

Summary and comments on the revised manuscript
tc-2023-43 entitled

**Reconciling ice dynamics and bed topography
with a versatile and fast ice thickness
inversion**

initially presented on 04.04.2023

by

T. Frank et al.

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 consider this manuscript for publication in *The Cryosphere*.

Major Comments

COMPARISON

Following the editor's 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.

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.

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.

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.

FIGURES

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