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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

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1 General comments

The manuscript by D. Pollard and R. M. DeConto entitled *"A simple inverse method for the distribution of basal sliding coefficients under ice sheets, applied to Antarctica"*, presents a simple algorithm for inferring basal friction by matching surface elevation. Most inverse methods generally rely on fitting surface velocities and use an adjoint method to infer the basal conditions. Here, a simpler procedure is presented: an Antarctic flow model is run forward in time and every 5,000 years, the model surface elevation is compared to the present-day surface elevation and basal friction is tuned to decrease the misfit between the modeled and observed data. If the modeled surface elevation is too high, basal friction is decreased to enhance basal sliding, and if the modeled surface elevation is too low, basal friction is increased so that more ice accumulates and the ice naturally thickens. The description of the method is followed by an application to the Antarctic Ice Sheet and promising results are presented.

This approach is radically different from other inversion methods. One of the issues that most ice sheet models have today is that they are gaining mass after initialization with present-day datasets, which is not consistent with all the observations that are available (GRACE, InSAR, ICESat, etc.). As described by the authors, some models show an error in surface elevations of several hundreds of meters, which is not a realistic projection but due to the model initialization. Here the authors address this critical issue by proposing a method to fit the present-day surface elevation, assuming thermal and geometrical steady-state, by tuning basal friction. The method is well described, the text clear and rather well written and the examples well explained. I highly recommend this paper for publication with some changes that are described below.

2 Specific comments

I think this manuscript should be a little bit more concise. Three different methods that bring incremental improvements are introduced and tested. A sensitivity analysis to enhancement factors is also presented. I would suggest shortening the text and focusing more on the final method rather than presenting all three methods. I would remove the *First inversion* section as this first approach does not work well, which is not a surprise as the basal friction and the thermal model are not consistent. The sensitivity to E should be moved to the appendix, as it is not at the core of the method. These are suggestions, I only think that the idea presented in the paper is excellent but

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is buried in sensitivity experiments and incremental improvements. This would make the manuscript clearer.

The presentation of the paper should be polished in places. Landau notation for example, $O(n)$, is used extensively throughout the text and never defined when, I think, plain text would be clearer. In general, it is better not to use mathematical symbols in the text.

As mentioned in the text, this method cannot be applied to higher-order or full-Stokes ice sheet models because these models cannot run for 200,000 years (page 1409). I was wondering if one could use smaller time intervals than 5,000 years between changes in basal friction, because that is the component of the algorithm that makes the method slow. These time intervals are closely related to the amount of change that is applied to the basal friction. I would imagine that changing the basal friction slightly less at each iteration would allow the use of smaller time steps. This is reinforced by the impression I had watching ht video in the supplementary material: it seems like the friction is changed too drastically as the surface error oscillates between red and blue before stabilizing.

I found the comparison between Rignot's velocity map and the model rather good, contrary to the authors' negative judgment, especially as these surface velocities were not used to constrain the model. Furthermore, this comparison should not be in the Appendix, because velocity misfit is something that a lot of people look at to evaluate a model performance.

Here is a list of some other minor comments:

- page 1408 line 5: define O(100's m), and again, should be written completely here in the main text (same for all other O)
- page 1408 line 12: even though I agree that these errors are mainly due to erroneous friction coefficients, the surface mass balance can also have a strong

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impact (as mentioned in Appendix C page 1427 lines 20-22)

- page 1409 Larour et al. 2009, not peer reviewed and not available online anymore
- page 1412: There are a couple of details that are missing in the model description: (a) What is the vertical resolution (number of layers)? (b) What is the time stepping ? (c) Is the thermal model a cold ice model?
- page 1414 line 22: 10^{-9} say $\rightarrow$ e.g., 10^{-9}
- page 1421 line 14: Fig. 4 show*s*
- page 1422 line 3: the numerous transverse strips in Joughin et al. (2009) might be due to a lack of regularization in their inversion. This might only be a numerical artifact.
- page 1433: issues of canceling errors are inherent to all inverse methods, the proposed approach is not worse than adjoint-based inversions
- page 1426 line 8: 0 ka $\rightarrow$ initialization
- Fig. C1. (c), the unit of the color bar is not really standard, it would be better to have expressed the misfit in m a-1 and still use a logarithmic color bar (like Fig. C1. (b)). For example log(v_{model} - $v_{observed}$) in red if v_{model}>$v_{observed}$ and log($v_{observed}$ - v_{model}) in blue for $v_{model}<v_{observed}$

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