> I believe that the reviewers simply wanted more explicit mention of what is
> and is not represented by the model — and this is understandable because,
> while the assumptions and limitations of the model are quite clear to a
> reader who "speaks the language" of the manuscript, there is likely to be a
> large contingent of people who don't speak the language using the PISM
> hydrology model. Already PISM has a wide user community, and I expect it to
> grow substantially with the release of its hydrological component---and based
> on what I perceive to be a very large scientific community of researchers
> interested in subglacial hydrology, a very small portion of whom are familiar
> with numerical solution of PDEs, there is an ever-increasing need for this
> type of explicit clarity. Just as the authors are concerned about
> misconceptions when non-expert users write and publish high-impact papers on
> the result of this hydrological model---and they will---the reviewers are
> concerned too, and I share this concern.

One might suppose from this comment that it is never mentioned that conduits are not in the model. However, the draft in question (date 14 February 2015) already includes the following four sentences, listed with line number, explicitly stating that there are no conduits in the model:

95   Conduits are not included.

651 Two-dimensional models which include conduits \citep{Schoofmeltsupply} are
not reductions of our model.

658 Our model has no conduit-like evolution equations at all, though the
gradient-descent locations of characteristic curves from models using idea
(ii) may correspond to the locations of conduits in some cases.

1118 ... our model has no ``R-channel'' conduit mechanism, in which
dissipation heating of the flowing water generates wall melt-back.

That you have included these statements is not disputed in the least in the above passage.

The introduction and the conclusion, in particular, include statements to this effect. Generally good scientific articles say precisely what they do, not what they do not do. Our work, apparently, must follow other practices, as follows:

> Thus, I would like two things made more explicit:
>
> 1) In the paragraph beginning at line 95, the rationale for not including
> conduits is reasonable from a mathematical standpoint; however, it should be
> acknowledged that channelized flow does occur in Greenland, and is a
> potentially (I say "potentially" yet am confident it is more of a
> "definitely") important physical process to subglacial hydrologic transport

> that is not captured by this model. That applied mathematics has not yet
> produced a coarse-grained continuum representation of conduits does not mean
> it is not important.

We never claim, of course, that conduits are not an important process.

I did not claim that you do.

Rather we point out that existing models of conduits
are inadequate to the task of inclusion into a continuum theory of subglacial
hydrology in two dimensions. For this reason, they are not included in
our model, which does not, should not, and even cannot (because of
unknown-in-advance user choices), fix a lattice/grid or even a lattice-spacing
scale in advance.

It is apparent that the nonconformity of this claim is driving editorial
overreach here. There is a demand for a particular statement, one clearly
without consequence or real scientific content, as a condition for publication.

My request was regarding the introduction to the paper, and not other sections. In the previous draft Introduction, regarding conduits, there was discussion of what other *models* had done, and the reasoning for not including them, i.e. that they cannot be described by a continuum process. There was no mention of their importance, or lack thereof, to the physical processes being modelled. The reviewers' comments led me to believe that they hold conduits to be important to the physical processes being modelled. Therefore it was my opinion that a simple *brief* statement was absent from the introduction, and I saw as topic editor it was my responsibility to ask for this.

It seems we greatly disagree on this. The issue of editorial overreach is one on which I cannot comment as the authors seem to think there is a bias or an agenda held by the topic editor, and thus, as the topic editor, I cannot by definition argue this.

To proceed past this barrier, we have replaced the leading sentence on line
95, i.e. "Conduits are not included." with these two sentences:

  Channelized subglacial flow is widely-assumed to occur
  in Greenland, based on borehole and moulin evidence (Andrews et al., 2014,
  for example). This important physical process for subglacial hydrologic
  transport is not captured by our model because conduits are not modeled.

Thank you.

> 2) As I see it, eq 32(b) and eq 18 (and the "cap" for W_til) ...

For clarity, what is referred to here are the equation

$$dW_{til}/dt = m/\rho_w - C_d$$

and the inequalities

$$0 <= W_{til} <= W_{til}^{max}$$

> ... could be included with the ice dynamics component of PISM, and eqs 32(a,c)
> could be solved horribly incorrectly, and this would not impact ice dynamics
> in any way.

But it would impact mass conservation completely. If one only had (32b) and the constraint this would be the old, well-tested PISM model. In other words, this is exactly the context which explains why the *title of the paper* starts with the words "mass-conserving".

I am not arguing this point in the slightest.

