REPLIES TO ALL REFEREE COMMENTS ON *MASS-CONSERVING SUBGLACIAL HYDROLOGY IN PISM*

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We thank the reviewers for many thoughtful comments. Indeed, there are 19 pages worth of (formatted) reviewer comments below, and our replies take another 21 pages, yielding this long 40 page document.

As emphasized by the editor and some reviewers, the paper should be shortened, and we have done so. Specifically we replace section 6 and subsection 7.4[1] by brief text summaries. Table 3 and Figure 6 have been removed. Just as significant, exposition has been made briefer. As a result the two-column-format length of the text portion of the paper, i.e. not counting Tables and Figures but including References, has shrunk from 18.75 pages to 15.5 pages, a reduction of 17%. The accompanying `latexdiff` output is enormous because essentially every paragraph has been revised, and most shortened.

While the reviewers disagree, at times, with the way we describe our results, and with the processes we choose to include in the model, these reviews do not assert that anything in the paper is *wrong*. There is no assertion that our model is wrong in the sense of not being compatible with physical principles, deductively wrong, or that numerical schemes are inconsistent or nonconvergent.

Instead, the majority of the reviewers' comments amount to asking us to add process models and add commentary, or to change our conceptual picture to match theirs. This has been resisted. Furthermore, it is frustrating that none of the reviewers seem to be seriously interested in, or have apparent experience in, applying models of any type at whole ice sheet scale, which is clearly our emphasis. Our model is already a super-model of four (identified) important subglacial hydrology models in the literature, two of which are essentially always applied to ice sheets and not glaciers [21, 22], and we have demonstrated this super-model at unprecedented scale. We have a sense that this is all overlooked in the quest for conduits and true englacial storage, which we have good, and clearly-stated, reasons for not including. Regarding conduits, we make clear our intent to stick to continuum physics, as that physical paradigm is the only accepted one in climate and fluid modeling.

Scalability, so that the model can apply at high resolution to a whole ice sheet, and *configurability*, so that climate modelers inexperienced in fiddling with subglacial hydrology models can still use it, are goals which dominate the design of the model. These goals motivate many replies below.

Comments by Editor Goldberg.

- *There are now 3 very helpful and insightful reviews from 3 very qualified and industrious referees. I think that all their major concerns have merit, and I ask that you make efforts to address these concerns. There maybe a couple of typos in*

Date: February 12, 2015.

[1]Unless otherwise stated, numbering refers to the *old* version, not the revised manuscript.

reviewer 3's review, and I disagree that W_{eng} is unaccounted for (it just may need to be added to some early equations)—but there are some very good points made about the difference between your model and the Schoof 2012 model with respect to the regions where pressure is either overburden or zero. . . . I hope that you can address all of these concerns, as I expect this to be a very valuable addition to GMD.

We believe we have addressed the reviewers' comments. Furthermore, we have non-trivially revised the paper in useful ways for the reader.

While we appreciate the industriousness of the referees, we would also like to point out the narrowness of their expressed concerns. The history of ice sheet dynamics modelling suggests there is a huge difference between equations that might work at glacier scale versus equations that make sense at whole ice sheet spatial scale, on long time-scale integrations, and at high resolution, simultaneously. There is no evidence that subglacial hydrology modelling is, or will be, any different. One has to make nontrivial theoretical compromises to extend real physical principles to large scale. These compromises, and their motivations, are a major feature of the paper. Applications to which a scalable model might be put (esp. subglacial lake identification under ice sheets, and the modeling of the thermodynamically-dominated basal shear stress under ice streams) are indicated as motivation of our work in many places. The paper has "ice sheet" in the title and it was submitted to a "model development" journal which will have primarily a large-climate modeling audience, and we believe our model's scope and choices are highly-appropriate to this context.

- *I want to highlight something that Dr Bartholomaus mentioned, offhand that the coupling is essentially one-way, because melt rate affects N_{til}, and thus yield stress, locally and P/W do not in any way feed back on it. . . .*

In our model basal melt rate affects both W_{til} and W, through conservation of mass, and this results in changes in P. This causal direction is already very important for understanding hydrology. Exploring the consequences of hydrology at large scale under ice sheet has barely begun in the literature.

However, only the effective thickness of water in the till (W_{til}) has an immediate effect on the effective pressure on the till (N_{til}). Thus only W_{til} has an immediate effect on the till yield stress τ_c, and this key fact in the model is something we think is a feature; *our model extends the best-understood hydrology/dynamics feedback, which is the plastic bed till-based model of* [22]. Our conservative, i.e. drained, version of that model improves it, as opposed to just launching into an unknown region of parameter space. *Not* guessing wildly about how cavity and/or conduit pressures affect the plastic bed paradigm is a feature here, not a bug.

Indeed, our point in modeling till is that the actual evidence for sustained patterns of weak and strong bed under significant areas of ice sheets is based on till, and thus our use of the Tulaczyk et al. (2000) [22] model is highly appropriate. Our use of a sliding law is also physical and appropriate; see our responses to Dr. Bartholomaus' comments on this topic.

On the other hand, as explained in multiple PISM-related and other papers on membrane stresses, there are extensive non-local connections between the basal shear stress, i.e. on coefficient τ_c by the sliding law, back to sliding speed $|\mathbf{v}_b|$ and basal melt rate m. The last two quantities affect W_{til} and W and P in highly non-trivial ways. The coupling is already complicated, and adding complexity is not always desireable.

- *. . . This is why I asked initially if there was some way of allowing conduit pressure to influence till storage. I don't remember this being emphasized anywhere in the text, and that it should be. (this also bears on Dr Bartholomaus's comment on the mixing it is indeed odd for the ice flow to be opening up cavities, and yet the normal stress of the asperities not affecting basal velocity.)*

There are many ways to connect conduit pressure to till storage. Indeed, there being no observations to constrain such a parameterization, we could implement in PISM whichever one you liked, according to whatever dynamics you intend to generate.[2]

Regarding Dr. Bartholomaus' comments on "mixing" soft-bed and hard-bed concepts, there are two ways to answer. One is that the large hard-bed subglacial hydrology modeling literature ignors the presence of till at the bottom of every borehole drilled to the bed where the presence or absence of till can be assessed; we know of no exceptions to this rule and none are offered by reviewers. The other is that there is a perfectly reasonable expectation that cavities form in soft bed through a sliding instability [19].

Comments by Dr. Bartholomaus.

- *In this manuscript, the authors present a novel extension of the existing Parallel Ice Sheet Model that includes the most complete treatment to date of subglacial hydrology in a large-scale ice sheet model. Subglacial hydrology is immensely important in glacier dynamics, but is often neglected in the major ice sheet models used to predict future sea level rise. The computational expense of tracking changes in the rapidly evolving subglacial environment has generally prevented all but the crudest of parameterizations (see table 2 of Bindschadler 2013's summary of the SeaRISE experiment).*
  Thus, the present work is novel and worthy of publication in GMD. The writing is generally clear and fluent. Both the theoretical development of the continuum equations and the numerical implementation are clearly outlined.

