Review for Menthon et al. : **Comparison of calibration methods of a PICO basal ice shelf melt module implemented in the GRISLI v2.0 ice sheet model**

Submitted to *Geoscientific Model Development*

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

Summary

Ocean-induced melt of Antarctic ice shelves and its representation in ice-sheet models is one of the main sources of uncertainty for ice-sheet projections. The parameterisations used to bridge the gap between ocean properties and the basal melt remain uncertain. The authors revisit this uncertainty and explore a new calibration approach to reduce it. To do so, they implement the PICO ice-shelf basal melt parameterisation into the ice-sheet model GRISLI and test different novel calibration approaches, resulting in a more robust parameter choice. They explore a variety of conditions that affect the calibration such as the metric, the resolution, the geographical specificities, and the forcing conditions.

The study is a timely contribution because the parameterisation of the melt at the base of Antarctic ice shelves remains one of the largest sources of uncertainty in projections of the future evolution of the Antarctic ice sheet and its contribution to sea-level rise. The approach explored by the authors is a useful addition to the community as it provides a set of parameters that seem to be more robust across conditions compared to previous sets. It could also be explored for other parameterisations in the future.

The manuscript is very clear and thorough. This thorough description is very well suited to inspire other researchers to explore the presented methods on other parameterisations and/or other ice-sheet models. Overall, I have a few larger comments as some aspects of the study appeared unclear to me and other more minor comments. I therefore suggest minor revisions.

GENERAL COMMENTS

I want to start by thanking the authors for making this manuscript very pleasant to as the manuscript and figures are very well done and clear.

#1 The results of this study are very encouraging. However, there is a strong limitation, which is not discussed much. In Figure 3, it becomes clear that using bins improves the melt rates for the "middle" bins but, in all cases, the spread between members remains high for the low and high-end bins. In particular, for the lowest melt, an anomaly of 0.5 m per yr could be quite high compared to the actual melt. For the highest melt, the order of magnitude is maybe lower compared to the actual melt. However, it is important to check this further because the points with the highest melt are also the points that influence the future of the ice sheet the most. Would it be possible to complete this by a metric that looks at the percentage formed by this anomaly compared to the melt value? Just to have a better idea of what this means exactly.

#2 I am not completely convinced by section 4.2. I do not understand how the authors can calibrate on different input temperatures (e.g. 1K warmer) but same target melt and make conclusions from this. If the parameters do not change much, does that not mean that they are not sensitive enough to temperature changes? Ideally, you would expect higher melt for higher temperatures if you use the same set of parameters, no? This makes me unsure about the whole approach. Does that mean that the parameterisation cannot react to changes in forcing and that the parameters are too strongly set? This would not be useful for projections and is what could be interpreted from the low sea-level rise contribution in Fig. 7c. To reassure the reader, I recommend that the authors clarify the implications of this result, discuss them more in detail or reformulate to avoid misunderstanding.

DETAILED CONTENT COMMENTS

L3 : It is not only about limited computational resources but also about a lack of observational data. I suggest completing "limited observational data and computational resources".

L31: Not sure if the formulations only differ in the complexity of the "melt physics" themselves. In most cases, assumptions differ on the simplification of the ocean circulation in the cavity. Can the authors reformulate to clarify?

L39-40: This is great! Calibrations that do not need regional corrections are what are currently needed!

L104-108: This paragraph is unclear in regard to the authors' own contribution vs what has been done in PICO-PISM before. I suggest reformulating it to clarify.

L114: It is unclear where the value for the combined factor comes from. As far as I know K_T is a parameter that needs calibration. So how has it been calibrated in this case? I suggest the authors add the source or briefly explain the calibration.

L122-124: It remains unclear to me why the authors use the coupled GRISLI-PICO setup if the geometry is kept constant anyway. Could the authors clarify why they do not use a standalone version of PICO then?

Table 1: Very useful table!

Table 1: Maybe I did not think this completely through but aren't ADA and ADA of bins the same metric. Taking the mean of a mean should, I think, result in the same as taking the mean of the whole ensemble directly. This would also explain why the results are so similar between the two. Can the authors comment on that?

L218-219: I am not 100% sure that a narrow set of parameters is a guarantee for "better". I agree it is useful for modellers but I would be careful with such kind of statements. If a large range of parameters are possible, this could also be linked to the formulation of the

parameterisation. Still, in L294-296, the authors explain the advantages of having a narrow set of parameters. Maybe this could already be mentioned here?

L241: It is unclear to me what is the difference between the distribution curve and the magnitude of the spatial patterns. Is one the shape of the curve and the other the actual number?

Figure 8: Just a remark: The 2D results are interesting and give the feeling that C does not really play a role in the calibration. Out of curiosity, have the authors thought about what this could mean?

L330: Agreed that Joughin et al. (2021) is a good study to refer to here but it should not be forgotten that enough other studies (e.g. Reese et al. 2018) show that localised melt has a strong effect on buttressing. I suggest that the authors reformulate a little more carefully.

L350: This is not completely true. The quadratic term of the quadratic parameterisation is there to mimic the effect of the overturning circulation in a very simple way.

L360-362: Yes, it can be clearly seen in the sensitivities to warmer forcing in Burgard et al. (2023) and Lambert and Burgard (in press) that the quadratic parameterisation is an outlier towards high melt sensitivity. However, as we do not know what is the "right" sensitivity, this is not enough to say that one is better than another.

L363-366: This is not very clear. I suggest that the authors reformulate to clarify.

DETAILED WRITING COMMENTS

L20: Replace "warmth" by "heat"

L22: "on the other hand" does not really work in this sentence. I suggest leaving it out.

L34: Leave out "However", it is confusing.

L41-49: This could be shortened.

L54: Replace "are dependent" by "depend"

L79 and 84: For the results, I suggest to stay consistent with present instead of past tense: "made" => "make", "combined" => "combine"

L83: Correct the citation format (\citet{} instead of \citep{})

L98: Rephrase to "refreezing in some areas"

L142: Replace "but also" by "and"

L148: To improve reproducibility, I suggest that the authors add the information if the results are in m ice per year or in m w.e. per year.

L158: I suggest that the authors reformulate, the formulation is very unclear.

Figure 1 caption, last sentence: Replace "is" by "are"

L190: Can the authors clarify if they are writing about the sensitivities of PICO and QuadNL to ocean warming or to something else?

L209 and later: "side-by-side" sounds awkward. I suggest reformulating, maybe with something like "close" or "similar".

L220: Replace "explain" by "explained"

L224-225: I suggest reformulating as this is not a complete sentence.

L232: "led" => "lead"

L235: remove "is"

L236: missing "methods" in front of "without"

L269: "Elemer" => "Elmer"

Figure 6 and later: Would it be possible to replace CalibXX by an indication of the metric it was calibrated with? That would clarify the legend.

L284: I suggest reformulating "we further discuss the ISMIP 2300 …"

L359: "rates" => "rate"

L401-402: This is not a complete sentence.

Supplementary material: There is A LOT of material and the captions are sometimes very short. I wonder if it would be possible to reduce the amount of figures or add one sentence explaining the core of the figure or set of figures when appropriate?

References

- Reese et al. 2018 : https://doi.org/10.1038/s41558-017-0020-x
- Burgard et al. 2023: https://doi.org/10.1029/2023MS003829
- Lambert and Burgard, in press: https://doi.org/10.5194/egusphere-2024-2358