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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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Here are our responses we propose according to your remarks and suggestions. Corresponding corrections will be in blue in the paper such that you can easily track changes.

1 Main remarks

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).

This is indeed an interesting point but as acknowledged by the reviewer it remains beyond the scope of the present paper. Our work aims at investigating the discrepancies between models for short term projections (two centuries). Consequently, our simulations aim at reproducing changes of GL positions plausible with this duration, i.e. of the order of tens of kilometers.

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.

As previously mentioned, we seek to investigate the discrepancies of the transient behaviour of various models for a couple of hundred years. The essential aim is to investigate the impact of various numerical schemes in terms of sea level contribution from ice sheets. Regarding the SSA-H-FG model, we deliberately choose a low resolution (with respect to the other models). This choice has been dictated by one essential reason: such models that implement the boundary layer theory, are used with corresponding resolution (approx. 10 km) to infer the coming contribution of Antarctica to sea-level rise. This is for example the case for the PennState-3D model used in the seaRISE initiative (Bindschadler et al, 2012). For the numerical approaches of SSA-FG and FS-AG, they are known to fail to compute consistent steady geometries whether a too large resolution is used (Durand et al, 2009). This would therefore have no sense to investigate their transient behaviors with too coarse a mesh. To our opinion, this justifies our choice of different mesh sizes from one model to the other according to the (i) capacity of each model to correctly compute steady states at a given resolution, (ii) the mesh size currently used to predict ice sheet short term response. The justification has been improved accordingly in the text (p.11).

Furthermore, we weren't clear by saying that "at higher resolution the results better match the other models". We would like to emphasize the fact that at higher resolution, the SSA-H-FG model would lead to a much smoother variation of GL position (less numerical artefact) implying more realistic behaviour. This part of sentence has been removed.

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 approx. 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?

This is a very pertinent point! Simulations with much finer resolutions (1.25, 2.5 and 5 km) have been done using the SSA-H-FG model to check whether the resolution or the physics implemented is responsible of the disagreement with other models. As can be seen on the following figure, despite a fine resolution the transient response in terms of ice discharge is extremely similar for coarse (10 km) and fine (1.25 km) meshes. So, this is the implementation of the boundary layer theory that is responsible of the too

fast response. This is now discussed in the paper (p.11).

Regarding the proposition of the reviewer to run the FS-AG model with a coarser resolution, we know that a too coarse mesh would prevent the grounding line from retreating (Durand et al., 2009b). As previously mentioned, resolutions have been chosen at the condition that the considered model gives sensible results for steady geometries. That would not be the case for FS-AG with a 10 km resolution.

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.

This is a good point, similarly pointed out by S. Price, and the corresponding discussion was particularly misleading. One can see on Fig.2 that the retreat of the grounding line for SSA-H-FG is significantly faster compared to the other models (this is particularly clear for $C_F = 1$ and at the very beginning of the simulation). It can also be seen on Fig. 3, despite the high-frequency numerical noise, that the rate of surface thinning is higher for SSA-H-FG compared to other models (this is particularly clear for high resolution simulation where dx=500m, which is note shown here). This clearly explains the larger VAF release observed for that model. Discussion has been modified accordingly (p.12 and p.13).

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.

Rather than presenting the VAF evolution as suggested, we finally choose to add the evolution of absolute change in VAF, $\Delta VAF(t) = VAF(t) - VAF(0)$, for each model (see Fig. attached to this file). Indeed, $\Delta VAF(t)$ clearly demonstrates the discrepancies between models during the transient in terms of sea-level contribution. This further has the advantage of removing the information on the initial volume which is different for every model and would unnecessarily complicate the figure and make the discussion more confused (p.11). Futhermore, we decided to keep the panels presenting the relative difference of ΔVAF with respect to ΔVAF_{FS-AG} for two essential reasons: this enables to magnify the difference in between models and further shows that this relative difference appears to be pretty similar irrespective of the given perturbation. We believe it is worth mentioning this point.

