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Interactive comment on "Parameterization of basal hydrology near grounding lines in a one-dimensional ice sheet model" *by* G. R. Leguy et al.

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This paper presents a more physically-based approach to regularizing the discontinuity in basal friction across the grounding line than was previously done in Pattyn et al (2006), and shows that a continuous basal shear stress allows convergent results to be computed at lower cost than a discontinuous one. This is a very useful contribution.

There are a few things that I was not particularly keen on in the general description of how the present work fits into the literature. The notion of a "transition zone" is mentioned in multiple places. I would argue that the main "transition" around the grounding line, in terms of large-scale ice sheet dynamics, must be the transition from an extensional-stress dominated flow in the ice shelf to a vertical-shear dominated flow in the ice sheet. That transition occurs whether we prescribe some continuous change in basal friction parameters away from the grounding, as is the case here through the dependence of τ_b on N and therefore its assumed dependence on H), or whether we keep basal friction parameters constant, for instance by putting $\tau_b = C|u|^{m-1}u$ as in the MISMIP experiments.

(Let me say right away that I understand the confusion that typically arises here. "Vertical-shear dominated" does not have to mean that the velocity field inland is dominated by vertical shear, nor does it even have to mean that vertical shear stress is much larger than extensional stress. What I mean is that vertical shear stress dominates force balance, so terms like $\partial tau_{xz}/\partial z$ are much larger than $\partial \tau_{xx}/\partial x$, which is ultimately the basis of "shallow-ice" type models.)

The relevant "transition length scale" can be identified as the extent of the boundary layer in Schoof (2007b). No other transition zone is *necessary* though it is clearly possible to invent additional physics that leads to more transition length scales, for instance by having a sliding law that has different asymptotic behaviours for small u/N^n and large u/N^n, as is done here. Those transition zones (here, the transition from a low, Coulomb-like shear stress to a high power-law like one) are presumably secondary to the one transition that remains unavoidable, namely the extensional- to vertical-shear-stress transition. This should probably be reflected in the text.

I'll briefly go further along this path. On page 374, line 6, the paper states that "this simplified friction law leads to a set of equations with an accurate semi-analytic approximation (Schoof, 2007a), whereas the more complex friction law in Eq. (15) does not, to the best of our knowledge, lend itself to a similar semi-analytic solution."

The boundary layer formulation in Schoof (2007b) can be actually be rewirtten for the present friction law rather straightforwardly, illustrating how the change in friction law introduces additional parameter dependences into the the formula linking discharge Q through the grounding line and ice thickness H_f at that location (which is then no longer computable analytically)

The relevant boundary layer problem would be

$$(UH)_X = 0,$$

$$4(H|U_X|^{1/n-1}U_X)_X - \left(1 + \frac{\nu|U|}{H^n(1-H_f/H)^{np}}\right)^{-1/n} |U|^{1/n-1}U - HH_X = 0,$$

with

$$4H|U_X|^{1/n-1}U_X = \frac{1}{2}(1-r)H_f^2 \qquad \text{at } X = 0$$

$$H = H_F \qquad \text{at } X = 0$$

$$UH \to 0 \qquad \text{as } X \to -\infty$$

$$U \to 0 \qquad \text{as } X \to -\infty$$

which is identical with the original boundary layer model in Schoof (2007b) except for the term

$$\left(1 + \frac{nu|U|}{H^n(1-H_f/H)^{np}}\right)^{-1/n}$$

appearing in the friction law — which however tends to unity as $H \to \infty$ in the matching region with the rest of the ice sheet, and therefore does not affect the solvability of the boundary layer problem, which could presumably be attacked with the same method as in e.g. Schoof (2012, JFM, appendix), except that the arguments for Q having a power-law dependence on H_f no longer apply.

The main difference with the original boundary layer problem is the appearance of the parameter $\nu = k[U]/(\rho_i g[H])$, where $[U]$ and $[H]$ are the scales for velocity and ice thickness in the boundary layer identified in Schoof (2007b). This parameter, as much as p, should dictate discrepancies between the formula for Q in Schoof (2007a,b) and the results used here. You would need large ν to have a boundary layer whose length significantly exceeds the boundary layer scale $[X]$ estimated in Schoof (2007b); otherwise the boundary layer size (which I would maintain is the most sensible transition zone extent) will remain the same as that in Schoof (2007b).

