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Reply on RC1

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-AC1>, 2022

Response to comment tc-2022-103-RC1 by Anonymous Referee #1

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

There are some confusions or inaccuracies at times about the two inversion methods. This is especially the case when referring to inversions overall and discussing the question of steady-state: while the steady-state assumption is used for the inversion based on transient simulations, this is not needed for "snapshot" inversions based on variation data assimilation. The geometry and velocity observations are used in the stress balance equation only, and the ice sheet does not need, nor is assumed, to be in steady-state. The combination of the given velocity and geometry will prescribe whether the ice sheet is in steady-state or evolves over time. Such terms are mostly used in the abstract and introduction and should be clarified to remove any possible ambiguity.

While there are nowadays transient inversion methods in use that do not make the steady-state assumption (e.g., in CISM a new approach is currently being tested that inverts for both geometry, velocity, and thinning rates), we agree that most of the time this is still the case. We will clarify the distinction between the two families of inversion methods in the manuscript.

The results proposed show the impact of errors in various ice sheet fields on the inferred basal friction values in a relatively comprehensive way, but does not put this work in the context of previous studies. Previous work on inferring several fields (Arthern et al., 2015; Gudmundsson and Raymond, 2008), impact of rheology (Seroussi et al., 2013), or role of errors in observations (Habermann et al., 2012) has been done in the past and should be referenced and discussed to better describe the improvements and new results proposed here.

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.

There is limited discussion on the choices made to perturb the different fields (viscosity, surface mass balance, etc.) and I would be curious to understand how these choices were made. Also, how do these changes compare to each other between the different fields (are these large or small bias) and how do they compare to our knowledge of the different fields and the current uncertainty for each of them? Such information would help better inform the results, and in particular the uncertainty in future evolution (Fig.9).

The chosen parameter ranges are based both on upper-bound estimates of the uncertainty in the physical nature of the Antarctic and Greenland ice sheets, and on the desire to stay within a range where the errors in these parameters can still be compensated for by changing the bed roughness.

For Glen's flow law factor, we assume that this depends on the englacial temperature through an Arrhenius relation. The uncertainty in the surface temperature through the last glacial cycle is about ± 1 K for Antarctica (Jouzel et al., 2007), and about ± 4 K for Greenland (Alley et al., 2000; Kindler et al., 2014). Furthermore, in realistic applications a flow enhancement factor is often applied to account for anisotropic rheology and damage. Estimates for the values of these enhancement factors differ significantly (Ma et al., 2010). Based on the literature, an uncertainty of an order of magnitude in the ice flow factor seems plausible. We chose a smaller range (increase/decrease of 25 %) to ensure that the inversion procedure was still able to reproduce the target geometry.

For the surface mass balance, we chose an admittedly small range of ± 5 % in the accumulation rate. This is because, for simplicity's sake, we only changed the uniform accumulation rate. While observations of local accumulation rates, melt rates, and SMB contain significantly larger uncertainties, caused by both measurement errors/uncertainties and interannual variability, the integrated mass balance of the entire Greenland / Antarctic ice sheets is better constrained through e.g. GRACE observations. Based on the Greenland Surface Mass Balance Model Intercomparison Project (GrSMBMIP; Fettweis et al., 2020), a range of ± 5 % for the long-term mean integrated mass balance seems plausible.

For the basal mass balance (or rather the sub-shelf melt rate, as we apply no melt underneath grounded ice), uncertainties are much larger (e.g. Burgard et al., 2022). Here too, we applied the simplest possible option of a uniform melt rate. As for the flow factor, our choice of parameter range was constrained not so much by observations, but by the need to stay within the window where the inversion procedure is still able to reproduce the target geometry.

For the subglacial topography, we chose a range of ± 10 % of the ice thickness. While the surface elevation of the Greenland and Antarctic ice sheets is generally known very accurately, estimates of ice thickness and bedrock elevation are based on interpolation of local radar measurements. In the BedMachine Greenland v4 dataset (Morlighem et al., 2017), the reported uncertainty in the bedrock elevation exceeds 10 % of the ice thickness over about 30 % of the ice sheet. Our choice of increasing/decreasing the estimated ice thickness by 10 % everywhere serves as an upper bound, as it is unlikely that all of the data and extrapolations are biased in the same direction.

