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

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

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by

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Summary

In this manuscript, the authors present an iterative approach to simultaneously infer the poorly constrained basal topography beneath glaciers as well as to initialise a glacier system model into a self-consistent state for prognostic simulations. Input requirements are surface observations on ice geometry and ice velocity as well as surface mass balance estimates (SMB). Thickness measurements are not ingested but withheld for validation. The approach is applied to a synthetic ice-cap setup and to a real-world glacier, i.e. Kronebreen on Svalbard. The synthetic setup serves to present a performance baseline for non-slip and sliding regimes under idealised input conditions. It is also exploited to analyse the sensitivity to input uncertainties and a-priori parameter choices. The method is then put to the test on Kronebreen, for which further methodological refinements - also during post-processing - are introduced. The performance of the iterative optimisation approach convinces for the synthetic setup, as the bed is retrieved with high accuracy. In the real-world, the misfit remains elevated as input fields and model parameters are less well known. Nonetheless misfit metrics are comparable to other approaches when observations are not ingested.

When I accepted the review, I was mostly attracted by the fact that this approach allows for a simultaneous initialisation of a forward model. Although this remains a side aspect in the manuscript, I consider this initialisation a big asset of the approach presented in this manuscript. I want to congratulate the authors to their concise presentation of this initialisation strategy. During my review, I however identified some major concerns on methodological details and the experimental design. The manuscript is well written and easy to follow, yet the structure can be improved. Moreover, there is a clear need for extending the discussion, certainly with regard to an assessment of the performance.

In summary, I am very positive about this manuscript and I recommend that the editor should continue to considered it for publication in *The Cryosphere* after my concerns have been alleviated. This will certainly imply a major revision.

Major Comments

BECHMARK VALLEY GLACIER

I think you should extend your setup to one of the ITMIX2 benchmark glacier, preferably a valley glacier. ITMIX2 is the reference benchmark for such approaches. As glacier, I suggest Austre Grønfjordsbreen as it is also on Svalbard and as most input is available. I ask for this because you deliberately forward regional-scale applicability on which such valley-glacier setups are regularly encountered. Moreover, sliding is less important and might be a challenge for the hybrid SIA-SSA model variant of PISM that uses an empirical function to combine sliding and creep. For a valley-glacier setting, I wonder how your initial viscosity choice will affect your performance even when allowing for sliding updates. Moreover, an ITMIX setup would provide possibilities to directly compare to the performance of other reconstruction approaches.

THICKNESS OBSERVATIONS

I truly appreciate that you compare your Kronebreen results to thickness observations. Yet I wondered why you did not use them during the iterative optimisation to better constrain your bedrock result. As you do know the bed in some locations, you could simply apply a 'restoring' in your bedrock update (Eq. 1) that drags the bed in each iteration towards these observations. In this way, you could further reduce the final bed misfit that currently exceeds100m in the Kronebreen setup (Fig. 4a). I also want to emphasise that reconstruction models in the 2nd round of ITMIX were asked to ingest thickness measurements. Moreover, ITMIX2 did highlight the importance of direct measurements and gave suggestions on acquisition strategies. Therefore, your approach would highly benefit from the capability of assimilating thickness measurements.

ON ITERATIONS AND ALTERNATIONS

If I understand your alternating optimisation strategy well, the basal and surface topographies are iteratively adjusted during prescribed 1000 steps followed by a single friction update. I wonder why the frictions coefficient is not updated iteratively as well. Friction seems decisive. Did you try this? I wonder if this could speed-up the convergence. As it stands now the convergence will very much depend on the actual value for λ.

I further wonder about the stopping criterion, which is set as a fixed number of iterations (which is not mentioned in the methods but during the experimental description - please adjust). In this way, much computing time is spent in negligible bed updates. An objective criterion on effective basal topography changes per iteration could help to further speed-up the optimisation. In addition, it would imply actual convergence.

SENSITIVITY ANALYSIS

I deeply appreciate the sensitivity analysis for your synthetic ice-cap setup in

the appendix. To me the most unconstrained parameters/variables are the climatic mass balance (CMB) and the ice viscosity. As I understand it, the synthetic geometry is in equilibrium, which means that the specific CMB should be zero. In Figure A1, you present relative CMB values with regard to a reference, which I suspect should be zero in a specific sense. So how can this value be increased by 25-75%. This is certainly a misperception from my part (I might sense that you actually increase the point values by these relatvie values –¿ which would imply that the specific values remains unchanged; in reality we do not know the specific CMB). In any case please explain and think about rather providing absolute specific perturbation values in metres ice equivalent per year.
Concerning ice viscosity, I think your analysis is convincing, certainly as you also present results without friction update (Fig. A4). Finally the sensitivity test with regard to the time step dt of the forward model seems dispensable. In my view, no matter the choice, equation (1) should internally compensate the time step scaling. The only constraint is that the mass conservation implementation remains numerically stable.

MANUSCRIPT STRUCTURE
In my view, you should better distinguish between methods, results and discussion. I somehow like your division by synthetic and real-world setup in terms of experimental setup and results. Yet the methodological updates in the latter setup, concerning the control parameter update (on β) and the post-processing, are confusing. I would introduce both concepts already in the methods section and apply them consistently both in the synthetic and real-world setup. Please streamline both setups in terms of methods. The manuscript would be easier to follow. Moreover, the last section on 'Discussion & Conclusions' is confusing. Please separate both aspects into dedicated sections.

