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Interactive comment on "Committed near-future retreat of Smith, Pope, and Kohler Glaciers inferred by transient model calibration" *by* D. N. Goldberg et al.

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The paper entitled "Committed near-future retreat of Smith, Pope, and Kohler Glaciers inferred by transient model calibration" by Daniel Goldberg, Patrick Heimbach, Ian Joughin and Ben Smith presents the results of a transient model calibration applied to the region of Pope, Smith and Kohler glaciers in West Antarctica, using the MITgcm and automatic differentiation. The authors show that parameter estimation through a transient model performs better in terms of matching the observed trends than the classical "snapshot" inversion widely used in the ice sheet modeling community. Using the transient calibrated model, they show that, over the next 30 years, this region of

the Antarctic ice sheet will keep losing mass at a steady rate, even when no melting is applied in the region where the grounding line retreats.

This is a very timely paper, very well written, concise and clear. Transient calibration is a field of active research for many research groups in glaciology and this team did a great job in putting these tools together and applying it to a real system successfully for the first time. I highly recommend this paper for publication, and I only have minor comments/suggestions.

1 General Comments

The comparison with the snapshot initialization is useful but a little bit biased in my opinion because it is based on an old (and probably bad) surface DEM (2002), with velocities that are 10 years younger. This exercise highlights the difficulty of using snapshot calibration because it is not always possible to find datasets that cover the same time period. But I would be curious to see the 30 year run with a $\sim$2010 snapshot calibration (with both surface DEM and velocity) rather than a mix of 2002–2010 data. It would not be surprising if this snapshot calibration, with better and more consistent datasets, yields results that are in a better agreement with the transient calibration. I understand that the authors want to compare the model output with existing data but it is not 100% clear whether the difference between the transient and snapshot calibration for the 30 year run is due to an inconsistency between datasets in the snapshot calibration or if it is just due to the difference between the two types of calibrations.

This is more a comment than a suggestion, but I was somewhat disappointed that the model does not include a floating ice shelf downstream of the 1996 grounding line, because this is most likely the region where important processes (such as melting at the ice/ocean interface) triggered the acceleration and thinning that this region is undergoing. Ignoring this region and using boundary stresses as a control felt like putting all these critical processes under the rug. I understand the author's rationale, but I would have loved to see the melt rates as a control and see if the transient calibrated model could tell us more about how the pattern of melting might have changed over the past decade (even with big error bars).

The other problem with having the stress at the grounding line as a control is that there is no unique solution (as mentioned by the authors p.4470), and it is even worse for the snapshot calibration. An increase in basal friction β^2 has the same effect on the cost function J_{snap} as an increase in normal stress σ. If, for some reason, the algorithm ends up with a σ that is too small, the model will artificially increase β^2 in this region, generating an increase in basal friction right next to the grounding line (see Fig. 4b). With such a high increase of basal friction near the grounding line, it is not surprising that the grounding line does not retreat. Again, I agree that the transient calibration probably does a better job, because it is constrained by more datasets, but including σ in the control space will make the snapshot calibration worse than if the ice shelf was included.

Finally, I found the paragraph about the Rignot et al. [2014] paper not very convincing (but I might be a bit biased). First, I totally agree with the authors that Rignot et al. [2014] is based on a qualitative assessment and actual modeling is required to test this hypothesis. I also agree that when the fjords are narrow, the walls of the valley can exert enough resistance to prevent grounding line retreat along retrograde slope. Now, I am pretty sure that if melt rates were applied at the grounding line and in its vicinity, which is not the case here, grounding line migration would have been more dramatic. This is a very conservative simulation and provides a lower bound to the contribution of this region to sea level and grounding line retreat, and we cannot rule out more dramatic scenarios.

2 Minor comments

I am not a big fan of the title for two reasons. First the term "near-future" is a bit vague, and "inferred" generally refers to the results of inverse modeling. This is just a suggestions and I will leave it to the authors to decide if they want to change the title but I would just take out the second part of the title: "Committed retreat of Smith, Pope, and Kohler Glaciers over the next 30 years".

- p.4460 l.1: keep present tense "is calibrated".
- p.4460 l.12: I don't really like the term "steady-state" because snapshot inversions do not assume steady state (i.e. they do not assume that time derivatives are 0).
- p.4461 l.6: As such, (comma missing)
- p.4461 l.15: "ice thickness" is not really a surface properties. How about surface height?
- p.4461 l.18: "stiffness" generally refers to elasticity, viscosity might be more appropriate
- p.4462 l.4: you might want to cite Seroussi et al. [2011]
- p.4462 l.19: integrated $\rightarrow$ run
- p.4463 eq.2: It is not really standard to add a factor of 2 for the constraints.
- p.44634 l.1: Minimizing J is not equivalent to minimizing J', because otherwise you see that by taking $L_i > 0$ and $\mu_i \rightarrow -\infty$, we would achieve $J' \rightarrow -\infty$. We actually want to find the *saddle point* of J'.

- p.4464 eq.3: since basal friction opposes motion, you probably want a minus sign
- p.4464 l.16: the Lagrange multipliers are not the gradient of the cost function J generally. But they can be used together with the state variables to compute the gradient of J pretty easily.
- p.4471 eq.6: I am not sure to understand the equation, I would have defined the ice height above floatation as follows:

$$h_{AF} = s - R + \min\left(\frac{\rho_w}{\rho_i}R, 0\right) \quad (1)$$

- p.4472 l.8: Thus, (missing comma)
- p.4472 l.10: I kind of disagree with this statement (if I understood correctly). When the grounding line retreats, the velocity increases over the entire domain instantaneously (e.g. Seroussi et al. [2014]). The only way to make sure that the inflow boundary does not affect the model is to go all the way to the divide, assuming that the position of the divide does not change. Now, given the time scale involved in this paper, I don't think the imposed flux affects the model significantly.
- p.4472 l.15: Finally
- p.4473 l.20: Thus, (missing comma)
- p.4474 eq.8: you probably forgot a factor $1/5$
- p.4475 l.22: "rigour" → rigor (most of the paper is in American English)
- p.4482 l.3: Eq. B2 (no parentheses)
- p.4482 l.14: Thus, (missing comma)

- p.4483 l.14: Thus, (missing comma)
- p.4483 l.26: parentheses missing for references.

3 References

Rignot, E., J. Mouginot, M. Morlighem, H. Seroussi, and B. Scheuchl, Widespread, rapid grounding line retreat of Pine Island, Thwaites, Smith and Kohler glaciers, West Antarctica from 1992 to 2011, Geophys. Res. Lett., 41(10), 3502–3509, doi: 10.1002/2014GL060140, 2014.

Seroussi, H., M. Morlighem, E. Rignot, E. Larour, D. Aubry, H. Ben Dhia, and S. S. Kristensen, Ice flux divergence anomalies on 79north Glacier, Greenland, Geophys. Res. Lett., 38(L09501), doi:10.1029/2011GL047338, 2011.

Seroussi, H., M. Morlighem, E. Rignot, J. Mouginot, E. Larour, M. P. Schodlok, and A. Khazendar, Sensitivity of the dynamics of Pine Island Glacier, West Antarctica, to climate forcing for the next 50 years, Cryosphere, 8(5), 1699–1710, doi:10.5194/ tc-8-1699-2014, 2014.

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