

Interactive comment on “A confined–unconfined aquifer model for subglacial hydrology and its application to the North East Greenland Ice Stream” by Sebastian Beyer et al.

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General Comments

In ‘A confined-unconfined aquifer model for subglacial hydrology and its application to the North East Greenland Ice Stream’, the authors develop a new model of subglacial hydrology based upon the idea that subglacial water flow can be approximated as a layer of variably permeable material (a linked cavity system or an actual till aquifer) topped with a mostly impermeable cap (the glacier). The paper makes the claim that this approach has lower computational requirements than more explicit models and can

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simulate effective pressures suitable for forcing sliding laws in ice sheet models. Several examples illustrate model results, both synthetic (SHMIP) and inspired by reality (NEGIS).

Specific Comments

Model formulation

I appreciate the novel thinking showcased in this paper; it's always useful to approach an old problem in a new way, and by switching from the classical model formulation used in most contemporary ice sheet models to this new viewpoint, the authors certainly introduce a new way of thinking. Unfortunately, I think that this viewpoint lacks physical justification. The primary way in which this paper departs from previous subglacial hydrology modelling efforts is that differences in flux are accounted for by changes in the conductivity, rather than a change in the average cavity size. However, the equations for evolving cavity size (which are well understood from a theoretical perspective) are used to model the change in conductivity, with units made to match by simply multiplying by the conductivity. How is this justified? Without any theoretical justification, the model becomes strictly heuristic, and if this is the case, why is this model formulation any better than any other random model formulation that happens to achieve results that compare favorably to SHMIP? If the authors can provide such physical justification, I will happily withdraw this criticism. If not, then I want to see this point stated prominently in the paper.

There is a sign error in Equation 9: the creep term should be negative. Also, v_{melt} as written implies that melt is always based on a fully saturated aquifer. The b in that term should be replaced with $\min(b, h)$.

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Coupling to basal sliding

The chosen formulation neglects the coupling between sliding and hydrology. Most models of subglacial hydrology allow for the opening of cavities (and hence an increase in the effective transmissivity) by accounting for ice cavitation over sub-grid scale bedrock asperities. In contrast, this paper assumes that transmissivity (what I can only view as a proxy for the opening of cavities or channels) opens only by dissipative melting. This is problematic for several reasons. First, if the authors don't think that this is an important mechanism in making space for water to move around in below a glacier, then they need to say so. Essentially all work on this subject recognizes this as a major process, particularly in cases with significant sliding. Second, it is fairly well understood that in the continuum approximation, when this term is dominant, the problem always leads to runaway channelization, precluding the presence of linked cavities (hence the use of edge-based formulations for modelling channels in e.g. Werder (2013)). Why is this not a problem here, in both a numerical and a physical sense? Finally, the paper includes an extra parameters K_{min} , which makes it so that there is always transport capacity, and this is identified as a sensitive parameter. This parameter strikes me as a hack to solve a problem that would be solved in a more principled way by including a term that increases conductivity proportionally to sliding speed.

Transmissivity formulation

The principle variable entering the mass conservation formulation is h , defined as the piezometric head, or the potential relative to some fixed datum. This is fine, so long as the appropriate modifications are made when the head drops below the bedrock elevation (this does not seem to be handled at all in this model). However, the transmissivity $T(h)$ is formulated as if h were the height above bedrock. Either the transmissivity

should be

$$T(h) = \{ K (b - B), h \geq b; K(h - B), B < h < b; 0, h < B \} \quad (1)$$

and b should be redefined as the aquifer thickness plus bedrock elevation, or h should be redefined as the local height above bedrock and the mass conservation equation should be changed to

$$S_e h_t = \nabla \cdot T(h) \nabla (B + h) + Q. \quad (2)$$

Note that this error makes no difference when the bedrock elevation is uniformly zero (SHMIP, for example). However, it would lead to some very questionable results when there are significant variations.

Effective storage coefficient

Why does b appear in $S_e(h)$? if $h > b$, then the head is rising through glacier ice with permeability S_s (this is regularization to make the equation parabolic rather than elliptic, see Schoof (2012)). Why then would the head increase depend at all upon the thickness of the underlying aquifer? It is also worth noting that S_s as presented has units that don't make sense ($\text{Pa}^{-1} \text{m}^{-1}$), and that the porosity ω cancels out in Eq. 2. I also think that there is a misunderstanding with respect to the meaning of the effective storage coefficient. This is simply the void space in the aquifer versus in the glacier: it makes sense to say that the aquifer is more porous than the glacier (i.e. the head changes faster in a confined aquifer than an unconfined one), but not that more water is released from an unconfined aquifer than a confined one.

Discretization

Given that you're using central differences and forward Euler to discretize (not exactly revolutionary), this section could be moved to an appendix or supplement, or even omitted all together.

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What cannot be omitted is a discussion of stability under time stepping. In particular, the Courant-Friedrich-Lewy criterion imposes a time-step restriction for stability in the case of explicit time steps. For the chosen discretization, it is very small indeed. I would like to see some verification that the CFL is being respected. I suspect that it is currently not, which would provide a potential explanation for the obvious oscillations (i.e. checkerboard pattern) that appear in the results.

Also, how bad is the mass conservation problem? If a considerable amount of mass is being lost or gained, then this affects the validity of the results.

Parameter choices

The choices of ω and b are incompatible in most plausible scenarios for a glacier base. In the case of a sediment aquifer, then a value of $b = 10$ m is reasonable, but $\omega = 0.4$ (40% void space!) is not at all reasonable. Conversely, if the 'aquifer' is the linked cavity/conduit system, then $\omega = 0.4$ might be reasonable but $b = 10$ m is too large by an order of magnitude. A much stronger effort needs to be made to state the type of physical system that the model is supposed to simulate, and parameter values with regards to this need to be better justified.

Figure 3j

Is there any transport between channels? It doesn't seem like it from this figure. Shouldn't efficient channels reduce the pressure, causing water to flow in laterally, eventually leading proximal channels to merge?

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The use of ‘reduced ice overburden pressure’ as a comparison in this case is a bit of a straw man. As it appears in Huybrechts (1990), this refers to a water pressure given by bedrock elevation below sea level, which is only reasonable when very little basal or surface melt is expected (as in the Antarctic context for which it was initially used). For NEGIS, a much more defensible comparison would be that the water pressure is bounded below by sea level height, but that otherwise it is a constant fraction of overburden. Also, isn't $N_{CUAS}^{-1}/N_{HUY}^{-1} = N_{HUY}/N_{CUAS}$? Why the exponents?

On the definition of ‘improvement’

What constitutes an improvement per ‘we can considerably improve the velocity field in ISSM ...’? An improvement with respect to the eyeball norm? Or is it possible to be somewhat more quantitative, e.g. computing the misfit between these results, PISM, and ISSM without the hydrological model?

Technical corrections

P1L16 Citation needed

P2L1–3 Citations needed

P2L24 How do you know that this strategy captures the overall behavior?

P2L30 Should cite Schoof (2012) or Bueler (2015) or some other paper that discusses the implications of assuming that the system is always full

P3L20 This statement is not true in the subglacial hydrology literature

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P16L16 Citation needed

P16L32 Should be 'bed model of Morlighem (2014)' rather than 'data'. The results of a PDE-constrained optimization scheme are not data

P19L14 What does 'empirical nature' mean?

P19L17 Not sure that 'restitution' is the right word.

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