The Cryosphere
Discussions

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

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Response to comments from reviewer Steve Price.

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

The discussion of the dimensionless coordinate, lambda, on p.1069, lines _15-20 is a bit confusing. Specifically, is the "edge" of the domain the upstream edge or the downstream edge (I assume the former)?

Upstream aka landward, text clarified.

For the grid point x_i, where is the "distance" measured from : : : also from the up-

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stream edge of the domain? I assume the idea is that, at the g.l., lamba is equal to zero? If so, then it is confusing that lamba is defined (line 14) as a real number existing on the interval from 0 to 1. Is lambda only defined for the two grid cells that contain the grounding line?

Lambda ranges from zero at the last grounded grid point to 1 at the first floating grid point. In general it is not zero at the grounding line. It is only defined for the grid cell containing the grounding line, i.e. for the region between the last grounded and first floating gridpoint. Text clarified.

In the discussion on how the parameterizations are constructed (section 3), it would be nice to have some idea for how often the linear extrapolation, cubic interpolation, and harmonic mean procedures "break down", forcing reversion to linear interpolation. For the experiments conducted here, is it not, fairly, or very common that the procedures revert to linear interp?

With the linear extrap this happens frequently to the extent that the end result is basically the same as the linear interpolation GLP. In the other GLPs it is very rare. We've added a comment to this effect near the beginning of the results section.

Lines 5-10 in the "basal drag" section (3.2.2) – At least for the 2d case discussed here it seems like one could calculate the g.l. flux from the Schoof analytical solution, in which case one could use the thickness profile and the Schoof g.l. flux to (at least partially) reconstruct the "correct" velocity profile across the cell containing the g.l. This better constrained velocity profile could then be used w/ equation 29 when calculating BĔĘ2 at the midpoint (a half-baked idea that might be worth some more thought at some point).

The Schoof calculation makes assumptions about thickness profiles that are reasonable approaching steady state but less so during our spin up process. But this is a larger question than can be addressed here. We are looking into the Schoof parameterisation separately from the current study.

p.1083 ("time evolution" section) lines 15-18 – Can you confirm here whether or not the thickness change (local to the g.l. that is) also occurs in steps, or is it smooth and continuous in time (whereas the g.l. motion is not).

I have not looked at the evolution of thickness at the grounding line. I do not expect it to be smooth. It may even oscillate. It must be increasing while the g.l. position is stagnating, but whether it increase more slowly or possibly even drops slightly as the g.l. makes its rapid advances I don't know. This is not a standard output for the simulations that were carried out. If you think it is important to include in this paper then let me know and I can re-run a few simulations and plot out thickness evolution.

Lines 2-28 -The whole issue of whether or not these (or other) parameterizations can be improved any further seems to lie w/ understanding this "jumping" behavior. I'm still not sure I understand it. Is there a specific (numerical?) reason why the g.l. should get "stuck" near a grid cell? If the authors have ideas, perhaps they could speculate on them here? Perhaps they are speculating on them and I'm not entirely getting it : : : but it seems like a better understanding of this behavior is the key to this whole problem.

We could speculate on this but prefer not to at the moment until we are in a position to provide a more firm grounding to our speculations. The reviewer (Steve) has indicated he is happy with this position (personal communication, can be confirmed with Steve if need be).

p.1085 – I don't follow the explanation at the top of the page.

Do you mean the paragraph that carries over from the previous page? The essence is that the metric RMA is entirely based on the model's self consistency whereas ACC relies on comparison to an external solution. This means that in terms of convergence RMA is easier/more reliable to assess. We've modified this text a little.

p.1086 - lines 2-3: Note why the non-linear drag law is a bit "easier" on resolution requirements and may show slightly faster convergence (relative to the performance Met-

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rics that is) with increasing resolution; when m=1/3, the basal "stickiness" decreases as the sliding velocity increases, in which case it becomes increasingly more slippery (and the transition zone one needs to resolve widens) as one approaches the g.l. This is not the case for m=1.

This is a reason to expect better absolute performance from the non-linear drag law but are you sure this should also give better convergence?

p.1086 - Line 26 – I see where the factor of 8 comes from, but I don't follow where the factor of 16 comes from. Perhaps it would be good to clarify where these numbers come from (e.g. you get 2x2 = 4x increase for a doubling of horiz resolution (in 2d map view) and also (presumably) a halving of the timestep (assuming an explicit advection scheme and CFL limitation), which gives you another factor of 2).