Finally, we believe that adding final geometries would make the discussion more complicated for the reader. Indeed, one can see on Fig.2 that SSA-H-FG and FS-AG have relatively close grounding line positions at the end of the simulations despite SSA-H-FG undergoes a much larger loss in volume (because SSA-H-FG initial geometry was more extended). So the final geometries do not directly reflect the change in VAF. Therefore, as the discussion of the figure is focused on the changes in volume we believe that adding the last surface profiles could be confusing for the reader.

2 Technical points

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

We have modified our figures so that axes numbers and labels could be easily read with no need to zoom the display.

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

This figure should normally appear in the new version.

References:

Bindschadler R., S. Nowicki, A. Abe-Yuchi, A. Aschwnaden, H. Choi, J. Fastook, G. Granzow, R. Greve, G. Gutowski, U. Herzfeld, C. Jackson, J. Johnson, C. Khroulev, A. Levermann, W. H. Lipscomb, M. A. Martin, M. Morlighem, B. R. Parizek, D. Pollard, S. Price, D. Ren, F. Saito, H. Seddik, H. Seroussi, K. Takahashi, R. Walker, W.L. Wang, submitted to J. Glaciol., 2012.

Durand, G., Gagliardini, O., Zwinger, T., Le Meur, E., and Hindmarsh, R.C.A.: Full-Stokes modeling of marine ice sheets: influence of the grid size, Ann. Glaciol., 50, 2009b.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C2532/2012/tcd-6-C2532-2012-supplement.zip

Interactive comment on The Cryosphere Discuss., 6, 3903, 2012.

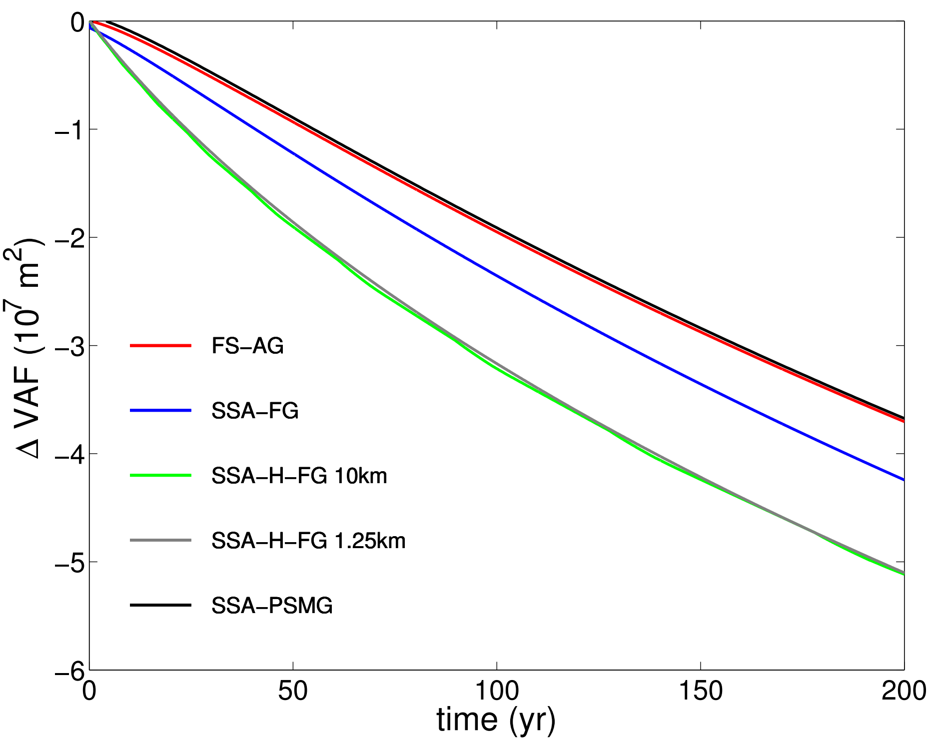

Fig. 1. Change in VAF, ΔVAF as a function of time for the four models used in our study and for SSA-H-FG with a resolution of 1.25 km.