

Response to reviewers #2

The authors thank the editor and both reviewers for their constructive feedback on the structure of the manuscript and recommendations for additional discussion on limitations and atmosphere-ice sheet feedbacks. We agree that these points would help improve the readability of the manuscript and provide a more complete understanding of the results in a broader context. We are happy that the reviewers support the publication of this work following these modification and will now outline how we have addressed all the points raised.

Editor comments

1. Separation of results and discussions. This point has now been raised by three separate reviewers. A separation of results and discussions is also in line with the usual layout of a TC publication. I am aware that such a separation seems forced at times as it can be much more intuitive to read both together. However, it generally leads to a clearer structure and easier navigation through the manuscript. I would thus encourage you to take up this feedback from the reviewers.

The results and discussion have been separated into new sections. The new structure is:

3. Results

3.1 Ensemble

3.2 Parameter sensitivities

3.3 Ice dynamics

4. Discussion

4.1 Comparison to reconstructions

4.2 Influence of parameters on ice sheet configurations

4.3 Atmosphere-ice sheet feedbacks

4.4 Limitations and recommendations for future work

In addition to moving text from the results into the discussion section, Figures 4, 5, 8, 11 and 12 have also been moved and are now Figures 10, 11, 12, 13 and 14 respectively. Additional text has been added to the discussion under sections 4.3 and 4.4 to address the reviewers additional comments.

2. Discussion of effects of resolution, constant vegetation and lapse rate. So far most of the resolution discussion revolves around the ice sheet model resolution. E.g. On lines 319-338 you discuss the imprint of resolution on temperature biases which is great and addresses a major point raised by one reviewer. Effects on precipitation are also touched upon but both points can be easily missed by readers and reviewers due to the combination of results and discussion.

We have expanded on the discussion of limitations including climate model resolution and use of prescribed SSTs and moved this to its own section so that it is not as easily missed – Sect 4.4.

3. Ice-atmosphere feedbacks. One review pointed out that a discussion and illustration of atmospheric circulation changes due to ice-sheet-atmosphere feedbacks are so far missing. A figure on atmosphere circulation changes has been suggested. This is a valid point and would nicely align with the great discussion on ice sheet evolution.

We have added a new section in the Discussion (Sect. 4.3) discussing the feedbacks that may occur between the atmosphere and ice sheets and included a figure showing the effects of the different LGM and PGM ice sheets configurations simulated in this study on the position of the jet stream, precipitation rate and surface temperature (Figure 15). However, we want to highlight that these results should be highly caveated by the low resolution of FAMOUS which is not suitable for studying these effects in detail (L550-552).

Reviewer #1 comments

1. While the combination of the results and discussion section works well for the discussion of the results, it also leads to a sparse discussion of limitations of the simulations, e.g., effects of the low resolution, non-dynamic vegetation set to present-day, use of a constant lapse rate. The manuscript could benefit from discussing the limitations more and whether the authors expect their parameters and NROY reconstructions to hold beyond these limitations (e.g., for higher resolutions or a coupled ocean) and why. In particular, is there an argument for prescribing SSTs besides the cold-bias in the ocean model and would the authors expect no (significant) improvement even with an ocean component without such a cold bias/ with a corrected cold bias?

We have now separated the results and the discussion and expanded on the limitations of the study in a separate Limitations section (Sect. 4.4). We have touched on the effects of using a coarse resolution climate model on precipitation and smb etc (L554-574), impact of no precipitation downscaling (L575-578) and limitations of prescribing SSTs and difficulties in running fully coupled simulations including a dynamical ocean (L579-594). We discuss whether the results will hold beyond these limitations in L517-523 and L576-578.

We expect that including the ocean component could provide useful information on the role of ocean-ice feedbacks on ice sheet evolution, however, biases in the modelled ocean would need to be corrected via tuning for these results to be of use. This would involve additional work and resources that was beyond the scope and constraints of the study. In addition to minimising biases, prescribing sea surface conditions allows us to control aspects of the Earth System and isolate feedbacks and mechanisms. We expand on this discussion in L585-594.

2. L102-105 & L129: It should be mentioned here that atmosphere only is used

This has now been added (L89 and L115)

3. L281-298/Sect 2.4: Considering how similar this is to Patterson et al., 2024, including in figure design (Fig.2), I would suggest referencing that study here again and highlighting commonalities/differences.

This is now included (L257, L259-261)

4. L456-464: It would be good to mention in the beginning that this paragraph discusses the LGM simulations.

Added 'LGM' in L453

5. L510-515: The spacing before and after this paragraph is confusing. Considering that the paragraph belongs with the following ones more than the previous one, I would expect the extra spacing between paragraphs before and not after it. Further, the first sentence (L510-511) could be shortened and combined with the next one (e.g., "Among the four most relevant parameters (elevcon, cw, tgrad and beta), the sensitivity of the LGM NAIS...")