This seems to be a complaint that our model of till saturation is directly tied to ice dynamics, through Mohr-Coulomb, while water in the linked-cavity network has a no influence on ice dynamics, though the ice dynamics influences the linked-cavity network. This is a correct description of our model.

I would have used the term “statement” rather than “complaint”.

As we point out repeatedly, our model is an extension of the Tulaczyk et al (2000ab), Le Brocq et al (2009), and Schoof et al (2012) work. None of these tie the cavity or drainage system pressure to the basal shear stress, in the first case because the model does not have such a system. In any case we do not know how to tie the basal shear stress to the P and W in our model.

Of course, the Tulaczyk et al (2000ab) work underlies the actually observation-supported connection between basal melt rates and ice stream flow, that is, the extended theoretical and field work done on the Siple Coast ice streams. That work was all based on the notion that there must ultimately be drainage of the subglacial water in excess of the capacity of the till. There is still no evidence that the pressure gradient in the at-the-time-unobserved drainage network affects ice flow. By contrast, there is much ongoing subglacial modeling based on ideas that cannot explain ice stream flow. We are trying not to abandon an effective model for the majority of subglacial-water-modulated fast flow (i.e. ice stream flow) just so we can model the latest popular curiosity, the small effect of dynamics on the slow warm parts of Greenland. (Note that fast outlet flow in Greenland is remarkably insensitive to Moulin input, especially on the scale of a one-month ice sheet model step.)

I am sorry you do not agree with me on the need for explicitness and transparency, but as I wrote before, I am simply trying to ward off misconceptions regarding the dynamics of the model when it is used by non-mathematicians, and I believe in being conservative on this front.

I reiterate, *“I am not saying this is *wrong*, or that hydrology can cavity size *should* affect basal stress”.*

Regarding whether (32a,c) are solved correctly, this is the first paper to bother to verify that the coupled solution is correct in more than one horizontal dimension, through the construction and use of an exact solution.

It was poor choice of words on my part to mention the notion that someone might solve 32(a,c) incorrectly. I was certainly not implying that you have done so.

> ... This is what I believe reviewer 1 meant by 1-way coupling. If I am wrong I
> must apologize, but I must be convinced. The authors emphasize that the
> velocity solve is nonlocal and so yield stress does not determine basal
> stress locally; this is accepted and besides to point I am making. I am not
> saying this is *wrong*, or that hydrology can cavity size *should* affect
> basal stress (though in reality I believe it could, see below), but I insist
> this is briefly pointed out in section 4 or 5.

It is a bit hard to respond to this comment. Three times we explicitly make the point that we are testing the model in a one-way mode:

1032 This nontrivial example demonstrates the model at large computational scale
  using real ice sheet geometry, with one-way coupling from ice dynamics giving a
  realistic distributions of overburden pressure, ice sliding speed, and basal
  melt rate.

1080 Thus only one-way coupling was tested: a steady ice dynamics model fed its
  fields to an evolving subglacial hydrology model.

Yes, followed by “a steady ice dynamics model fed its fields to an evolving subglacial hydrology model”, implying that this is not the typical mode of usage.

1179 The current paper only demonstrates one-way coupling, in which the PISM ice
  flow and thermodynamics model feeds basal melt rate and sliding velocities
  to the hydrology model. Two-way coupling will appear in future work.

The last two sentences, in particular, are not obscure. They are the last two sentences of the conclusion.

Yes, I agree, I had overlooked this last line of the conclusion and I apologize for claiming the direction of coupling is obscure, this is not the case regarding the statement in the conclusion.

The editor says "I insist this is briefly pointed out in section 4 or 5."
Presumably "this" is a statement that "cavity size [does not] affect basal stress".

Making a change in section 4 makes no sense because it is about closures to the equations for the evolution of the subglacial variables (i.e. W_til, W, P). The basal shear stress applied to the base of the ice is not closely-related.

So we have tried to state what is "insisted"-upon in section 5, at the end of subsection 5.1 as follows:

In this model the pressure P does not feed back to ice dynamics through changing the basal shear stress applied to the ice. Thus modeled cavity size, i.e.~the thickness W of the water in the linked-cavity system, also does not affect ice dynamics. Instead, as clearly stated in section 4, the yield stress τ_c is determined by the amount $\Wtil$ of water in the till. Under general conditions of significant basal melting, or surface input, so that $m > \rho_w C_d$, the second equation in system (32) causes W_til to increase up to its limit W_til^max. Ongoing significant melt then causes water to pass into the linked cavity system, at which point W generally increases according to the first equation in the system, and P evolves according to the third. Under these conditions the term $\partial \Wtil/\partial t$ is zero in the first and third equations because W_til is unchangingly at its maximum value. In summary, water input is first put into the till and then "cascades" into the linked-cavity system.

