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The Cryosphere Discussions

Interactive comment on "On the substantial influence of the treatment of friction at the grounding line" *by* O. Gagliardini et al.

S. L. Cornford (Referee)

s.l.cornford@bristol.ac.uk

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This paper discusses the effect of changes to the representation of the basal stress faround the grounding line in the Stokes model Elmer/Ice. It is a fairly brief manuscript, and I am inclined to think that its results do not entirely support the discussion. I don't even think they are very well described by the title – in that I don't think the influence of the different formulae is substantial. I think the results need to be available, but they would sit better in a longer paper, which compared them to the (presumably) lower error seen with the kind of friction law proposed in Tsai (2015), or a modification that resulted in much lower error in the conventional power law case.

For comparison, Seroussi 2015 also discussed modifications to the ISSM hydrostatic model(s). These modifications had a notable impact on ISSM's results - in effect they reduced the size of their numerical error in MISMIP3D by an order of magnitude.

The authors of this paper note that the modifications available to hydrostatic models are not available to a Stokes model, which seems correct. The modifications that are made are rather more modest in effect, and have to do with the interpolation of the traction coefficient (rather than thickness above flotation) between nodes. There are three formulae, FF, LG, and DI. LG was the original, DI (as the authors note) seems intuitively to be correct and its results tend to fall in between the other two. However, all three appear to have a similar sized numerical error. Looking at fig 4, for example, the measure of error (distance between advance and retreat GL) is about 40 km in each case for $h = 400m$, 20 km for $h = 200m$ and so on.

Does the figure refer to

$$\int |\sigma_{ij}(LG) - \sigma_{ij}(FF)| dS$$

or

$$\int \sigma_{ij}(LG) dS - \int \sigma_{ij}(FF) dS?$$

I assume the first, but the wording could also mean the second - then the total traction at the bed (plus some stress at $x = 0$?) balancing the total gravitational force, which is the same in all cases because the geometry is imposed.'

The sequence of figs 6-7 make the error in DI look smaller than the other two, but not by an order of magnitude. DI sees the grounding line at about 9 km after 100 years with 20 lateral elements and at 12 km for 80 elements. At the same time (e.g) LG advances as far as 19 km with 20 elements but only advances to about 14 km for 80 lateral elements The error in both seems to be a few kilometers at the coarsest lateral resolution, as is the difference in initial grounding lines. The conclusion I draw from this is that the orginal Elmer/Ice error was a good portion of its MISMIP3D P75 results, but not enough to account for the difference between its steady state results and the SSA results (RHI,HGU,DGO,DMA, but also ISSM's SSA results in Seroussi

2015). In other words, the claim that Elmer/Ice produces a different steady state due to its different stress balance is not disproven. It's P75 dynamics do look increasingly like the hydrostatic models (e.g the centerline grounding lines starts to retreat toward the end of the 'perturbation on' period, and is upstream of the initial point at the end of the 'pertubation off' century) as the lateral resolution is improved.

The fact that all three formulas produce the same result when the friction does decay to zero with distance from the GL does not imply that thay have lower error in that case - they just have the same error. Imagine a case where the true Tb was constant to the midpoint of the last grounded element, then decays. FF, DI, and LG look the same, but will still each incur error of the same magnitude as discussed before.

The recommedation to prefer (say) the Tsai 2015 friction makes sense, presumably it will result in less error for everyone. It does seem as though the MISMIP experiments may be needlessly hard in some ways.

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