The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-289-RC2, 2018

The Cryosphere
Discussions

Interactive comment on "Grounding-line flux formula applied as a flux condition in numerical simulations fails for buttressed Antarctic ice streams" *by* Ronja Reese et al.

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Received and published: 27 March 2018

This paper presents a systematic comparison of remotely-sensed ice fluxes through Antarctic grounding lines with the fluxes predicted by a suite of "flux formulae" based on a particular boundary layer model for ice flow at the grounding line. The basal friction parameter — a key variable in the flux formula — estimated using an inversion of the same velocity data set. In general, the procedure adopted shows terrible agreement, demonstrating that said flux formulae do not work at all well when applied to present-day Antarctica. I believe the result is robust, and I am generally happy for the paper to be published more or less as is.

There are a few items that one could go after a bit more. My view of the review process has become pretty cynical (is my role really to hold up decent work just because it could be improved? Where does that process end?). In short, I am under no illusion that the points I raise will, or even should, feature in a revised paper, and so I have no desire to force the authors to address them. Rather, my argument is that writing off the methodology behind the offending flux fomrulae may be somewhat premature, and this could be investigated further. In that vein, here goes:

1. The discussion regarding the reason for the discrepancy between observed fluxes and fluxes predicted by the flux formulae is a bit weak. The discrepancy is primarily put down to large buttressing factors. I actually think that is inaccurate, in the following sense: if I really had a locally uniaxial flow, and buttressing due to a larger-scale reduction in extensional stress at the grounding line, I'd expect an amended flux formula (with buttressing factor theta_1 as defined in the paper here) to work pretty well, and I wouldn't expect theta_1 = 1/4 to cause major issues. In fact, it had been my impression that Gudmundsson et al (2012) had found reasonably good agreement, at least where the grounding line cuts across the channel in the geometry used in that paper, rather than at its sides (where the flow is presumably heavily affected by shearing parallel to the grounding line).

2. The flux formula being discussed was dervied under a number of conditions (for this you really have to look in detail at Schoof 2007 in JFM, the JGR version won't help), which can really be boiled down as follows: there is a boundary layer near the grounding line over which extensional stress decays to values compatible with a shallow ice approximation further inland. In order for the flux formula being tested to work, the following conditions must be met a) the boundary layer must be in a pseudo-steady state. This is justified by a separation of time scales: the boundary layer should equilibrate much faster than the ice sheet as a whole, so in the absence of rapid changes in forcing (or in bed condition due to internal feedbacks! - see Robel et al 2016 in The Cryosphere) b) the flow at the boundary layer scale must not depend on the transevrse

position (that is, if I move parallel to the grounding line by a distance comparable with the boundary layer length scale, the flow field should still look the same) c) the flow must be unidirectional and perpendicular to the grounding line. The condition that depth-averaged extensional stress ("R") at the grounding line is equal to 1/2 rho(1-rho/rho_w) g H is really not essential at all, which is where the simple correction factor theta in Schoof (2007) came from. It is true that making theta very small should change things - in fact, the extensional stress can become comparable to those that are experienced in a shallow ice flow and the need for a boundary layer almost goes away, as discussed in the appendix to Schoof (2007, JFM), Kowal et al (2013, JFM) as well as the supplementary material to Schoof et al (2017, The Cryosphere). A superficial reading of Reese et al would suggest that the value of theta is the crux of the problem, and I think that obscueres a few things (in the sense that some of the theta values observed, notably the negative ones, are likely to be the symptom rather than the cause; bear with me).

3. Out of the conditions listed above, c) is actually the easiest to surmount (I am currently working on an extension to the unidirectional flow model that would take shearing parallel to the grounding line into account, this changes the relationship between flux, thickness and extensional stress to take account of grounding-line-parallel shear stress, but nothing too exciting happening here). My impression is that the conditions that are most likely to be violated by the real data considered in this paper are a) and b). I'll touch on both below, in a way that may be at least partially testable.

4. Pseudo-steady state (point a above). This is likely to be violated relatively frequently, at least *in the data*. By that, I mean that bedmap ice thicknesses and ice velocities are taken at face value here, and the inversion is done purely as a snapshot inversion to find C, presumably at the cost of generating quite large ice flux divergences, if one were to compute them. If so, then it is likely that a "progonostic" forward computation of the model with time stepping would lead to significant transients that result from an incompatibility between velocity field and bed geometry, and these transients may not be

"real". See the work by Morlighem et al on inverting for bed topography to suppress the effect, and also the work by Goldberg and Heimbach on the use of data assimiliation techniques to avoid the pitfalls of using snapshot inversions for bed properties.

From the MISMIP model intercomparison in one horizontal dimension (Pattyn et al 2012, The Cryosphere), we know that violating the pseduo-steady state assumption (in that case, due to step changes in the ice viscosity parameter) can lead to singificant but sort-lived departures from the "flux formula", with the ice flux at the grounding line potentially settling back onto the flux formula over a time scale that is equal to the advective time scale for the boundary layer (boundary layer length / scale for velocity in the boundary layer). I'm not saying that this is likely to happen here, but it's worth illustrating - if you run your model forward over the time scale I identify, do you end up somewhere closer to the flux formula?