For the sliding law parameters, we chose a parameter range based on values reported in the literature. Zoet and Iverson (2020) report a range of transition velocities between 50 and 200 m/yr, whereas in CISM a default value of 200 m/yr is used, sometimes going as high as 1,000 m/yr in sensitivity experiments. For the exponent, Zoet and Iverson report a value of 5, CISM uses a value of 3, and using a value of 1 makes it a linear sliding law (which is still used in some ice-sheet models), so testing a range of 1 to 5 covers all those options.

We will include this information in the manuscript.

Finally, there is an improvement described for the inversion technique, but this impact of this improvement on the inferred bed roughness and misfit with observed field is not shown. It would be interesting to add an experiment to compare the results with and without this improvement (e.g., using the non-steady-state case).

The inversion technique described here improves upon the one used in CISM. That one uses the mismatch between the modelled and target surface elevation and surface velocity, but does not have the flowline-averaging procedure. The difference is mostly noticeable in the stability, the speed of convergence, and in the reduction of artefacts at ice-margin or grounding-line grid cells. The final inverted roughness fields are nearly identical (apart from those artefacts), which is logical as the inclusion of a proper regularisation term guarantees that there exists a unique answer to the inverse problem. We prefer to avoid a detailed comparison of the technical pros and cons of the different inversion techniques in the manuscript, as this is beyond the scope of the project. We will reflect these thoughts in the manuscript.

Technical comments

p.1 l.22: "retreat" -> "mass loss"

We will change this.

p.1 l.26: the ABUMIP experiments are not only idealized cases but also extreme experiments remove all the ice shelves around Antarctica. These results are therefore showing an extreme case, and are difficult to compare with experiments using more nuanced forcing. It would be good to nuance this paragraph.

We will clarify that this is an extreme case.

p.2 l.30: maybe not just basal sliding and roughness but basal conditions overall.

We will change this.

p.2 l.34: this paper demonstrated the role of the basal sliding law used, but did not really conclude on the bed roughness itself.

We will clarify this.

p.2 l.53: as explained here, this second method is performed using observations at a given time, and the model is not run forward in time as part of the inversion procedure. The geometry is therefore "given" but not really "kept fixed" as there is no notion of time.

We will reflect this conceptual difference between "nudging" vs. data assimilation methods here and throughout the manuscript.

p.2 l.54: it would be important to mention how this method works: a cost function, measuring the distance between some observed fields and their modeled equivalent, is defined and this cost function is minimized during the inversion procedure.

We will include this short technical description in the manuscript.

p.3 l.65: you need to make a distinction between the two methods here: what is described only works for the first inversion technique, in the second one, there is no impact of the ice sheet geometry as part of the inversion and therefore no "steady-state" ice sheet. This

should be rephrased to either distinguish the two methods or to make the description generic enough to cover both methods. (same with "thinning the ice" on l. 66)

We will clarify that these compensating errors will affect both families of inversion procedures in similar but different ways.

p.3 l.78: remove "still"

We will do so.

p.3 l.82: remove "of" (of as a result)

We will do so.

p.3 l.75-83: this is an accurate description of what is done in the paper, maybe the abstract is a bit too generic, which can lead to confusions about the two overall methods to infer properties (variational data assimilation and adjustment during a long transient run)

We believe that the two families of inversion procedures are fundamentally similar enough (particularly since ours also includes a velocity term, and is generally well able to match the observed velocities even when compensating errors are present) that the more general conclusions stated in the abstract are justified.

p.4 l.101: add some references in this paragraph (sliding laws, etc.)

We will include references to Weertman (1957) for the power-law sliding, and to Iverson et al. (1998) for Coulomb sliding.

p.5 l.135: explain what the "I" variables are. Also, why use the entire flowlines and not a region of influence with a given region of influence around the various points/regions?

The "I" variables represent the normalised half-flowline integrals of the ice thickness and velocity errors. The scaling functions w_u and w_d ensure that errors along the flowline close to p receive more weight than those further away. We did not experiment with limiting the length of flowline to integrate over. The length of the region influenced by changes in the bed roughness might well be variable, so that using the entire flowline seemed like a simpler first choice. We will include this information in the manuscript.

p.5 Eq.6-7: It's not clear if/how these variables defined for a single point are extended to the entire domain. Are the I variables defined and used locally or globally?

