Review of Blasco et al.: "Antarctic Tipping points triggered by the mid-Pliocene warm climate"

By Tijn Berends

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.

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.

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?

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. …)…"?

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?

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