Thank you.

> Minor issues:

> l157: m is described as being either due to basal melt or to drainage from
> the surface. But in 2.3 I might be wrong but I believe it is referred to as
> coming exclusively from basal melt ...

Section 2.3 never mentions m or the concept of basal melt or drainage from the surface.

I deeply apologize, I do not know why I said 2.3 and this must have been a typo.

In any event I was seeking clarification on the question of whether basal melt and surface drainage might be treated differently, and whether this may have simply been overlooked since the experiments in this paper consider basal melting only. You have provided this. Thank you.

> ---otherwise it is implied that surface drainage goes directly into the till

Indeed that is what we imply.

> ---which I think is in contrast with the Tsai and Rice (2010) view of the
> process and others as well. Shouldn't drainage from the surface be a direct
> source to the hydrological system in addition to
> \rho_w(\partial W_{til}/\partial t - melt/rho_w)?

No, our model is not a good one to use to describe "a basal crack driven by the rapid drainage of a surface meltwater lake near the margin of the Greenland Ice Sheet", to quote the abstract of Tsai & Rice. Must our model be designed for this?

As the authors of the model and the paper I believe this is up to you, and I regret my mention of the hydrofracture process as I realize this is not the only process by which water reaches the bed.

> l532 harder, not hardest (which sounds awkward)

O.k. Changed.

> l533 confusing sentence; suggest to lead with the numerical advantage and
> follow up with the drawback, e.g. "By contrast, if \phi_0 is larger then the
> numerical solution of equation (31) is easier; but local changes in
> subglacial pressure P are damped and spread at the speed of influence to
> other parts of the connected subglacial hydrologic system."

Yes, it is an awkward sentence. Because numerical problems at the other extreme are already stated, "numerical" need not be mentioned in this sentence. We have changed to the simpler sentence:

By contrast, local changes in subglacial pressure P propagate to other parts of the connected hydraulic system in a damped way if ϕ_0 is large.

> Finally, I make a few comments regarding other issues I saw in the
> reviewer-author discourse. Action is not required, so to speak, but I urge
> the authors to at least consider them.
>
> 1) Reviewer 1 commented on the odd combination of a deforming-till basal
> stress parameterization with a cavity opening/closure hydrologic system
> evolution. The authors interpreted this to mean that cavitation does not occur
> on deformable beds, and maybe that is what was meant. But in their response
> the authors cited Schoof (2007). In this paper, the ice-cavity interface is
> treated as shear stress-free, ...

We are not experts on the stress boundary condition in water-filled or air-filled cavities. We do, however, presume that the *shear* stress applied to the base of the ice by the water or air in a cavity is very small. This is exactly the approximation also used for ice shelves, for example. In an ice shelf the contact area is certainly larger, and probably the water velocity is higher, and yet the applied shear stress is still ignored.

Thus we are not surprised that Schoof treats the ice-cavity interface as shear stress-free.

> ... and I do not know the literature well enough but

> presumably other models of deformable bed cavitation use the same treatment.

We don't know of other *models* of deformable bed cavition at all.

> ... I would think this would mean, as with hard-bed cavitation, that the
> effective coarse-grained basal stress would decrease with some measure of
> cavity size.

Why? The contact with the till (i.e. other than the cavities) could be quite strong because large cavities in a linked cavity system could more-effectively drain the till which contacted the ice. Weak till is not automatically tied to large cavities.

This is a fair point (albeit not one made in the section of manuscript that describes the effective stress treatment, which led me to inquire after it)

In any case, we are simply pointing out in our model that:

1) till is present in observations of the bed under glaciers and ice sheets

2) sliding over bumps and till can both credibly make cavities

Are these wrong?

No, but *I* was not commenting on their veracity, although I cannot say that Rev 1 was not questioning point (2).

> Such a dependence is not present in the yield stress formulation
> used in this model.

"Such a dependence" presumably means that the basal shear stress must be a function of cavity size. No, we do not have that in the model.

Thank you.

