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

We 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.

> *Line 163: In general, the presence of regularization terms does not guarantee the uniqueness of the minimum. Moreover, local minima might be present so that different convergence paths might lead to different local minima. Please explain or rephrase the sentence.*

We will rephrase this sentence.

> *Line 543: I don't understand why the fact that the inversion method converges implies that the method does not overfit. Please explain, maybe add a reference, or remove the sentence.*

Without a regularization term, short-wavelength terms in the solution can continue to increase in amplitude as the model is run forward; the effect of these terms on the velocity solution displays diminishing returns, so that bigger and bigger changes in the solutions are needed to reduce the velocity/geometry misfit. This shows up in the convergence plot by a bed roughness rate of change that soon starts to exponentially increase (up to a certain point; in our model code, the till friction angle phi is limited between 0.1 and 50 degrees, so at some point the solution "stabilizes" by reaching theses limits everywhere). The Gaussian filter-based regularization term in our approach prevents this type of overfitting from occurring. We will state this in the manuscript.

> *Fig A2, right: can you explain why both curves of the bed roughness rate of change seem to be increasing towards the end of the simulation? Are these simulation not converged yet?*

For the CISM approach, we believe the wave-like features arise from an under-damped, slow oscillation between the bed roughness and the ice geometry. In the upstream part of the ice stream, where velocities are very low, the ice thickness responds very slowly to a change in bed roughness. The initial guess for the roughness is too high, causing the ice to slowly accumulate; the inversion will start lowering the roughness, but since the ice thickness changes very slowly, it lowers the roughness too much, causing the ice to eventually become too thin. With the current choice of timescale (tau = 40,000 yr) these oscillations do eventually dissipate, but it takes a long time. Including a dH/dt-term in the calculation of dphi/dt removes this problem, which is what Bill and Tim now use in their newest inversion approach. In our own approach, the velocity term in dphi/dt has a similar effect, since velocities respond instantaneously to a change in bed roughness (in theory).

For our own inversion procedure, we believe the noise-like features in the dphi/dt-curve to be caused by an interaction between the velocity term in our inversion, the iterative solvers used in our stress balance solver (both for the linearised problem, i.e. with fixed effective viscosity, and for the non-linear viscosity), and the dynamic time step used for the ice thickness equation. The combination of these iterative solvers with a dynamic time step causes (very) small errors to continuously appear in the velocity solution, only to be repressed by the subsequently reduced model time step. For the fast-flowing ice of this particular geometry, these velocity errors start to affect the bed roughness inversion before they are repressed by the dynamic time step, which causes the “noise” that is visible in the dphi/dt-curve of our approach. Using smaller tolerances in the stop criteria for the two iterative solvers in our stress balance solver reduces this problem, at the expense of increasing the model’s computational cost. Since the left panel of the figure shows that the resulting errors in the roughness solution do not accumulate, we deem this to be acceptable.

We will state this in the manuscript.

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

We 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.

Overall comments

> *The results of the different experiments made in the paper are described and discussed one by one in section 4, and there is limited discussion comparing the different results together and addressing some overall questions. Two important aspects that should be discussed in more details: 1) the problem of overfitting mentioned by a previous reviewer that should be included in the main text*

The problem of overfitting is explored in detail in Appendix A, which is now extended based on the new comments of Reviewer #1 to discuss how Fig. A2, panel B shows that overfitting is not problematic with our method. We will additionally refer to this Appendix again in Sect. 2.2 where overfitting is mentioned.

> *and 2) the question of which parameters cause the most problems based on the perturbation experiments done in the study, and the ones we can relatively safely ignore, based on the values used in the manuscript and the current uncertainties in these fields.*

We will add a few lines to the first paragraph of the Conclusions, discussing the relative importance of uncertainties in the different parameters.

Technical comments

> *l. 55-60: could use more references*

We will additionally refer to Athern and Gudmundsson (2010), Gagliardini et al. (2013), and Arthern et al. (2015) as examples of studies that have used velocity-only-based inversion methods to estimate basal slipperiness or traction.

> *l. 139: the exponent is p =*

The current sentence is grammatically correct.

> *l. 164: has a unique solution*

This sentence has been replaced based on the comments by Reviewer #1.

> *l. 194: I_2 and I_3 are reversed in the description compared to the equations*

They are not.

l.211: These values are

We will change this.

l.320: It is stated that the initial conditions do not impact the results. It would be good to provide some numbers for that.

The conclusion that choosing a different initial uniform value for the bed roughness does not affect the final inverted roughness field, was based on preliminary experiments, which we unfortunately did not store.

l.325: "very small" should be quantified

The errors in the unperturbed experiments are typically < 5% for the bed roughness, < 5 m for the surface elevation, and < 5% for the surface velocity. We will state these numbers in the manuscript.

l.445-450: Additional references needed

We will refer to Rignot et al. (2019) to support the claim that the present-day Antarctic ice sheet is not in equilibrium, and to Seroussi et al. (2019) to support the claim that many ice-sheet models used for future projections (implicitly) assume that it is.