Summary and comments on the manuscript
egusphere-2024-1525 entitled

A new glacier thickness and bed map for Svalbard

presented on 03.06.2024
by

Ward Van Pelt and Thomas Frank

SUMMARY

The title directly sets the stage for the overarching objective, i.e., to forward a new map of glacier ice thickness for Svalbard. For this purpose, the authors employ a state-of-the-art method that builds on surface observations of geometry and velocity as well as model estimates of surface mass balance. Calibration target is the abundant record of thickness measurements. For the actual mapping, the authors distinguish three glacier types using specific treatment for active surges. Apart from the actual thickness distribution, the authors ultimately report a total ice volume of $6\,800 \pm 238$ km^3, which is within the range of previous estimates.

Admittedly I am very excited about this new mapping effort on Svalbard. The reason is that all previous attempts have their specific weaknesses. The manuscript is well written and strikes with clearness and high-quality illustrations. Altogether it is very easy to follow. When I read the manuscript, one question got stuck in my head. Should this new map replace previous efforts (new standard) and if yes, what are the key arguments for the quality increase. In my view, the manuscript fails to explain this. Apart from that, I see some methodological aspects requiring further justification or adaptation (e.g., glacier classes, calibration, multi-model approach, uncertainty). Overall, I remain very positive about this manuscript and I recommend that the editor should continue to considered it for publication in *The Cryosphere* after my main concerns below have been alleviated.

MAJOR COMMENTS

GLACIER CATEGORIES

The criteria for defining several glacier classes are well presented. You distinguish in terms of area (threshold 100km2), termination type (land/marine) as well as observed surge activity in 2017/18. Yet later in the manuscript (L116-117), you diffuse these categories again by joining well-connected ice geometries and with it combining different classes. So a land terminating glacier smaller than 100 km2 that is connected to a larger ice-body will be treated differently than its stand-alone homologue. The same is true for surge-type glaciers. All surge-type glaciers are embedded in larger icefields (Fig.1c). Does actually any glacier remain in class 3? If yes and they fall into an icefield, what is done at the its internal boundary? In summary, the classification appears somewhat confusing to me. I am sure you find a more consistent strategy.

METHODOLOGICAL MIX
I understand that you sell the glacier classification, and with it the specific methods per class, as a strength of your approach. The Parallel Ice Sheet Model (PISM) is used for the larger glacier compounds (class 2), the Instructed Glacier Model (IGM) for the smaller land-terminating glaciers (class 1) whereas surge type glaciers (class 3) are treated with the perfect plasticity approach (requiring minimum input). While it is clear that surge type glaciers need to be treated differently, I do not get my head around it, why two approaches are necessary for the other two classes (1 and 2). You argue that IGM comprises higher-order dynamics necessary for the smaller glaciers. Yet, higher-order dynamics would also be preferential for the larger ice bodies. Some of these are marine terminating and show significant flow speeds near the ice front. I do not see the advantage of using two models especially as the glacier class definition is a bit diffuse (see above). Last but not least, you need to say something about the IGM capabilities of inferring ice thickness itself without integration into a transient assimilation. Please add this to the discussion.

PROGRESS & DISCUSSION
As much as I like it that a new thickness map of the Svalbard ice cover is presented, I wonder about the improvements with respect to existing products. You can either show that the quality of the input data is higher or that your method is more sound. Alternatively, you simply show some performance measures by comparing the different thickness products and leave the decision to the reader. I am not sure what is the best way forward in your case. The most practical is to extend your discussion by additional analysis. I suggest that you simply add previous results to some existing figures (Figs. 5, 6 and 7). This extension will help readers to better assess your new thickness product. In case you find further arguments

to promote your map product, please stress this prominently throughout the text and certainly in the abstract. One last thought that got stuck in my head: the two previous estimates (Fürst & Millan) seem to differ in volume and spatial distribution. Your map reproduces the volume of one approach and the pattern of the other. Why is that? Could this give an argument?

INPUT DATA
I would appreciate a little regional overview of all input data in Sect. 2. Many of these data sets have global coverage and it is difficult to assess their quality on Svalbard. You should comment on that briefly. Admittedly, you have Fig. 1 but nothing is said about uncertainties/quality. Possibly some of them are better suited than what was used for previous Svalbard maps.

