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

Thomas Frank, Ward van Pelt, Jack Kohler

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1 Response to reviewer 1

We would like to thank reviewer Samuel Cook for his remarks that helped us to improve our manuscript. We reply below by highlighting the reviewer's comments in italics above our answers.

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. I think this is a good paper that makes a useful contribution to the field of glacier thickness inversion, but I have a few major concerns around the general framing and structure of the paper that I feel need to be addressed before I can recommend it for publication. I do not think any of these are particularly difficult to solve, however, and I think the authors should be congratulated on coming up with an ingenious new solution to a difficult conundrum!

We would like to thank the reviewer for his overall positive comments.

1.2 Major comments

The title and abstract of the paper major on this being a fast method for inverting for thickness, but I think the paper doesn't necessarily back that up.

- *An easily fixable issue here is that the paper never actually says how fast the method is, beyond saying it's 'fast'. The authors present results in terms of the number of iterations they took to achieve, but never say how long it takes to run an iteration. So, somewhere, I think the paper has to provide an actual time per iteration or per experiment to allow the reader to judge how fast the method is*

- *I feel it's also difficult to say the method is fast when its speed depends on the user's choice of ice-flow model. Presumably, if you were using a full-Stokes model, the method would be considerably slower?*
- *I also think that the speed of the method is not really the important thing about it. I would say the real advantage of the method is that you end up with a self-consistent model glacier that you can stick into a prognostic simulation without having to do any relaxation. After all, you can invert for thickness very quickly if you just assume perfect plasticity and have a DEM. The resulting glacier will be a horrible mess, but it will be fast. What we don't have so much yet is a method that shortcuts all that relaxation running time, and that's where this method really innovates, I think.*
- *So, overall, I would strongly encourage the authors to reframe the paper towards the 'self- consistent model glacier' aspect and away from the 'fast' aspect. I think changing the title and the abstract would go a long way towards doing that – the discussion already points this way, so I really think this is more-or-less a cosmetic front-end change than anything else. Though I think the authors really do still need to give somewhere an idea of how long the method takes to run in actual time, rather than just giving a number of iterations, as that's a useful comparator with other methods, regardless of any claims the authors do or don't make about how fast the method is.*

We agree with the reviewer that our claim of a **fast** inversion technique has not been discussed enough in the original manuscript. At the same time, we do not think that it is very meaningful to give an actual time per iteration, as suggested, since the computational time of every inversion depends on a range of glacier-, hardware (as in computational capacity) and ice flow model characteristics as well as model resolution that will vary greatly between applications of our method. As correctly pointed out by the reviewer, running a Full-Stokes model, for instance, would make the method slower. So, instead of giving a time per iteration, the revised manuscript will contain are better discussion around speed of ice thickness inversion methods. Specifically, we will define that *fast* must be understood as *comparatively fast* considering what other approaches yielding a similar output would cost in computational time. Based on our test glacier Kronebreen, we will demonstrate that running 8000 iterations, each of which comprises a forward run of 0.01 yrs, results in a net computational time equivalent to running a forward model for 80 yrs. This can be compared to other inversion approaches, e.g. van Pelt et al. (2013) (requires 1000s of model years, Pollard and DeConto (2012) (100,000s of model years), Le clec'h et al. (2019) (1000s of model years) or expensive adjoint-based ones (e.g Goldberg and Heimbach, 2013).

On re-framing the study, we do agree that the abstract could better reflect the 'self-consistent model glacier' aspect. We will stress this part more in the abstract and instead move the focus away from the large-scale applicability of the method.

Section 3.2 (on model sensitivity) needs some rewriting. I'm very glad the authors tested the sensitivity of the model to both input errors and model parameters – that's a really useful thing to do – but I think this section needs quite a bit more work to make it really helpful for the reader. As it stands, it's much too vague and imprecise to provide a clear picture of model sensitivity – I came away from it feeling I didn't really have much more of a clue as to how sensitive the model really was compared to when I started reading it. I know the authors give a lot of the details in the Appendix, but I think there are some headline statistics the authors could add to the main body of the text to make this section useful without having to make the reader dig down into the appendix.