All equations are evaluated (i.e. a flowline traced in both directions, the integrals calculated, and the rate of change of the roughness calculated) for every ice-covered grid cell individually. We will mention this in the manuscript.

p.6 l.155: Can you detail these artefacts and the impact of the scaling values used?

The artefacts present as individual or clustered grid cells (typically on the ice margin or grounding line) where the iterative bed roughness adjustment overshoots, quickly diverging to extreme values. Also, sometimes short-wavelength oscillations in the roughness field can occur when the roughness is adjusted too fast relative to the amount of applied regularisation. The scaling values serve to limit the rate of adjustment so that these divergences do not occur. We will clarify this in the manuscript.

p.6 l.160: τ was used for the basal shear stress so it might better to use a different

letter for the time scale.

We will replace τ with t_s .

p.6 l.171: How do these terms compare to the regularization terms used in other methods?

In velocity-based inversion using more complex mathematical tools to minimise an explicitly defined cost function, typically term related to the gradient or curvature of the roughness solution is included in the cost function, which suppresses small-wavelength terms in the solution. Pattyn (2017) applies a Savitzky–Golay filter during the nudging process, similar to our Gaussian filter; Pollard and DeConto (2012) do not report any regularisation term. In CISM, no regularisation is applied, although the inclusion of a dH/dt -term in the calculation of $d\phi/dt$ likely results in some smoothing. We will mention this in the manuscript.

p.7 l.191: target geometry and velocity

We will change this.

p.9 l.217: "a deep oceanic trough" -> "a deep ocean" (there is no trough on the ocean part on Figure 3)

We will change this.

p.9 l.221: What value is used for A?

The flow factor $A = 1.13928E-17 \text{ Pa}^{-3} \text{ yr}^{-1}$ is tuned to obtain a steady-state mid-channel grounding-line position at $x = 450 \text{ km}$. We will state this in the manuscript.

Sections 3.1 and 3.2: How do you grow these two configurations to a steady-state and how long does it take to grow them? What is the resolution of the model?

A steady state is achieved in both experiments by running the model forward for 50,000 years. For experiment 1, we use resolutions of 40, 20, and 10 km (only 20 km for the perturbed inversions). For experiment 2, we use 5 and 2 km (only 5 km for the perturbed inversions and the retreat simulations). We will state this in the manuscript.

p.10 l.232: What is the impact of using a different initial value for ϕ ? 5 degrees is the most common value in this set-up so it might be good to make sure this initial value does not influence the results of the inversion!

Choosing a different initial value generally does not impact the final inverted roughness field. However, choosing a value that is far away from the target means it can take longer to converge to the correct answer, as the ice-sheet geometry will initially adjust to the "wrong" roughness, and will take some time to relax back to the correct geometry. We will state this in the manuscript.

p.10 Fig.5: It would be good to change the colorbars for the surface elevation and surface velocity differences and better see the errors. In caption, change "ice-sheet geometry" to "ice-sheet surface elevation". Finally, the colorbars for Fig. 4 and 5 are the same, but the velocity is very different for the two simulations, so it would make sense to adjust the values and really focus on the velocity modeled for each experiment.

We will change the colormap for the velocity errors to make it clear that these panels show a different quantity than the ones showing the till friction angle error. We will

change the colour limits to show the errors more clearly. We will change the caption to mention surface elevation instead of geometry. Lastly, we will update the figures, as the caption stated that the relative velocity errors were shown but the figures themselves accidentally showed the absolute errors. We will apply these changes to all figures.

We do not think that using a different scale for the target velocity fields in Figs. 4 and 5 (the unperturbed experiments) is desirable. The narrow ice stream on the southern margin of the ice sheet in experiment I (Fig. 4) reaches a maximum velocity of over 700 m/yr (visible as the narrow purple strip in the figure), which is not that much slower than the ~ 1000 m/yr reached by the shelf in experiment II.

p.11 Fig.5: In caption, change "ice-sheet geometry" to "ice-sheet surface elevation" and "three unperturbed" to "two unperturbed". Same as Fig. 4 for the colorbars of the surface elevation and surface velocity differences.

