The Cryosphere Discuss., 6, C2534–C2545, 2012
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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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Received and published: 19 December 2012

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

1 Main remarks

1. 3904, lines 11-14: The statement that "large discrepancies...are observed in terms of ice sheet contribution to sea level" is hard to rectify with the previous statement that "their overall response ... is found to be consistent in terms of g.l. position, rate of

surface elevation change, and surface velocity". If all of those things are similar then it is difficult to understand how the sea level contribution between the models can be significantly different.

This part has been partly rephrased (in the abstract p.1 and in the text p.13), according to similar remark made by D. Pollard (point 4).

2. 3907, line 6-22: Suggest checking the terminology for "plane flow". Is this what is more commonly called "plane strain" or "plane stress"? Perhaps it would also help to clarify in this section that these are all "flowline" models.

Yes, we replace plane-flow by flowline models (p.3, p.5).

3. 3910, line 11: Is it really the basal shear stress or is the magnitude of the basal traction? The latter contains normal and horizontal shear stress components in addition to the vertical shear stress. For a higher-order / Stokes model this will generally not be equal to the shear stress (assuming that by "shear stress" you mean the "vertical shear stress" at/near the bed).

By basal shear stress we mean the tangential component of the traction stress applied along the normal to the surface. We rather use this term in the text (p.6).

4. 3911, line 2-4: For the SSA model, I don't think that it is true that the lateral (marine) margin b.c. is already included in equation 11 (this seemingly conflicts with what is written in section 2.5).

Yes, we should mention that this is only the basal boundary condition that is implicitly included in the equations (11). The lateral boundary condition at the front is explained in section 2.5. This has been clarified p.6.

5. 3916, line 21-24: Isn't the smooth retreat behavior of SSA-FG mostly attributable to the small grid spacing? The Pattyn interpolation helps a bit, but you really need very fine resolution to begin with (i.e. the interpolation might save you a factor of 2 or so in resolution).

The smooth retreat behavior of SSA-FG is attributable to the small grid spacing, but also to the interpolation in a consequent way. For example, the pattern for the FS-AG model, which has a small grid size of 50m, presents large oscillations in the rate of grounding line migration. This is the direct consequence of the discrete implementation of the grounding line migration.

6. 3919, line 9-11: I don't necessarily agree that the ΔVAF between models are extremely similar. They seem fairly different to me, especially within that first couple of decades and especially for the model that specifies the g.l. flux based on Schoof.

We did not expect to write that "ΔVAF between models are extremely similar", but we rather wanted to point out that the relative differences of ΔVAF of each of the three SSA models with respect to ΔVAF of FS-AG are similar for all perturbations. The sentence has been slightly rephrased to avoid confusion (see p.12).

7. 3920, line 1-9: I think this section could be explained a bit more clearly, e.g. in terms of where the numbers come from. Is the range given for a single model or all models? Is the additional SLR given on top of the 4.6 mm already quoted?

The range given here is the range obtained considering the model FS-AG as a 'reference', i.e. representing the rise of 4.6mm, and the largest difference (+300% for the SSA-H-FG model and -30% for the SSA-PSMG model). This leads to the range of sea level rise of 3 mm (4.6-0.3*4.6) - 18 mm (4.6+3*4.6). This has been clarified in the text (see p.12).

8. 3920, line 25-27: "consistent results" and "major divergence" seem somewhat mutually exclusive here. This should be clarified.

We agree. This has been clarified (see remark 4 of previous reviewer).

9. Discussion and Conclusions: The finding that the boundary layer theory may significantly overestimate the flux at the g.l. (and thus the thinning and retreat rates at the g.l.) during the transient phase of evolution seems more significant than is stated here. The emphasis here seems to be on this difference applying only over short time scales, with some suggestion that this may be ok in the end since all models eventually

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apÂ■proach the boundary layer theory. This seems like a serious understatement for two reasons. First, for many prognostic simulations targeting estimates of future sea-level rise, the timescale of interest in the experiments conducted here is similar to that a policymaker might be concerned with (âĹij100-200 yrs). Over that timescale, there is a large difference in the volume above floatation (which is directly relevant to sea-level rise) between the model using the boundary layer theory and the other models. Second, these experiments only apply to the response to a single perturbation. In reality, an ice sheet might undergo multiple, repeated perturbations over time. For example, one could argue based on observations that a perturbation every decade for the entire 200 yrs was plausible. In this case, one might expect a repeating (and additive?) series of curves like those shown in Figure 6. The overall effect would be that relatively more of the time series would be dominated by the portion of the curve that is closer to year 0 in Figure 6, and for which the difference between the model employing the boundary layer theory and the other models is much more significant. These differences would presumably be even more significant for simulations over much larger timescales (e.g. Pollard and DeConto, Nature, 458, 2009).

We partly agree with the reviewer. Indeed, short term simulations of ice discharge have to be taken with caution, particularly when using models with flux condition at the grounding line. We believe this is clearly stated in the discussion and conclusion (e.g. “This intercomparison strongly suggests that models prescribing flux at the GL according to the boundary layer theory most probably overestimate ice discharge.” or “models that prescribe the flux at the GL should be used with particular caution when dealing with small spatial and temporal scales.”).