We appreciate this summary and the comments below, which have improved the text.

- *Beyond these over-arching strengths, I have four critiques that I believe would significantly enhance the impact and accessibility of the manuscript. These four opportunities for improvement are below. My line edits follow these more significant points.*

[2]Acknowledging that this is a "snarky" reply, we feel this really describes the situation. A model that has till, and cavities, and conduits, and englacial storage has a lot of parameters. It can do anything you want. We are already at great risk this way, and it is time to stop adding parameters to our model and start trying to use ice-sheet-wide observations to constrain it.

—Four significant opportunities—
+ The authors offer some comparison between their model and those of Werder, Hewitt, Flowers, Schoof, etc., but these are generally smaller scale models that have yet to be implemented or applied at the ice sheet scale, and rarely to the complex geometries of existing glaciers or ice sheets. Some discussion regarding how the new PISM hydrology model compares with the hydrology models of other major ice sheet models, such as those discussed in the SeaRISE project would be very valuable. At present, comparison to existing ice sheet models is entirely lacking. Without much knowledge of these models myself, I suspect that the present model may represent a significant advance over the implementations in other ice sheet models. If appropriate, the authors may consider adding a sentence regarding this comparison to the abstract. Also, by way of review, please consider adding a table comparing features of presently-used ice sheet models.

We *do* compare to existing large-scale models, by describing and citing the work of [9, 14, 16, 21]. Such find-the-subglacial-lakes-and-drainage-paths modeling, which either uses an ill-posed version of our well-posed overburden-pressure-based `routing` model, or a balance velocity model, is the only whole-ice-sheet-scale work we know about.[3] We have also added a citation to [12], which describes the construction of a related hydrology submodel within the Community Ice Sheet Model, but which is applied only at the scale of a single idealized mountain glacier in [12].[4]

We believe it would be inappropriate, and a surprising use of space, to add a table comparing features of presently-used ice sheet models in a PISM model-description paper. It is already an inherent deficiency of model description papers that they can only describe a snapshot of an evolving piece of software. Snapshotting other software projects, many of which, unlike PISM, do not have open development heads, would only make this worse.

- *+ Considering that efficient, low-pressure conduits are such important features of the subglacial hydrologic system, some discussion/justification of why a model without conduits is useful is necessary. While consistent model behavior under grid refinement is certainly tremendously valuable, if one of the fundamental processes (i.e., transport of water in conduits) is entirely neglected, then all the model results may be called into question. The present model is still an improvement on the general lack of subglacial hydrology in existing ice sheet models, but ideally, conduits will be included in future generations of ice sheets.*

We discuss why a conduit model is not included in our model, and we have amplified our points in the revised version.

At present, *all* 2D conduit models are *not physics* by the normal standards of the field (e.g. the field "climate modeling" or "fluid modeling" or "continuum physics", according to taste). Note that "consistent model behavior under grid refinement," in the sense used by this reviewer, is normally called "continuum physics", and has been the standard for

[3] We care about this whole ice sheet scale application. The fact that we are building an improvement of the [9, 14, 16, 21] models is important to us, clearly stated in the paper, and never mentioned by reviewers.

[4] It has not yet been applied at whole ice sheet scale (S. Price personal communication).

physics since Fourier and Maxwell.[5]

We clearly state that the existing lattice models of 2D conduits can't work in a user-controlled large-scale ice dynamics model, and that for that reason we do not add them. That these models are useful for process exploration is not denied or disputed.

Linked-cavities could also be forced onto the nodes of a 2D lattice, but we, and *all* existing 2D models, do not put them on a lattice. That is because we (collectively) *do* have the PDE which describes the effect of a linked cavity system as continuum physics. We have added this point to the paper.

We encourage this reviewer, and the other reviewers who we also believe (by their questions) are interested in including conduits into subglacial hydrology models, to proceed in the normal manner of physics and attempt to develop a PDE description (i.e. a lattice-free formulation) of conduit effects. Or they can apply an actual conduit formation model to whole ice sheet scale, that is, one the causes a conduit to appear at the location where the data suggests it should, not where lattice location input data forces it. To complain that we have not invented the former ourselves, or made a model trillions of times more efficient than existing models so that we can claim the latter, is unfair.

That "all the model results may be called into question" is the normal state of affairs in climate modeling. But this phrase profoundly explains why we *don't* use lattice models. We will not risk having a user of PISM, in runs coupled to a GCM, have a reviewer of the results correctly point out that there was a single subsystem in the entire coupled mess which was not using the usual translation-invariant structures of physics . . . namely a 2D lattice model of subglacial conduits.

We completely agree that "ideally conduits will be included in future generations of ice sheet [models]".

- *+ This manuscript and model includes an ambiguous mixing of hard-bedded and soft-bedded ideas. For example, the model includes opening and closing of cavities at the glacier bed, driven by basal sliding (section 2.5). This is generally considered a hard-bedded view of basal hydrology and motion. However, the description of the Mohr-Coulomb yield stress for till (section 3.2) is appropriate for soft beds and basal motion accomplished by deformation* within *the till, not at the interface between the till and the glacier ice. Similarly, the sliding law that depends on the till's yield stress (section 3.3) is also a soft-bedded concept. The combination of soft- and hard-bedded ideas in this model appears to be inappropriate or at least confusing. . . .*

We are not quite sure why our mixing of hard- and soft-bedded morphology is "ambiguous". The equations are clear.

Though the reviewer may not have read it, we cite [19] which models the formation of cavities, by sliding, in a deformable subglacial till. This is precisely a "combination of soft- and hard-bedded ideas" for "opening and closing of cavities at the glacier bed, driven by basal sliding" in the sense used by the reviewer. We also cite [23] which uses till essentially

[5]Numerical model behaviour under "grid refinement", as used in the field of numerical approximations of continuum physics, and as normal in geoscientific models, is the very different concept of numerical convergence to the solution of a differential equation. It is directly addressed by our verification case.

as we do, combined with a conduit in a 1D model.

The literature of subglacial hydrology usually avoids including till in model-based exploration of hard-bed processes, but we can't find a single published (or unpublished) example of a till-free bed-reaching borehole in ice, and none are offered by reviewers. Because we expect, based on the observations that do exist, that the majority of the ice overburden pressure of an ice sheet, in non-frozen areas, is supported by saturated till, we include a model for its strength, namely Mohr-Coulomb.

