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Interactive comment on "From Heinrich Events to cyclic ice streaming: the grow-and-surge instability in the Parallel Ice Sheet Model" *by* Johannes Feldmann and Anders Levermann

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This paper describes and application of the very respectable PISM ice sheet model to idealized simulations of surge cycles in marine ice streams. It differs from other studies in the same sort of area by modelling both longitudinal and lateral stresses (as opposed to just one or neither) without parametrizations. I think it has the basis of a good paper, but I think it needs an extra result or two to make it a really good paper. Ideally, I would have like to have seen the experimental design include the MISMIP+ reverse slopes (where buttressing or the lack of really matters), but I think that might be too much to ask for. So instead, I'd like to suggest that the authors also carry out some simulations with the linear sliding law. On page 6, I see that the paper suggests that either the

nonlinear sliding law or the more natural treatment of buttressing is responsible for differences with the Robel 2016 paper (linear sliding, parametrized buttressing). By choosing a basal traction coefficient such that the ice sheet is comparable (e.g. same GL position at furthest advance), this can be tested in some more detail (I.e, if the results are still different, it must be the buttressing treatment)

General Comments ————————-

The feedback diagrams are a nice idea to make the subject easier to understand. I wonder if the first of these figures (and the text that describes it) needs a little work. It is not so difficult to understand that there are some negative feedbacks e.g H → + → V → - → H and some positive feedbacks e.g W → + → V → + → W . but the key to all of this is in the detail of when and why one dominates. I don't really read that from the diagrams. Also, there is a mix of degree-of-freedom variables (H,V,W) and derived quantities (basal traction, flux), I think this could be simplified.

I wanted to read some discussion of the relationship between the various equations and time scales comes about (e.g, what is the source of the 1.8 ky scale – the drainage rate, or the time taken to advect cold ice from the divide, or something else. Should it be a surprise that it is not much affected by SSA stresses, which tend to have limited importance far upstream from the GL)

I'm not sure about the stabilization phase (P5, L17) being a separate negative feedback system (blue loop). First,it has the same time scale as the surge phase. My naïve reading of this is that at some point, the thinner colder ice means that melt-rate starts to drop, so that dW/dt < 0, then the same positive feedback that caused the surge)ie W → + → V → + → W works in reverse (W →- → V → - -> W). I'm no surge expert though – do other authors agree with you?

The surge-damping results are interesting, I think you could extend perhaps them . At the moment you have undamped surging (phi = 10) and decay to states that maintain a steady thin ice stream (phi <= 8), where presumably the bed is not frozen. Do steady

’thick and slow’ systems occur when phi » 10. Like wise, it would be interesting to see what happened if you switch to phi = 10 from the phi = 8 system.

The manuscript seems to somewhat over-rate its novelty e.g

(1) abstract, ’we identify .. the central feedbacks’ – that’s a big claim. Surely others have noted the same.

(2) P2, L10 “In particular, and in contrast to many of the previous studies, our simulations use a sliding law that is based on the stress balance of the ice and thereby has stress boundary conditions.”

Some papers have considered non-linear sliding, membrane stresses, etc in studies of thermo-mechanical instabilities. Obvious examples include Hindmarsh, G.RL, 2009 which is not cited, and Beuler and Brown 2009 (which is cited), which also describes the original version of the SSA/SIA scheme and much else regarding the PISM model used here, the major exception being Aschwandens 2012 improved PISM thermodynamics scheme. OK, the “many” makes P2,L10 technically true, but this is not the only statement of this sort, the cumulative effect is to appear to be claiming too much.

(3) The connection to Heinrich events, with a ice plus basal water model (not such a nice one) is described at length in Roberts et al, Clim. Past, 12, 1601 (doi:10.5194/cp-12-1601-2016)

Specific comments ——————

P2, L31 “A linear interpolation of the freely evolving grounding line and accordingly interpolated basal friction enable realistic grounding-line motion similar to models of higher order (Feldmann et al., 2014).”

I don’t think Feldmann 2014 shows this, exactly. The interpolation may represent a modest improvement but the time-dependent behaviour in Feldmann 2014 is clearly not close to convergent unless the mesh is resolved to around 1-2 kilometers., and indeed, the *non-interpolated* (model A) results at around 1km have features seen in

demonstrably resolved SSA (see the MISMIP3d paper) and Stokes (see Gagliardini 2016) models that the interpolated (model B) results lack . Probably the SSA/SIA physics and 1 km resolution chosen in this paper is adequate, but Feldmann 2014 is not the main reason even if it helps. You could say

“A linear interpolation of the freely evolving grounding line and accordingly interpolated basal friction, together with the use of one-sided differences* in the driving stress close to the GL, permit SSA physics to be treated with mesh resolutions of around 1 km (Feldman et al 2014)”.

*Correct? I thought you did this. I do too because I found it made a big difference, e.g (sorry to mention my own papers) [Cornford 2013 http://dx.doi.org/10.1016/j.jcp.2012.08.037] .whereas the interpolation helped only a bit [Cornford 2016] https://doi.org/10.1017/aog.2016.13.

P4, L7 “The superposition of both components yields a bed trough which is symmetric in both x and y directions” → reflection symmetric about y = 0, but no x-symmetry in the formulas given. I think (from other parts of the paper, that you meant symmetry about x = 0 so instead of b(x) you have b(|x|)? however, you could just say that a reflection condition (dh/dx = 0, u = 0, dv/dx = 0) is satisfied at x = 0

P4, L10 “Resulting convergent flow and associated horizontal shearing enable the emergence of ice-shelf buttressing, having a stabilizing effect on the grounding line...”. Not really “stabilizing” – even with no ice shelf there are no obvious unstable equilibria of the MISI sort in this geometry. Presumably the steady GL is further downstream than it might if the shelf was removed.

P5, L33, “...explained by assuming that a thinner ice sheet before the surge leads to a less dramatic surge [fine by me] and thus to a larger minimum [not fine by me]”. A less dramatic surge starting from a thinner sheet could lead to the same finial thickness as a more dramatic surge starting from a thicker sheer, or pretty much any other combination.

Fig 7. The frequency (w) and amplitude (A) of surges decays with a. Seems like there might be a critical a between 0.05 and 0.075 where the surging is turned on/off. I wonder how w, A behave around that point? That might be an unreasonable request, depending how long the model takes to run.

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