> 2) Reviewer 3 is right about the "corner case" mentioned, though he might not
> have worded it clearly enough. In the extreme case, if an infinite slab of ice
> of uniform thickness rested on a uniform layer of water of pressure that
> exactly balanced the overburden, I don't see what would be driving the
> (infinitely wide) cavity to close. Replace "infinite" with "very wide" and
> this is the case the reviewer referred to.

No it is not the case the reviewer referred to.

Note the VERY clear hypothesis that "P < P_o" in Reviewer 3's setup. This hypothesis is not accidental; the sentence describes a "decrease [in] input" so that the prior P=P_o case is converted to P<P_o.

This means there is a force. In particular, N > 0 and thus v_c(N,h) in Schoof's model is positive. On the other hand, v_o(h) is zero if you believe its meaning (i.e. opening of cavities is driven by contact of the ice with the bed; without ice/bed contact the cavities do not open). So the cavity closes.

Reviewer 3 very clearly says "This contrasts to Schoof et al. (2012) which keeps P = Po until W <= W_r." Reviewer 3 thus claims that P is somehow pushed back up to P_o in the Schoof model. We don't think that is true, because we are actually looking at the equations in Schoof et al. (2012), apparently.

You say "an infinite slab of ice of uniform thickness rested on a uniform layer of water of pressure that exactly balanced the overburden". That is not "P<P_o", and clearly not the case Reviewer 3 has in mind.

There is NO mention of the lateral extent of the cavity in Reviewer 3's comment. You have made up "infinite" and "very wide" out of whole cloth.

In *your* setup our equations (and Schoof's too) say that there is no closure of the cavity. In *your* setup with the explicit "pressure that exactly balanced the overburden" assumption, then N=0 and the closure term is zero. Our formula for opening also gives zero because of "(W_r - Y)_+" is zero, assuming in your setup that the water is thicker than the roughness scale (i.e. W = Y > W_r so W_r - Y < 0). So both (13) and (14) give zero and thus dY/dt = dW/dt = 0.

In *your* corner case, necessarily assuming no boundary conditions on the system so as to make sens of your case, our equations and Schoof's equations don't close the cavity.

I will not argue your contention that I misunderstood the “corner case” that Rev 3 referred to, and as I said this is not a major point.

> ... Furthermore, the authors say that
> "none of the models in the literature tie the creep closure rate to the
> roughness scale". Well, perhaps they do not call it "roughness scale", but
> this is really just semantics. Plenty of models account for the geometric
> arrangement of clasts and obstacles in the closure rate, for instance Creyts
> and Schoof (2009) and Kyrke-Smith, Katz and Fowler (2015).

This is a significant change of subject. These papers assume a thin film of water. We agree in this case that the film had better not be thicker than the clast size because then the old Walder argument applies and the film is unstable. But these models still don't tie the closure rate to the roughness scale, to my knowledge.

e.g. Kyrke-Smith, eq 2.7, in which l* will, at times, depend directly on the ratio H_c/l_o, which I would have referred to as a roughness scale, but I will defer this point as it is incredibly inconsequential at this point.

What is the point here?

To respond to a point made in an open discussion which I believed was problematic – not to influence the manuscript in any way. If it is not my place to do so I apologize.

> 3) As the authors point out in the manuscript and response to referees, the
> use of the fixed drainage term C_d is a straightforward extension of Tulaczyk
> et al's UPB model. It is not the only possible one, however; Van der Wel et
> al 2013 is cited solely as an example of one-horizontal-dimension drainage,
> but it is not acknowledged that their model is *another* possible extension,
> which takes account of vertical transport, something which this model does
> not do. Furthermore, I don't think anything is straightforward about this
> drainage term. In Tulaczyk et al 2000b (p485) it is stated that the mechanism
> of drainage is far from clear and while a constant parameter for drainage is
> assumed for simplicity, a mechanism that depends on void ratio (and by
> extension till interstitial water pressure via your eq 19) cannot be discounted.

We agree. Where did we "discount" these concerns? Where did we say that our version of the Tulaczyk C_d mechanism was the "only possible one"?

We *do* point out something that seems not to concern the editor. A more complicated till model is one of many, many ways to add more totally-unconstrained-by-the-available-data parameters. We prefer to reduce their number not increase it.

You do not say it is the only possible extension – and with the primary goal being a smaller set of unconstrained parameters I accept this is likely the best. As you say in the reviewer response, it will not have a strong influence in the Northern Hemisphere at any rate.