DISCUSSION
As it stands, your discussion focusses on the benefits of the sliding updates, limitations from regularisation and the post-processing. I miss some comparison of how your approach performs with respect to others. You forward the mean absolute error as a measure of performance - is this quantity available for other approaches. To my knowledge the global consensus estimate by Farinotti et al. (2019) can be exploited for a direct comparison to your Kronebreen results.

Minor comments

L1-4 I totally understand your intrinsic excitement/motivation to raise global scale applicability in the first sentence. Yet in L4 your phrasing already moder-

ates this applicability to local and large scales. As this study presents a new method, the abstract should rather focus on performance not so much on the outlook. This outlook is indeed exciting and should/could be an aspect for your conclusion section.

L6-7 Here you claim that your iterative approach also serves for model initialisation into a self-consistent state. Your experimental setup does not substantiate this claim. It is taken as a fact from the methodological design. It could be worth to run the synthetic ice-cap setup forward in time after bed retrieval. Ideally the geometry would not change much in this equilibrium setup.

L92 You formulate that the observed elevation change is the primary target quantity for optimisation. I am a bit worried on the model capabilities to reproduce these rates near the glacier margin (no matter if land- or marine-terminating). It is known that this regions is critical in terms of flux divergence. In your synthetic ice-cap setup, you deliberately exclude the margin from the bed retrieval (L183). For the real-world glacier you even introduce strong gradients by the applied masking. Together with the SIA aspect in the ice-dynamic formulation, I wonder about any consequences for the applicability. This is certainly another point that should be picked up in the discussion.

L151 As your iterative approach infers the bedrock topography and the friction coefficient simultaneously, you should discuss potential ambiguities. Is this problem well posed? Is there only a single solution and are you convinced that the target parameters can be well differentiated.

L183-184 Do you also apply this margin masking for the real-world setup.

L267 This initial Gaussian filtering seems vital for application of the SIA. Still for more complex valley-glacier geometries, this step might not remove all artefacts. I therefore wonder if you also tried an initial relaxation with a prognostic run for which the geometry is not allowed to evolve too much (by capping the elevation change rates). This strategy could be more robust and beneficial as a prior step to your reconstruction.

L282 The masking of ice cover to the Kronebreen outline seems a bit harsh as it will introduce extreme gradients in surface elevation for example at the divide with Kongsbreen. I suggest to rather keep the full ice geometry also outside the Kronebreen outline and only update the basal topography within the mask (and prescribe/freeze it outside). It should not be difficult. Probably this is anyway what you have done.

L295-203 Many details on parameter choices of this paragraph can be added to Table 1. See below comment on Table 1. In this way, I sense that this paragraph can be reduced.

L317 The iterative increase of this relaxation parameter seems very fundamental in terms of methodology. I therefore urge you to include it in the main method also covering the synthetic setup.

L342-343 In my view membrane stresses are in general captured by the hybrid SIA-SSA ice-dynamic variant in PISM. It might be that the PISM strategy to merge SIA and SSA anyway suppresses this effect. Please be more specific

and rephrase.
L348-365 Here you introduce a post-processing correction in the middle of your results section. This comes as a surprise to the reader. I suggest that you rather introduce it as an optional post-processing filter in the methods. I am sure that, there, it can presented more concisely.
L366-376 This entire paragraph has a discussion character and does not fit into the results section. Please adjust according to my main comment on the manuscript structure. **L372** 'too high' $\longrightarrow$ 'too low'

FIGURES
Generally, the figure quality can be improved to better guide the reader to the important details by structural and visual re-formatting.
Fig. 2 I think this figure tries to serve two purposes. First it presents a schematic of the iterative approach. Second it introduces the synthetic ice-cap setup. I would split these two aspects in two individual figures. The introduction of the synthetic ice cap is the second figure and it should be formatted similarly to the present Fig. 3 (see respective comments). I would also not blend input fields and results in one figure. I therefore better like your presentation of the real-world setup in Fig. 3 and Fig. 4. Please try to present the synthetic setup analogically. For the results of the iterative figure (currently Fig. 2b), I urge you to also show iterative reduction of the mismatch between observed and modelled velocities and ice thickness. This is more easily done once you present the results as an individual figure. The velocity mismatch should decrease during the friction updates. I wonder what happens during the subsequent 1000 iterations of bed updates.
Fig. 3 I miss the thickness observations in this figure. Please add another panel.
Fig. 4 Please add the requests on Fig. 2. Furthermore, can you rather show relative thickness errors instead of absolute bed errors in panel c. This will facilitate the assessment of the importance of these differences. In addition, I request that you do not focus on this central location for the velocity comparison but rather show an extra figure covering the entire Kronebreen catchment showing modelled and observed velocities. Then the reader can better assess your velocity results, which are otherwise not presented. The latter could be an extra figure.
Fig 5 What do the colours mean in panel c? To me it would make sense to have panel c also presented for the synthetic ice cap. This could serve as a baseline for an ideal setup and help to assess your approach.

TABLES
Table 1 Please extend this table to cover all experimental setups (ice cap, Kronebreen and potentially Austre Grønfjordsbreen). In this way, comparison is facilitated (also see comment to L295-302).