Blue color: Editor's/Reviewer comment
Black color: Authors response

=== Reviewer ===

The authors have put in a lot of effort to address the issues raised by myself and the other reviewer. In general, I think this has improved the manuscript, providing a better context for the modelling choices made by the authors in setting up their experiments, as well as for the conclusions they draw from their results.

The one point I think needs some more attention is the initialisation procedure. I don't entirely agree with the authors' rationale for not optimising their basal friction coefficients, and while I do not think this invalidates their experiments, I do think some more context is appropriate.

We thank Tijn Berends for reviewing the manuscript again and for appreciating our effort. We have taken into account his major concerns in the new manuscript.

Major points

In your rebuttal, and in the newly updated Discussion section of your manuscript, you describe some additional experiments where you included a "nudging" procedure, adapting the spatially variable basal friction over time to achieve a stable ice thickness close to the present-day observations. You state that you performed this nudging over a period of 30,000 years, but that the ice-sheet "is not yet in equilibrium" at the end of this period. You illustrate this with the new Fig. S9, which shows the change in total ice volume over time in a 30,000 continuation run, where you keep the friction field fixed in its nudged state, and the climate fixed to the present-day. You state that similar problems were reported by Seroussi et al. (in review) and Coulon et al. (2023).

I do not believe Fig. S9 shows what you think it shows. During the first ~2,000 years, your modelled ice volume changes by about 1 m.s.l.e. I doubt that this is simply a continuation of the trend at the end of your 30,000-year nudging phase. Instead, this initial sharp change, followed by the slow relaxation you see after ~2,000 years, is indicative of a "model shock". This is a common problem when using a nudging approach; during the nudging phase, it is almost unavoidable to put some (temporary) restrictions on the modelled ice geometry, to prevent (parts of) the ice sheet from collapsing before the friction is sufficiently nudged to keep them stable. Some modellers artificially reduce the rates of thickness change during this phase, others limit how far the thickness is allowed to deviate from observed, yet others simply do not allow any thickness change at the grounding line.

This is a tricky thing to get right, especially when you do not simultaneously nudge the sub-shelf melt rates (which you don't mention doing). While not knowing exactly what approach you chose, I suspect that the "jump" at the start of your continuation simulation is a result of problems from these implementations, rather than a fundamental problem with the nudging procedure (as there are several other models out there that have used it to achieve a much more stable ice sheet than what you have shown here).

Also, maybe a minor point, but I could not find any mention of such problems in the work by Coulon et al. (2023). Seroussi et al. (in review), as far as I'm aware, only mention these model drift problems for models that invert for velocities, rather than for geometry.

I think it's acceptable to show your results from the non-optimised simulations, but I don't think it's fair to dismiss the optimised simulations for the reasons you wrote. The argument that the nudging procedure (under a length list of assumptions) finds the present-day basal friction field, and that that might be different from the friction field during the Pliocene, is a better reason for not using it in this context. Also, I myself would have no problem with it if you simply stated that your optimisation approach is still a work in progress, and was not yet ready for application when you began your study.

Indeed, in this case we did not nudge the sub-shelf melt rates, which could explain the drift jump observed in our simulations. Also, we did not allow for any temporal restrictions which could help for stability if we seek for a stable PD. Though we now have a nudging scheme with satisfactory results (Juarez-Martinez et al., 2024), by the time of this study it was still work under progress, therefore it is not completely implemented in our model version. We will remove Figures S8 and S9 showing results for optimized friction coefficients, since results are similar to non-optimized fields, and they should be analyzed with caution. We maintain our argument that the present-day basal friction field might be different from the friction field during the Pliocene.

Regarding the work of Coulon et al., (2023), they optimize towards PD ice thickness and simulate a trend as observed in Figure 2, under PD forcing. They simulate WAIS collapse by Year 3000. However, in their simulations they first nudge melt rates and later apply PD oceanic conditions which explains such a drift (personal communication).

Minor points

Abstract and other places: I am unfamiliar with the notation you use (e.g. …a mean contribution of 2.7^{+0.1}_{−0.4} mSLE to 7.0^{+0.1}_{−0.1} mSLE…). Are these uncertainties of uncertainties?

This uncertainty range refers to a particular AOGCM for dynamical parameters. We removed the uncertainties in the abstract since it can lead to confusion.

L 125-126 At the risk of offending Lev, I believe that the previous phrasing of "terrain-following coordinates" is more informative and easier to understand than "sigma-coordinates" (as not all models use the letter sigma for the scaled vertical coordinate). Perhaps "…terrain-following coordinates (also known as "sigma coordinates" in some models, e.g. …)…"?

We included both names in the description.

