Review of revised manuscript by Mas e Braga et al. "Sensitivity of the Antarctic ice sheets to the peak warming of Marine Isotope Stage 11"

November 6, 2020

General comments:

This is the second review of Mas e Braga et al. following an initial round of reviews. I think the manuscript is much improved in comparison to the previous round of reviews and most of the points raised by the reviewers in the first review have been addressed. However, there still remain a few questions/suggestions for improvements listed below that I would like to see addressed before publication.

Scientific comments:

L79-82 In the first review I asked to motivate the choice of ensembles and parameter changes a bit better and the authors have added a paragraph that discusses this. However, I feel the argument that these parameters have not been addressed before a bit weak. Many other parameters (e.g. type of basal sliding law, different ocean parameterisation) could also have been chosen, so it would be good to state why you selected these particular parameters. Are they what you consider most important for sea-level rise or are they the most unconstrained? Please clarify this.

L181 Could you briefly say why you picked the EDC record for your ensemble here?

Sections 3.3. and 3.4 I think you could combine these two sections and simply state that they are not important for ice-sheet volume differences. Especially regarding section 3.3., volume differences are basically non-existent. At least I cannot see much difference at all in Fig. 4c. Similar arguments apply for Fig. 4e. Also, regarding the difference in floating ice volume at the beginning of the simulation, is that a result of forcing the same initial geometry with different sea-levels or why do they not start from the same value?

Caption of Fig. 8: I think that your statement: "Everywhere where Q_{bm} >SMB, ice shelves are thinning" is questionable. I think if you think about a Lagrangian framework this is correct, but if you think about it in an Eulerian framework (more common in ice-sheet modelling), you could have higher basal melt rates than SMB, but still have local thickening because thicker is is being advected from upstream. This is also true if you consider entire ice shelves. You can have a negative budget from SMB and basal melting, but still gain mass, because thicker ice is being advected from upstream. So I recommend deleting this statement.

Paragraph starting at L304: I think it is not surprising that ocean melting does not do as much to the EAIS as it does to the WAIS. WAIS is a marine ice sheet with large shelves providing a lot of buttressing, while the EAIS has only small ice shelves which provide less buttressing and is predominately not marine-based.

Paragraph starting L340: I think here it would be good to add a qualifier that these numbers are for your particular ocean melt parameterisation. If you use a different paramerisation (e.g. linear relation), these thresholds would most certainly change as well.

L349-354 I do not find your different resolution experiments particularly helpful or well thought-through. So you test, 20, 16, and 15 km. This seems like a random choice of model resolutions. Especially the step from 16 to 15 km, is really small. If you want to do this more rigorously, you would have to do a convergence study (see for example Cornford et al. 2016 or Schannwell et al. 2018). I am not suggesting that you should do this, but I think you are definitely overstating your results and should be more cautious with your conclusions. In your Fig S15c, it is hard to tell because it is rather small, but you definitely see differences in sea level, even from 16 to 15 km. So your results are certainly to a degree mesh resolution dependent. From my own experience, if you increase your resolution to 10 km you start resolving ice streams a lot better and you would probably see more differences. I think that your different mesh resolutions are not fine enough to make the claim that results are mesh resolution independent. Rather, from the evidence that I see the contrary is the case. If you cannot run higher resolved simulations with your model because of computational restrictions that is fine, but it should be clearly stated.

In you conclusions you state "...WAIS collapse was caused by the duration rather than the intensity of warming...", but in the discussion you say that both conditions have to be met and that even a shorter, but more intense ocean warming may also lead to WAIS collapse.

Figure comments:

The main point that needs improving are Figure sizes, Figs 3, 5, 7, 8 are plotting continent wide grounding lines. But each panel is so small, it is nearly impossible to see any differences. So please make each panel a lot bigger. If it helps, you could change to a 2x2 panel format. You can also use the full width of the page to make them bigger. For example, Figure sizes are much better in the supplement.

Figure 8:
I am sorry, but the hatching where basal melting is dominating SMB and vice versa is not visible. Even with a 300% zoom it is hard to see. You would probably have to have a zoom-in into the regions you talk about in the text in an additional Figure to see this.

Please also make Figure 10 a lot bigger. The insets are so small I had to use 300% zoom to see everything. No chance on the printout.

Technical corrections:

L98 "controlled by a temporally fixed"

L278 "CFEN equivalent run" maybe

L304 "close to grounding lines"

References

Cornford, S., Martin, D., Lee, V., Payne, A., and Ng, E. (2016). Adaptive mesh refinement versus subgrid friction interpolation in simulations of Antarctic ice dynamics. Annals of Glaciology, 57(73), 1-9. doi:10.1017/aog.2016.13

Schannwell, C., Cornford, S., Pollard, D., and Barrand, N. E.: Dynamic response of Antarctic Peninsula Ice Sheet to potential collapse of Larsen C and George VI ice shelves, The Cryosphere, 12, 2307–2326, https://doi.org/10.5194/tc-12-2307-2018, 2018.

Sincerely, Clemens Schannwell