This is relevant because the paper focuses on p as the main control parameter in regularizing basal friction, when k is at least as important.

The second point that struck me was that the description of the regularization of basal friction was repeatedly described as being rooted in physics. This is true, but only to a very limited extent. I don't actually think that what is going on here is a particularly good description of water pressure and therefore effective pressure near the grounding line — the main thing that the model in the paper does is to ensure that τ_b goes continuously to zero, while it approaches a more canoncial form $\tau_b = C|u|^{1/n-1}u$ inland. This is done by making the "effective" sliding coefficient C_e that gives τ_b through $\tau_b = C_e|u|^{1/n-1}u$ depend on ice thickness through

$$C_e = C\left(1 + \frac{k|u|}{\rho_i g H\left(1 - \frac{H_f}{H}\right)^{pn}}\right)^{-1/n}.$$

The particular formulation for how C_e goes to zero has H_f is approached is almost neither here nor there, because the dependence of N on thickness is simply made up. In that vein, I'd be happier if the "parameterization" of effective pressure was simply recast as a "regualrization" of the transition in basal friction.

I could come up with many other simple models that look different, without even involving draiange or thermal physics. Why not for instance assume that water pressure is always equal to that in the ocean at the same elevation as the bed, which would give

$$N = \rho_i g H - \rho_w g b$$

b being downward-positive in the (slightly silly) sign convention of Schoof (2007a). This would have the same property of $N = 0$ at the grounding line. It would lack the arbirary

TCD
8, C345–C355, 2014

Interactive Comment

control parameter p, which would then be replaced by k. This is not to say that the version of N above is better than the one in the paper and should be implemented (though it seems more obvious) but the verbiage about things being physics based is perhaps a bit too strong.

Detailed points, apologies for any repetitions of the above:

abstract: in my view, the "transition zone" is best viewed not so much where ice lifts off the bed - that would be the grounding line, which in a depth-integrated model is not a zone but generally a curve, and "resolving" that curve is probably not what is meant by "Adequate resolution...". The transition zone is probably more sensibly defined as the region where an extensional-stress dominated flow (the ice shelf) transitions into a vertical-shear dominated flow (the sheet); this is the boundary layer defined in e.g. Chugunov and Wilchinsky (1996), Schoof (2007b). I don't see any other sensible definition of transition zone if it is a "zone" not a "line" (in which case it may as well be called the grounding line). The Pattyn et al (2006) "transition zone" is a red herring - it is a made-up length scale over which a transition from effectively no slip to free slip is regularized.

p 364 line 25: The phrase "Full-Stokes" should be struck from the glaciological dictionary. "Stokes flow" refers precisely to the equations Durand et al solve; anything else is an *approximation* of Stokes flow. Also "gold standard" is a weird concept — we're not building cars or houses here. "The Stokes equations contain the fewest approximations of all the widely used ice flow models" would be better (make no mistake; they are still an approximation.)

page 365 line 6: Bueler and Brown (2009) is actually not a shallow ice model but a hybrid between shallow ice and shallow shelf. I think it is misleading to refer to shallow ice here because - at least as far as I can tell - it is now well-established that a shallow ice model on its own (without some representation of what happens in terms of coupling with the shelf, for instance as per the work of Pollard and DeConto) does not give a

TCD
8, C345–C355, 2014

sensible representation of groudning line migration.

page 365 line 16: "The physically based parameterization...results in further reduction of this error, with the added advantage that the width of the resulting transition zone is essentially independent of model resolution." That did not make any sense to me, for two reasons. a) Define "parameterization". You've talked a lot about different models that differ through the degree of approximation in various stress components and the extent to which they can be depth-integrated and therefore be made computationally cheaper. Your "parameterization" is nothing to do with this. In fact, it is not so much a "parameterization" as a *regularization* of the transition from free slip to frictional slip that happens at the grounding line; it is that transition that leads to the extensional- to shear-stress-dominated flow transition referred to above b) "the resulting transition zone is essentially independent of model resolution" — if you have a convergent numerical scheme for a well-posed partial differential equation model, your solution needs to become indpendent of model resolution when that resolution is high enough. You seem to be suggesting that this is not the case without your regularization, implying that all previous models are based either on an ill-posed set of partial differential equations, or employ numerical methods that are not convergent. Presumably that is not what you mean to say, so please re-write this part.

