Review for Shafer et al. : **Considering ocean conditions within ice-shelf cavities moderately reduces projected mass loss from the Antarctic Ice Sheet**

Submitted to *The Cryosphere*

*Reviewer: Clara Burgard*
*I do this review un-anonymously to clarify from which background and level of expertise some remarks might come from and because it makes the conversation during review more transparent on both sides.*

## Summary

The authors investigate the effect of resolving sub-shelf ocean cavities in an ocean model on the simulated basal melt and sea-level contribution of an ice-sheet model. To do so, they use the cavity-resolving Earth System Model E3SM and the land ice model MALI. They conduct experiments where they compare basal melt inferred from the thermal forcing computed by E3SM in the cavity and basal melt inferred from the thermal forcing extrapolated from E3SM ocean conditions in front of the cavity. They find that differences until 2100 are not very large in most of the regions, except around Totten ice shelf, Amery and Stancomb-Brunt. In the resulting sea-level contribution, most of the difference can be seen in the Totten and Ross regions. Beyond 2100, they expect discrepancies to increase, showing that it will be increasingly important to resolve the ocean circulation in the cavities for better sea-level rise projections.

The experimental design is very clear and the analysis is conducted very precisely without meandering in different directions. While they are a few limitations (no geometry feedback from MALI, constant SMB, …), the limitations are well and clearly discussed too.

The study is a timely contribution to the field as more and more discussions are taking place on the need to couple ice-sheet and ocean/climate models for better climate and ice-sheet projections. This study highlights that, on the short-term, not resolving cavities only introduces a negligible error, which might be reassuring news for modelling centers which are not working on opening the sub-shelf cavities. However, for projections on the long term, it seems that there is no way around it.

I really want to congratulate the authors on this manuscript! It was a very pleasant and smooth read. The majority of the questions arising in my head got answered in the following paragraphs and the figures are very clear. I only have a few small comments and therefore suggest minor revisions.

## GENERAL COMMENTS

### Choice of parameterization

The authors chose to use the quadratic parameterization from Jourdain et al. 2020 in their experiment. As the uncertainty around the choice of parameterization is very large (Burgard et al. 2022), I am wondering how much the conclusions would change with another parameterization. Is there another one built in MALI that could be used to redo the same experiments? Do the authors have an idea of the order of magnitude?
This point could be addressed in the discussion.

**SMB forcing**
I understand why the authors keep the SMB constant to focus on the ocean-ice interface. However, for the part about the sea-level contribution, I wonder if the SMB could play a role in changing the numbers from the conclusion. Couldn't it mitigate the inland response of ice sheet (Fig. 6)? Is there any way to have an estimate of the potential effect? I would be curious and it is the limitation of this study which bothers/puzzles me the most.

**Comparison to ISMIP results**
I am not completely convinced by section 4.1.1. The authors compare their results (fixed SMB, scenario SSP3-7.0) to results under changing SMB and higher emission scenario and argue that it is promising because they fall within the same range. I am not sure there is a clear added value in this comparison. I suggest the authors rethink about it.

**Abbreviations**
In general, I found that there were too many abbreviations. I know that it is a question of personal style but I suggest that the authors write out "thermal forcing", "Antarctic Ice Sheet", "Ross Ice Sheet", … all the time. It increases the reading comfort.

**DETAILED COMMENTS**

**L28:** I suggest adding "only" after melting.

**L29:** The transition between the two sentences reads weirdly. I suggest reformulating.

**L32:** I would suggest replacing "for CMIP6" by "Phase 6" as I am not sure that ISMIP is directly embedded in the CMIP framework. But maybe I am wrong. To be double-checked.

**L201:** Add an "s" to parameterization

**Table 1:** It is encouraging to see temperature corrections that remain below +/-1°C!

**L340-341:** Replace "greater" by "larger" twice

**Figure 5:** I know from experience that it is very challenging but is there any way to see more of the ice shelves and less of the grounded ice sheet? It is impossible to see real details on this figure. Also, when the grounded ice sheet is shown in black, it is difficult to distinguish from the dark red (like in Totten). Have the authors tried other colors? And, final remark, the values go beyond 5 m/yr as mentioned later in the text. Is there a reason to cut it off there? And if the authors want to keep it at 5 m/yr, I suggest making an "extended" colorbar (little arrow on the top).

**In general:** For future readers: Can the authors clarify somewhere if it is meters of ice per year or meters of water equivalent per year?

**Figure 6:** I find it interesting that the effect on the grounded ice is so limited. Do the authors have an explanation for that?

**L446:** The correction approach is interesting and it is a little disappointing that it only appears in the discussion. It could have been great to estimate this update in the beginning and do the experiments with this \gamma.

**Figure 9k:** I suggest annotating where East and West is. I always get confused although I am an expert in the field.

**L521:** I suggest adding “for most ice shelves” after “short” because the sentence before talks about long flushing timescales and it is therefore confusing.

**L526:** I suggest starting a new paragraph here. Then there is one paragraph about overestimation and one about underestimation.