Reconciling ice dynamics and bed topography with a versatile and fast ice thickness inversion - response to reviewer 1 II

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1 Response to reviewer 1

1.1 Summary

This paper presents a new method for inverting for ice thickness and, optionally, basal friction at a glacier by iteratively minimising misfit between observed and modelled dh/dt through adjusting an initial guess at the bed (to which the method is insensitive), and by adopting the same approach with observed and modelled surface velocity for the basal friction field. The authors test their method on a synthetic ice cap in order to quantify its performance and sensitivity, and then apply it to Kronebreen, a tidewater glacier in Svalbard, finding that the method generally reproduces the bed well, and is fairly robust when subjected to a wide range of model parameters and input data perturbations. They also show that their method allows the creation of a self-consistent model glacier that can then be used in prognostic simulations without any relaxation. This is a revised version of the paper, which I also reviewed in its original state. I congratulate the authors for their thorough and considered response to both my review and that of the other reviewer, and feel that this revision adequately responds to all the concerns I raised in my original review. Beyond picking up a few typos, I have no further points to raise at this review stage and recommend that the paper should be accepted for publication. Well done!

We would like to thank the reviewer for taking the time to review our manuscript again and for his positive comments. All the typos are fixed!

Reconciling ice dynamics and bed topography with a versatile and fast ice thickness inversion - response to reviewer 2 II

Thomas Frank, Ward van Pelt, Jack Kohler

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1 Response to reviewer 2

1.1 General

First of all, I want to truly thank the authors for their in-depth replies to the review comments. They took quite some effort by attempting to directly assimilate thickness measurements, by suggesting other stopping criteria, by extending the sensitivity analysis and by reorganising the manuscript structure. I especially appreciate the last two aspects. In summary, I am very positive about this revised manuscript. Despite my wish for a 'standard setup' that could serve for comparison, I only remain with some comments that can be picked up in the discussion section. Minor revisions seem at order. I therefore recommend that the editor should continue to considered this manuscript for publication in The Cryosphere.

We are happy to see that the reviewer agrees with our first revisions and would like to thank him/her for again taking the time to provide further comments which we reply to below.

1.2 Major comments

Comparison
Following the editors suggestion, I also want to urge you again to introduce a 'standard setup' from ITMIX which will facilitate comparability. It can also help in the cal/val strategies. I appreciate the new chapter on comparing your result to other studies on Kronebreen. Yet you only show a single quantity for comparison. Another idea could be that, instead of presenting this table, you could insert a figure showing observed vs. inferred/modelled thickness values for all approaches (scatter plot similar to Figs. 3f, 7c). In each figure panel, you can add one or several misfit quantities such as the mean absolute bed misfit.

As mentioned in the previous response letter, we were somewhat sceptical to the idea of adding ITMIX glaciers due to the danger of inflating the length of manuscript, as well as questions on the suitability of the available test glaciers to be run within the framework described in the manuscript. As rightfully pointed out by the editor, ITMIX indeed does not only comprise valley glaciers; there is also Austfonna as the only ice cap with available dh/dt data. However, there is a strong inconsistency between the provided surface mass balance and dh/dt for Austfonna, resulting in a negative apparent mass balance gradient which is unphysical and would result in a modelled zero ice thickness for almost the entire ice cap (as confirmed by experiments we conducted). Indeed, the only inversion method in

ITMIX 1 that requires dh/dt as input and submitted results for Austfonna (Fuerst) set dh/dt to zero since uphill ice flow would have occurred otherwise (Farinotti et al. (2017), supplement). That being said, following the editor´s and reviewer´s strong recommendation of including ITMIX glaciers, we tested the three synthetic ITMIX glaciers and found that they can be reasonably modelled with our framework. In the interest of brevity of the manuscript, we have therefore included a concise section in the results chapter where we briefly describe the inversion setup and the results (including a table), and we inserted references to that section at different places in the manuscript. A figure for each test glacier was also added to the appendix. We are happy to report that we find excellent agreement with the provided thickness 'observations', constantly ranking our results among the top two of all solutions submitted to ITMIX 1.

