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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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Response to the 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

Anonymous Referee 1

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General comments:

This paper presents a thorough and clear description of a new ice sheet model, akin to hybrid dynamics SIA/SSA models currently used for Antarctica, but with some reasonable and innovative simplifications so it is computationally fast. The model is implemented in MATLAB and will be a useful tool to engage students in teaching and workshop environments, as well as being capable for many research applications.

In this paper the model is thoroughly tested against established benchmarks (EISMINT, MISMIP) and validated vs. modern Antarctica. Sensitivity experiments of Antarctic re treat for simple warming perturbations are described. One important result is that much larger grounding line retreat is obtained with a Coulomb-friction based parameterization of grounding line flux, compared to that based on power-law sliding, but further testing may be desirable (see below).

I would like to thank the referee for this early and very detailed review, which gave me ample time to check out in more detail the concerns that were raised. Here, I will answer to the major questions raised by the referee and how new implementations hopefully improved the model and the model results.

Specific comments:

(1) The treatment of ice temperatures is based on classic vertical profile equilibrium solutions which allow for vertical ice velocity, and then time lagged with an e-folding relaxation towards these solutions at each grid point. The timescale of the e-folding lag is based reasonably on the local Peclet number (pg. 17, eq. 42). This is probably the most drastic simplification from other 3-D hybrid models, and neglects horizontal advection (which cools mid-level interiors as cold surface ice is advected downwards and outwards, and cools the cores of ice shelves supplied by flow across thick grounding lines. A fairly arbitrary compensation for this lack of cooling is attempted by reducing the strain heating (pg. 17, line 6). This simplified temperature treatment is evident in the benchmark intercomparisons in the Appendices, where basal temperature is the only field with poor results.

As a suggestion, perhaps basic horizontal temperature advection could be added to the model, ust by adding an additional term in Eq. (41): ... + u dT/x + v dT/dy with (u,v) given by (12) and T is the column mean temperature. That probably would not require much CPU or slowdown of the model.

The referee is correct that this is probably a drastic simplification. It was also one of the first simplifications I made to the model. However, my major concern was not so much omitting horizontal advection, which it is relatively well counterbalanced by frictional heating, as the EISMINT I experiments show. I admit that this has not so much of a physical basis and the coupled experiments in EISMINT II were not very convincing. My major concern with this approximation is related to the time-dependent evolution of the temperature, which in its current form is not suited for paleo-climatic studies. Therefore, I revised the temperature calculation completely by solving the time-dependent thermodynamic equation in three dimensions, similar to Pattyn (2010). It includes besides vertical diffusion and advection also horizontal advection, internal heating and frictional heating. In order to improve calculation speed, the whole subroutine was optimized and given the stability of the numerical scheme, the temperature field is only updated every 10 to 20 iterations.

Nevertheless, there are a couple of simplifications made: (i) the temperature field is calculated using shape functions for both horizontal and vertical velocity (Hindmarsh, 1999) as well as for velocity gradients based on the deformational SIA velocity for a vertically-integrated value of A; (ii) the flow parameter A is still determined for a given column fraction. Despite these simplifications, the model is now in agreement with both the EISMINT I (Huybrechts et al, 1996) and EISMINT II (Payne et al, 2000) experiments. Even the 'unstable' basal temperature patterns according to some experiments in EISMINT II are now reproduced.

Given this concern, I suggest that a map of the models basal temperatures for modern Antarctica be shown, and compared with existing model and data based maps (of

which the author is a leader).

It was my mistake not to have shown the basal temperature field for the Antarctic ice sheet, especially since the temperature calculation was a major approximation. The basal temperature field was different from the one given in Pattyn (2010), as it does not include the optimization of geothermal heat flow using observed temperature profiles and subglacial lake distribution. However, it was more in line with other basal temperature fields obtained by other model studies (Pollard and DeConto; Huybrechts, ...), which gave me confidence in the approximation. A comparison between the new basal temperature field and the approximation of the submitted manuscript reveals also the same pattern, which demonstrates that the initial approximation was quite well representing the ice-sheet temperature field. It is therefore expected that the forcing experiments using the new temperature calculation will not be so different from the previous ones, especially on the time scales that are considered. Nevertheless, the concerns on the temperature field are now taken away.

(2) It is puzzling why the inverse procedure for basal sliding coefficients (p. 23-24, Fig. 5) yields quite large errors in surface elevation (âĹij200 m) in some regions of the interior East Antarctic plateau. The inverse procedure should reduce them to 10's m (Pollard and DeConto, 2012b) (even if the bed elevations are in error, model or observed, cf. pg. 24 line 19).