Yes, the Mohr-Coulomb model for the yield stress for till is appropriate for soft beds and basal motion accomplished by deformation within the till. However, basal *ice* deformation may occur in a thin (meters) layer of temperate ice with high water and sediment content. This deformation, and also notional hard-bed sliding if it occurs, are all modeled in the current literature by power-law sliding relations. An ice sheet model, and the actual data available to constrain it,[6] cannot distinguish these mechanisms occurring close to the bed.

Finally, as stated in section 3.3, our computed yield stress value τ_c is used as a physically-meaningful coefficient in a power law for sliding [1], and such power laws are effectively regularized Coulomb stress models in the range of powers we use [18]. Having the coefficient of the sliding law be physically meaningful, and being tied to modeled basal water pressure so that it can physically evolve, is both conceptually and practically better then providing a sliding law with no physical meaning of, or physically-based way to model the temporal- or spatial-variation of, the coefficient.

- *. . . Furthermore, the description of 1-way and 2-way coupling could be more clear. If the rate of basal motion (u_b or v_b) is an input to the model, then why is there a section on the sliding law (section 3.3)?*

Section 3.3 is included exactly to give meaning to the yield stress τ_c as a submodel *output*, something clearly stated in several places, including Table 2. Because this is a model description paper for a submodel of PISM, we are obliged to state what the inputs and outputs of the submodel are.

One connection between $\mathbf{v}_b$ and τ_c is our hydrology model. The other is the whole ice dynamics model of PISM, which ice-sheet-modeler readers know takes boundary stress as an input and produces velocity as an output. This ice dynamics model is well-described in literature we cite.

As hinted above, in the most common use of sliding laws in ice sheet modeling, the coefficient in the sliding law has *no* physical meaning. It is often set by inversion of surface velocities, thus totally by-passing a process-based description of how it might evolve. We think that having mass-conservation for liquid water in the subglacial system, and using a physically-based computation of the coefficient in a sliding law, is a preferred situation.

- *+ It is interesting and surprising to note that you find an inverse relationship between water pressure and basal motion for systems at steady state. This is contrary to almost all prevailing sliding laws. Is [it] a result of the 1-way coupling (v_b that does not directly depend on water pressure)? Whatever the cause, it is sufficiently surprising to warrant additional discussion.*

[6] Esp. DEM, surface velocity, and bed elevation, but also, increasingly, isochrones and layer geometry.

We include an analysis of steady states because this analysis is not done elsewhere for the (now) standard linked-cavity system equations [10, 20]. Our first point is that these equations imply a functional form $P(W)$ at steady state, and thus that Flowers and Clarke [8] are not crazy to propose such.

Indeed there emerges a "inverse relationship between water pressure and basal motion for systems at steady state" from this analysis. Why is this surprising? Basal motion generates cavities (i.e. space for the water to fill), so the pressure drops. Said another way (as we do in the paper), sliding increases the opening rate, so if creep closure must balance it then the pressure will drop, to speed the closure, unless there is a simultaneous increase of water into the system (which does not happen at steady state). We presumed this observation, *which is not offered as a "sliding law" at all*, and indeed should not be used that way, was standard. In any case it follows from the equations, the reviewers don't believe our analysis is in error, and we have included it with the prominence we believe it deserves.

Presumably the idea is surprising because the reviewer believes in sliding laws. Regarding the idea that the sliding velocity "does not directly depend on water pressure", we remark that sliding laws usually relate the basal shear stress applied to the ice *and* the water pressure *and* the ice base velocity. Equation (25) in section 3.3 is thus a sliding law, and it is the one that we offer. It is utterly standard, except that it (appropriately) includes the Coulomb case ($q = 0$) and it has a water-pressure-linked coefficient with physically-meaningful units.

The presence of longitudinal stresses in the ice implies that there is globalized connection from water pressure back to sliding velocity, via (e.g.) stress boundary conditions at the boundary of the ice fluid mass (i.e. the glacier). This connection via a stress balance is outside of the scope of this submodel description paper, but is described at length in [5] and other citations.

Our analysis of steady states is not offered as a sliding law, and in that sense the reviewer is correct that our analysis "is a result of the 1-way coupling." That is, conservation of momentum in the *ice*—especially, the applied stress on the base of the ice—plays no role in the relationship. The relationship simply follows from equations (13), (14), (15) in their steady state cases, as clear in the paper.

- *—Line Edits—*
  p. 4706, l. 26 Also consider citing Walder 1982 if your purpose is to highlight some of the early work here.

We cite Walder (1982) on line 22 of page 4708. We use Creyts and Schoof at this point on page 4706 because only stable (i.e. viable) models of aquifer geometry are worth listing as alternatives which might go into a subglacial hydrology numerical model.

- *p. 4707 l. 5 I think the best reference for englacial porosity is Fountain et al. 2005, from Storglaciaren.*

These citations here are about models. We already cite Fountain later when describing observations.

- *p. 4708, l. 24 It may be worth mentioning that wall melt in linked cavities is generally expected to be small (Kamb, 1987 and Bartholomaus et al., 2011)*

We have added this comment, thanks.

- *p. 4709, l. 1 What are the ramifications of neglecting to model conduits? Many observational studies (including work by Nienow, Mair, Anderson, Cowton, Harper) have shown that efficient conduits are a fundamental component of subglacial hydrology. How will your model provide insightful and realistic results without a conduit component?*

We had assumed that models like [3], which seem to explain the behavior of (rare) well-observed hydrological-plus-glacier-dynamics systems without using conduits, were of some value, but we may be wrong.

Remember that the model is intended for whole ice sheet use. We want, therefore, a *well-posed extension* of the models used for identifying subglacial lakes [14, 21], and we want a *mass-conserving extension* of a successful (in terms of explaining surface velocity observations) ice stream basal stress model [1, 5, 22]. While these purposes are all quite prominent in the paper we actually wrote, they are essentially ignored in reviewer comments.

- *p. 4712, l. 17 It is not immediately clear to me why $\nabla H \gg \nabla W$. Where does this suggestion/observation come from?*

We have revised the relevant sentence to say: "If P scales with the overburden pressure P_o then the first term will dominate in the situation $|\nabla(H+b)| \gg |\nabla W|$". We no longer assert the situation is "common".

This comment is in the context of explaining why, for nearly the first time, we have included "$\rho_w g W$" into the formula for hydraulic potential. We presumed that the reason it is left out of nearly all prior literature is because that literature assumes that flow along the gradient of the (prior) hydraulic potential $\psi = P + \rho_w g b$ dominates over the gradient of the other part (i.e. the gradient of the added part "$\rho_w g W$"). This is the *only* case in which it would be acceptable to leave out the now-added part. Furthermore, it is widely-accepted in the literature—for example, in the review paper [6]—that gradients in P follow gradient in the overburden pressure $P_o = \rho_i g H$, so that gradients in P follow gradients in H.

