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The Cryosphere Discussions

Interactive comment on "A simple inverse method for the distribution of basal sliding coefficients under ice sheets, applied to Antarctica" *by* D. Pollard and R. M. DeConto

M. Habermann (Referee)

marijke.habermann@gi.alaska.edu

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General statement

The manuscript "A simple inverse method for the distribution of basal sliding coefficients under ice sheets, applied to Antarctica" presents a new approach to reconstructing basal sliding distributions beneath ice sheets. Formal inverse methods that minimize the misfit between observed and modeled surface velocities have been used widely. The approach used here is to instead minimize the misfit between observed and modeled ice surface elevation. After every 5'000 years, the local basal sliding value is

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Interactive Discussion

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adjusted according to the local difference between the modeled and the observed ice surface elevation. This is repeated until an equilibrium is reached.

The scientific significance of the manuscript is excellent because it presents a substantial new method to infer basal sliding. The resulting basal sliding distribution leads to ice elevations that are an order of magnitude closer to observed present-day values compared to commonly used basal sliding distributions. The methods and results are stated clearly in a very well written text. The presentation quality is good with many well explained illustrations and examples. I highly recommend this manuscript for publication after addressing the changes described below.

Specific comments

The paper assumes that the bedrock data set is 'the truth'. One has to acknowledge that the DEM is based on large amounts of interpolation over vast areas of the ice sheet. This is now mentioned in terms of subgrid phenomena, but the issue is that for many grid points, there isn't an actual corresponding observation anywhere in the vicinity. This needs to be discussed, as it clearly influences the results. The errors due to changing bedrock elevations (currently discussed) are probably entirely trivial compared to that.

The presented method minimizes the difference between observed and modeled ice elevation, therefore it is not surprising that this error is small for the resulting basal sliding distribution. Showing maps of a small error in ice elevation does not show that your basal sliding solution is actually recovering the 'real' basal sliding distribution. Even if the fit was perfect, that in itself does not justify the method (it could be good for all the wrong reasons). A relevant question to address in this context is how good the fit should be, given errors in the data set (especially given the possibly large errors in the bedrock data), i.e. should the inverse procedure be a minimum or a fit to an expected error.

Synthetic tests could give a greater confidence in your method. Picking a ‘target’ basal sliding distribution, running the model to equilibrium, adding noise to the resulting ice elevations and using this as the ‘target’ ice elevation would provide a framework to test the method more thoroughly. One advantage of this type of synthetic test is that it isolates the issue of imperfect model physics from that of recovering C(x,y).

The presented method might be ‘easier’, but it is still computationally expensive, and possibly more expensive than adjoint-type inversions, because of the large amount of forward models necessary to reach equilibrium.

Structure: I think sect. 8 could be left out, or greatly reduced. All statements are very qualitative and the closer comparisons with Pine Island and Thwaites area are not very valuable. Appendix C could be put into the main article, maybe instead of the current sect. 8, it would also be interesting to see the velocity comparison for the 1st and 2nd (without topographic influence) inverse methods and possibly the simple two valued distributions. The log-log comparison of modeled and observed velocities makes these results look better than they really are, but there are also some good reasons (discussed in the Appendix) why discrepancies should not be a surprise. I recommend making this a more integral part of the paper. Appendices A and B seem important enough to be moved to the main article, maybe in a shortened format. Both of these are important to justify your assumptions. I would leave out Appendix D. Sec. 6 mostly contains text about the enhancement factor, this text could be shortened.

- p. 1407, line 23: You argue that neglecting longitudinal stresses has a lesser effect than basal sliding distribution, is there any evidence for this? Major advances in reproducing ice thickness of ice-sheets were based on the introduction of longitudinal stresses. Additionally, C(x,y) is only adjusted in areas of sliding and in these areas (at least when sliding is fast) longitudinal stresses are important.

- p. 1410, line 10: The achieved solution of basal sliding is consistent with the model and therefore doesn't change in subsequent forward runs. But this doesn't mean that the basal sliding solution is actually the real one. Maybe it would be good to point out that a model with consistent sliding distribution might work better for predictions of past and future than a model with 'real' sliding distributions that are not consistent with the used model.

- p. 1410, line 23: Please give a little more information about the non-inverse run; is the climate forcing constant?, how long is this run?, is it again run to equilibrium?

- p.1410, line 27: "..., when prescribed in subsequent normal runs, maintains the same optimal fit to modern ice elevations and thicknesses". If I understand your method correctly, the 'inverse' iterations are performed until equilibrium is reached and the full model is used in the 2nd inversion procedure, so it is not surprising at all that surface elevations in 'forward' runs with the prescribed C(x,y) do not change. If there is a difference between the model runs used in the inversion and the once used in the non-inversion runs please state it clearly (Freely varying grounding lines and ice sheets?). (Similar statement starting on p. 1417, line 18).

- p.1414: Please state your initial estimate of C(x,y)? Were different initial estimates of basal sliding tested out? How do different initial estimates influence the solution? It would be great to see some test results in this direction.

- It is often difficult to asses, just by visual inspection of the figures, how well results agree. Some type of metric, for example standard deviation or cross-correlation coefficient, would add to the value of the statements that are made.

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Technical corrections

- Please check for consistency in wording: 'integrated', 'normal integration', 'run forward', 'forward calculation', 'run forward in time', 'non-inverse runs'. important to make the distinction between forward model in the inverse sense and forward in time.
- p. 1407, line 10: How was this range of C(x,y) values for hard rock vs. deformable sediment determined?
- p. 1412, line 16/17: 'EAIS' and 'WAIS' define.
- p. 1413, line 14: This sentence makes it sound like both Eq. 3a and 3b are used in the paper, but earlier it is stated that only Eq. 3a is used in this paper. How much does the choice of eq. influence the solution?
- p. 1416, line 3: Typo: 'values' is written twice
- p. 1417, line 21: State that the figure shows the results of a non-inverse run.
- p. 1419, line 1: Which 'results' are you referring to? First sentence is a bit confusing, maybe reword.
- p. 1421, line 25: List which figures of Joughin et al. (2009) and Morlighem et al. (2010) you are comparing with.
- p. 1422, line 7: As mentioned by the other reviewer, the Larour et al. (2009) reference is not publicly available. Also Lingle et al. (2007) is merely an unpublished slideshow and not suited for comparisons.
- p. 1423, line 19: How would you combine the inverse method with statistical ensemble techniques?

- p. 1426, line 19: Leave out ‘that the method has already corrected for’

Figures:

- All figures: Leave out the word ‘errors’ in captions for $\triangle h$.
- Fig. 3: Add ‘(s.a.)’ to it’s definition in the caption.
- Fig. B1: a) maybe use the same scale as in previous figures
- Fig. C1: Caption for c) ‘with imposed minimum of 2 m a-1’: Can you clarify this?

Supplementary materials:

- Could you add a movie of the evolution of C(x,y) during this same model run? It would be very interesting to see how the features are adjusted throughout the inversion.

Interactive comment on The Cryosphere Discuss., 6, 1405, 2012.

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