

Interactive comment on “Melt at grounding line controls observed and future retreat of Smith, Pope, and Kohler Glaciers” by David A. Lilien et al.

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Review of Lilien et al.

This paper presents models of recent thinning and grounding line retreat of the glaciers feeding the Dotson and Crosson ice shelves. The models are also used to address possible future scenarios for this system of glaciers. The paper is hugely relevant. While a lot of attention is currently focused on the neighboring Thwaites Glacier it can easily be forgotten that the Pope/Smith/Kohler glaciers have undergone some of the largest changes observed anywhere on the planet. In addition, it has the potential to affect ice evolution in the larger area, as thinning can spread inland rapidly and lead to divide migration, with consequences for this entire sector of the ice sheet.

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I do recommend publication in TC for this paper, but I also have a few general comments that I hope will be useful.

1) Generally, the paper could do a better job in outlining what works well in these models and what doesn't. This could be accomplished by a slight reorganisation of the Discussion and some expansion of the Conclusions. Otherwise, it is easy to read this paper and get sidetracked by model-data mismatches. This starts with Figure 1: Fig. 1b-d show velocity model-data mismatches that are quite large. It is easy for a reader to then be skeptical of any conclusion reached in the paper. I suggest that the paper first emphasises the conclusions that are most solidly supported and then discusses all the qualifications. For example, continued mass loss over the next century of order >6 mm sea level seem inescapable. Grounding line position is fiendishly difficult to get right and varies a lot between models. Etc.

2) What is the criterion for the choice of models for the prognostic simulation? It seems like you don't hold much faith in some of these. Could you more clearly outline, which range is most realistic, given the model performance over the period of observations.

3) I would love to see a bit more discussion on initialization. You generally do a good job outlining the challenges. Is there a way to assess how important the initial temperature distribution is? You make a steady state calculation here; if I read it correctly. For example, when you invert for flow rate factors over the ice shelf, how does that compare to the derived temperature distribution? Also, one measure of success for initialization is to look at thinning rates. How well do the models do with that?

4) How is calving at the ice shelf front handled?

5) Could you comment a bit more on the choice of multiplying observed melt rates rather than multiplying parameters as in prior studies (line 224-230). What are the benefits of this choice?

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6) Parts of the discussion on weakening (l.383-394) reads a bit odd in the sense that it sounds like you discuss weakening of margins as a possible cause. But what would cause weakening in the first place? Ice doesn't just get 5 deg warmer or more anisotropic; there would have to be some other driver. So weaker margins could lead to an amplification of an otherwise triggered change. Some rewording would clarify that.

7) Figures are a bit hard to read with small fonts, at least on a printed out copy. I would prefer a Figure 1 that is more of an overview. In particular, having results in there already (velocities) is actually distracting

8) This is a bit of a repeat of comment 1): What should the reader take away from figures such as Fig. 7? I can look at it and say that this model is terrible: on Pope the largest thinning is off by a factor of 2 in the best case. Similar things could be said for grounding line positions and velocities. But that is obviously not your main point. Help the reader a bit in what you consider the successes and challenges of this modeling effort. I think a bit of a restructure of Discussions would go a long ways here.

Small edits:

l.30: I would say 'peaked temporarily'. There is no reason that this would have to remain a one-time occurrence.

l.175: I think this is not quite correct. The effective pressure assumption here essentially implies infinite hydraulic conductivity (a flat water table). The implication is that any sort of pressure gradient that is required to drain subglacial water means that water pressure further away from the grounding line needs to be higher, which extends Coulomb like deformation inland. Therefore the model is likely to underestimate inland velocity response.

l.203: ... comparison BETWEEN modeled ...

l.277: ... compared TO the observed ...

I.324/25: Why those particular choices (see also comment 2) above)

I.445: to -> from

I.449: Where is the Haynes Glacier (maybe show in Fig. 1?)

I.463: There are A variety ...

I.509: error -> errors

I.516-519: Also, hydraulic gradients would lead to lower effective pressure inland, as per comment above.

I.526: .. as much AS the ...

I.526: How do you know that?

I.530: its -> it

I.531: ... we did NOT have ...

I.567: 'relatively modest' is in the eye of the beholder, you're describing some major changes here with global impacts from a single basin

Conclusions could be expanded a bit.

I.759: one of the 'thin for 2Obs' should be 'thick for 1Obs'

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