

Response to referee #1

I would like to thank the authors for the extensive changes that they made, which considerably strengthen their manuscript. This makes for a very nice and interesting read and introduces a very useful framework for current regional-scale glacier modeling. Upon reading this updated version I still had a few comments which would need to be addressed before acceptance of this manuscript.

Thank you for your second review on our manuscript. We agree with your comments and implemented the suggestions in the revised manuscript.

Missing references: In general, while the new version of the discussion reads very nicely, it is somewhat very limited when it comes to linking this study to the current literature which does cover some of the aspects touched upon here.

We agree that links to related work were missing. We added references to the recently published work by Frank et al. as an alternative approach for ice thickness inversion. We also added references to Schmitt et al., which presents alternative approaches for both thickness inversion and data assimilation. In addition, we now refer to other velocity products that could be used as alternatives to the Millan velocity dataset.

Frank, T., van Pelt, W. J. J., Rounce, D. R., Juvet, G., and Hock, R.: Global glacier-free topography reveals a large potential for future lakes in presently ice-covered terrain, *Nature Communications*, 17, 3985, <https://doi.org/10.1038/s41467-026-72548-9>, 2026

Schmitt, P., Maussion, F., Goldberg, D. N., and Gregor, P.: The Open Global Glacier Data Assimilation Framework (AGILE) v0.1, *EGU-sphere*, pp. 1–35, <https://doi.org/10.5194/egusphere-2025-3401>, 2025.

Missing discussion on snowlines: While I liked the answer that you provided in your detailed response to my comment on the EoS SLAs used for validation and the origin of the potential mismatch, I was disappointed to not see more details about this in the updated discussion.

Thank you for pointing out that the section was missing a discussion of the offset between EoS SLA and ELA. We have incorporated the relevant parts of the previous response into the Discussion section.

Prior distribution of model parameters: the numbers reported in the response to reviewers differ from those now written in the main text. Also, it's worth noting what kind of distribution is used – gaussian? Uniform?

We double-checked the response to the reviewers, the manuscript, and the actual code and found no inconsistencies. The confusion may have arisen because the manuscript reported the mean and standard deviation derived from the GLAMOS dataset, whereas our response referred to the prior distribution used for the ensemble, for which the standard deviation was set to three times the value obtained from the GLAMOS analysis. We revised the manuscript so that the reported values now directly correspond to the actual prior distributions used in the experiments. We also clarified that the priors follow Gaussian distributions. Thank you for pointing out this potentially confusing formulation.

L108-109: no need to specify 'we added a suppl. Figure...', just refer directly to the figure

Done

L205-213: for both Plaine Morte & Giétro it is apparent that there are some strange artefacts occurring in the modeled ice thickness and SMB fields, maybe caused by the velocity? These patterns look quite concerning and it would be useful to know from this study 1/ where these come from and 2/ whether similar artefacts are visible on other glaciers or if these are extreme cases.

We agree that the wave patterns visible for Plaine Morte and Glacier du Giétro are model artefacts and do not represent realistic glacier geometries. In discussion with the IGM development team, we concluded that the inversion scheme in version 3.0.0 is not fully stable. A plausible explanation is that the simultaneous training of the ice-flow emulator together with geometry updates leads to unstable optimization, since the ice-flow model is not trained on evolving bedrock topography. In most cases, the regularization terms are sufficient to ensure realistic bedrock geometries.

We share the supposition that these wave-like patterns may be triggered by artefacts in the observed surface velocity fields, particularly the small patches of unrealistically high velocities. However, similar artefacts in the velocity observations are also present for other glaciers, but in other cases they do not lead to visible wave patterns after inversion. We verified that such pronounced wave artefacts in the final thickness maps are only visible for these two glaciers.

We added a short discussion on these artefacts in the manuscript.

Response to referee #2

I thank the authors for carefully addressing my comments. I only have a few remaining minor technical/editorial suggestions:

Thank you for your second review and for identifying the remaining inconsistencies. We really appreciate your careful reading.

- L6: "timer"

Done

- L94: I'm not 100% sure as I'm not a mathematician but when I see bilinear I think of a function in two variables (e.g. like in bilinear interpolation) rather than a two-segment piecewise-linear function

Thank you for pointing this out. We agree that a two-segment piecewise-linear function is a better description of the SMB model.

- L95: "the a"

Done

- L98: Fig. S5 is probably wrong here. S9?

Done

- L105: double check the period, in the response it's mentioned "between 15 August and 30 September"

The actual period is 15 July to 30 September. We apologize for the mistake in the response.

- Fig 2: regarding my comment "- I would force integer tick labels for panel (a)." sorry for not making it clearer: I meant the colorbar should have integer tick labels since it shows the number of glaciers. Seems fine in the new version anyway.

Thank you for clarifying your previous comment. We agree that integer ticks for colorbar is more appropriate.

- L132: the r value (0.74) matches the one from Fig. 3 (a) but the MAE values are different (97 m vs ~106)

Corrected