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The Cryosphere Discussions

Interactive comment on "Retreat of Thwaites Glacier, West Antarctica, over the next 100 years using various ice flow models, ice shelf melt scenarios and basal friction laws" *by* Hongju Yu et al.

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This paper describes a set of ISSM ice sheet model simulations to the Thwaites Glacier. It plays to one of ISSMs notable strengths, namely its ability to be switched between the three suitable model types (in order of fidelity, 2d hydrostatic , 3d hydrostatic, and 3d non-hydrostatic 'full Stokes' models) for this sort of application. It shows that although the three model types result in some variation, that variation is smaller than the influence of differing treatments of friction at the ice bed interface. It also adds the general body of model results in Thwaites glacier, with projections that tend to confirm those

of similar models. I think that the manuscript is in good shape, and could be published with only very minor revision

General comments

I think the manuscript could more obviously distinguish between choice of physics and choices that affect numerical error. Two of the authors at least are very familiar with the issue of melt on partially floating cells, and I think that they have - correctly - concluded in recent work that it is a design error, rather than a straightforward choice. The text does acknowledge that the numerical error can be reduced arbitrarily, so I don't think this is a major issue

Specific comments (and corrections)

P1,L16 (and 19) : dischargers? An unusual word for this case. 'Outflow' or 'sink' might be a more conventional choice.

P2, L13 'conditional' -> 'conditionally'

P2, L17 'we need numerical models'. I'm not sure that everyone agrees on 'need', but at any rate follow text supports the common use of numerical models rather than their proven utility.

P2, L24 'a transition in stress field' - I think something more specific is needed here about the type of transition, i.e from gravitational stress being balanced largely by local (in x,y) basal traction in the interior to being balanced by distant (in x,y) basal stresses via englacial viscous stresses.

L2, L30 : Here is an example where physics and numeric could be more clearly seens as distinct.

P4, L11. Melt and Nomelt don't seem like a good choice af name to me. A lazy reader that look at the figures without reading the text, might think there was no sub-shelf melt in the nomeltl experiment. There is a Seroussi et al paper that talks about friction

schemes (in hydrostatic models) that names schemes like NSEP, SEP1, and so on that are clear but don't mislead the lazy. The 'Melt' scheme sees like a melt version of SEP1 (If I recall correctly), so it could be SEMP1? And Nomelt becomes SEMP0 ?

L21: You should comment on the different behaviour of non-linear rules in the literature, it is especially important in the Joughin 2010 Pine Island Glacier paper, and others have commented too.

L23 'ensemble' -> 'combination' ?

L29; Dirichlett condition - I think here you have modelled only part of the catchment, so that you need observations rather than setting divide conditions u.n = 0 etc. You just need to say why this is OK (because there is very little flux leaving the region along those boundaries)

P6, L9 '8 layers'. This seems a common choice for full stokes, but is it enough? How do you know?

L11 'conduct an inversion of' -> 'solve a typical inverse problem to estimate'

L14 'relax the model' -> 'relax the geometry'?

Fig 3 : Odd units in the top row. Why the ˆ(-$\frac{1}{2}$) ?

P11, L6: This paragraph is about mathematical issues o not a numerical issue, since it would occur even in (no-existent but still imaginable) analytic solutions. I think this whole subsection needs a rewrite; it mixes up physics, mathematics, and computation performance, sometimes within a paragraph e.g L14-

P. L20; This is a numerical issue, but is preceded by a choice of phsyics (SSA/HO/FS) then is followed by a choice of physics (friction rule). Perhaps re-order?

P12, L1 'friction is reduced with the Budd friction law' : Because Cw = CbN in the first instance, so your Cb has to be much lower inland where N is large in the initial state. That might work out differently if our knowledge of N was poor (e.g due to hydrology)

L10. Though the extra parameter, f in Tsai 2015 is O(1) rather than being able to take on any value.

L27: 'TG is retrograde' - and, the channel widens too.

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