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Discussions

Interactive comment on "Neutral equilibrium and forcing feedbacks in marine ice sheet modelling" *by* Rupert Gladstone et al.

Anonymous Referee #2

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This paper aims to examine grounding-line behavior in advance and retreat scenarios, particularly examining cases in which the author's models show examples of what could be termed neutral stability (or multiple steady-state grounding-line configurations for the same forcing) of grounding line position, while also demonstrating that GL reversibility, in and of itself, is likely not a sufficient test for demonstrating that a model is sufficiently resolved.

The paper is well-written and clear, although would perhaps benefit from a statement of the goals of the experiments at the beginning. The approach taken is well-described (I think I could re-run these experiments on my own if I wanted to), and the figures are for the most part clear and well-documented (the figure illustrating stability is a useful one). It is a useful addition to the literature, and I support publication after a few fairly

minor points are addressed.

I think the biggest thing missing from this paper is much, if any, discussion of mesh resolution. It's not controversial to state that an insufficiently-resolved ice sheet model will exhibit artifacts in its grounding-line response (even alluded to that in the discussion). It would be very helpful to present some sort of mesh convergence result to demonstrate the regime being operating in for this paper. Resolution is mentioned at the beginning (implying operation in an under-resolved regime), but then don't do anything to place the experiments in context in this sense. Without that sort of discussion, it's tempting to label the results here as "odd things that happen when a grounding-line problem is under-resolved"), and attribute the multiple steady-states to hysteresis due to under-resolution. It would be very useful if you picked a few of the initial cases (say a= 0.2, 0.7, and 1.7) and show (a) the convergence of the GL and area at steady-state with mesh resolution, and then (b) how the experiments behave in fully- and under-resolved regimes. Otherwise, you essentially seem to be making the point that GL reversibility is not a sufficient test by itself to demonstrate that a model is sufficiently-resolved (which is an important point – that the only reliable way to assess whether one is sufficiently-resolved is via a convergence study along the lines of Cornford et al (2016) – but it's not a point that's being made explicitly in this paper).

The role of mass balance also isn't mentioned in the results – You appear to have chosen a test case in which the additional mass flux onto the grounded ice due to the increased surface area for an advanced grounding line is exactly balanced by the increased flux through the GL due to the increased ice thickness at the GL (hence the apparent multiple stability points). A useful test would be to try this experiment again with (for example) a different bed slope, which would in principle change that relationship.

1 Specific comments:

1. page 1, line 9, 11, 16, etc: The word "convergence" has a particular meaning in numerical modeling describing how a model behaves as the mesh spacing, timestep, etc are refined (or possibly the tendency of, say, a solver, to reduce its residual to a prescribed tolerance). In at least some places, you appear to use "convergence" when you likely mean "steady state". I'd suggest a careful check on all of the uses of the word "convergence" to ensure that it's being used consistently. Otherwise, there is a tendency for confusion when a single word has multiple meanings and connotations. I'd even suggest the use of "convergence with resolution", etc...

2. page 2, line 8: I'd suggest also citing Seroussi and Morlighem (2018) on discretizing melt forcing near grounding lines here.

3. page 2, line 9: I think the choice of flowline modeling and Weertman sliding law are unfortunate here – flowline because it's perhaps overly simplistic given the current understanding of the effect of buttressing and other effects that are not present in a flowline model; I would have suggested using either a MISMIP3D or MISMIP+ configuration as a testbed. Weertman is unfortunate because as the authors point out, it produces much more of a forcing discontinuity at the grounding line, which is likely amplifying the effects described in this work; something like the Tsai Coulomb-limited sliding law would have been a useful counterpoint. That said, none of these specifically discount the conclusions drawn in this paper, but instead leave important questions unexamined.

4. page 2: (problem description) – how long is your domain in the x-direction?

5. Figure 1:

   (a) Is there really no vertical shear in the velocity field? That's surprising, but is the impression I get from the vertically-constant coloring of the velocity field.
   (b) The use of the intensity-based colormap doesn't work well with the two-profile plot as designed, since the semi-transparent colors can't be distinguished from different speeds for the second profile. I'd suggest switching to a colormap which isn't intensity-based if you want to present the second profile as a lighter-shaded version of the primary colormap. Another option would be to simply show only one representative velocity magnitude plot, but overlay the outlines (in black) of multiple profiles, which would also allow for more than one alternative profile.

6. Figure 2:
   (a) Did the 0.2 m/a run ever actually achieve steady-state? It's not obvious from the area plot.
   (b) It appears that all of the cases for which the advance-phase a is greater than 1.6 m/a all collapse onto the 1.6 profile. Is that the case? I'd say this is really odd behavior; do you have any idea why? Are the GL's all getting stuck on the same cell boundary? It seems like the behavior is very different above and below that threshold.

7. Section 3 (discussion of multiple steady states) – it might make sense to move this section to before what is now section 2.2; if you did that, the initial experiment (section 2.1) would be followed by the discussion of the initial experiment, and then the follow-on perturbation experiments would have a context when they're introduced. It would also lessen the number of times a reader has to page back and forth between experiment description, results, and discussion.

8. page 5, line 10: A useful test for steady-state would be to compute the time derivative of the area and plot that.

9. page 5, line 10: "grouding"-> "grounding"
10. page 7, line 2: suggest changing "a ball on a hill" to "a ball perched on the summit of a hill"
11. page 7, line 2: "after begin" -> "after being"?
12. page 7, line 6: suggest changing "A large perturbation" to "A large-enough perturbation"
13. page 8, line 25: This suggests something isn't quite right, since one would expect the friction to vary smoothly throughout advance and retreat if the subgrid-scale friction discretization is done correctly. If that's the case, then one wouldn't expect to see mesh-related artifacts of advance and retreat in the friction field, would you?
14. Figure 3: Why isn't experiment P2 shown in 3(b) and 3(c)?
15. Figure 5: What happens if you allow the system to reach steady-state after the perturbation is applied (rather than discontinuing the forcing after 1000 years? Does the GL finally advance, in that case?

2 References:

1. Cornford, S., Martin, D., Lee, V., Payne, A., Ng, E. (2016). "Adaptive mesh refinement versus subgrid friction interpolation in simulations of Antarctic ice dynamics". *Annals of Glaciology*, **57**(73), 1-9. doi:10.1017/aog.2016.13
2. Seroussi, H. and Morlighem, M., (2018), "Representation of basal melting at the grounding line in ice flow models", *The Cryosphere Discussions*, **2018**, 1–16, doi:10.5194/tc-2018-117

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