L 141-142 "Here, cf is a dimensionless field representing the basal properties of the base, such as soft /hard beds (cf =0.1) or hard beds (cf =1.0)." This phrasing suggest that cf = cf(x,y), while in your rebuttal, you state the cf is "a unitless coefficient", which suggests that it has no spatial variability. Which one is it?

Indeed, cf is no field. We changed it to value.

L 244 “…grounded-ice differs by less than 2% from observations…” Do you mean the grounded ice area or volume?

We mean area. We changed it in the description.

References:

- Juarez-Martinez, A., Blasco, J., Robinson, A., Montoya, M., and Alvarez-Solas, J.: Antarctic sensitivity to oceanic melting parameterizations, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2023-2863, 2024.

=== Editor ===

Given the latest re-review by Tijn Berends and the current state of the revised thesis, to expedite completion, I'm requesting a few minor revisions, thereby avoiding a further external review stage. To make this work, please address Tijn's remaining concerns. Their main concern about the friction coefficients issue is I take mostly a communication issue of what your own tests imply. For even 100 kyr glacial cycle scales, I have strong concerns about PD optimization of friction coefficients as they generally do not account for changes in basal temperature over say a glacial cycle or within a surge/dormancy cycle of an ice stream.

Note however (contrary to Tijn's comment), "sigma coordinate" (if this is indeed what you are using) is a more precise (and standard, cf eg wikipedia..) term than terrain following coordinates (of which there are a number of different ones). So please retain "sigma coordinate" if accurate. If not accurate, do precisely specify the vertical coordinate.

We thank the Editor for considering minor revisions. We maintained the term sigma coordinates in the manuscript but added in parenthesis Tijn’s suggestion of terrain-following coordinates.

Also, expanding on Tijn's comment about uncertainty of uncertainties, your interquartile range expressions are comprehensible in the main text given that you parenthetical assign the different values to different models, but for those who first read the abstract, this will be incomprehensible.

Indeed, since it can be confusing we removed the uncertainties in the abstract.

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Concern 1:

The classical central limit theorem (CLT) requires independent identically distribution (IID) random variables. What are your IID random variables?

The application of descriptive statistics does not equate to the specification of a probability distribution. And if 30 simulations from a high dimensional non-linear modelling space were all that was needed to make confident inferences, robust inference of past and future earth system change would be a lot easier than it is.

It is fine to use descriptive statistics to summarize ensemble results. Do make explicitly clear that your "probabilities" are empirical probabilities (ie relative frequencies) based on your ensemble results. Cf even the fine wikipedia page on this if you are not clear on the distinction between probability and empirical probability.

And please clarify on first use whether A \pm B (when taken from cited literature) is nominally one or two sigma ranges.

We agree that the distribution of sea-level equivalent with respect to the dynamic parameters does not follow a gaussian distribution, so the CLT should not be taken as valid in our study. We will make clear that our probabilities are empirical probabilities.

The uncertainty ranges of the cited literature refers to 1 sigma range. We made this clear in the following submission.

Concern 2:
The interpolation of AOGCM fields to the interior sub-ice-shelf regions is another potentially major source of uncertainty in your experiments, at least for the marine sectors of the AIS. Given the potential for changes in submarine melt driven grounding line retreat to induce the tipping points you are considering along with the potentially large discrepancy between an extrapolated AOGCM field to the actual temperature distribution under an ice shelf, this warrants at least a bit more consider discussion than the "slightly different final states" claim which I suspect is invalid. It's an ongoing a challenge for all of us modelling ice sheets with significant ice shelves.

We agree that interpolation of AOGCM fields in the ice-shelf cavities is a big uncertainty which we may have underestimated in our study. We reformulated as follows the following paragraph:

“Note that the AOGCMs do not provide any oceanic information under Antarctic ice-shelf grid cells. Since that grid information is required to force our ice-sheet model, we interpolate to that grid point using the value of the nearest neighbor at the same depth. Of course, applying other interpolation schemes - and increasing the spatial resolution of the grid - will change the oceanic conditions and lead to different final states. The ideal outcome would be to include ocean models that resolve the ocean circulation below the shelves, which is not the case of the PlioMIP2 ensemble. Jourdain et al., (2020) propose an extrapolation protocol which is another possibility but it would add another source of uncertainty. We used the nearest neighbor interpolation scheme for simplicity but extrapolation of oceanic conditions inside ice-shelf cavities is an ongoing challenge within the scientific community.”

References:

- Jourdain, N. C., Asay-Davis, X., Hattermann, T., Straneo, F., Seroussi, H., Little, C. M., and Nowicki, S.: A protocol for calculating basal melt rates in the ISMIP6 Antarctic ice sheet projections, The Cryosphere, 14, 3111–3134, https://doi.org/10.5194/tc-14-3111-2020, 2020.