Thanks for remarking this. I have been looking into this in more detail. First of all, it seems that better convergence is reached when letting the optimization run for 200 ka instead of 100 ka. Secondly, the use of the regularization term (smoothing) improves the fit near the borders (compared to Pollard and DeConto, 2012b), but increases the error in the interior. Therefore, the global fit improves, but the referee is right to point that the regularization results in a poorer match for the interior ice sheet. I should state it otherwise.

Perhaps these larger errors are due to regions of the bed erroneously being frozen. In frozen basal regions the inverse procedure cannot reduce the model's surface elevation errors. So this is an additional reason to request a basal-temperature map.

Some errors are due to frozen zones, since the optimization procedure does not perform across these zones. However, as shown by the similarity between the basal temperature fields (old and new), this seems not due to a mismatch of frozen/temperate areas.

nb: "ice thickness", pg.24 line 1, should probably be "ice surface elevation".

Indeed it is.

(3) One important result is the greater grounding line retreat with TGL (Coulomb-friction based grounding line flux parameterization, Eq. 25), vs SGL (power-law sliding based, Eq. 23). All experiments shown use power-law sliding (Eq. 15) for the interior grounded ice, and none use Coulomb sliding (Eq. 21). My concern is that the combination of TGL with power-law interior sliding is not compatible, and the mismatch in the physics may lead to spurious behavior in grounding zone regions. (The discussion on pg. 13, lines 24-27 may be relevant).

To address this concern, I would request additional runs be made with Coulomb friction law (Eq. 21) and the TGL grounding line parameterization. This would ideally also involve re-doing the optimization spin-up for basal properties, which may still be feasible by changing phi (till friction angle) instead of $A_b inEq.(55).Alternatively, thecombinedEq.(22)couldbeusedinsteadof(21).$

This has been looked into with greater detail. First of all, I don't completely agree with the non-compatibility between the Coulomb boundary condition at the grounding line and the Weertman sliding law inland from it. As shown in Tsai et al (2015), the Coulomb friction leading to vanishing effective pressure at the grounding line is a physically correct condition, whilst with the power law,

where the effective pressure is non-zero at the grounding line. Furthermore, the crossover from Coulomb conditions at the grounding line to power-law conditions inland is a very narrow transition zones (with exception of perhaps the Siple Coast region where streams experience a very low drag for a wide area). The contact with the ocean will always be influenced by marine sediments (characterized by a till friction angle), which makes the combination of both conditions (power law sliding for the ice sheet and Coulomb friction for the grounding line) valid. To demonstrate this, I carried out different experiments with varying values of till friction angle at the grounding line. Only for high till friction angles $\phi > 50°$) does the grounding-line sensitivity diminish, but still remains more sensitive than the grounding line conditions according to the power-law sliding. Moreover, I also included an optimization scheme for the Coulomb friction law (on the suggestion by the referee). This optimization changes tan(phi) (and not phi as the referee suggested). The resulting fit is less well than with the power law, but it makes phi vary between 2- 70°. Higher/lower values would be really non-physical. The resulting response is obviously less sensitive than with the one where phi is prescribed, but still more sensitive than the power-law sliding and Schoof-condition at the grounding line. Both results are interesting and will be discussed in detail in the paper.

(4) The use of driving stress instead of basal stress in the basal sliding law to avoid iterations (pg. 10, Eqs. 15,16) is one of the features used to speed up the model. But maybe the 20

Basal sliding with the hybrid model IS a function of basal shear stress (or basal drag). So this effect of driving stresses being balanced by driving stresses is not correct (I should write this better in the revised manuscript). The equations are correct for $m = 1$. However, for a power law with $m = 2$, for instance, Eqs 15,16 make the sliding law more viscous than plastic by introducing the term tau_d. However, the revised model now properly calculates the effective viscosity in

the SSA equations (see response to referee 2), hence requiring iteration, so that this approximation is not made anymore. The resulting effect is rather limited, as I expected.

(5) The subglacial water pressure p_w in Eqs. (19) and (20), pg. 11, is assumed to depend on elevation minus sea level, which is a common step in many models. But it is hard to see how the subglacial water system can sense hydrostatic pressure from the ocean at all, more than 100 or 200 km inland from the grounding line.

I know. That is exactly why I did only use the Coulomb condition at the grounding line, because here the effective pressure is zero by definition. Given the fact that I now have introduced the optimization of the Coulomb friction law for the interior ice sheet, the approximate definition of p_w can be seriously questioned (which I will discuss). As I already mentioned in the manuscript, a subglacial hydrology model would be more appropriate and physically correct.

Technical points:

Will be answered with the new version of the manuscript.

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