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Interactive comment on "Sea-level response to melting of Antarctic ice shelves on multi-centennial time scales with the fast Elementary Thermomechanical Ice Sheet model (f.ETISh v1.0)" *by* Frank Pattyn

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This manuscript presents the details and some experiments carried out with a new ice sheet model code (f.ETISH), which is designed to represent the major process in near future ice sheet dynamics (e.g changes in grounding line flux due to ice shelf thinning) well enough to be meaningful but not requiring fine spatial resolution and the attendant computational cost. The new model is similar in that respect to the model of Pollard and DeConto (sans cliff collapse), but makes some additional approximations for the sake of speed. Is this a useful new model? Perhaps. It could be very well

suited to long-term integrations, though that is equally true of Pollard and DeConto. It could be useful in large ensemble construction, where I think it makes a better job of representing the physics than (say) Ritz et al 2015. It does seem to contain new but not really well justified approximations in rheology and temperature structure, but given that these things are largely unknown and must be tuned anyway, that may not be very serious problem.

The paper is a bit rambling in parts (maybe this review is too), especially the model description and the discussion. Sometimes it includes expressions that are well known, then ignores them, e.g the effective viscosity is treated this way, and the discussion of Coulomb friction laws seem to be a bit extraneous too. I think it really does need a substantial review and edit.

General Comments

One message of the paper seems to be that treating the flux across the grounding line according to Tsai (TGL) rather than Schoof (SGL) dramatically increases the retreat rate. This could be because Tsai depends on a higher power of flotation thickness (more acceleration of retreat on retrograde slopes), but might to some extent be attributed to the addition of another free parameter (tan phi) . Given that the model is so quick, maybe some runs with larger $\tan(\phi)$? Looking at eq 18, for much of WAIS where bedrock elevation, is < -1 km, $\tan(\phi)$ is around $\tan(\phi_{min})$ = 0.2. What happens if $\tan(\phi) = \tan(\phi_{max}) \approx 0.5$ – presumably we see about half the rate of SLR? On the same note, TGL is double sided - flux increases more quickly with grounding line thickness, which would mean that the grounding line accelerates more readily in unstable configurations (retrograde slopes, without buttressing) but decelerates more readily in stable configurations (prograde slopes) - and the formula for tan(phi) should amplify this effect somewhat. The East Antarctic results in section 5 seem to differ from this, with the introduction of TGL leading to retreat over prograde slopes (Totten, Wilkes Basin, Recovery Glacier a little upstream from the present day GL) where there was little in the results with SGL

I am suspicious of approximating the effective viscosity by assuming that the stress that enters it is that of an free floating 1HD shelf (eqs 8 and 9). Can this really be a good approximation in buttressed ice shelves like (e.g) PIG, Amery, Totten, where there are regions with little along flow stretching, but strong lateral strains ? To me, the example in appendix D is not especially convincing – cross flow gradients are too low, and the flow field too smooth. This might not be a big error in itself, since at higher melt rates all that will matter is the TGL / SGL with no buttressing, but a more convincing test is needed. Why not re-run a middle melt-rate experiment with the normal nonlinear rheology?

Given that there is a well known test - MISMIP+ - with published results that include both the Tsai friction rule and ice shelf buttressing - why not test f.ETISH against that?

Specific comments

Abstract

(and elsewhere) "The higher sensitivity [in the case of the Tsai 2] is attributed to higher driving stresses upstream from the grounding line." I'm not sure this makes sense – and I suggest it is at least partly the other way round. Because q(TGL) is larger than q(SGL), but both are only applied at the GL, dh/dx is going to be bigger at the GL for TGL with all else being equal. The same – plain Weertman - friction law is applied upstream.

Section 1

"The majority of these interactions demonstrate non-linear behaviour due to feedbacks, leading to self-amplifying ice mass change." -> "Some of these . . ."

"thicker ice grounded in deeper water would result in floatation, increased ice discharge, and further retreat within a positive feedback loop." -> thicker ice grounded in deeper water would result in increased ice discharge, and further retreat within a positive feedback loop.

". . .. based on boundary layer theory (...Ritz et al., 2015. . .)". I don't think the Ritz et al., 2015 GL is based on boundary layer theory, does it? But imposes retreat rates sampled from some sort of probability distribution.

Section 2.1

"The main advantage of SIA is that the velocity is completely determined from the local ice-sheet geometry." That might be called the main disadvantage too.

SSA+SIA : "a simple addition still guarantees a smooth transition" - why wouldn't it? SIA isn't smooth in the same way as SSA, but so long as the surface elevation is smooth, it will be. More to the point, is this a good approximation? How about at the ice shelf calving front, where grad(s) is large, there is no basal stick and SIA makes no sense? I don't think Schoof and Hindmarsh 2010 gives us a reason to think that SSA+SIA is any more sensible than plain SSA.

"Basal velocities in the hybrid model are defined through a friction power law, where" Basal traction, no? The velocity is related but depends also on viscous stress at least close to the GL in the SSA+SIA case.

2..1.7. The Coulomb friction law plays no part in the results, except for its involvement with the Tsai flux. I suggest cutting this (longish) section 2.1.5 entirely and describing tan(phi) in 2.1.7

“where ‘spy’ is the number of seconds per year” Why switch units mid-expression?

