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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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GENERAL COMMENTS

This paper summarizes a model intercomparison study focused on identifying similarities and differences in prognostic model behavior for a number of different treatments used for accurately modeling grounding line dynamics. The models are all flowline based and treat the problem of accurate grounding line dynamics with different technical approaches (e.g. enhanced resolution at the grounding line) and/or using different approximations to the governing Stokes equations.

Adequate background material to the grounding line problem is first given; both from an observational perspective (e.g. why do we care about grounding lines) and also from a modeling perspective (e.g. why are grounding lines difficult to model accurately). A clear (and adequate) description of the Stokes model and the Stokes approximations used in this study is also given. This is followed by a discussion of grounding line treatment, which provides a succinct summary of the approaches currently employed by numerical models (and used in this study). A section describing physically motivated perturbations applied at model lateral boundaries provides a clear description for how these perturbations are implemented consistently within the various models. Care is taken to ensure that the chosen values for the forcing parameter results in dynamical changes in the model (e.g. rates of elevation change, changes in velocity, and rates of grounding line migration) that are approximately in line with observations. The experiments and the experimental setup are discussed and are well motivated by previous work.

The results of the paper are significant in pointing out the degree to which the differing approaches result in similar and different transient grounding line behaviors. Perhaps the most significant finding (in my opinion) is that the approach of specifying the g.l. flux directly based on the steady-state boundary layer theory of Schoof may significantly overestimate the flux at the g.l. (and thus the thinning rate at the g.l. and the retreat rate) during the transient phase of evolution.

In general, I found this paper to be well written, easy to read, and easy to follow. Most of my comments below are minor and aim to clarify a few areas of the paper I found to be ambiguous or slightly confusing.

SPECIFIC COMMENTS

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.

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.

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).

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).

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).

3919, line 9-11: I don't necessarily agree that the delta_VAF 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.

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?

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

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

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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 approach 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 (~100-200 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).

TABLES / FIGURES

Table 1: Define ULB, BAS, etc?

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

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.?

Figure 3. Note that the colorbar ranges are different on the different rows of figures (same applies to Figure 4).

As noted by the other reviewer, Figure 6 does not appear in the printer friendly version of the .pdf.

TECHNICAL CORRECTIONS

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

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

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

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

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

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.

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).

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

3910, line 6: "non-penetration" should be "no penetration"

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

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

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

3916, line 7: change “higher” to “larger”

3917, line 3: Elaborate on or define “dithering”.

3919, line 19-22: Aren’t grounding line migration rates given in Figure 2b?

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).

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