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The Cryosphere Discussions

Interactive comment on "Dynamic influence of pinning points on marine ice-sheet stability: a numerical study in Dronning Maud Land, East Antarctica" *by* L. Favier et al.

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This study describes glaciological simulations of a region of coastal East Antarctica. The simulations are performed using the BISICLES ice sheet model that has previously been shown to resolve stresses and velocities accurately at the grounding line when run at sufficiently high resolution. A novel feature of this study is the investigation of several different melt rate parameterisations and an investigation into the consequences of pinning points where detailed local surveys show that the ice is grounded, but the available continental scale bathymetric charts would suggest otherwise. The paper is logically organised and clearly written. The sensitivity studies performed make sense, the figures are appropriate and the conclusions are important enough to be

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Discussion paper

published in The Cryosphere.

My main concern with this paper is that I think more needs to be done to demonstrate that the results converge under grid refinement. The effect of grid resolution might be especially important if basal melting makes a sudden jump across the grounding line from a finite value to zero, or if the basal melt depends sensitively on ice draft that varies rapidly near the grounding line. Both of these conditions are relevant here and the melt parameterisations are different from those used previously, so I don't think it is enough to rely on previous investigations with this model to assess the sensitivity to grid refinement.

Major considerations

There are at least three motivations for investigating the sensitivity to grid resolution more thoroughly than is done here.

1 km resolution at the grounding line seems quite coarse unless a sub-grid scheme is used to parameterise basal drag and driving stress at the grounding line. Was a sub-grid parameterisation used? Are the results sensitive to the mesh refinement?

Another important consideration is that both melt rate parameterisations have the potential to apply non-zero melt rate directly at the grounding line, with a sudden transition to no melt for grounded ice. This might impose a discontinuity in the gradient of ice thickness at the grounding line. Again, it would be good to see evidence that the model can resolve this adequately.

Also, both parameterisations depend on ice draft. Basal slopes at the grounding line can be steep. Again, this means the authors should show that their results do not depend sensitively on the grid refinement used.

I think a simulation should be included with a modified parameterisation for which the melt rate goes to zero at the grounding line (perhaps by setting G=0 in Equation 1). This would reveal whether the retreat is driven mostly by melting directly at the grounding line, or by reductions in buttressing induced by melting elsewhere.

Minor corrections

P1 - Line 22. This section includes the following statement

A prior retreat of the grounding line (e.g. ocean driven) resting on such an upsloping bed thickens the ice at the grounding line, which increases the ice flux and induces further retreat, etc., until a downsloping bed is reached.

This description is misleading. The retreat needn't stop when a downsloping bed is reached. In the simulations of Schoof (2007) the grounding line doesn't stop at the deepest point, which is where a downsloping bed is first encountered. There is the possibility for a stable equilibrium to exist on downsloping beds, but only if upstream snowfall balances local flux at the grounding line. There is no reason for this condition to apply just because the grounding line has reached a downsloping bed. Rather, the grounding line will continue to retreat until (i) upstream snowfall balances local flux at the grounding line AND (ii) the bed is downsloping. Some glaciologists seem to think that grounding line retreat will necessarily stop when a downsloping bed is reached, modellers shouldn't be adding to this confusion.

P2 – Line 25. Make clear compression/extension is in flow direction.

P5 – Explain more clearly which domain is bounded by the dotted line.

P5 – Eqn 1. I don't think G and A are melt rates. What values were used for these parameters?

P6 – How does p vary? Give more details of exponential decay rate.

P7 - Line 3. It needs to be clearer which equations are being solved. The text is currently ambiguous and leaves open three different possibilities. Is it (a) the L1L2 system described by Hindmarsh (2004), (b) the L1L2 system described by Schoof and Hindmarsh (2010) or (c) the SSA* system described by Cornford et al. (2015). If the equations are the SSA* approach described by Cornford et al (2015) then these are not the same as the model described by Schoof and Hindmarsh (2010) and shouldn't be referred to as such.

P7 - Eqn 5. Is any regularisation used in the sliding law for low velocities?

P8 – Imposing a trial and error value of basal freezing to prevent grounding line motion during relaxation seems slightly imprecise. An alternative approach is to fix the thickness of floating ice shelf to prevent grounding line migration during relaxation (see Arthern et al. 2015, DOI: 10.1002/2014JF003239). This approach can also incorporate dhdt observations on the grounded ice so the grounding line retreats at the observed rate during the forward simulation. It would be worth pointing out that alternative approaches to constraining the grounding line during surface relaxation are available.

P16 – Line 9. Capitalise Weertman.

Table 1. Check the units for heat capacity.

Table 1. The parameter α_2 is described as a tuning parameter for M_{b2} not M_{b1}.

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