So either: (i) all the prior literature is worthless (perfectly possible), or (ii) the case $|\nabla(H+b)| \gg |\nabla W|$ is worth considering as a way to relate our formulas to prior literature, and that is the spirit in which we consider it. Our model only *assumes* $|\nabla(H+b)| \gg |\nabla W|$ in a minor part of the given formula for the flux; see the next reply.

- *p. 4712, l. 23 If here you assume that $W \ll b$ or P, and thus can be neglected in eq. 8, why have you made the distinction in eq. 2 and the discussion that follows to include the W term?*

This question only makes sense if "you assume that W $\ll$ b or P" is interpreted as "you assume that $|\nabla W| \ll |\nabla(H+b)|$". We do not compare values of W (a thickness) to b (an elevation) to P (a pressure); we are comparing only gradients of distances to each other.

As noted, the simplification is for simplicity, in particular for simplicity in the final

implementation. Despite the simplification we *keep* the part of the flux proportional to ∇W, so the model is more complete than any other applied at ice sheet scale. Furthermore, the simplified model is always diffusive for any pressure closure, and so, in particular, the `routing` model is well-posed unlike the related models in the prior literature [14, 21].

The simplification occurs inside our formula for the effective hydraulic conductivity K, which is wildy-uncertain in practice anyway. That is, in the formula $K = kW^{\alpha-1}|\nabla\psi|^{\beta-2}$, which appears throughout the literature, the correct values for coefficient k and the powers α, β are all subject to minimally-constrained speculation.[7] We note that the simplification in question makes no difference at all if $\beta = 2$, the value used in half of the cited work.

- *p. 4713, l. 1 Near here, or somewhere else within the paper, please compare your values for hydraulic conductivity [k] with those that may be calculated from field observations. Are your values in line with those found in the field? Googling "subglacial hydraulic conductivity" yields several points of comparison.*

We have looked and not found. There is not a single observational paper we can find which argues that a directly-measurable value of the hydraulic conductivity is describing the average effect of a linked cavity system over the area of a grid cell relevant to this work (100 m to 5 km squares, say). Values are, of course, always given when these papers include a *model*—our Table 1 cites the default value of k as from [20]—but one should be very skeptical that a value from applying one model to the data (supposing this is done) is still the right value when applying a different model to the data.

Of course hydraulic conductivity for *till* is given in literature, based on specific *in situ* observational work. Such values appear in the literature we cite, and they dominate the results from Googling "subglacial hydraulic conductivity". But the till hydraulic conductivity value should not be used as k. The conductivity of till is so low that water does not move laterally through till in a time, and over distances, which could explain any of the apparent behavior of water under glaciers and ice sheets. Rather it is the macroscopic conductivity of the connected cavity network which is relevant. Such a network can be present even as there is sediment (till) lying around; this is the situation we are modelling.

To quote [3], which we already cite,

> *Each of these three parameters, γ, $[k =]C\tau_b^n$, and ϕ, is only weakly constrained by observations reported in the literature. ... $[k =]C\tau_b^n$ has units that depend on the exponent, and varies from $1.5 \times 10^{-5}\, m\, s^{-1}\, Pa^{0.18}$ to $1.1 \times 10^{-3}\, m\, s^{-1}\, Pa^{0.4}$ (Jansson, 1995; Sugiyama and Gudmundsson, 2004).*

Is this "weakly constrained" result the kind of "field observations" meant by the reviewer? Why should space in this model description paper be used to recapitulate such a weak and uncertain state of affairs? The source of the default value of k (Table 1) is, of course, cited, but we actually want to avoid asserting that any particular value of any constant is correct. This is because we are building the model so users can relate its relatively-few parameters, k among them, to rich, but often indirect, available data. As pointed out in our paper, "Darcy flux parameters α, β, k are also important [to the distribution of water thickness in

[7] We make this point in citations in subsection 2.3, which give a wide range of values for the same situations.

the model results]. Parameter identification using observed surface data, though needed, is beyond the scope of this paper."

- *p. 4714, l. 15 Here and nearby: define c_1, c_2, and A.*

  We have done so.

- *p. 4715, l. 6 Phrasing is ambiguous, as it makes it sound as though your model potentially does not include till water storage beneath some parts of the ice sheet.*

  The issue is that the majority (by area) under the world's ice sheets does not have *liquid* water under it, though it may have till. The equations for till storage, transfer into the transport system, and weakening of the saturated till, must all reflect the amount of *liquid* water there, not frozen water. We model frozen locations as not having liquid water in the till, so $W_{til} = 0$, and as being strong because N_{til} is small (from (23) and (24)). We have attempted to make this point clearer, without increased length.

- *p. 4715, l. 20 Why not include lateral transport of water through till if vertical transport is included? Till is often regarded as having an anisotropic hydraulic conductivity (e.g., Jones, 1993, "A comparison of pumping and slug tests. . ." in Ground Water vol. 31(6)). Horizontal conductivity can be at least several times greater than vertical conductivity.*

  The reason for not including horizontal transport is the standard fact of ice sheet modeling generally, a fact which is even more applicable here: the flowing layers in ice sheet models are *thin*. In particular, any till thickness ever given in the literature is 1/1000 (or less) of the lateral distances traveled by subglacial water. Anisotropy is irrelevant unless the horizontal conductivity can make up for this thinness. As the reviewer's figures suggest, the conductivity in the horizontal is *not* one thousand or more times the conductivity in the vertical.

  Of course, there is presumably a transport network of cavities, conduits, or thin sheets in addition, which has a low (lateral) macroscopic conductivity. We attempt to model the first of these morphologies because stable continuum physics evolution equations are available for it. The overall structure of the model is exactly what we believe is appropriate for water moving underneath ice sheets which have much of their overburden supported by saturated till: we model transport in combination with till storage, and the till is modeled as Mohr-Coulomb.

- *p. 4715, l. 20 Is m in eq. 16 the same as m in eq. 1? If so, these terms cancel out of eq. 1.*

  Yes, m in equation (16) is the same as in equation (1). Yes, they cancel out when $W_{til} < W_{til}^{max}$, so that no water enters the transport network (i.e. so that $\partial W/\partial t = 0$ in (1)) in that case. But we conserve water. Thus if the right side of (16) is positive and also $W_{til} = W_{til}^{max}$, at a given location, then $\partial W_{til}/\partial t = 0$, i.e. we put no more water in till, and the water goes into the transport network (W) according to equation (1).

We have attempted to clarify this logic here in section 3, and also in section 7 where numerical schemes are nailed down.