The first sentence in original L510-511 has been removed and the start of the next sentence now begins a new paragraph in Section 4.2 (L465).

6. L580-583: This sentence is quite convoluted and grammatically incomplete. I would suggest splitting it in two.

This is a typo and the missing full stop is now added (L362)

7. Fig. 11: Panel labels are missing

They have now been added (Fig 13).

8. Fig. 12: Panels b and c are hard to compare as the y-axes differ, I would suggest combining the panels into one

Panels b and c have been combined into one and the units have been changed to kg^3/yr for clarity (Fig. 14)

9. Typos/ editorial comments:

L44: “One period...” I would suggest starting a new paragraph with this sentence. [Corrected L32](#)

L425 “reconstrcutions” [Corrected L434](#)

L436: “degalcaition” [Corrected L437](#)

L441: “CRHONO” [Corrected L442](#)

L488: “paraemters” [Sentence moved and reformulated L463-464](#)

L556: “Beta” should be lower case [Corrected L505](#)

L587: switch order of “(123) Massey Sound, (129) Prince Gustaf Adolf Sea” to agree with the clockwise ordering used in this section [Corrected L368](#)

L607: “that ice stream locations were...” [Corrected L385](#)

L609: plural “different configurations”? [Corrected L387](#)

L614: “extends” instead of “extent” [Corrected L394](#)

Reviewer #2 comments

1. I strongly support the previous reviewer’s recommendation to separate these into two dedicated sections. As written, a combined section is difficult to navigate and makes it harder to place the current findings in the broader context of existing literature.

[This has been done, see response to previous comments above.](#)

2. I find it a bit surprising that a paper on “coupled atmosphere-ice sheet model sensitivity and calibration” (quote from the manuscript title) does not include a single figure showing the atmospheric circulation. This seems like a significant oversight. The growth trajectories of the Northern Hemisphere ice sheets affect both themselves and each other through feedbacks with the large-scale circulation --- for example, it is well documented that the Laurentide altered the large-scale circulation over the North Atlantic in several fundamental ways, which fed back onto the size and spatial footprint of the downstream Eurasian Ice Sheet. Modeling studies have shown that this influence is nonlinearly proportional to the size and physical extent of the Laurentide. Therefore, including at least one figure illustrating the large-scale atmospheric circulation (ideally in different seasons and under different ice-sheet geometries) seems essential for publication.

[Since the resolution of FAMOUS is so low, it is unsuitable for studying the effects of ice sheets on atmospheric circulation. The aim of the study is to produce more realistic simulations of the LGM and PGM ice sheets, taking into account feedbacks between the ice-sheets and climate during the different time periods, improving on previous studies that have prescribed the SMB forcing or used simple ice sheet models. Due to the low resolution model, this study focuses on the larger scale atmosphere-ice feedbacks such as ice-albedo and temperature-elevation and on tuning these model parameters to produce realistic ice sheets for both periods. For a more detailed investigation on how the ice sheets impacts smaller scale features, such as](#)

stationary waves, a higher resolution atmospheric model would be required as discussed in Section 4.4. However, we have taken this feedback into account and included some discussion of the feedbacks between the ice sheets and atmospheric circulation with the low model resolution as a caveat (Section 4.3). We have also added the requested figure (Figure 15).

3. Related to the comment above, the atmospheric circulation model that is used to generate the climate for the ice sheet component is both simplified and of a very low horizontal resolution. While the former is not necessarily a major problem in and of itself, the latter could be a more major issue for the realism of the atmospheric simulation. Lofverstrom and Liakka (2018) showed that the realism of an LGM simulations deteriorates substantially when the grid spacing becomes smaller than the deformation radius of baroclinic waves (essentially the radius of extratropical cyclones), which has profound implications for the growth of ice sheets; they showed that the breakdown happens around 3-degrees, which is quite a bit higher resolution than the 5x7.5-degree grid used here. In fact, they showed that a sufficiently low horizontal resolution actually creates a warm anomaly over the Laurentide Ice Sheet (similar to the results presented here), which they argued is due to a lack of convergence of the dynamics, poorly constrained model physics, and a high diffusion rate needed to ensure numerical stability. I think the authors ought to acknowledge this limitation and cite this paper.

As mentioned in the reply to the comment above, we have included a discussion on how the low resolution of our model is unsuitable for simulating small scale atmospheric effects. We have also expanded on the limitations of using a low resolution model and the effects it may have had on our results, citing this paper and others in section 4.4.

4. Lastly, this is a minor point, but the paper by Kageyama et al. (2021) should also be cited in the manuscript. It provides a comparison of all available PMIP3 and PMIP4 LGM simulations and is directly relevant to this work.

Added in L221