



EGUsphere, referee comment RC1
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Comment on egusphere-2022-993

Anonymous Referee #1

Referee comment on "Deglacial climate changes as forced by different ice sheet reconstructions" by Nathaelle Bouttes et al., EGU sphere,
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Review of
Deglacial climate changes as forced by ice sheet reconstructions
by N. Bouttes et al.

The manuscript presents the results of the transient simulation of the last deglaciation with iLOVECLIM, following experimental designs of PMIP4. The authors present seven experiments with two different ice-sheet reconstruction datasets and different treatments in bathymetry or freshwater influx to the ocean. They show the evolutions in temperature, ocean circulations, and other climatic variables. They discuss why the simulated climate fields differ between experiments, and discuss the link between climate forcing and climate changes.

Overall, this article's experiments, results, and analysis are very good, and the manuscript is written and easy to follow. The systematic experiments in this study would help other climate modeling groups. The article is worthy of publication in the *Climate of the Past*. Still, I would appreciate it if the authors addressed minor points as detailed below.

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Title of the manuscript: I wonder if "different ice sheet reconstructions" or something like this phrase might be better to describe this article, as ice sheets are not the only forcing of the last deglaciation. And the evaluation of seven different experiments (two ice sheet reconstruction datasets + different boundary condition treatments) is the advantage of this article.

L20: recommend adding "global-mean temperature".

L25: The thing that the "link" refers to might not be clear: Link between [reconstructed freshwater fluxes and recorded AMOC] or [reconstructed freshwater fluxes and their representation]? I would recommend rephrasing this sentence.

L31: global mean surface temperature

L32: deglacial "surface temperature" changes?

L44-46: Reading this paragraph (L29-46), the logic of "difficult to disentangle the links" may not be clear.

L79: If I understand correctly, no previous study performed simulations evaluating the relative effects as in the following paragraph. So I would recommend stating "has not been evaluated" instead of just "unclear".

L107: Please clarify the modifications. Modification in codes or just applied time-dependent forcing?

L117: Please indicate the treatment of the terrestrial biosphere in the transient simulations. Is this prescribed or forecasted in the vegetation/land surface model?

L218: the main text says Figure 5 is 21-8 ka, but the caption says 21-10 ka.

L234 (Figure 6): As the salinity is conserved in the simulations, the global mean ocean salinity would be simply the inverse of the ocean volume (Figure 2a), so I wonder why Figure 6 is necessary. Does the mean salinity provide peculiar information?

L243: Please indicate that the vertical axis of Figure 7 uses different scales between the model and ice core data (like normalizing between LGM and Holocene).

L250: Please indicate the data references of the NGRIP and the EDC.

L253: I'm not convinced with "with a larger amplitude than at a global scale". I recommend writing out the number of global-mean/NGRIP/EDC temperature changes in the text following Figure 7.

L255: One standout from Figure 7c-f is the significant difference between ice core data and two simulations because the AMOC keeps its intense mode (shown in later in Figure 11). I recommend to briefly noting here that the AMOC was mostly intense in these two simulations, and that's one reason for the significant model-data difference in Figure 7c-7f.

L257-259 (Figure 8): It is unclear which panel is explained in the main text. Does "15-14 ka onward" refer to the panel of 65-90N?

Also, I would ask authors to consider adding a 2-d map of albedo at key time slice (e.g., Southern Hemisphere at 13 ka with two ice-6g_c experiments), which might help to understand.

L262: Is it possible to indicate the typical values of the albedo of continental surface and sea ice?

L268 (Figure 9): why there is a sharp increase in ice-6g_c evolving bathy exp at the end of the simulation?

L374: It seems FWF/3.5 in Figure 15a not used in the manuscript

L404-411: I agree with the authors that we should account for bathymetry changes, but discussion from model-data comparison in this point would be necessary. For example, the Figure 7e experiment has a sharp warming at 13.5ka, which seems to be absent in ice core data. I wonder if this warming only in model results might be a "right" climate response but a different time period, or if it is technically still challenging points to account for realistic bathymetry/coastline changes in the model, or uncertainties in ice sheet reconstructions. Please discuss this point further.

L441-443: It may be noted that some previous studies (appears in Figure 13) used simplified areas in freshwater, compared to this study utilizing river routing.

L446-448: Please clarify the logic of this sentence, because

- The sentence discusses 18-15ka, but results from Zhu et al. (2014) exhibit AMOC reduction mainly after 15ka.

- According to Figure 16b, "smaller evolution of sea level equivalent than in the reconstructions " may not be necessarily true in terms of total sea level rise between 18-15ka.

- I guess you expect weaker AMOC at 18-15ka (based on introduction L40-41), but not sure from this paragraph.

I agree with the final sentence, "either the ice sheet and sea level reconstructions should be revisited...", but I think you may refer to previous studies (e.g., Ivanovic et al. 2018 paleoceanography), and how the present study improves the discussions on this topic.

L24 (abstract): the phrase "This questions the links..." might be somewhat strong compared to the discussion subsection 4-3. I would recommend reconsidering the abstract sentence.