**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 #2

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This manuscript describes a model-data comparison for the mid-Holocene focusing on the northern high latitudes. A new method similar to the identification of analogues in meteorology is presented for comparing different model simulations with multi proxy data. This approach looks interesting and is a nice addition to already existing methodologies. My general suggestion is that the authors should include more details in the discussion of the results, both with regards to the proposed winter warming mechanisms (NAO and ocean-sea ice change) and with regards to comparing their results to other model-data comparison studies. The paper is generally well written. However,

there are several typos and excessively long sentence that should be corrected. I think the paper fits nicely with the scope of CP and may be published after considering the comments below.

Major comments:

1. I agree with previous comment made by E. Zorita regarding the weight function used in the cost function. The authors should check this carefully.

2. The cost function does not take into account the model uncertainty. The Arctic is usually a region with large natural variability, and thus a relative low signal-to-noise ratio. How is the natural variability in the models? Could the authors comment on this?

3. The authors point to the role of shift towards a more positive NAO during 6k as one possible mechanism for explaining the winter warming seen in the data. I think the discussion of the possible role of the NAO in explaining the winter warming signal over northern Fennoscandia and northeastern Eurasia is too short and needs more details. In particular a comparison to the Gladstone et al. 2005 paper would be welcome. For instance according to Fig. 7b and the text the MRI model shows a mean shift towards a positive NAO. However, according to Gladstone et al. 2005 the MRI shows little change in the NAO at 6k compared to 0k. Could you please comment on this? Is the SLP response very different in the MRI-OA version?

4. According to Gladstone et al 2005 the FOAM, HadCM and MIROC models were all closer to NAO+ regime in the 6k simulations. Yet, both MIROC and HadCM3 have very high CF values for winter (Fig. 5). It would be interesting to see the winter temperature and SLP response for these models as well. Could it be that the circulation is more zonal in the MIROC and HadCM models, so that the warming anomaly does not penetrate as far north as indicated by the proxy data?

5. Another mechanism for explaining the winter warming the authors suggest is related to an accumulation of heat in the ocean during summer and subsequent release of this heat to the atmosphere during winter. However, as noted by E. Zorita in his comment, the maximum warming occurs over the Barents Sea in regions that presumably are covered by sea-ice. Wouldn't this prevent the heat from being released to the atmosphere? A map of the sea-ice extent for 6k compared to 0k would be helpful here. How much of the sea-ice in the Barents Sea are retreating in the simulations during winter? What models show the largest change in the Barents Sea sea ice extent?

6. What is the role of the ocean circulation in the FOAM and the MRI models? You could imagine a more positive NAO pushing more warm Atlantic water into the Nordic Seas and subsequently into the Barents Sea, melting more sea ice and exposing more of the ocean to the atmosphere. A figure showing the surface currents in control together with the anomaly would be nice. At least it should be part of the discussion.

7. Why do you not show the MRI model in Fig. 8? When you use the CF to pick two models you should at least stick with these models, and show comparable results for them.

Minor comments:

1. In Fig. 1 the authors show the distribution of the different temperature reconstructions. For easier reading I suggest that you give each season a different color (for instance red for summer, blue for winter and black for annual).

2. Overlapping contour lines will help to read the scale of the SAT and SLP changes in Figs. 4 and 7.

3. P1661, line 23: "Climate model simulations . . ." This sentence is not well formulated and should be rephrased.

4. P1666, line 18: The models were run for a longer period than 10 and 100 years for PMIP1 and PMIP2, respectively. Please correct.

5. P1669, line 3: I think you are referring to Fig 3d here and (autumn response) and not Fig. 3a (winter).

6. P1670, line 4: "We selected the six simulations with summer temperature responses closest in magnitude to the average . . .". Which average?

7. P1675, line 12: "Again, the reason for such marked differences between the simulations is the lack of proxy data to constrain the models for these regions". What are you trying to say here? The lack of proxy data is not the REASON for model differences (that is related to the different physical schemes, parameterizations, resolutions etc used in the different models). Please rephrase.

8. In Fig. 8a the label on the y-axis says "Percent". I think this is wrong. It looks more like 1000*km2. Please check.