A corollary of this is actually the negative theta values computed. If I supposed that the flow were unidirectional and laterally homogeneous as per points c) and b) above, then the only way I could have a negative theta value would be if du/dx < 0 (u and x being measured along the flowline, naturally. If that is the case, then the assumption of the boundary layer model of a fixed flux through the boundary layer, corresponding to a pseudo-steady state, must fail, since such a flux would require d(hu)/dx = h du/dx + u hd/dx = 0 and we can assume that u, h > 0 and, as we ought to be thinning towards the grounding line, dh/dx < 0. If also du/dx < 0, both terms are negative and there is no way that we can have h du/dx + u hd/dx = 0.

That leads to two possibliities: either the pseudo-steady state condition is violated (so a) does not apply, which could occur due to short-time-scale changes in forcing) or the assumption of laterally homogeneous flow (point b) above) must be false. I expect we are looking at the latter rather than the former: the flow of the ice streams in question slows as the grounding line is approached. Mass is not lost here, but rather, I expect that the flow simply spreads laterally: as the centreline slows, a wider region flows at ice stream speeds. I confess I haven't bothered to check this in the data set, but it would certainly be worth doing that

5. Lateral homogeneity. This is probably the big one. The original boundary layer theory did not deal with this at all, and I fully expect that ice streams violate this assumption pretty much every time. There is plenty of evidence for the force balance of ice streams to be singificantly affected by lateral shear stresse, and I expect this holds true near the grounding line. This is intrinsically a loss of lateral homogeneity: we have gradients in the velocity component normal to the grounding line with respect to the coordinate that measures distance parallel to the grounding line, and an extra term appears in the force balance that does not feature in the original Schoof (2007, JFM) boundary layer theory. In that sense, I would not expect the resutls of the latter paper to hold, though I know how one would update the boundary layer theory presented there to account for it. In fact, it may be worth pointing out that various papers have already tried to do that, though only in a form where lateral drag is parameterized. The most complete version of this can be found in Schoof et al (2017, The Cryosphere), with parts of the problem also addresed by Pegler (2016 and earlier papers).

Is this testable? The answer is yes. Both Schoof (2007) papers give estimates for the size of the boundary layer as a function of the various model parameters, such as C, m etc. All you would have to do is check whether any mdoel parameters (such as extensional stess at the grounding line, or C, or ice thickness at the grounding line) vary singificantly within that boundary layer length scale. It is not acutally enough to do that *along* the grounding line, you should really also check that C in particular does not vary by an O(1) fractional amount when going a single boundary layer length scale inland. If there is singificant variability, you immediately have good reason to say that the flux formula won't hold.

6. I'm not particularly bothered by wanting to "defend" simulation models that use a flux formula. I can ceratinly see their appeal in simulating long time scales, and for testing qualititative ice sheet behaviour without getting lost in computational detail. But I haven't built a career on such a model. That said, I would be quite interested in a com-

parison of long-time-scale prognostic modelling using a flux formula model - admitting that it is going to give locally terribly wrong fluxes at a given point in time - and a fully resolved model like Ua. If I care about grounded ice volume changes over long time scales, how different are the predicitons? And are they *qualitiatively* different, in the sense that irreversible retreat occurs not just at different values of forcing parameters, but depends in a fundamentally different way on the forcing that is imposed?

Minor comments:

"Within the context of the shallow ice-stream computational models âĂŤ a commonly-used flow approximation for describing the flow of ice streams and ice shelves (e.g., Morland, 1987; MacAyeal, 1989) âĂŤ it has, for example, been suggested that for many applications a horizontal resolution of around one ice thickness or less is suitable (Gladstone et al., 2012; Pattyn et al., 2012; Cornford et al., 2016)" This is a somewhat bizarre thing to say; the whole point about a shallow ice theory is that it does not know about how long a horizontal distance equal to one ice thickness is: the limit of a small aspect ratio is already implicit in constructing a shallow ice theory. The fact that a grid or node spacing comparable to something like 1-3 km (or whatever) is regarded as adequate should not be equated with a mesh element size of one ice thickness. More relevant is what fraction of the linear domain size the typical distance between grid points or nodes should be — probably 1 in 1000 is really implied here.

equation 14 "I(f) = ..." what does the argument "f" signify, given that f does not appear on the right-hand side of equation (14)? Should this be I(v)?

page 9 "can \emph{not} be used": this should be "\emph{cannpt} be used"

figure 4: The Ua flux looks terrible - as in, very non-smooth. I'd expect H1-type convergence of ice velocities under grid refinement - is the jaggedness mostly a result of a misalignment between grounding line and mesh, or of forcing the grounding line to lie along a mesh (and therefore having sharp corners at every node?) Probably worth explaining.

page 13 bottom "However, in the presence of ice-shelf buttressing no such simple conclusions can be drawn (e.g Goldberg et al., 2009; Gudmundsson et al. ,2012; Gudmundsson, 2013; Pegler, 2016)." Without wishing to advertise my own work too much, Schoof et al 2017 in the Cryosphere also gives a qualitatively different example (with calving, which I believe differs from the other references given here) where the usual stability argument is reversed.

multiple instances, for instance figure caption B1. "Exemplary" is not usually used synonymously with "an example of". A brief internet search gives me the following meanings 1. serving as a desirable model; representing the best of its kind. "an award for exemplary community service" synonyms: perfect, ideal, model, faultless, flawless, impeccable, irreproachable; More excellent, outstanding, admirable, commendable, laudable, above/beyond reproach; textbook, consummate, archetypal "her exemplary behavior" antonyms: deplorable 2. (of a punishment) serving as a warning or deterrent. "exemplary sentencing may discourage the ultraviolent minority" synonyms: deterrent, cautionary, warning, admonitory; raremonitory "exemplary jail sentences" You might want to consider whether this is what you mean

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