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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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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

S. L. Cornford (Referee)

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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.

I should like to thank Stephen for this thorough review and I will try to give a non-rambling response to his queries. A series of improvements to the model have been made and are detailed in my response to Referee 1. I will therefore refer to my response to that referee for points that were already raised. The major changes to the model are (i) the full temperature calculation, including horizontal advection and internal heating, (ii) an optimization scheme for the Coulomb friction law (and therefore tests on different values of ϕ at the grounding line, (iii) the SSA solution based on a properly calculated effective viscosity (instead of a crude approximation).

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(φ)? Looking at eq 18, for much of WAIS where bedrock elevation, is < -1 km, tan(φ) is around tan(φmin) = 0.2. What happens if tan(φ) = tan(φmax) $\approx$ 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.

See my response to Referee 1. Higher values of ϕ lead to lower sensitivity of grounding line retreat, but it is not half the amount of SLR for a doubling of ϕ. Even for very high values of $\phi > 70°$, this leads still to a mass loss that is significantly higher than for the Schoof-condition. I included also an optimization of the Coulomb friction law, whereby values of till friction angle are optimized, hence also at the grounding line. There will be a more thorough discussion on the retreat experiments for different conditions of ϕ and the sensitivity on prograde slopes in the revised manuscript.

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?

I have been looking into this and calculated the effective viscosity now as it should be (according to eq. 5). It also required an iteration, but this doesn't seem to slow down the model as much. The resulting effective viscosity is different (as would be expected), and in few cases the response as well, mostly related to differences in buttressing factors. However, the major sensitivity of the model still remains with the treatment of the boundary conditions at the grounding line (Tsai vs Schoof), and the magnitude of change is comparable to the results presented in the submitted manuscript. A new set of results will therefore be presented in the revised manuscript.

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?

It is of course an interesting idea that will require quite some work and also fall outside the scope of the present paper. In term, it is envisaged to perform those tests, but it will require major changes in the model code with respect to the adaptability of the boundary conditions.

Specific comments

These will be answered in detail with the revised manuscript.

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