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The Cryosphere Discussions

Interactive comment on "Grounding line transient response in marine ice sheet models" *by* A. S. Drouet et al.

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This paper presents a sensitivity study of a basic flowline marine-ice perturbation, using 4 models ranging from full Stokes to hybrid/SSA. The results offer valuable cautionary messages for the application of hybrid/heuristic models such as SSA-H-FG to perturbations on the scales addressed here (200 yr, 100 km). The paper is clear and very succinct, and suitable for publication with only minor changes in my opinion. My main suggestion is to add more perspective discussing the relevance to different scales of application, that would refine the main conclusions on the suitability of the various models to different scenarios, as follows.

In assessing model differences, a pertinent question is: What accuracy is needed for what types of problems? As mentioned (pg. 3917, line 17), the scales in the paper

are relevant, for instance, to whether the Pine Island/Thwaites Glaciers will retreat by 10, 20, 100 km or more in the next 10, 20 or 200 years. Robust predictions for that kind of problem demand accuracy on the order of ~10's km and 10's years. That will require high resolution (much finer than 10 km almost by definition), and perhaps full Stokes/higher-order physics; model discrepancies on the order of those in the paper would be significant.

In contrast, for large space-scale and long time-scale applications (~10,000 years, 2000 km), such as the last deglacial retreat across the Ross and Weddell embayments, the only models that are currently feasible in two horizontal dimensions are SSA/hybrid with coarse ~10 km resolution. For those applications, if model-model differences and errors remain on the order of a few 10's km and 10's years as found here, that would be very acceptable, i.e., much smaller than the overall scales of interest.

Specific points:

1. It is currently unknown whether model-model differences like those in the paper, or larger, would be found in large-scale applications. Perhaps the authors' modeling framework could be adapted to run such a 1-D flowline experiment, for all 4 models; for instance, LGM to modern retreat (~15 kyr) on a ~3000 km transect running between 2 locations in the outer Weddell and Ross Seas through central WAIS, with realistic bed topography. If the FS-AG model could be run feasibly, that would be a valuable test for the accuracy of the other models on large scales. (nb: That could be the subject of a future paper, and is not suggested for this one.)

2. The rationale for using 10-km resolution here in the SSA-H-FG model, much coarser than the other models, is given on pg. 3917, line 36 to pg. 3918, line 4, where it is noted that at higher resolution the results better match the other models. Perhaps this rationale could be rephrased and combined with new discussion suggested in the main points above.

3. In any case, 10-km resolution only leaves a few grid points to resolve the relatively

small length scales here, as apparent in the slope of basal ice within $\sim$20 km of the grounding line (inset, Fig. 1). Maybe the SSA-H-FG model discrepancies are due mainly to this coarseness, and not to intrinsic physics and parameterizations. This may become clearer with the new figure and inset suggested below (point #5, regarding VAF). Additionally, would it be feasible to run the FS-AG model at 10-km resolution, to see if this causes similar departures from its nominal results?

4. It seems strange in principle that Volume Above Flotation (VAF, Fig. 6) is the one result with significant differences between models, while in contrast, grounding-line positions and surface geometry are consistent (as mentioned on pg. 3920, line 25-26). VAF is basically determined by ice geometry, grounding line position and sea level, so why is VAF alone different? Perhaps just rephrase that sentence to explain, or to tone down the implied contrast.

5. Showing relative VAF changes (dividing by one model's result, Fig. 6) seems to be an unnecessary step, and may obscure some information. Why not show absolute VAF's versus time for each model, as in the other figures? I suggest: a. In Fig. 6, show absolute VAF versus time, not relative VAF. b. For the Cf=1 case for instance, add a figure like Fig. 1, showing the ice profiles in the final year-200 state, with insets as in Fig. 1 for basal profiles near the grounding line. See also point #3.

Technical points:

- The labels on most figures are too small to be read easily without zooming the display, and should be enlarged.

- Fig. 6 seems to be missing in the printer-friendly version.

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