- *In particular, in Section 3.2.1, it would be helpful if the authors could provide some quantification of how much the perturbations to the input data impacted on the final bed/friction field, beyond just saying 'large bed responses', which is a bit vague – 10 m, 50 m, 100 m?. The authors state* *that the ice volume is pretty insensitive to all the perturbations (though I would like some idea of what the percentage change in volume across all the simulations is), and helpfully explain all the mechanisms by which the errors feed through into glaciological changes, but I didn't really get any sense from the section as written what the magnitude of those glaciological changes were. As it stands, the section leaves open the possibility that it could well be that the perturbations don't affect the overall ice volume, because they lead to the bed being 500 m too high in half the domain and 500 m too low in the other half. I would also want to know how much the authors had to increase theta to mitigate the issues – 1.5x, 3x, 5x, 10x? - and I think there needs to be a bit more detail around 'more dependent on initial conditions'. The best solution is perhaps a table showing the mean bed and dh/dt mismatch for each simulation (or some other metric of the model's performance), and then maybe a figure showing some of the final beds to show what 'more dependent on initial conditions' actually means.*

- *For Section 3.2.2, I would similarly like some idea of the range of values of beta and theta the authors tested (the authors give the range of values for T, for example, in Section 3.2.1; the same should be done here). The authors repeat their previous observation about larger theta leading to greater dependence on initial conditions here, with a little more detail as to why, but I'd still like to get some sort of sense of what that looks like or means in practice without having to dig through the appendix. If the authors do put together some sort of figure to show that, I think it fits better here than in 3.2.1, as this is the section about explicitly testing theta (in which case, the authors could just stick a 'see below' in Section 3.2.1 when they mention that larger theta lead to greater dependency).*

We appreciate the suggestions to modify section 3.2. As mentioned in the manuscript, the influence of errors in the input data on the calculated bed depend on several glacier characteristics and are thus hard to generalize - hence why we refrained from giving specific numbers. However, we follow the reviewer and will provide more quantitative metrics, both in the text and in a new table. We will include metrics for each perturbation experiment, namely the ratio between the ice volume given the perturbation and the true ice cap volume, as well as the absolute bed misfit. Together, these two metrics reflect whether or not a perturbation results in the correct ice volume and if the bed is similar in shape to the true bed. A figure on varying theta will also be included. Furthermore, we will provide the range of tested values in sec. 3.2.2 as suggested.

As a more general point, I'm wondering what would happen if the authors updated friction first, then the bed. Would the method converge towards the same friction/bed combination, or would you get something different? Fundamentally, how do the authors deal with the problem of equifinality (as in, there are lots of possible friction/bed combinations that would reproduce the observed variables – how do the authors know theirs is a sensible one?)? Some discussion of this would be a useful addition, either in Section 5, or elsewhere where the paper discusses the robustness of the model. One additional simulation changing the order of the friction and bed updates would, I think, be very instructive and I would encourage the authors to see if it makes a difference.

The question of equifinality is indeed an interesting one that has not been discussed enough in our initial submission. Recall that the concept of the apparent mass balance shows that dh/dt and mass balance together determine the total mass which is fluxed through a glacier. This mass may be transported either by a thick but slow glacier, or a thin but fast glacier. By forcing our model to reproduce observed velocities via the friction coefficient there would only be one ice thickness that matches the mass flux if ice thickness and ice speed were independent of each other. However, since ice thickness also influences ice speed by changing the driving stress, there is a small realm of overlap between two regimes, namely when observed velocities can be reproduced either by a change in ice thickness or in basal friction. Due to the different characteristics of sliding (non-local, vertically uniform velocity profile) and shearing (local, vertically dependent velocity profile) ice physics, we think that it is typically very unlikely that changing thickness or basal friction would lead to the same ice velocity and mass flux, and therefore, that equifinality is an issue. Given the fact that all of our experiments, synthetic and real-world ones, converge to a sensible bed even with different initial conditions corroborates this idea. We will include this discussion in the revised manuscript.

On the comment of changing the order between friction and bed updates: Friction updates have a near-instantaneous effect on ice velocity and therefore ice thickness, leading to a rapid convergence to a new state after a friction update. And although the effect of a friction update applied at one grid point is non-local due to the mathematical nature of the SSA, the radius of affected cells around is limited, again indicating that a new state is reached rapidly after a friction update. Bed updates, on the contrary, lead to changes in the mass 'export' of an affected cell, and thus affect all downstream grid cells which will also change their mass 'export'. Because of that, bed updates are travelling downstream through the entire glacier, implying that it takes much longer to reach a new state after bed updates. In fact, through this concept, it can be understood that the convergence time to a new bed is equivalent to the response time of the glacier to a mass balance perturbation. So, given the difference in convergence time of bed vs. friction updates, it does not make sense to swap the order between them. We did conduct experiments that show just that, but for the sake of conciseness suggest that a discussion as done here will suffice in the revised manuscript.

1.3 Minor comments

p.1, l.14: 'fieldwork' not 'field work'

Will change.

p.2, l.24: 'allow the derivation of' not 'allow to derive'

Will change.

p.2, l.30: 'former' not 'previous'

Will change.

p.2, l.47: Some examples of where the conditions/assumptions in the SIA aren't met (sliding glaciers...) would be good here - here the text assumes that people will know and don't explain, unless the reader goes back a few lines and works out that 'considering only internal shear' means 'not valid where there's something else going on'. It just makes the reader's life slightly easier.

