

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 #3

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General comments

The paper by Zhang et al. concentrates on the non-trivial and necessary task of comparing model results to paleo reconstructions for the Mid-Holocene, thereby focussing on the high latitudes. Model simulations of different generations and complexity performed within the PMIP framework are evaluated against available proxy data by means of a cost function approach.

The paper is well written in its overall structure. I would recommend, however, to modify

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the paper's current state in order to more clearly emphasize the gain of knowledge by this specific study in contrast to existing PMIP intercomparison studies. Technically, the paper should be corrected with respect to minor language and typographical mistakes which are still quite numerous.

Specific comments

1. The abstract is rather long. I would suggest to shorten the first part referring to PMIP and PMIP results. It is not directly evident for the reader whether the authors are referring to earlier studies and what is actually part of the present study. It is important to more clearly emphasize the focus and major outcome of this paper. Is it the model-data comparison? Is it the focus on high latitudes? Is it the application of a cost function? Something like Section 1, p. 1663 l.9... would fit into the abstract as well.
2. To be more consistent with the paper title, the abstract should emphasize more the high latitudes since this is one major aspect of the paper.
3. Section 2: If the companion paper by Sundqvist et al. gets accepted, this section probably could be considerably shortened by referring to the Part 1 paper.
4. Section 3: The last paragraph of this section can be omitted. This has been stated earlier in the section already.
5. Section 4 p. 1669 l. 21: "The averaged temperature ... closer to reconstructions....". Either a citation has to be given here or this should be the subject of the next chapter on the model-data comparison.
6. Section 4, p. 1669 l. 23: "It is clear..." This does not necessarily have to be the case. Due to models' deficiencies, discrepancies with observations might be even larger.
7. Figure 5: I am not sure to which extent calculating the average of the different cost function values is actually meaningful. Seasonality has been shown (by this and other papers) to be important, so averaging different CF values will lead to a loss of

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information gained by the discussion on a seasonal basis.

8. Section 5, p. 1671 l.2: Not everyone might be familiar with this meteorological method. What exactly do the authors mean by that?

9. Section 5.1: As the authors correctly state at the end of this section, it is not easy to identify the simulation which best corresponds to the reconstructions because the CF value itself is hard to interpret objectively. So the problem I see with this method is that the objective method presented needs a subjectively defined CF threshold beyond which a model is considered to well fit the reconstructions. The authors should discuss this problem. Would their line of arguments change if not just FOAM and MRI OAV models had been selected as “best fits”?

10. Section 5.1, p. 1672, l.5-8: Figure 3 does not show the reconstructions and therefore does not allow any conclusions about the agreement between models and reconstructions.

11. Section 5.1 / Table 3: It is unclear whether first the ensemble mean (e.g. of the SST-f experiments) has been calculated and then the CF value given in Table 3 has been determined or whether first the CF value for each simulation within an ensemble (such as PMIP2-OA) has been calculated and the average of several CF values is given in Table 3. This should be described in more detail to avoid misunderstandings.

12. Section 5.1, p. 1672, l. 18: “It again objectively confirms...in better agreement”. This sentence needs to be clarified. Do the authors mean “better than PMIP1-SSTf” or “PMIP2-OAV better than PMIP2-OA”?

13. Section 5.3: Gladstone et al. (2005) also discuss the NAO at 6 ka in PMIP2 simulations. It would be interesting if the authors could put their results more in context with Gladstone et al.’s results.

14. Minor comment on p. 1665 l.9: “warmer temperatures” is wrong. Temperature is a physical quantity. I would write “higher temperatures” or “an increase in temperature”.

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Technical corrections

1. p. 1662 l. 15: “Existing data-model....” This sentence is too long and complicated. It can easily be split up in several parts.
2. Section 1, p. 1662 l. 21-26: I would add a citation on this evidence for polar latitudes. If the authors are referring to Masson-Delmotte et al., the citation should be repeated.
3. p. 1669 l. 8 versus l. 10: “This is shown....” versus “....(not shown)”
4. p. 1671 l.20: “..larger uncertainties contribute less..”
5. p. 1675 l.24: “...towards a more positive phase of the North Atlantic Oscillation...”
6. The authors should try to consistently use the PMIP acronyms PMIP1-SSTf, PMIP2-OA, PMIP2-OAV. Section 5.1. provides a mixture of these (PMIP2-OA vs. PMIP-OA).
7. Tab. 2: I would mention “high latitudes” in the caption.
8. Tab. 2: The description of the experiments is slightly misleading because greenhouse gas forcing is also included in the simulations. So I suggest to use the standard PMIP abbreviations such as “PMIP1 SSTf, PMIP2 OA, PMIP2 OAV” in the table itself.
9. Figure 1: It would be nice to indicate by different colours which of the individual sites represents July/August, January or annual mean values.
10. Figure 3, caption: I would add a remark such as “Note that the anomalies are computed with respect to the control simulation of the respective model.”
11. Figure 5: The curves are a bit misleading. I would just show dots or bars to emphasize the discrete nature of the results.
12. Figure 5, caption: “The valueS of the cost function IS...” (singular vs. plural)
13. Figure 8: Are really both y-axes of panel a) given in percent?

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