on the choice of the number of predictors, the location, the type of proxies and if one fits on annual, summer, extended summer, full northern hemisphere, land only, extratropics, etc? In my opinion, the reader gets confused in this aspect and the interpretations do not help in that respect. There are a few other methodologies recently applied or which could be incorporated to reconstruct large scale temperature. Those are ordinary least squares, total least squares, MBH (1998), Kalman Filter, a mixture of those, RegEM, as well as neuronal networks. Most of those methods have been described in the introduction. However, what is missing is an analysis applying all those methods to the 'union predictors'. That would show the full amplitudes of past climate change . Further, filtered uncertainties should be calculated and plotted. That would help for the interpretations. In my opinion, a paper that intercompares and evaluates millennial temperature reconstructions should take into consideration all methods to show the full range of past climate variability, with all the advantages/disadvantages inherent in those approaches. 4) The second related point to check whether a specific method seems to perform 'better' would be to use coupled paleo runs. There are two 1000 year long runs which can be used. Without such an analysis in a surrogate climate it will not be possible to give one method a higher priority and another not. This of

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course depends also on the sensitivity of the paleo climate simulation. 5) I suppose, the authors show annual averaged temperature data? That is not clear. Further, I have my doubts about the choice of the predictor data in the new union reconstructions. For instance, if annual data is the target, then tree ring from Scandinavia more reflect the summer season. Another example is the tree ring data from Morocco, which are a clear winter precipitation signal. I still do not understand how such a predictor can be used for an annual temperature reconstruction? Is there a teleconnection through the winter and finally also annual NAO, if yes, the authors may show that this teleconnection is significant and stable through time. The choice of the predictors is really an important step and in my opinion, the criteria which ones to choose and which ones to leave out is not well described. I have also some doubts about the choice of the proxies in terms of location and distribution over the Northern Hemisphere. There are new high resolved climate proxies available for instance from North America and Eurasia that were recently published and that could be used as well. That would provide a better spatial coverage over the Northern Hemisphere. 6) Other issues that might be worth addressing are: Sensitivity to the calibration period, whether to detrend or not as well as the color of noise related to the model data. 7) It would also help if the authors could come up with some recommendation concerning the use of those methods for different applications. When should we use which method and why? Also, how do those methods perform if, like in the case of Moberg et al. (2005, Nature) proxies with different temporal resolutions are combined? Could the authors say anything about the methods that aim at sub-hemispheric reconstruction, resolve seasons and other climate parameters such as rainfall?

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