p 365 line 23: " Both models have the drawback that the accuracy of the grounding-line dynamics strongly depends on grid resolution" As per the previous comment, this may be misleading. "The models have the drawback that very high grid resolution is required for convergence."?

p 365 line 25 ". A tolerance of a few kilometers in the grounding-line location requires a resolution on the order of tens to hundreds of meters" You should probably make it clear that this refers primarily to fixed grid models.

page 366 line 9 "...with the goal of reaching neutral equilibrium..." I am not sure what neutral equilibrium has to do with this; to my understanding there is no indication that marine ice sheets exhibit "neutral equilibrium" in the sense of Hindmarsh (implying a locally non-unique steady state grounding line position)

page 367 line 6 "Another type of basal water channel forms through pressure- induced melt. Channels of this type that form within 50 to 100km of the grounding line are also likely to connect to the ocean (Cuffey and Paterson, 2010)." I'm not sure what you are talking about here (a page reference to Cuffey and Paterson may help). Pressure-induced melt for a start is a thorny subject; all that pressure does is to change the melting point. The melting has to come out of assorted heat fluxes or heat sources — geothermal and frictional heating being the obvious candidates.

In fact, I would avoid talking at all about hydrology to the extent that the paper currently does, as doing so suggests you will actually be modelling the processes that control effective pressure (which would be untrue). As far as I can see, the main argument here is that you expect effective pressure N to be continuous up to the grounding line, where it is zero. This will ensure that the basal shear stress goes continuously to zero as the grounding line is approached, which makes the numerically difficult discontinuity in τ_b go away. Furthermore, this can be done in a way that the sliding law inland agrees with the sliding law used for instance in the MISMIP experiments. That argument could be stated in a single sentence (or maybe two) without getting tied up in extraneous physical processes and observations that actually raise more questions about the model in this paper (for instance, lake draiange is clearly non-steady and there is not necessarily a persistent hydrological syste; if there is channelized drainage, its effects on effective pressure at the bed could be quite localized).

If you do wish to persist with the present list of reasons, beware any spurious arguments about tides making your formulation more appropriate — if you want tides, you might have to start resolving the migration of the grounding line over the tidal cycle and its time-integrated effect on the evolution of mean grounding line position, which presumably starts to involve a rather complicated viscoelastic formulation. I'd want to stay well away from that.

page 367 line 28: "Based on this parameterization we give a new definition of the transition zone." - see comments at the start of the review regarding the meaning of a "transition zone"

page 368: I'm not a fan of the $\tau_l + \tau_d + \tau_b$ notation. Even though τ_l has units of stress, it is actually the divergence of a depth-integrated stress. Equation (2) also obscures the fact that we are looking at a second-order elliptic problem for u.

Also, the sign convention for τ_b is odd, as it requires the sliding law to be stated with a minus sign throughout the rest of the paper. I'd suggest switching τ_b to $-\tau_b$ everywhere to agree with standard glaciological usage.

pages 372/373: "When $H_f/H \ll 1$ and the bedrock is below sea level ($b > 0$), the fraction of the bed with water-pressure support approaches p" — how do you define "the fraction of the bed with water pressure support"? And why is it clear that p is equal to that fraction? Or is it simply convenient to interpret p as a fraction if it lies between 0 and 1? This is reiterated later (p 353 l 15), but if you want to stick with that characterization, you really need to explain it better.

p 373 line 16: . "Our model represents only the portion of water- pressure support related to the ocean; basal water pressure in the model falls to zero when the bedrock reaches sea level (b = 0). More sophisticated models of basal till find that the basal water pressure remains a significant fraction of the overburden pressure in much of the ice-sheet interior (Tulaczyk et al., 2000b; van der Wel et al., 2013). A more complex model might include a network of channels as well as water-laden till at the base of ice streams. This hydrological network would influence the basal friction through water-pressure support outside the transition zone."