Will change.

p.2, l.51: The text doesn't quite explain explicitly why using beds derived using standard thickness-inversion processes will require you to do model relaxation if you use them as inputs in a higher- order model. The text states previously that you'll get errors in the ice thickness using one of the standard approaches, but why do those errors then require model relaxation (the velocity and thickness fields won't match so there'll be artefacts so...)? It might also be worth explaining what model relaxation actually is. As with the previous point, the text is perhaps assuming slightly too much knowledge on the part of the reader and making them work harder than they perhaps should in the introduction!

Will change.

p.2, l.55: 'the present day'

Will change.

p.3, l.57: 'allows the inclusion of' not 'allows to include'

Will change.

p.3, l.80: 'way' not 'part'

Will change.

p.3, l.82-3: How well do we actually know the mass balance at most glaciers? This approach assumes the surface variables are well-known (usually true for the surface topography) and that the errors in the model are therefore entirely due to bed errors, but I'm not convinced that assumption is valid at most glaciers. I realise this is dealt with later on, but some acknowledgement here that it is an assumption that isn't always going to be true would be good.

What we state here is the assumption (not the requirement) under which our method is applied. We state that the surface shape and mass balance need to be *sufficiently* well represented, i.e. good enough to derive a reasonable bed estimate. How good an observation of mass balance needs to be to suffice to that end is a rather complex question. In our sensitivity experiments, we show that it depends on glacier characteristics and that the mass balance error can be quite large for non-sliding glaciers without causing significant bed errors (e.g. a 75% mass balance overestimation causes only a 12% ice volume overestimation). Due to this complexity, we do not think that sec. 2.1 is the right place to discuss mass balance errors.

p.4, l.89: 'point out' Habermann et al is plural.

Will change.

p.5, l.126: 'that gently induce'

Will change.

p.5, l.127: 'that it cannot otherwise reproduce'

Will change.

p.5, l.132: 'that could lead' I think I'll stop pointing these out now, but the general rule is that 'which' is used when introducing a new clause, otherwise use 'that'. Pretty much, if you've stuck a comma in, use 'which', if there's no comma, use 'that'.

Alright, thanks for the clarification.

p.9, l.219: 'error' not 'errors'

Will change.

p.9, l.220: 'lead to similar outcomes' not 'induce similar mechanisms'

Will change.

p.10, l.252: 'the subject'

Will change.

p.13, l.332: 'it can be higher' – you're talking about the mismatch (singular). Linked to this, how much higher is 'higher' here?

Will clarify.

Section 4.5: I'm not entirely convinced by the post-processing step here, as it seems a little counter-intuitive to me to use an equation based on the SIA to correct errors caused by using the SIA. I don't deny that it works, I just don't like it conceptually. To be fair, I'm not sure what else could be done that would be reasonable, but I suspect readers might react in the same way as me, so if the authors can just add a sentence acknowledging that using the thing that caused the problems to fix the problems is what's going on and that the reader's possible confusion is understandable, it might make it a slightly easier pill to swallow.

We will add a note on that!

p.17, l.418: 'such as' not 'such that'

Will change.

Section 5: I think the approach presented in this paper could be usefully applied to the ITMIX dataset to benchmark it against other widely-used inversion methods. Are the authors considering doing this? Either way, a statement on yes/no and why/why not towards the end of the section would be good, because it's otherwise a bit of an elephant in the room

We chose Kronebreen as a test case due to its exceptional coverage with ice thickness observations which is even better than for the ITMIX glaciers. Nevertheless, a comparison with one of the ITMIX glaciers is a reasonable suggestion which we also considered including in our original manuscript. However, there are reasons that speak against that: First, describing another setup will inflate the length of the manuscript considerably and lead to an overall confusing structure of the paper. Second, running the hybrid SIA-SSA model on valley glaciers, as are mainly found in the ITMIX setup, is challenging. In fact, using PISM for such a setup would stretch the model´s capabilities and possibly lead to poor results. A model more suited for valley glaciers (i.e. a higher-order or Full Stokes model) would hence need to be used as a forward model, but again, describing this model in the manuscript would inflate the length and divert the focus away from the key points of the paper. We, therefore, suggest to refrain from adding further test glaciers. However, we are currently preparing another manuscript where we use the Full-Stokes emulated ice-flow model IGM (Jouvet, 2022) to apply our method on a regional scale which includes ITMIX glaciers. We will add an outlook in the discussion section on that. Furthermore, to aid comparability of our method with other inversion techniques, we will more explicitly discuss how the results of Kronebreen compare to other thickness products available for that glacier in the revised manuscript.

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