CALIBRATION If I understand your manuscript correctly, you calibrate several parameters (spatially uniform) for both the IGM and the PISM inversion. IGM and PISM only differ in the a-priori choice of the rate factor and the sliding coefficient as well as in the SMB correction strategy. Actual calibration parameters are β, Θ, τ and α_{min}. These mostly relate to the initial guess for ice thickness and the iterative inversion procedure. I cannot understand why you would need different initial guesses for these two models with $\tau_{PISM} = 0.52kPa$ vs. $\tau_{IGM} = 100kPa$ and $\alpha_{PISM} = 0.014$ vs. $\alpha_{IGM} = 0.04$. Isn't the perfect plasticity approach only calibrated once? Second, I am puzzled why the iterative inversion method requires so different values: $\beta_{PISM} = 0.25$ vs. $\beta_{IGM} = 1$ and $\Theta_{PISM} = 0.4$ vs. $\Theta_{IGM} = 0.15$ (also no friction update in IGM). Can you please explain? These calibration differences cast doubts on keeping a consistent map product while applying two glacier-system models.

MINOR COMMENTS

L91 Please clarify why you chose the perfect plasticity approach for surge-type glaciers. In my view, your main reason is the temporal consistency of the input data. The method by itself is not more adequate for such glaciers. I am not sure if all readers immediately get this.

L107-108 It is not very clear why you limit your surge-type class to the years 2017-2018. Koch et al. (2023) present surging glaciers for a longer time period. Why did you refine your selection? Your DEM dates back to 2010. So many other surges are imprinted. Do you see a problem from that even using the perfect plasticity.

L126 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.

L136 In the in-line equation here, you relate two stresses to each other, namely τ_d and τ_c. Velocity is the scaling factor. In this definition, τ_d and τ_c cannot both be stresses. I would consider one of them a basal friction coefficient. I therefore would not use the symbol τ.

L166 Here, you present the first calibrated parameters. Θ represents the strength of the surface update. I am very surprised by its magnitude of 40%. This means that if you have to adjust the thickness at one location, 40% of this change will be imprinted in the surface. This is a lot. I wonder how much your modelled surface then deviates from the observed one after convergence. Please clarify.

L205 & L226 I am happy to see such small threshold values here. Please give them in degrees.

L219 time periods --> time stamps

L254-256 Your uncertainty estimate of the mean thickness of 205 m is $\pm$7 m . As you say, this is about $\pm$3.5%. Measurement errors of thickness observations typically exceed 10-20% and therefore strongly challenge your estimate. These measurement errors are not considered in your uncertainty analysis. Moreover, I do not get my head around your argument (L255) that the standard deviation of modelled vs. observed thickness values must be divided again by the number of glaciers with observations (i.e., $\sqrt{169}$) Please explain better or remove this division.

L270 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)

FIGURES

Fig. 1 I like this figure very much. In panel (c) you indicate the locations of all surge-type glaciers. They all belong to larger ice-fields. So are they now modelled with PISM or the perfect plasticity approach. In the latter case, how do you ensure that you do not get internal fringe lines in the thickness field.

Fig. 2 Could you add the reference run with $M_{corr} = 0.4$ m w.e. yr^{-1}

and $\Theta = 0.4$.

Fig. 4 In panel (a) and (b), the ice thickness decreases towards the calving fronts of marine-terminating glaciers. Why is that? On Hansbreen, you could directly compare to a dense survey grid included in GlaThiDa.

Fig. 4 & 5 Please extend these figures by values from the existing thickness maps. It might also help in evaluating & promoting your results.

Fig. 7 How does this distribution look like for Millan? It would be good to add for reference. It might also help in evaluating & promoting your results.

Fig. 8 Please use the updated thickness map (v1.1) from Fürst et al. (2018) at data.npolar.no.

TABLES

Table 1 Please indicate the version of GlaThiDa.

Table 2 Please use the numbering you introduced for the glacier classes (1-3).

CODE AVAILABILITY

Is the inversion method available via an open repository?