This kind of misses the point. You are *not* modelling any of the processes that could conceivably control effective pressure, but simply imposing a form of N that ensures N approaches zero continuously as the grounding line is approached, and therefore basal shear stress goes to zero continuously. To pretend that the paper does anything more than that is disingenuous. Just state what you do, what you don't do, and leave it at that. Start mentioning papers like Tulaczyk et al etc and readers may legitimately ask questions about things like surges, the thermal state of the bed etc.

page 373 line 26 "This formulation does not require the introduction of an arbitrary length scale of basal transition, as in the parameterization proposed by Pattyn et al. (2006)" — Again, a bit of a fixation on the Pattyn et al concept of "transition zone", see comments at the start of the review.

page 374, line 6, "This simplified friction law leads to a set of equations with an accurate semi-analytic approximation (Schoof, 2007a), whereas the more complex friction law in Eq. (15) does not, to the best of our knowledge, lend itself to a similar semi-analytic solution." — see comments at the start of the review.

page 376 line 21 "The Chebyshev code produces grounding-line positions that match the semi-analytic solution of Schoof (2007a) to within millimeters (when the appropriate terms are neglected)." — you are about to repeat this in the next section, I would discuss the matter there. See immediately below.

page 378 line 6 "We configured our benchmark code with the same simplifying assumptions, and found that we were able to reproduce grounding-line positions from Model A to within fractions of a millimeter (the error tolerance of the Chebyshev solver). When we include the full longitudinal stress in the Chebyshev model, we found that differences with the Model A grounding-line position increased to $\sim$ 1km." Two points: i) The second result is actually more important than the first; you are demonstrating that model A works remarkably well for an asymptotic model in reproducing steady states of the depth-integrated marine ice sheet model it is meant to approximate. This is perhaps worth pointing out because some of the previous literature is rather confusing on this matter: Durand et al (2009) take a rather more negative view of the performance of model A based on its discrepancies with grounding line positions computed from a Stokes flow solver. What the result reported here shows is that that discrepancy is not primarily the result of the asymptotics done in Schoof (2007b), but because of the difference between a Stokes flow model and the depth-integrated approximation that is the basis for Schoof (2007b) as well as for the present paper. This is actually relevant for the interpretation of the MISMIP results in Pattyn et al (2012, 2013), where the sources of discrepancies between numerical solutions are perhaps not identified as clearly as they could — some are due to numerical error (which is avoidable) while others are due to different model formulations (which is unfortunate but cannot be improved on by better numerics) ii) It is not clear how the code is reconfigured "to the same assumptions" as in Schoof (2007b) — in that paper, there is no magic line at which extensional stresses suddenly go to zero; the whole point of an asymptotic solution is that extensional stresses naturally tend to zero when moving inland from the grounding line, and that a length scale for the associated decay can be identified. There is no "assumption" that they *are* zero inland however — just that they are sufficiently small to be neglected at a suitable order of approximation, which can be identified in terms of the relevant small parameter (ϵ in the notation of Schoof (2007b)

page 385, line 9 :"The largest errors occur near the local maximum in bed elevation at around $x = 1.25 \times 10^3$ km" — you might want to state here that this is completely predictable if you believe the Weertman (1974) argument about stability. A steady state grounding line near that maximum effectively signals that the system is near a bifurcation, at which the steady state near that maximum will be extinguished if A is decreased further (a kind of saddle-node bifurcation); in the vicinity of a bifucration, solutions will always be more sensitive to changes in parameter values (in fact, infinitely sensitive *at* the bifurcation)

page 366 line 12: "we have shown several advantages of a novel, physically based basal-hydrology parameterization together with an appropri- ate basal-friction law." — there is a certain point of view here that it's worth being clear about. The "advantage" corresponds to *assuming* different physics. It is true that the physics in all ice sheet models is presumably wrong, but to hail the approach here as an "advantage" is actually not to say "we have found a better way of solving a problem that has presented problems in the past" but to say "we have found a different problem that kind of looks the same but is easier to solve". Which is fine, but it's important to be clear about the difference. This also applies to the last sentence on page 386, where suddenly numerical method (applicable regardless of details in the formulation) are put on the same footing as actually changing the problem that is being solved.

This permeates most of the rest of the conclusions. At no point is there any mention of the need to figure out how basal hydrology actually works and how rapidly N changes away from the grounding line. Without any real knowledge of that, the present formulation has a semblance of better physics, but really (in my view) amounts to little more than a regularization of the discontinuous jump in basal traction — which is very similar to what Pattyn et al (2006) did, although with a bit more physics, and the advantage that sliding does not go to zero inland but rather that a canonical sliding law $\tau_b = C|u|^{m-1}u$ is eventually "reached".

Interactive comment on The Cryosphere Discuss., 8, 363, 2014.