We also follow the reviewer and added a scatter plot analogous to Fig. 3f for all approaches. To facilitate comparison, we plotted our results in that same figure. To avoid repetition, we instead removed panel c) from Fig. 7.

Sensitivity analysis

The extended analysis on the sensitivity of the approach to input uncertainties is really valuable. In particular, I like the concise presentation in Table 2. I fully agree with your assessment that this table can serve to infer overall uncertainties. Yet I want to raise that any bias in the velocity or mass-balance input transmits directly into the volume estimate. An overestimation of the mass balance by 75% results in an ice volume overestimated by 34% (or 12%). A similar underestimation translates into 68% (or 24%) smaller ice volumes. This brings me back to the assimilation of thickness measurements. These can help you to drag the results towards the right magnitude in thickness values. I see no other reason how your approach can accommodate/compensate such biases. I know that you tried to use these observations without much success. So I think you need to, at least, state that your approach transmits biases into the final thickness results. No worries, all approaches do this ... you can moderate your statement by saying that this transmission only applies if no reference observations on ice thickness are available.

As the reviewer states, Tab. 2 demonstrates how biases in input data result in biased ice volume results. But indeed, the reviewer has a good point in that we haven´t discussed yet that thickness observations could help to remove such biases. In the revised manuscript, this is made more clear.

Parameter equivocality

I appreciate that you added a paragraph on the ambiguities when simultaneously inferring basal topography and friction. You are very optimistic in your assessment. This is substantiated by your good results on the ice cap. Indeed impressive. Anyway, I think that you should mention the viscosity parameter, which is prescribed and also determines the modelled velocity values. This viscosity adds another layer of ambiguity. Please add in the discussion.

The term equivocality is not familiar to us in this context; we assume the reviewer refers to equifinality.

The discussion on unique solutions is, of course, always under the assumptions that the inputs and ice flow parameters are the same. If the ice viscosity is changed between two runs, one will obviously not obtain the same result. Since equifinality is about whether the method converges to the same solution (not necessarily the correct one) even under a range of different inversion 'designs', we do not fully understand how this is related to a discussion on how changing the ice viscosity impacts results. The latter is a discussion on how to obtain the right solution (not a unique one), and the sensitivity experiments on ice viscosity which we have carried out and discussed in several places (e.g. L444-446, L498, 514-517) clearly demonstrate the effect of different assumptions made. With that said, it is not entirely clear to us what else the reviewer wants to see discussed.

1.3 Minor comments

L523 *For Svalbard, Farinotti et al. (2019) exclusively used the results from Fürst et al. (2018). The reason was that this approach had a much larger amount of ground-truth data.*

Thank you for pointing that out! We were aware of that fact, but the wording in the manuscript was misleading and has been corrected now.

1.4 Figures

Fig.3 *Please use the same colours for the different input fields (cmb, velocity,...) for the two setups. This facilitates the reading.*

This is a good suggestion! We changed the color scale for cmb and dh/dt in Fig.2 (which we assume the reviewer refers to, since Fig.3 does not show any input fields) to align it with Fig.5. The color scale for velocity already is the same.

References

Farinotti D, Brinkerhoff DJ, Clarke GKC, Fürst JJ, Frey H, Gantayat P, Gillet-Chaulet F, Girard C, Huss M, Leclercq PW, Linsbauer A, Machguth H, Martin C, Maussion F, Morlighem M, Mosbeux C, Pandit A, Portmann A, Rabatel A, Ramsankaran R, Reerink TJ, Sanchez O, Stentoft PA, Singh Kumari S, van Pelt WJJ, Anderson B, Benham T, Binder D, Dowdeswell JA, Fischer A, Helfricht K, Kutuzov S, Lavrentiev I, McNabb R, Gudmundsson GH, Li H, Andreassen LM. 2017. How accurate are estimates of glacier ice thickness? Results from ITMIX, the Ice Thickness Models Intercomparison eXperiment. The Cryosphere 11:949–970. doi:10.5194/tc-11-949-2017.