- *p. 4715, l. 20 If m/ρ is almost always bigger than C_d, then dW_{til}/dt is always increasing up to the cap W_{til}^{max}. It would be useful to lay this out more explicitly, and include eq. 21 in this subsection. Essentially, you have a Boolean relationship, where in some places there is wet till and other places the till is frozen. Is model sensitive to selection of W_{til}^{max}?*

Yes, when there is positive[8] basal melt, then m/ρ is almost always bigger than C_d, so that dW_{til}/dt is always increasing up to the cap W_{til}^{max}. The figures in section 9 reflect this.

Though we would not say we have a "Boolean relationship," we agree with the spirit of the reviewer's assertion. We repeatedly emphasize that we enforce inequalities including (21), the bounds on W_{til}. It follows that in some places there is wet till and other places the till is frozen; well-known reference [5] covers these ideas. No, the model is not very sensitive to the selection of W_{til}^{max}, at least in areas of substantial basal melt rate.

- *p. 4715, l. 24 Inclusion of C_d with fixed value is poorly justified and seems very ad hoc. Even if used by Tulaczyk, why is it necessary here and what is the model sensitivity to the selection of 1 mm a^{-1}? A constant rate of till water drainage into the subglacial hydrologic system, that does not depend on pressure gradients, seems very odd.*

We agree that Tulaczyk's [22] use of C_d is ad hoc. His model needs such a background loss of till-stored water, and ours too, but unlike him we have implemented a *conservation* model. We keep track of all the water globally, but we need previously wet areas which no longer have water input to not be eternally weak (i.e. permanently remain with till full of liquid water).

In areas resembling anything in the present-day northern hemisphere, with relatively high basal melt rates, the model is insensitive to C_d. In areas of very low melt rate (e.g. EAIS) there is a time-scale sensitivity. We have no time-dependent information about changes in EAIS subglacial melt rates with which to constrain values. The implication that we should use something complicated (which is the only alternative to "ad hoc" here) simply implies adding more unconstrained parameters.

If the reviewer has a 2D, data-supported, physics-based, applicable-at-large-scale model of how till and a linked-cavity (or other) system interact, then we hope he publishes that. We can't find it, and this paper proposes a simple alternative which arises from the only observation-supported literature which we know about which is related to these processes [22]. We seek relative simplicity and few parameters, instead of implementing process speculation.

- *p. 4717, l. 1 What is the effect of this choice? How was it selected?*

The value $\delta = 0.02$ is based on the observations that subglacial water pressure at the bottoms of boreholes, i.e. in till, have pressure within a few percent of overburden pressure.

[8]Note that the majority by area of ice sheets are assumed to have frozen base, so $m \leq 0$ there.

The particular value used means that fully-saturated till has water pressure which is $(1 - 0.02)P_o = 0.98P_o$. This parameter *is* very influential on sliding, and is explored the right way (i.e. by using lots of observations of surface velocity) in [1], using the earlier PISM model of non-conserving subglacial hydrology.

- *p. 4718, l. 16 I recommend changing the title of this section to "Basal motion relation" or some other phrase. "Sliding law" implies slip at the interface between the ice and its bed, whether bedrock or sediment, whereas your equation for yield stress (eq. 17) is appropriate for till deformation.*

Ice sheet modelers use "sliding law" the way we do, that is, Equation (25) is called a sliding law by all readers familiar with ice sheet modeling, the target audience of this paper.

Ice sheet models can't make the distinction implied by this reviewer. That is, there is no distinction in results in any existing ice sheet model between modeling slip at the ice-bed interface and a meter down within the till. Vertical resolution like this is only in the heads of process modelers.

- *p. 4718, l. 21 q is already used for flux (even if printed in bold-face to identify its vector character). I suggest using another variable name.*

Exponent q is used in prior literature, including [1], and in the PISM users manual.[9] There is consistent use of bold for vectors in the paper, thus the flux is $\mathbf{q}$ while the power is q, so no confusion will arise.

- *p. 4718, l. 23 Previously (eq. 14), v_b was the rate of basal motion. u and v_b are used inconsistently throughout the paper.*

This has been corrected. Only "$\mathbf{v}_b$" is used for the ice base sliding velocity, with "$|\mathbf{v}_b|$" for the sliding speed.

- *p. 4719, l. 4 What value of q have you selected for your simulations? Justification?*

Value $q = 0.25$ was used in the spinup that preceded the hydrology run [1]. The sliding law equation (25) is, as stated above, included so that the reader knows that τ_c is a model output and how it is used, so the particular q value is unimportant. More important content, explaining the meaning of the $q = 0$ and $q = 1$ extremes, is given instead.

- *p. 4720, l. 1 While "velocity" is technically correct, it is an odd choice for a thickness change. I suggest using "rate."*

Sorry. We mean that $\tilde{\mathbf{V}}$ is a velocity, not $\partial W/\partial t$. This has been clarified.

- *p. 4720, l. 5 Define h- the ice surface elevation.*

This is simply a typo. It should be H, the ice thickness. Corrected.

[9] Despite the content of all reviews . . . oddly enough we are actually trying to publish a model description.

- *p. 4722, l. 7 "... does not exist for tidewater glaciers or ice sheets." This may not be strictly true see Gulley et al, 2009, in QSR, where they report exploring many englacial conduits. In subsequent work, Gulley has mapped subglacial conduits. A safer statement would be that "vapor/air-filled cavities are not known to exist far from glacier margins." The distinction regarding tidewater glaciers or ice sheets is unnecessary.*

  The relevant sentence has been removed in the revisions which shortened the paper. As a general matter, however, we believe that vapor/air-filled cavities are not a feature well-supported by observations in a model intended for ice sheets.

- *p. 4722, l. 10 "observed in ice sheets and glaciers" instead of "observed in ice sheets"*

  We have clarified that we only mean ice sheets here by only citing Das 2008.

- *p. 4722, l. 21 Add that the englacial water table is intended to represent the mean over some large area of glacier, perhaps* > 1 *km*^{2}*. Here, it is best to avoid the extreme complications of, e.g., Fudge, 2008 in J Glac, where subglacial water pressures vary significantly over very short distances.*

  Our point is not that there is variation over any particular scale, but that efficient connection to the subglacier implies a close connection between subglacial pressure and the height of water englacially. This is not, fundamentally, contradicted by Fudge (2008). We agree that our (notional) englacial water table represents a spatial-average of the nearly-unobservable englacial macroporous network.

- *p. 4723, l. 8 You might add that we can expect phi to be large everywhere that* dP/dt *would be large (a highly fractured temperate glacier in coastal Alaska), and that phi would be small only where* dP/dt *is small (ice sheet interiors). Thus, even hydraulically/numerically "stiff" ice sheets shouldnt experience physical or numerical shocks.*

  Actually, we think dP/dt may be very large in ice sheet "interiors", namely during abrupt subglacial lake filling or drainage (observed in Antarctica) or moulin drainage of supraglacial lakes (observed in Greenland). However, we don't really expect the model to be good for either highly-fractured temperate glaciers in Alaska, or for modeling the temporal or spatial detail associated to these ice sheet dramas. Our point with englacial porosity regularization is that it eases the solution of a stiff problem.

