

Second round of 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

We, the authors of the paper, thank the two reviewers for their time and comments to finalize this paper. We respond to each comment in blue in the text below.

From reviewer Clara Burgard:

I thank the authors for answering my questions and taking my comments into account. The manuscript has improved since the last version. The unclear points have been clarified and the restructuring is appropriate. I have a few minor comments left but, all in all, I think the manuscript is ready for publication.

L35-36: This sentence is redundant. I would suggest something along the lines of :”The parameterisations rely on the definition of their parameters.” But even this sounds awkward. Maybe leave it out altogether?

Indeed, we removed it as suggested.

L106-108: This is not a sentence. If a sentence starts with “whereas”, the second part of the sentence should be in contradiction with the first part. I don’t think it works they way the authors use it here. But this can be left to the proofreading maybe?

We suggest changing it to the following:

“In contrast, in PISM-PICO Reese et al. (2018a) did not include the grounding line of ice rises ...”

L167-168: This is not a complete sentence either. Or it is formulated in a very convoluted way that got me lost.

We agree and rephrased the sentence as: Since no squaring is used in the error computation of the MAE, the MAE is less sensitive to outliers than the RMSE.

L171: The word “adding” could be removed here.

Done as suggested.

L242: “analysis” => “analyse”

Done as suggested.

Figure 3 caption: “bins values” seems imprecise. Can the authors reformulate in a clearer way?

We rephrased as follow: “Anomaly of the values of the bins for the five best members...”

L253-260: Using past tense here for the verb “leading” in several instances makes the reading a little difficult.

We changed the tense to present and the verbs:

We see that four methods (ADA, ADA of bins, 2D RMSE, and 2D MAE) **results in a** spatial distribution with little contrast between higher and lower values, they do not

even have values more negative than -1 m.yr^{-1} in blue (Figure \ref{fig_MAPS-global-R16} panels (a) to (c)). It could be because this selection **gives** low γ_{T}^* values, $0.1 \times 10^{-5} \text{ m.s}^{-1}$ and $0.25 \times 10^{-5} \text{ m.s}^{-1}$. Whereas, the best single member following the RMSE of bins or the MAE of bins have a lot more contrast (Figure \ref{fig_MAPS-global-R16} panels (d) and (e)), which corresponds better to what is seen in the observations (Figure \ref{fig_MAPS-global-R16} panel (f)). These two methods **results in** higher γ_{T}^* values: $1.5 \times 10^{-5} \text{ m.s}^{-1}$ and $2.0 \times 10^{-5} \text{ m.s}^{-1}$.

L335-348: The answer to the question of the title is hidden in a large text about observational differences. I would suggest either merging this section with another one or finishing it with the main conclusion of the paragraph (what is now at L340-344). At the moment, it reads mainly like a description of the differences between the two observational datasets.

We added the following sentence to conclude clearly the paragraph:

“Hence, despite the important spatial differences, we conclude that the calibration methods are not significantly sensitive to the choice of the target dataset as they both give a similar selection of best ensemble members for each method.”

L388-390: There is a mix-up of sentences here.

Indeed, thank you. We corrected and two mix-up sentences are the following sentence:

“Prioritizing values over spatial correspondence within an ice shelf is in agreement with \cite{Joughin2021-sa} who argue that the ocean-induced melt volume, regardless of the spatial distribution, directly paces the ice loss.”

L448: The melt sensitivities to warming were explored in Burgard et al. 2023 and not in Burgard et al. 2022.

Done as suggested.

L459: I suggest adding “simulations of” before “future dynamics of the ice sheet”

Done as suggested.

L488-490: This sentence is tedious to read. Can the authors reformulate?

We reformulated as follows:

Reese et al. 2023 calibrated PICO to a sensitivity to temperature, but it required the use of temperature corrections. We suggest that using the MAE of bins calibration method could enable calibrating to sensitivity without additional temperature corrections.

References

- Burgard et al. 2023: <https://doi.org/10.1029/2023MS003829>

From reviewer Xylar Asay-Davis:

The authors have done an excellent job of addressing my recommendations. The manuscript looks to be in good shape to publish!

I have one small change that I would recommend but I am fine if the authors don't choose to include it.

l. 150 of the "diff" document: I would change "30-years relaxation" to "30-year relaxation". I had asked for an explanation of that relaxation and the authors kindly provided it in the response to my review but no further explanation is provided in the text, and I think some of the clarification I was given would also be of interest to readers.

Done as suggested. The text reads now as follows:

“The geometry of the ice sheet and the ice shelves is kept fixed to remove the influence of ice shelves geometry changes on the computed basal melt rate. The fixed geometry corresponds to Bedmap2 \citep{Fretwell2013-tc} with a 30-year relaxation with GRISLI. This relaxation is needed because with the basal drag coefficient inversion methodology used for ISMIP6, we compute the ice sheet internal thermal equilibrium with a long (60 kyr) experiment with fixed observed geometry. Thus, to avoid any artificial drift when releasing this constraint we run a 30 years relaxation experiment with the same boundary conditions as for the control experiment from ISMIP6 \citep{Seroussi2024-ef}.”