

The Cryosphere Discuss., author comment AC2
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Reply on RC2

Constantijn J. Berends et al.

Author comment on "Compensating errors in inversions for subglacial bed roughness: same steady state, different dynamic response" by Constantijn J. Berends et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-103-AC2>, 2022

Response to comment tc-2022-103-RC2 by Anonymous Referee #2

We'd like to thank the anonymous reviewer for their helpful comments on our manuscript, and would hereby like to address the concerns they raised. Reviewer comments are shown in italics, our responses in regular type.

Major comments

The literature review is not adequate and at times not accurate. In the introduction (lines 46-57) the authors mention several papers as examples of bed roughness inversion, where in fact most of those papers target the inversion of the basal drag (or basal friction), not the bed roughness. While these quantities can be related, they are certainly not interchangeable. Also I think there are some relevant papers that should be cited. Babaniyi et al, TC 2021, present a rigorous approach on how to account for model errors (in particular in the rheology) when inverting the basal friction. Other studies that look at the impact of rheology on inverted quantities are Seroussi et al., Journal of Glaciology 2013 and Ranganathan, Journal of Glaciology, 2020. A preliminary study of how errors in SMB could affect inverted basal parameters were featured in perego et al, JGR, 2014.

We agree that these are relevant references that should be mentioned in the manuscript. We will expand the introduction section of the manuscript to provide a more comprehensive overview of previous work on inversion methods, and specifically the effects of errors in model parameters and observations. We will also take care to clarify the difference in inverting for bed roughness, and inverting directly for basal drag.

The authors present a clever but involved and ad-hoc way to invert for the bed roughness. I find this anachronistic. Nowadays, the large majority of work performing inversion of ice sheet quantities uses PDE-constrained optimization approaches, which are very well understood and naturally linked to Bayesian inference problems.

Variations on the "nudging" method of inversion are used in e.g. CISM (Lipscomb et al., 2021), PISM (Albrecht et al., 2020), and f.ETISh (Pattyn, 2017), which are some of the most widely-used ice-sheet models of today.

Key parts of the PDE-constrained optimization problem are the regularization terms, that avoid overfitting, and the ability to weigh observations according to their trustworthiness

(i.e. root mean square errors in observations). The proposed method has a regularization step in the form of a Gaussian filtering, but it's not clear to me how to link that to the typical regularization term in the formal optimization approach. In my understanding, their method does not account for root means square errors in the velocity or thickness data, which is a significant limitation. I think the author should discuss these limitations and also investigate how different choices of the radius of the Gaussian filters affect their results. I suspect that there is too much overfitting in their inversion.

Regarding regularisation and overfitting: our inversion simulations are ran for 100,000 years. Typically, the inverted bed roughness converges to a stable solution within the first 50,000 years. This means that the regularisation (which indeed is done by way of a simple Gaussian smoothing) is working well, preventing the inversion from continuing to adapt the roughness solution when the misfit is no longer significantly reduced. Furthermore, Figs. 4 and 5 clearly show that no visible small-wavelength terms appear in the roughness solution, again indicating that there is no significant overfitting occurring.

The radii of the two Gaussian filters in our approach, were arrived at during preliminary experiments. The values reported here are the lowest values we found that effectively repressed small-wavelength overfitting terms in the inverted bed roughness. The target roughness in our experiments is relatively "smooth", with horizontal variations on a scale that is at least an order of magnitude larger than the grid resolution. Increasing the radii of the filters did not affect our inverted solution much until it was increased to several grid cells, so that it approached the spatial scale of the roughness variations. Roughness variations of a smaller spatial scale could therefore potentially be obscured by the smoothing in our approach. However, these would then quickly approach the ice-dynamical limit of roughness variations that can be resolved by inverting from surface observations (about 50 ice thicknesses; Gudmundsson and Raymond, 2008), so we do not believe that this would pose a serious problem in practical applications. We will reflect these thoughts in the manuscript.

Our method currently does not include weighting of the velocity/elevation mismatch based on uncertainty estimations in the observations. It would not be difficult to include these weights in the method, and it is certainly something worth considering when we move on to apply this method to the Greenland or Antarctic ice sheet. For the idealised experiments we present here, there is of course no observational error. We will reflect these thoughts in the manuscript.

Minor comments

eq. (1): In general, τ_b and u_b are vectors. Please write the equation in vector form (using the vector u_b and its magnitude $|u_b|$).

We will fix this.

eq. (1): How do you compute N ?

In the experiments presented here, we set the effective overburden pressure N equal to the ice overburden pressure, assuming no subglacial water anywhere. We will mention this in the manuscript.

line 172: how do you choose the radii of the Gaussian filters?

See our earlier response regarding regularisation.

section 4.2. Typically we distinguish errors in the data (e.g. in velocity/thickness observation and, possibly, SMB), from model errors (specific laws and model parameters

like A, p, etc). The latter are harder to account for. I think it would be better to do this distinction in your perturbed experiments.

We agree that this distinction is important to make. We will clarify this in the manuscript.

Figs 4 and 4: The range of the colorbar for the bed roughness is too wide. I would limit it to the interval [0.5,2] or so, rather than [0.1,10]. More ticks on the colorbar might help as well.

Based also on the comments of reviewer #1, we will change Figs. 4 – 8 to have a smaller range for the colour scales for all three errors (roughness, elevation, and velocity). We will also change the colour map for the velocity, to make it more clear that these panels show a different quantity than the roughness. Also, the velocity errors were accidentally still shown as absolute errors rather than relative; we will fix this.