- *p. 4724, eq. 34 As before, are these m's supposed to be the same?*

  Yes. See comment above.

- *p. 4724, eq. 34 This is an odd combination of equations, because the top equation is a component of the bottom equation, but the middle equation has not been incorporated in the bottom equation.*

Yes. This is "odd," but that is different from "incorrect," and the situation is complicated by very important bounds (inequalities) on the conserved variables. If the reviewer finds it incorrect (i.e. deductively incorrect) he should say so.

The context: As clearly stated in subsections 2.3 and 2.4, one can write either of two expressions for the flux, namely $\mathbf{q} = -KW\nabla\psi$ or $\mathbf{q} = \mathbf{V}W - D\nabla W$. Also, a term $\nabla \cdot \mathbf{q}$ appears in both the water amount evolution equation and the pressure evolution equation. The complications are that (i) we are indeed enforcing inequalities on W, W_{til}, P, and (ii) we want to handle the $\nabla \cdot \mathbf{q}$ terms by the same numerics in both equations in which it appears.

In this context, the theory is made most clear, given that we don't want to write a paper using variational inequalities which would only be understandable to mathematicians, when we describe the numerical scheme in section 7. After writing (and tossing) our paper several times with other expository choices, we find the current exposition most clear.

- *p. 4726, l. 3 Note that this is essentially the same as eq. 27.*

Yes. This redundancy in the exposition has been removed.

- *p. 4726, l. 23 Another connection is presented on p. 4721.*

Yes, we have noted this connection now.

- *p. 4727, l. 20 Around here, discuss that, in steady state, eq. 41 suggests that at water pressure decreases, the rate of basal motion increases. This flies in the face of most sliding laws. Can you offer any insight as to how we are to incorporate these two views in our understanding of hydrology and glacier dynamics? Is the one-way coupling of your hydrology model with a glacier dynamics model sufficient to gain insight?*

The reviewer has already made this comment above (page 6), and we address it there. In summary, we are *not* stating a sliding law and we *are* stating correctly-deduced consequences of well-known and widely-used equations (esp. equation (13) from [10]).

- *p. 4727, l. 20 Also note that P depends also on v_b, not on W alone.*

Here P depends on a lot of things, sorry. Our notation "$P(W)$" emphasizes that one can write the equation as a function which yields P given W, if all other symbols in the equation are defined. This is the usual convention in more than a century of exposition in theories using mathematics. A mathematician would presumably be equally-happy calling it "$P(A, c_1, c_2, W_r, P_o, |\mathbf{v}_b|, W)$," so as to precisely state the dependencies, but probably not the reader.

- *p. 4727, l. 23 I dont see the relationship between eq. 41 and the VW advective flux. Please elaborate.*

As noted immediately after this claim about equations (41) *and* (38), we explain it in the Appendix. It is nontrivial, so we explain it.

- *p. 4729, l. 5 Readers should not have to turn to the appendix to learn what s_b is. Move essential material out of the appendix and into the main text.*

  Good point. As noted at the top of this document we have replaced section 6 by a brief text summary, in section 5.4, and so this issue with s_b does not arise.

- *p. 4729, l. 6 Defining this new ω_0 variable seems unnecessary.*

  Yes. The relevant text has been removed, and the issue does not arise.

- *p. 4730, l. 5 What is the justification of the 5th power in the sliding speed?*

  In constructing an exact solution for the purpose of verification, specificity is essential, and qualitative reasonableness is of some importance, but uniqueness is not asserted or important. Here the power used is simple (an integer) and implies smoothness (because the power on $|\mathbf{v}_b|$ is 1/3 in the formula $P(W)$, smoothness requires that this power exceed 3). However, the reader is no longer bothered with the particular power because section 6 is replaced by a short text description of the construction of the nearly-exact solution. Figures 2 and 3 remain, which show the solution.

- *p. 4730, l. 17 Define what you mean by "under", "normal" and "over" pressure.*

  Yes. We have added this into the text.

- *p. 4731, l. 13 Give a few sentence introduction to the numerics here. The point is to discretize eq. 34. What is the order of calculations? What will feed into what over the next sub-sections of section 7? A thumbnail sketch similar to what is presented in 7.6 would be useful to guide the reader.*

  Our reordering of the exposition of the numerics now includes a brief introduction. However, precision requires defining most symbols *before* 7.6, where the important description of calculation order happens.

- *p. 4731, l .19 Near here, is it necessary for a model development paper to include a reference for "CFL" and "upwind"*

  We have defined "CFL" in the revised version. We believe that readers of this material will know what "upwind," "explicit", "finite difference", "centered," "second-order," "stability," and "convergence" mean, but in any case these are defined in the cited textbooks.

- *p. 4731, l. 21 Be sure to clarify that u and v are not components of v_b, but are for the water speed.*

  Thanks for the reminder. We have now defined u, v components at their first use.

- *p. 4732, l. 8 Parenthesis around the citation*

  Yes, got it.

- *p. 4736, l. 7 Is it important that the reader understand what it means for a scheme to be "flux-limited?" Without modeling expertise myself, Im not sure what this means.*

  Readers experienced with numerical advection schemes will know, but in any case the references to [13, 15, 17] are adequate.

- *p. 4742, l. 17 Because you report that your scheme is mass conserving so prominently in the abstract, you should report how much error is involved with step (x), where negative water thicknesses are discarded. This could be for the Greenland run of section 9.2.*

  The quantity in question, the error in step (x), is bigger than we would like. Quantitatively, in a run like the `distributed` run in section 9.2, the rate of loss of water from the subglacial hydrology into the ocean (esp. at outlet glaciers) is about 300,000 kg/s, the rate of loss of water onto ice-free land (i.e. rivers and proglacial lakes) is about 1000 kg/s, and the conservation error in step (x) is about 300 kg/s.

  The size of this conservation error is a result of the implementation of the *energy* conservation scheme at the ice base [2], not the hydrology scheme itself. Note that we only discard negative water thicknesses ($W_{i,j}^{l+1} < 0$) when, essentially, the energy-conservation-computed basal melt rate m is *negative*, i.e. in the refreeze case. The size of the discarded negative thicknesses thus has to do with the magnitude and extent of large negative basal melt rates, and not the quality of the hydrology model at all. The scheme itself is positivity-preserving.

  The size of this conservation error is thus not an issue in this model, but the fact that ours is the only paper which has ever even mentioned this quantity *should* be at issue. Noticing this error for the first time in a hydrology scheme is a feature not a bug. Figuring out the best way to reduce it, and demonstrating its reduction to zero under grid refinement, are for future research. But you have to notice it, or admit to it, before you can fix it.