Eq 25: What value does $tan($\phi$) tend to take?

Section 2.2

eq 28: should be an inequality? $hdu/dx \leq AhT_f$ (ie the maximum stretching is in free shelves)

“Ritz et al. (2015) use a slightly different prescription, but sensitivity tests showed that the extra terms in the mass conservation equation can be safely dropped, rendering the maximum strain check therefore independent of velocity gradients.” Which terms? The terms that have been dropped are $-udh/dx$ and $-vdh/dx$, both of which involve **thickness** gradients and are typically positive, so in fact

$dh/dt \geq a - -M - -h(du/dx + dv/dy) \leq a - M - 2AhT_f$

And you assume dh/dx is neglibigle?

“However, to compensate for the absence of horizontal advection in the model, only a fraction $fs \approx 0.25$ of the total strain heating amount was added. This value is determined from the EISMINT benchmark experiments (Appendix A).” Should this value not depend at all on ice speed? Is ESIMINT a sufficient test of this quite different dynamics?

Section 2.3

OK, these expression come from others. But are they justified in any way.

Section 2.4

Eq 33. Has some horizontal advection - needed to eliminate Tdw/dz and reduce vertical advection to wdT/dz, but neglects horizontal temperature variation? An even simpler solution might be possible if the conservative advection $d/dz(wT)$ was used and all horizontal transport neglected. Or did I miss something?

How is eq 41 based on the Peclet number? So τ_t is advection dominated when Pe is large and diffusion dominated when $Pe \to 0$, but does the code actually compute some function of Pe?

Section 2.6

Why solve eq 46 with BiCGStab? What preconditioner is used? ILU(0)? UMFpack is MATLAB’s default sparse solver, I think, and I guess the matrices are all small (coarse grid), so if there are large ice shelves (so A becomes poorly conditioned) this direct solver might be the better choice (or not)

Section 4.

“This further improves the final fit compared to the non-regularized case..” which is not normally the case with regularization - typically regularization results in worse (or no-better) fit to the observations for the sake of a smoother (or more plausible in some other sense) solution.

Section 5.1:

Sorry to bring this up, but Cornford et al 2016, Annals of Glaciology https://doi.org/10.1017/aog.2016.13 does a rather similar experiment (all-Antarctic response to sustained ice shelf removal), with a sub-km model, and the Weertman sliding results could be compared.

Is the rate of SLR labelled incorrectly in fig 10?

Section 5.2

“Melting is not allowed to be spread out across the grounded part of the 20 ice sheet near the grounding line as is done in some models (Feldmann et al., 2014; Golledge et al., 2015)”. Note that Feldmann and Golledge are not really trying to spread the melting about, they are just applying some melt to finite area grid cells whose centers are grounded but whose neighbours are floating, estimating a floating fraction by interpolating the thickness above flotation. This sounds pretty innocuous - even sensible - in which context the sentence above sounds like the wrong choice. Of course we know it is not the wrong choice, but maybe say something about why?

“[SLR] determined from the change in ice volume above floatation, hence do not represent the total grounded ice mass loss” Seems like an odd comment - how else would it be computed ? It makes me wonder if the section 5.1 SLR is from total mass loss (indeed the text of section 5.1 suggests that, ”the total mass loss for TGL is three times as large compared to SGL, i.e., a contribution to sea-level rise of 12 m ...”), when I assumed it had been computed from VAF

Fig 11. Although the ‘thick lines (SGL), thin lines (TGL)’ plot works for the large delta M, I can’t make so well out what is going on at small delta M. how about thin lines with a few symbols (say, circles, squares). Or drop the $\Delta M = 10$ m/a results?

Fig 12. To my mind, at least one more grid spacing (there are some runs 16km. right?) to be able to say much about mesh dependence. You can’t test convergence at all with just two, you need to show that results are getting closer to one another as $dx \rightarrow 0$

Section 6

“Another major difference pertains to the marine boundary, with a novel implementation of the grounding-line flux condition according to Tsai et al. (2015), based on a Coulomb friction law (TGL)” ‘novel’ seems a bit strong, given that Tsai derived the flux formula, and the implementation replaces a very similar formula (SGL) in an overall method to modify the Schoof flux to include buttressing due to Pollard.

p35 “unless sub-grid grounding-line parametrizations are used that generally allow for grid sizes of ≈ 10 km (Feldmann et al., 2014). “. Personally I think this claim in Feldmann 2014 is not supported by the results, which are better with the sub-grid scheme, but still need dx 1 km. Why should we believe that results in one idealized problem should be widely true?

“Nevertheless,comparison with high-resolution SSA and hybrid models show that while differences in transient response exist, results are in overall agreement with the other models (Pattyn and Durand, 2013).” That really was not the message I took from Pattyn and Durand 2013, at least regarding the transient.

“as the ice-sheet profiles ‘taper off’ towards a flattening upper surface, contrary to the power-law case,” - this happens to some extent in the power law case too, depending on the scale length for viscous stresses transmission.

“(so-called ‘aggressive’ grounding line in PISM).” Does Golledge really call it ‘agressive’ in that paper. I remember him saying it in a talk. Anyway, why not say what it is: a type of numerical error (aggression -> 0 as dx -> 0) rather than something that could be seen as physics.

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