I don't think it is needed, have just removed the mention of 16.

Somewhere in the discussion/conclusions it would be nice to see an explicit statement about the resolution requirements (for this study, this model, these assumptions, etc.) to achieve an "acceptable" level of error using the "best" GLP used here. Is it 100m? 500 m? 1km?

If we did this it would be somewhat arbitrary and could be taken out of context. The resolution requirements depend strongly on a number of factors such as bed slope and rate factor. And they depend massively on basal drag law and coefficient. This is a big question that a lot of people seem to be asking right now, but any answer we give based on this study would be plain wrong in all but a few real world situations.

Figures / Tables / References The figures, tables, and associated captions are all relevant and complete in my opinion. All of the references in the text appear in the "references" section and vice versa.

p.1065 – Vieli and Payne ref is to "2005" – should be 1998? Same mistake occurs in other parts of the text.

I don't follow. I am not aware of any Vieli and Payne 1998 paper (had Tony and Andreas even met each other back then?). I think we're correct on this one.

Ref. for Schoof "grounding line dynamics" looks weird : : : lots of extra numbers at the end?

TC seem to like to append all the page numbers on which the article is referenced in the bibliography section. I guess this is what you refer to.

Technical corrections

Have all been addressed more or less as suggested, exceptions below.

p.1066 lines 6-9 – another way to say this is that no one has clearly demonstrated yet that you need full Stokes in order to model g.l. behavior correctly.

I think Sophie Nowicki and the Grenoble group HAVE demonstrated that you need full Stokes in order to model the grounding line correctly (and Schoof asymptotic analysis supports this). I think the point you want to make is that it has not yet been demonstrated that using the floatation condition in a model without full stresses is significantly different to a contact condition in a model with full stresses in terms of large scale grounding line migration (aside from the numerical issues with grounding line migration such as addressed by this study). Surely the (not yet formally assessed) assumption is that for large scale studies the floatation condition is good enough.

Gravitational driving stress p. 1077 lines 10-12 – I'm not sure I follow this discussion.

The principal is this: Say we want to calculate a - b. Precision is to n significant figures. Suppose a and b are identical to x significant figures. The resulting sum is only meaningful to (n-x) significant figures, the rest is meaningless. I've found out that this is called 'catastrophic cancellation'. I don't want to take up space in the paper for what is really just minor point so I've removed the discussion from the text and just left the name of the phenomena. Anyone interested can easily google it and find a better description!

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p.1084 lines 8-17 – Why is it that the "jumps" in g.l. motion associate w/ the two parameterizations, both at the same grid resolution, occur at what looks to be different spatial locations? Is that an artifact of the plot, or is the lesser of the two GLPs getting hung up "at" a grid cell whereas the better of the two is not? Perhaps this is just a plotting issue or I'm not paying close enough attention to the axes : : :

I think you are referring to figure 6 rather than the text here? Perhaps you missed the two different y axis scalings. See also the one on the right.

p.1085 Discussion Line 21 – ": : :and the simplest GLP or changes (doubling) of grid resolution."

The differences *are* comparable to doubling of resolution.

p.1088 lines 15-22 – from the discussion, it is not clear whether or not Pollard et al. still get the stp like behavior w.r.t. g.l. motion. If so, are their steps just "correct", w.r.t. ice flux such that they get the right g.l. migration?

Sub-grid scale grounding line position was not a diagnostic of their model. The interpolation was used to calculate the Schoof flux and this was then applied at a grid point. I had not fully understood their method when this paper was written, I have updated my summary of the Pollard and DeConto work.

Conclusions p.1088 line 26 – ": : :centred on interpolating ice thickness, driving stress, and basal traction over the grid cell : : :"

We argue (in introduction) that the thickness profiles are key, which is why we prefer ": : :centred on interpolating ice thickness over the grid cell : : :"

Appendix A p.1090 lines 7-10 – Why is a linear forcing used here vs. a "step" function forcing, as in MISMIP? Perhaps not relevant, but please note if there is some significant reason for choosing one over the other.

The linear forcing was chosen originally because we carried out a large ensemble of

simulations sampled over key inputs and could not get these to all complete success-fully with a step change (a step change too small meant that retreat did not occur in some retreat simulations and a step change too large caused instability in some simulations). As you say, we do not believe this is relevant to the outcome so we merely document here the approach taken.

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

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