- *p. 4745, l. 22 Are the 2800 processor-hours on each of the 72 processors or divided amongst the processors?*

  It is the total number of processor-hours in the computation, as is standard in describing parallel computations and when using the units "processor-hours".

- *p. 4746, l. 16 Do you specify a geothermal heat flux? The handling (or lack thereof) of geothermal heat should also be specified earlier, where the model setup is described.*

  We specify geothermal flux from input data to the model. The relevant Shapiro and Fitzwoller data is addressed in the SeaRISE description paper [4]. The "handling of geothermal heat" in PISM is documented carefully at full paper length [2]; it is nontrivial.

- *p. 4746, l. 18 Please report if you identified any basal freeze-on ($m < 0$) consistent with Bell et al., 2014, Nat. Geosci., vol. 7?*

  Why this particular insistence? As noted, we allow basal freeze-on. Comparison to all existing observations is not the role of a model description paper.

- *p. 4747, l. 25 Again, this is a good place to discuss the ramifications of a model without R-channels. What are the limitations of your model? Is there a way that aspects of R-channels emerge in your model without explicit channel modeling?*

Yes, "aspects of R-channels emerge . . . ". We say, about the `routing` model, on page 4747, line 25 of the submitted text:

> *The continuum limit of the model would have concentrated pathways of a few meters to tens of meters width. These concentrated pathways could be regarded as minimal "conduit-like" features of the subglacial hydrology. As noted in the introduction, however, our model has no "R-channel" conduit mechanism, in which dissipation heating of the flowing water generates wall melt-back.*

Similar text is in the revised version.

- *p. 4748, l. 2 What about the eastern outlet glacier results makes them particularly suspect? 7, C1774–C1781, 2014*

They are suspect because of the quality of the "bed elevation detail provided by the SeaRISE data set," as stated in the paper, given that the bed elevation field there is from few flight lines. This sentence has been removed, however, and only a note that the bed elevation detail from SeaRISE is "limited" remains.

- *p. 4748, l. 4 You report on the run time for your spin-up with the null hydrology model, but what are the processor demands for the distributed model described here?*

Good point. We have added the numbers, which are very small because we are not modeling ice dynamics, namely 14.2 processor hours for `distributed` and 14.7 for `routing`. As noted, the higher modeled water velocities and modeled diffusivities in the `routing` model decrease the time step, which implies more computation, but on the other hand the per-time-step work in `routing` is less, so the computational times are very comparable.

- *p. 4748, l. 5 Another statement regarding the sensitivity of results to W_{til}^{max} would be useful here.*

Though this model has a substantially-reduced number of parameters, relative to the model the reviewer would want, there are still far too many to examine sensitivity of all of these parameters.

- *p. 4748, l. 20 Around here, worth mentioning that pressure as an increasing function of W is vaguely in line with the results of the Flowers (2002) model, although your model reveals additional complexity.*

We already make this connection at four different spots in the paper. Additionally cluttering-up the description of our model results is, we believe, unnecessary.

- *p. 4749, l. 17 "seemingly-disparate"*

Yes, thanks.

- *p. 4750, l. 20 Again, reference the observation that steady pressure here increases as sliding decreases, which is inconsistent with almost all sliding laws.*

  As noted, we are not giving a sliding law, the inverse relation in question also applies to all the published models which have both cavity formation through sliding and cavity collapse through creep [10, 20, 24, and others], and this is not an important result of the paper.

- *Table 3 Odd to present the melt rate as a function of water density. Change this to a straight scalar (i.e., 200).*

  Yes. Table 3, and other details of the construction of the exact solution, have been removed, so the issue does not arise.

Comments by Anonymous Referee #2.

- *This paper describes a new sub-component of the open source ice-sheet model PISM, which accounts for subglacial drainage of meltwater. The model and a number of subcases are described in considerable detail and then the numerical implementation is described. A simple steady state solution is used to test the numerical method, and the model is then applied to the whole of the Greenland ice sheet.*
  I enjoyed reading this paper. It represents to my knowledge the first serious attempt to include an evolving subglacial drainage model within an ice-sheet scale ice-sheet model, and the results are encouraging. As such, I would like to recommend publication. However, I have a few issues that I think need to be clarified or thought about first.
  The major comments are here, followed by some specific but more minor points.

  We appreciate this summary of the paper.

- *1. The first term of (33), involving the pressure derivative and which represents changes in englacial water content, ought to appear in (34a) also, since this term derives from the mass flux into/out of the englacial system, and it is the addition of this term to the mass conservation equation (34a) that gives rise to its appearance in (33). As it stands in (34), subtraction of the first and third equations puts the $\partial P/\partial t$ term into the opening/closure equation $\partial W/\partial t$, which I don't see justification for.*

  As we state, we only have *notional* englacial porosity, because it is used as a regularization. The fact that this pressure derivative term does *not* appear in mass conservation equation (34a) is a reflection of the fact that we use englacial porosity only to cause the otherwise-elliptic pressure equation to become parabolic. Actual englacial storage, which would enter into the conservation equation, would require another mass variable (e.g. with notation "W_{eng}" as suggested by anonymous referee #3) and then more coupling parameters between subsystems,[10] and more inequalities to deal with refreeze cases, would be

[10]Both transfers between W_{til} and W_{eng} and transfers between W and W_{eng} would need to be parameterized. We wrote such a model and paper and threw it away.

required.

The divergence of flux term $\nabla \cdot \mathbf{q}$ appears in both the water thickness evolution and pressure evolution equations, in any model which has these three aspects: (i) mass conservation, (ii) cavity thickness evolution, and (iii) full cavities. Such models appear in [10, 11, 20, 24]. Thus we agree that "subtraction ... puts ... into the opening/closure equation ..." is correct in our equations, and those in the just-cited work too. Our use of this manipulation is correct even if the reviewer does not already do it, or think of it this way.[11]

In any case we cannot know what "justification" should be in the reviewer's head. If the reviewer asserts our deduction is wrong here, or anywhere else, then he/she should say so.

- *2. p4738, l11, and this section generally—is it clear that these arguments prove stability for the* system *of equations in this model (in which the coefficients in (60), say are varying at each timestep due to the pressure evolution)? The analysis here seems to be for a standard advection-diffusion equation on its own, but it is not immediately clear to me that standard results can be used here. I have no doubt that the method is stable, but I think if the stability properties are to be discussed in this much detail, it needs to be done for the whole system together, and not for the individual components of the operator splitting separately. Or if there is an argument as to why this is sufficient, that should be included.*

Yes, these arguments prove the stability of the numerical scheme for the particular equation in the presence of irregular coefficients which might come from coupling. We are not using a linearized stability analysis here,[12] an analysis which would "break" if the coupling is present, but a *maximum principle* analysis. Though it often gives overly-pessimistic stability conditions, one of the benefits of max principle analysis is that the details of the coefficients, including the possibility of them coming from coupling to other equations, don't enter into the analysis.[13] We suppose that the reviewer has seen "standard results" for advection-diffusion equations which come from a linear stability analysis, as such results are closer to necessary and sufficient conditions in the constant-coefficient textbook cases.

