

On the theoretical limitations of joint inversion for basal slipperiness and effective viscosity in ice-flow models

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We are very grateful to both reviewers for their helpful advice and positive comments. We have incorporated their suggestions into a revised version of the manuscript. We have addressed the comments of Daniel Martin (highlighted in blue) as follows:

There are a few places where the tone of the prose leans toward a somewhat non-collegial tone – I've attempted to point out specific examples, but in general I'd encourage the authors to avoid phrasing which reads like "We're right, everybody else is wrong", which generally isn't helpful. It's sufficient to simply make your points without that level of editorializing.

Thank you for this feedback. Our attempt to motivate and put the results in context went a bit overboard, and we agree the manuscript is better with these sentences deleted.

One issue you don't touch on which I think is relevant given other developments in the field: given your finding of separation of basal friction and rheology/viscosity in your inversions, this implies that mismatches between model and "true" rheology will be expressed in, and isolated to, the component of the inversion which adjusts viscosity or A . In particular, mismatches in the Glen's Law flow exponent will be accounted for there. I suspect the implication that these mismatches likely can't be accounted for via basal friction is important.

Another point it would be good for the authors to address is potential limitations in this work due to the various simplifications and assumptions made. I think the work does a very good job of simplifying in support of extracting meaningful insight, but there are still likely impacts from these, and it would be good to include a short discussion along those lines.

In particular, I suspect that your choice of a linear rheology may oversimplify things somewhat. In practice, the Glen's Law rheology generally used for ice tends to result in concentrated regions of high strain rates, with a correspondingly relatively fine spatial structure of viscosity (more pronounced if $n=4$, which many observations seem to support), along with an interplay between velocity and viscosity perturbations. One could imagine that this would result in some coupling between basal friction and viscosity, unlike what you find in the linear model. It would be good if you were able to address this. If not beyond the scope of this work, one nice additional example would be to add a second U_a inversion example which uses the full nonlinear rheology.

I expect the impact of m in the basal friction formulation is less, but still worth mentioning potential impacts in your discussion. In general, I think a paragraph summing the ways your example model deviates from common practice and potential impacts would be a helpful addition to the discussion section.

Thank you for these suggestions, we have now added a new section "5. Limitations" ahead of the Conclusions which summarises the assumptions and simplifications that have been made in the study. In this section we discuss the potential impact of non-linear rheology. However, we feel that exploration of the effect of $n=4$, etc. is outside the scope of this work, and would be suited to a further numerical study exploring the wider parameter space.

Specific points:

Thank you for the detailed, helpful comments.

1. lines 24-25: I think it would be better to mention basal friction/slipperiness first, then rate factor/viscosity/rheology -- admittedly a personal preference which you're welcome to ignore, but in my experience, most modeling efforts start by inverting for basal friction and then move on to the somewhat-harder rheology.

Thank you for this suggestion. We have swapped the order in the manuscript.

2. line 25: I think you could more-generally call this the local rheology of the ice, rather than rate factor (and then mention that it could be rate factor, viscosity, some form of "damage", etc.). I think that's somewhat more evocative, and also covers the fairly wide range of approaches people take to this aspect.

Thank you for this suggestion. It now reads: *"The local rheology of the ice characterised by the viscosity, or the rate factor A in Glen's flow law (Glen, 1953) which is the rheological law commonly used in ice-sheet modelling, or some parameterisation of a damage model."*

3. line 103: I'd suggest "commonly-used in" instead of "appropriate for" here.
4. line 140: "surface velocity fields" -> "perturbations in the surface velocity fields"
5. line 154: I think "flow line at a given y" would be clearer than "each y flow line", because my initial interpretation was "y-directional flow"

We agree these are clearer, and have made all three changes to the manuscript.

6. 190-197: This is a very nice result! (even if I'm worried it may not completely hold for a nonlinear rheology).

Thank you! Our expectation is that higher-order n may actually increase how distinct the frequency profiles are for the viscosity and slipperiness transfer functions. We have added this to the "5. Limitations" section above.

7. line 257: "some data errors" -- can you expand here (or somewhere) what the form of your data errors/noise take for these experiments? If I'm reading correctly, "data errors" are only applied to the ice surface "measurements", correct? (as opposed to ice velocities or bed height).

These refer to the size of the data errors added to the surface velocity measurements, which we describe in the preceding paragraph. The wording of "some" was confusing. We have now modified line 257 to read: *"The difficulty in retrieval starts once we add meaningful data errors to the surface velocity measurements."*

8. line 274: I'd suggest a toning-down or removal of "This is in direct contradiction to many of the claims in the literature", which is needlessly confrontational. It's fair to lead with "Unlike others (cite), we find that.." or "Unlike (cite), we find that..."

Deleted.

9. line 282: I'd use "baseline" instead of "true" here, since the "true" fields in the context of these experiments are actually baseline + perturbation.

Agreed. We have changed this in the manuscript.

