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The Cryosphere
Discussions

Interactive comment on "Parameterising the grounding line in ice sheet models" *by* R. M. Gladstone et al.

R. M. Gladstone et al.

r.gladstone@bristol.ac.uk

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Response to comments from reviewer Frank Pattyn.

The reviewers recommendations are for minor corrections. These are addressed one at a time below. Frank's comments in normal font, responses in italics.

Page 1079: The determination of steady state is obtained by a visual inspection of the evolution plots. By definition, a steady state is a situation in which the ice sheet geometry does not change anymore with time. Fixing the run time to 35 and 80kyr, respectively is not equivalent to a steady state, which is confirmed by my *visual inspection* of the experiments displayed in Figure 2 for instance: the 35kyr experiments are clearly *NOT* in steady state. Comparing these results with analytical steady state

solutions is therefore strongly biased, since the difference is not only resolution dependent or influenced by numerics, but also by the time-dependency. If the authors do not want to run the experiments again to reach a properly defined steady state (say, by stopping the calculation when j@H=@tj < _), they should reformulate the steady-state issue. It should then be stated that experiments were run for 35kyr which is close to but not necessarily a steady state. Furthermore, a mention should be made that the differences with analytical (Schoof) results are also due to the *almost* steady-state condition.

This is a good point. We have made some alterations to the text as suggested. Specifically we refer to 'final grounding line position' rather than 'steady state grounding line position'. We feel that rerunning the simulations would slightly strengthen the main arguments in the paper but not change any of the results/conclusions. We have not yet had time to re-run the simulations though we agree with Frank that this would have been the ideal option. If the editor feels strongly that the simulations should be re-run we are ready to consider this, though it would mean significant delays in completing the revised paper. HPC resource is not a problem, but man hours are limited.

Page 1081: As shown by Durand et al. (JGR), advance and retreat paths may differ quite a lot using a full stokes ice sheet model. Not only is there a significant difference with the results from boundary layer theory (Schoof), the divergence is also a function of model resolution. Although Durand et al. applied the MISMIP 3 experiment (with upsloping and downsloping beds), these issues remain valid for the stable downsloping case as well. The return solution generally lies closer to the analytical solution than the advance solution, although the differences decrease with higher resolution. Taking the mean of advance/retreat is not appropriate, as the *correct* solution is not necessarily the mean, but lies closer to the retreat solution for low resolutions. The reason for this is something that will be explored further in the MISMIP intercomparison. A metric for advance and retreat separately is probably more appropriate. The comparison with the analytical solution is appropriate if it is mentioned that the differences may also result from the non-steady state condition.

Frank's comment 'The return solution generally lies closer to the analytical solution than the advance solution' presumably refers to the Durand Stokes model as it is not true in general. In particular the opposite is true for most of the simulations presented here. So saying that the correct solution 'lies closer to the retreat solution for low resolutions' is not true for most of our simulations.

Explanation of the motivation for definition of ACC. We want to get a prediction from the model. We want the best attempt to predict a steady state grounding line position from the model. We know we can achieve a range of predictions from the advance to the retreat grounding line positions. We know there should be only one valid grounding line position for our experimental set up. We don't have an argument to say that either the advance or retreat simulation is in general better. Therefore, in the absence of further information, we choose the mid-point of the range of values indicated by the model as being viable, that mid-point being the mean of the advance and retreat grounding line positions. At the end of the day this decision is not 'correct' in the sense of giving the exact grounding line position, but then neither the advance nor retreat simulations considered separately give the exact grounding line position either. We still feel the ACC is appropriate for the reasons given above. As ever, I am more than happy to argue the point further with Frank should he feel sufficiently motivated.

Having said all this, we would like to point out that the advance and retreat simulations are in any case considered separately in Figure 8.

We have modified the wording of the ACC definition to emphasize the point that there is no 'correct' way to define model accuracy here.

Page 1088: The analytical solution from boundary layer theory does however only evaluate steady state positions of grounding lines; not the transient state. This should be mentioned.

My understanding is that the calculation of flux across the grounding line is based upon assumptions that may be valid outside steady state, but I confess I have not worked through all the equations in the first Schoof 2007 paper. It seems that the assumptions are usually but not necessarily valid for steady state and are very unlikely to be valid far from steady state (one of the assumptions relates to thickness change as one moves inland from the grounding line). I've made a small modification to the text.

Minor remarks

The minor remarks have all been addressed as suggested except where noted below.

Page 1078, Line 6-7: I agree that the true velocity profile in the vicinity of the grounding line is not linear, but this applies to all other interpolated parameters as well, such as ice thickness, and gravitational driving stress. The statement should therefore be removed. One can however be more precise by stating that an improvement to the basal friction interpolation can be obtained through an interpolation of the ice flux / ice velocity.

I don't understand this argument, perhaps you have mis understood what we are trying to say in this paragraph. We are talking about basal drag, and basal drag in the model is a function of velocity. It is not a function of flux or thickness. That is why we make the point that velocity is not linear without mentioning thickness or flux at this point. Here we are just providing a motivation for wanting to improve on B1. The role of thickness and flux is introduced in the next paragraph. No change implemented.

Page 1087, Line 4: what is precisely meant by very high resolution? 2km, <1km, 0.1km?

See our response to the same question from the other reviewer, Steve Price. The required resolution is a function of many things, most importantly basal drag.

Interactive comment on The Cryosphere Discuss., 4, 1063, 2010.