Comments on the revised manuscript
egusphere-2024-1525 entitled
New glacier thickness and bed maps for Svalbard

presented on 03.09.2024
by

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The authors present a revised manuscript addressing the comments from the initial review round. Although I appreciate the authors effort to answer the major comments, I regret to say that some of their answers to several of my major concerns are not convincing - at least to me. I still remain very positive about this manuscript and therefore recommend that the editor should continue to considered it for publication in *The Cryosphere* after my concerns have been alleviated.

MAJOR COMMENTS

METHODOLOGICAL MIX

Thank you for including the comparison between PISM and IGM including RMSE values and thickness maps in the rebuttal (not in the manuscript). You argue that PISM performs better for regional application. The fact why you prefer IGM over PISM for small-scale glaciers remains vague (higher-order) and not convincing to me. I still do not see the ultimate argument to included IGM - certainly after you invoke the limitations of the current fast developments. I still wonder why you are not more consistent and apply PISM all over the domain (with prescribed perfect-plasticity values for surging glaciers). In this way, you would get a more coherent map product and a simpler method (also in terms of calibration).

PROGRESS

In response to this comment, you raise the argument that your approach *'can be used as a numerically stable spin up state for prognostic modeling'*. I agree that this initialisation is big asset. Yet the built-in IGM inversion for thickness and ice-flow parameters claims the same property. Please discuss. Do not misunderstand me, I can accept this argument as a clear benefit. Yet, I imagine myself to start modelling on Svalbard with your thickness map. I would then need to use IGM and PISM to do so consistently. It might be me, but it seems impractical to do so with two models that need to communicate/interact. Again, the solution is to reduce the method mix to PISM-only (with perfect plasticity).

PERFECT PLASTICITY

Your answers to my minor comments on L91 and L107-108 are not yet satisfying. You agreed that temporal consistency motivated your choice - specifically with regard to the 2010 DEM and 2017-2018 surface velocity observations. Following this logic, you should rather use the Copernicus DEM. Moreover, the perfect plasticity does not require surface velocities. So your argument should invoke the glacier outlines and the DEM. This also corroborates your argument on limiting the surge-type identification to the velocity fields from 2017-18. As I indicated before, the analysis of Koch et al. (2023) shows an extended time coverage for surge-type glacier identification. Finally, you did not comment on the applicability of the perfect plasticity approach for surge-type glaciers. Please add.

MINOR COMMENTS

L126 *'This comment was undressed (neither in the rebuttal nor in the revised manuscript).' From my understanding, the term yield stress relates to when deformation becomes possible whereas the term sliding law normally relates basal velocities to general basal stress conditions. Please check this terminology and be specific about any assumptions.*

L270 *'Follow-up on your answer to:' For the land- and marine terminating glaciers, you report mean thickness values of 42 m and 162 m, respectively. How can you reconcile this value with the archipelago-wide average of 205 m. I certainly miss something here ;o)*

Obviously, I missed the difference between median and mean values. This metric-mix makes it very hard to compare. Solutions would be to either use both metrics for each time you specifically provide averaging measures or only give either of the two metrics consistently throughout the manuscript. Personally, I prefer the former. Your decision.