10. line 284: I think it would be helpful to spell out what "i.i.d." stands for. I had to look it up, and if I had to, other readers will as well.

Done. It now reads: "... with independent and identically distributed (i.i.d.) Gaussian random noise

added..."

11. Figures 4 and 5: It looks like the perturbations are actually of the form $\sin(\log(A))$ and $\sin(\log(c))$, correct? It would be good to mention that explicitly since it's logarithmic in these figures and not in Figure 3.

The sinusoidal perturbations are added to η and C , it is only the inversion which is done in log-space. (The perturbation amplitude is 10%, and so the shape of the perturbation still looks sinusoidal in log space.) We've added that explanation at the point logs are mentioned at line 287 for extra clarity. It now reads:

"We have chosen to invert for the fields in log-space ($\log A$ and $\log c$), which is the typical inversion approach in $\dot{U}_a$ for numerical Antarctic ice flow studies, although the sinusoidal perturbations are applied to η and C , making it identical to the configuration in Figure 3."

12. line 320: "as has been suggested by some authors" is also unnecessary.

Deleted.

13. line 322: "we believe that" -- it's better to confirm things when possible rather than guessing at something in print. In this case, I can confirm that BISICLES generally performs its inversions to match $\text{mag}(\text{velocity})$ (which is stated in Cornford, et al, 2015.)

Thank you for the confirmation! We have deleted "we believe" from the text accordingly.

We have addressed the comments of Anonymous Referee #1 (highlighted in blue) as follows:

Below are two points that could be considered for a minor revision of the paper:

- **as the proposed experiment involves out-of-phase perturbations, it could be interesting to show if the results still holds for perturbations that would be in phase or not completely out of phase.**

Thank you for this suggestion - it raises an interesting question of whether the current experiment is a unique point in the parameter space, and so could give deceptively good or bad results. The alignment of the viscosity and slipperiness perturbations themselves is perhaps not what is important - what matters is the alignment of the resulting perturbations in the surface fields. The transfer functions are imaginary and so introduce a phase component of their own into the resulting surface field perturbations. The functional form of the η and C transfer functions are distinct, which is why the phase difference won't be preserved in the effect on the surface fields. The combined effect on the surface fields is frequency dependent and not necessarily in-phase or out-of-phase. Given this, we prefer not to include an additional experiment which might confuse the readers, but instead have included this point in the new section "5. Limitations" (see below) to highlight that the space of different frequencies and different phase differences for the perturbations hasn't been explored.

- **it would be interesting to add a discussion section to summarise the main assumptions and how this could affect inversions for real settings; For example, the proposed experiment assume a perfect model, linear friction and flow laws, uncorrelated observations errors, perfectly known surface and bed elevations, well constrained prior.**

Thank you for this suggestion, and we have now added a new section "5. Limitations" ahead of the Conclusions.

minor comments:

- **Section 2.1 : define the notation with bar on top for the background fields. This is defined only in the Appendix.**

Thank you for catching this. This definition has now been added to the manuscript body as well.

- **Line 203 "prior modelled using the Matern covariance function" and Appendix: equivalence between Tikhonov regularisation and covariance functions can be derived analytically for some special cases (see also e.g. Barth et al. (2014)) and helps to understand the role of the regularisation. But can be worth to mention that in general it will not be explicit. The effect of a prior error covariance matrix parameterised using Matern correlation functions can be reproduced using a diffusion operator, as it is done in e.g. FeniCs (Recinos et al., 2023).**

Thank you for these references and extra details, we have included them in the revised manuscript line 204:

"...maximising the a-posteriori distribution in the Bayesian MAP framework with uncorrelated Gaussian measurement errors and a prior distribution modelled using the Matérn covariance function (e.g. Lindgren et al., 2011; Barth et al., 2014). In general, the relationship between the covariance of the prior distribution and regularisation terms in a cost function is not explicit, although it is possible to reproduce a covariance parameterised with Matern correlation functions using a diffusion operator, as done in FeniCs (Recinos et al, 2023).

[2]Barth, A., Beckers, J.-M., Troupin, C., Alvera-Azcárate, A., Vandenbulcke, L., 2014. divand-1.0: n-dimensional variational data analysis for ocean observations. *Geoscientific Model Development* 7, 225–241.
<https://doi.org/10.5194/gmd-7-225-2014>

[3]Recinos, B., Goldberg, D., Maddison, J.R., Todd, J., 2023. A framework for time-dependent Ice Sheet Uncertainty Quantification, applied to three West Antarctic ice streams (preprint). *Ice sheets/Data Assimilation*.
<https://doi.org/10.5194/tc-2023-27>

- **Line 227 " T^{**} is an $m \times m$ " matrix; m is defined in the Appendix not in the main text and m is already used for the exponent of the friction law.**

Thank you for catching this. We have now added this definition to the manuscript body as well, and switched to dimension "d" everywhere.