We will do so.

p.11 l.251: Mention this is for experiment 1.

The perturbations listed here apply to both experiments. We will clarify this.

p.11 l.259: How about the role of local errors due to noise in observations? For example a random noise of 5 or 10% in the velocity, thickness, etc.? How long is the model run to reach the steady-state? And how do you know that this steady-state is reached?

The inversions are initialised with the target geometry, and are run for 100,000 years. Generally, the inverted bed roughness converges to a stable solution within $\sim 50,000$ years, so that this is more than long enough to reach a steady state. We will state this in the manuscript.

We have not performed any experiments concerning observational errors, apart from the "topo" experiments that represent a systematically over/underestimated ice thickness.

p.12 Fig.6: again I would focus the colorbar for the surface elevation difference given the relatively small range of errors. Caption: "steady-state geometry" -> "steady-state geometry and velocity". Add "from left to right" before the list of parameters studied (viscosity, surface mass balance, ...)

We will do so, and apply the changes to the colourmaps and colour limits mentioned earlier.

p.13 l.284: How do you define that these are acceptable? How do they compare to observational errors?

The errors in the inverted surface elevation and velocity here are undoubtedly larger than observational errors. We deem them acceptable because they are well within the range of errors for initialised ice models in intercomparisons such as initMIP-Greenland, etc. We will mention this in the manuscript.

p.13 l.292: Over what part of the domain do you perturb the basal mass balance (grounded, floating, both)?

We apply only a sub-shelf melt rate. We will clarify this in the manuscript.

p.13 l.294: "introduced errors" -> "errors introduced"

We will change this.

p.13 l.299: "except for the inversions with perturbed basal melt rate" (or rephrase to make that more clear)

We will change this.

p.13 l.299: missing word after "these"

We will fix this.

p.14 l.313: For which experiment?

For experiment II. We will state this in the manuscript.

p.14 l.315: I am not sure to interpret that correctly: is the inversion or the run providing the data for the inversion run only to 10% of the steady-state? The second case would better represent reality, though as the ice sheet is never in steady-state, neither case is ideal.

The target run providing the data for the inversion is run to about 90 % of the steady-state ice thickness. We will clarify this in the manuscript

p.14 l.318: These errors are actually not so large compared to some of the runs with perturbed fields and the errors are located only upstream of the grounding line.

That is correct.

p.15 l.326: remove "steady-state" (last case is not steady-state)

The ice sheet from the inversion run is in steady state (the target for the last inversion is not, but that is not relevant here).

p.15 l.328: indicate where along the y axis is this grounding line shown?

The time-series show the mid-stream grounding-line position. We will clarify this in the manuscript.

p.15 Fig.9: It would be good to add the percentage of mass loss on the right of panel A.

We will do so.

p.16 l.338: remove "negligibly"

We will do so.

p.16 l.340: Change tense in paragraph (We investigated instead of We have investigated, etc.)

We will do so.

p.16 l.340: How does that compare to the previous approach? It would be good to show one case with the previous and new approach to see the impact of the changes (maybe the non steady-state MISMP+).

See our earlier response regarding the difference in performance between the two

inversion methods, and the fact that the resulting bed roughness fields are nearly identical.

p.16 l.346: this is not really new

We agree, but since we still come across people who place a lot of trust in their inverted roughness fields, repeating this conclusion seems useful.

p.16 l.356: How do the different changes applied in the different fields compare to our knowledge (lack of knowledge) in these fields? What are the implications for comparing these uncertainties?

We believe that our choice of parameter ranges generally reflects the uncertainty of these quantities for the Greenland and Antarctic ice sheets (see also our earlier response regarding these choices). We will mention this in the manuscript.

p.16 l.361: "not clear": what did you try?

We did not perform any additional experiments. Our use of the phrase "discrepancy" here might be confusing, as it is maybe not surprising that not all physical processes affect both basal sliding and ice-dynamical response identically. We will remove this sentence.

p.17 l.363-369: need more references and justifications

We will add more references to substantiate the claims made in this paragraph.