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Climate of the Past Discussions

Interactive comment on "Northern high-latitude climate change between the mid and late Holocene – Part 2: Model-data comparisons" *by* Q. Zhang et al.

Anonymous Referee #1

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This manuscript presents an interesting attempt to compare model simulations of the mid-Holocene with multi-proxy temperature reconstructions for the northern latitudes, taking into account the uncertainties in the multi-proxy reconstructions. The methodology offers an interesting alternative to previous mode-data comparisons over Europe or high latitudes. It is difficult however from the present state of the manuscript to fully understand what we gain compared to what was already done. Additional discussions and comparisons with previous findings would be welcome. This is needed since none of the comparisons consider all the aspects of the changes in climate. This includes

in particular the studies by Guiot et al, 1997, Bonfils et al. 2004, Brewer et al., 2007, Wholfart et al. 2004 and 2008, for which either bioclimatic variables from pollen data of simulations of the vegetation using a biome model were used. The present manuscript refers to the compilation of data presented in Sundqvist et al. (Part 1, same special issue), which implies that its acceptance for publication is tied upon part 1 manuscript. I indicate also below the major comments that need to be considered before publication:

1. An interesting point is that this manuscript combines information from different proxy sources. Could the authors tell more of the advantage and disadvantage of this approach compared to the one from Cheddadi et al (1996), Davis(2003) or Biglow et al. (2003) who consider mainly pollen data?

2. In most PMIP model data comparisons over land the climate variables considered are not Summer, winter and annual mean temperature, but bioclimatic variables such as growing degree days, temperature of the coldest month and a humidity index that have been identified to better reflect the plant physiology. Even though this information was certainly not available in most of the publications that were considered in the data synthesis, it would be interesting to tell a little bit more about possible caveats between model variables and data reconstructions depending on the type of record. This may also have implication for the estimation of the magnitude of the signal, as well as for comparison with the results of previous studies.

3. Eduardo Zorita, made an important comment on the weight you use in the cost function, and I share similar concern. For the homogeneity of the formula you should have a variance and not a standard deviation in the wi.

4. You could also have considered the uncertainties in the model output by considering the model interannual or decadal variability. Monthly temperature values are available in PMIP database for most of the models considered. At least it would be great to know how model variability and noise impact the cost function for one or 2 models.

5. Even though you do not consider the same data, and not exactly the same region

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you should compare your results with those of Brewer et al. 2007 who also included most of the PMIP2 simulations. In addition Bonfils at al. 2004 also provided possible changes in atmospheric circulation that are compatible with data synthesis. Here the feedback from vegetation and sea-ice is an important factor that you seem to clearly isolate from the OA and OAV simulations. Additional analyses on the feedbacks that could complement Braconnot et al, 2007, by including both changes in the atmosphere and in the ocean circulation would be welcome.

6. I am not sure you can easily combine the different cost functions (summer, winter and annual) into one number. The reason is that annual mean temperature is not independent of the two others. Could you also tell what would be the cost function if a model produces 0 change compared to present day ?

7. It is interesting to see that OAV models have a better match with data. However, the number of model used is different between OA and OAV simulations, and the cost function seems to depend on the number or point considered. Also I believe that a statistical test would not be able to distinguish between the “best OA” simulations and the OAV simulations, as far as I can judge from figure 5. Could you argument on this a little bit more?

Minor comments 1. P 1666 . In PMIP1 the models were integrated at least for 11 years and the last 10 year average was stored in the database. For PMIP2 the models were run at least 100 year after the coupled model reached equilibrium and the last 100 yr of the simulation were stored in the database. Please modify slightly the text here so that the reader doesn’t believe the simulations are only 10 or 100yr long.

2. P 1669. It is true that the OAV simulations better reproduce the high latitudes, but you should not go too far in your conclusion, since other studies (Wohlfart et al., 2004, 2008) also show that they tend to produce a continental drying that is not seen in the data.

3. P. 1662. Brewer et al. (2007) included most of the PMIP simulations in the model

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data comparison, except the most recent one (mainly new OAV). Also check that you have the last version of the simulations in your analysis, because substantial errors were found in some of the simulations.

Interactive comment on Clim. Past Discuss., 5, 1659, 2009.