The cumulative effect of successive perturbations is speculative and would remain to be verified. Furthermore, on the time scale of the study by Pollard and DeConto (glacial-interglacial), we may presume that the model “switches” from a large, close to steady state, geometry (glacial conditions) to a smaller one (interglacials). Because, closer to steady conditions, the description of these steady states with various models will presumably be in a fair agreement whatever the approach used. This is during the transient phases between these states that, most probably, results would differ a lot from one modeling approach to the other. However, this remains highly speculative and would deserve a specific study. As previously mentioned, this is largely out of the scope of the present work.

We eventually reworked the conclusions according to these last ideas.

2 TABLES / FIGURES

1. Table1: ULB, BAS, etc ?

This has been done in the legend of the Table 1.

2. The axes labels on many of the figures are too small to read easily in the print version of the paper.

Figures have been modified such that axis labels could be easily read.

3. Figure 1: Suggest adding a legend to link a particular colored line with a particular model (same for other lined figures). In the text, it might be worth commenting on why the SSA-H-FG model has such a different shape at the g.l. than the other models. Is this also a result of the boundary layer theory approx. used to specify the flux at the g.l. ?

We added a color legend in the lined figures and commented on the SSA-H-FG model in the legend of the figure 1. The shape of the SSA-H-FG model is significantly different from the other models because it has a much coarser resolution (10km).

4. Figure3: Note that the color ranges are different on the different rows of figures (same applies to Figure4).

This has been done on purpose, because the perturbation with $C_F = 5$ implies elevation changes values that are far lower than those of the two other perturbations. So in order to clearly see the temporal and spatial pattern we keep a different scale for $C_F = 0.5$.

5. As noted by the other reviewer Figure 6 doesn't appear in the printer version of the .pdf.

This figure should normally appear in the new version.

3 Technical comments

1. 3904, line 16: "... our results question THE CAPACITY OF THESE SAME MODELS to compute..."

This has been changed (p.1).

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2. 3904, line22: Does buttressing first need to be defined?

We added a sentence to more precisely define the buttressing (p.2).

3. 3905, line 17: Give the dimensions of the boundary layer in terms of ice thickness rather than km?

As far as we know, there are no references that enable to express the length of the transition zone as a function of ice thickness at the GL, excepting the one of Chugunov and Wilchinski, 1996, but which refers to cases of frozen bed, conditions that are not fulfilled in our study. We thus keep the dimensions given in km as in Hindmarsh, 1996 and Schoof, 2007 (p.2).

4. 3905, line 27: Provide a reference for the MISMIP experiments.

Done.

5. 3906, line 17: Start a new paragraph here, e.g. “Unlike in the original MISMIP experiments, here we choose to investigate...”

Done (p.3).

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6. 3906, line 28-29: “Ice sheet modeling have previously only been achievable with....”. This statement is not really true, as a number of fully 3d and higher-order models have been used for large-scale, prognostic ice sheet modeling. Perhaps I’m not clear on what the authors mean here. At any rate, some clarification of this statement would be appreciated.

Yes, we agree that this sentence is a bit too strong. We tone down by replacing “only” by “mainly” (p.3)

7. 3907, line 7: For the Elmer model, provide some previous references for publications describing the model (which is more relevant than where the model is developed).

Done (p.3).

8. 3909, line 2: “...the mass flux (i.e., surface mass balance) at the surface...”

Done (p.5).

9. 3910, line 6: “non-penetration” should be “no penetration”

Done (p.5).

10. 3912, line 10: Why is it ""emi-analytical" rather than just "analytical"?

The variable θ, as explained p.7 , in Eq.(17) depends on τ_{xx} and h_g which will be given by outputs of the SSA models, justifying why it's semi-analytical rather than just analytical.

11. 3915, line 4: "We consider and ice sheet ......on a downward sloping bedrock". Reference Figure 1 here?

Done (p.9).

12. 3915, line 17: Use "e.g. Prtichard et al., 2012" or add additional and/or more fundamental references? Currently reads if this statement is solely attribuitable to the work of Pritchard et al. (which is not the case).

Done (p.9).

13. 3916, line 7: change "higher" to "larger".

Done (p.10).

14. 3917, line 3: Elaborate on or define "dithering".

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Done (p.10).

15. 3919, line 19-22: Aren't grounding line migration rates given in Figure2b?

Indeed, they are. We delete this sentence.

16. 3920, line 17: Suggest using “dynamics” or “momentum balance” rather than “physics” because for some modeling folks “physics” means something very different than the momentum balance).

Done (p.13).

References:

Hindmarsh, R.C.A.: The role of membrane-like stresses in determining the stability and sensitivity of the Antarctic ice-sheets: back-pressure and grounding line motion, Phil. Trans. R. Soc. A, 364, 1733–1767, doi:10.1098/rsta.2006.1797, 2006.

Schoof, C: Ice sheet Grounding line dynamics: steady state, stability and hysteresis,J. Geophys. Res., 112, doi:10.129/2006JF000664, 2007.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C2534/2012/tcd-6-C2534-2012-supplement.zip

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