On the other hand, the case of negative source term, which in this case comes from outside the model (i.e. through the energy-conservation-determined melt rate m not through the involvement of coupled equations for P and W_{til}), requires enforcing inequalities, and this is not in our stability analysis, as we state. Furthermore, so as to shorten the paper we have removed the subsection in which the scheme is shown to have positivity-preserving and stable properties. We have replaced this subsection with a brief text description.

[11] In fact our equation (32), which is verbatim from [20] in the till-free case, mixes opening/closing processes and the divergence of the flux, and so the same manipulation would put open/closing processes into the mass conservation equation in [20].

[12] E.g. von Neumann or Fourier analysis [17].

[13] Thus the max principle analysis of stability of schemes for the three abstract heat-like PDEs $u_t = (1+x^2)u_{xx}$, $u_t = (1+u^2)u_{xx}$ and $u_t = (1+w^2)u_{xx}$, where w is the solution of another equation, is the same if one wants sufficient conditions to ensure a maximum principle at each time step. The *positivity* of the coefficient is important here for the analysis to work at all, and its *size* is important in determining a sufficient condition on the time step, but the *origin* of the coefficient is irrelevant.

- *3. The boundary conditions should really be described in more detail. It'd be helpful to state mathematically what boundary conditions are imposed (in section 5 say), rather than having it algorithmically described in section 7. In particular, the diffusive nature of the W equation suggests that one should apply some sort of conditions on W at all boundaries, but these are rather hidden, ...*

On the one hand, the situation is much worse than portrayed by the reviewer, and it is not primarily about what is inside this paper. On the other hand, there is no boundary in PISM in one of the senses meant by the reviewer. We now explain these statements.

Our model is, as clearly stated in the paper, subject to inequalities on water amount (i.e. $W \geq 0$) and on pressure (i.e. $0 \leq P \leq P_o$). These constraints imply that a mathematically rigorous description of the equations needs *free* boundary conditions to be determined by a variational inequality or similar weak formulation.

This is clearly understood *for the pressure equation* by Schoof et al 2012 [20]. However, [20] does not consider negative basal melt rate (i.e. the case $m < 0$ in equation (4.5) for the evolution of water thickness h_w in [20]). As a result they miss the fact that *both* evolution equations, i.e. for water amount W and for pressure P (our notation), are subject to variational inequalities. Indeed, diffusive or otherwise, the continuum equation for water thickness W, as stated in [20] or [10] or our manuscript or elsewhere, does not maintain positive values of W if m can be negative (i.e. refreeze), so a free boundary appears which we must deal with.[14] Roughly-speaking, this free boundary delimits basins where ice streaming can occur. A numerical scheme for W evolution must actively enforce the inequality in some way, such as by restricting admissible functions or by truncation/projection in an explicit scheme.[15]

Thus the only mathematically-honest treatment of our continuum model, or that in [20], would require coupled variational inequalities. As just one variational inequality is hard to handle—see [24], who say it is "prohibitive" in 2D and then skip it—the complications of a coupled pair are great. We actually believe we *are* correctly (i.e. convergently) numerically solving this coupled pair of free boundary problems by an explicit scheme which truncates/projects to enforce the inequalities, but we are not close to proving that. If we took on this topic with mathematical precision then (i) the paper would be enormous and (ii) no one would read it.

On the other hand, note that the periodic domain (i.e. flat torus) version of our model, or of the model in [20], would have no classically-defined boundary conditions because the domain on which the continuum model is solved has no boundary. For whole ice sheet simulations in PISM, the ocean or ice free land surrounding the ice sheet has exactly such a periodic extension, that is, no boundary. This has little disadvantage in practice, and it allows the advantage that every grid point in PISM, on every processor, has the same physics. We do state how all free boundaries are handled numerically—this is what we are doing "algorithmically in section 7", by stating where inequalities are enforced by

[14] It is a free boundary not seen in [20] primarily because there is no allowance for coupling to an energy-conserving basal melt rate, which would sometimes be negative. In this sense the pre-determined water input of most subglacial hydrology modeling is addressing an easier problem than ours.

[15] Our scheme for mass conservation is explicit, but also the scheme in [20] is explicit.

truncation/projection—but we don't have classical boundaries at which to apply boundary conditions.

In summary, we describe what the numerical scheme actually does in section 7.6. Then we show verification results in a case where the exact continuum solution, subject to the two coupled (but unstated) variational inequalities, with free boundary, is known. We think this is actually addressing the boundary conditions in a manner which is more helpful to the GMD reader than some more-mathematical expository alternatives.

- *. . . and in section 9.1 it is claimed that there are convergence issues associated with a jump in W, which seems at odds with the diffusive term.*

We have rewritten some text on how the boundaries in this model are really all free boundaries. In particular, in section 5.1 where we summarize the continuum model, we explain that in ice-free land and ocean (i.e. ice shelf or ice-free ocean) locations, the hydrology model sees such a large (in magnitude) negative value for m that any water which flows to, or diffuses to, that location during a time step is immediately removed. Thus we have a marginal jump in W in the nearly-exact solution under consideration in section 9.1.

The low regularity of the exact solution dominates the convergence rate, because the jump occurs along a non-grid-aligned curve. (In polar coordinates one could do a 1D computation in which the jump is added as a grid point. This would tell us nothing about the performance of our schemes in the presence of irregular source terms.)

- *. . . I suspect the boundary conditions are mostly imposed by step (vi) on p4742, but I was not entirely clear on what is meant by 'not computing' the divided difference contribution to the flux divergence.*

The sentence in question on step (vi) is simply wrong, and should not be there at all as it describes an old state of the code. It has been removed.

Equation (55) is used as stated at all grid points, regardless of neighbor mask state.[16] Thus the boundary conditions, which can all be interpreted as *free* boundary conditions and which are motivated by concerns listed in replies above, are applied in steps (ii), (vii), (viii), (ix), and (x) in the list given in section 7.6.

We have added the following ideas about boundary conditions to the revised text, in section 5.1, before going into detail about numerics: *(i)* PISM always has a periodic grid for whole ice sheet computations, so there is no classical boundary to the hydrology system. *(ii)* Free boundaries occur all over the place as a result of enforcement of inequalities. *(iii)* In ice-free land and ocean (i.e. ice shelf or ice-free ocean) grid points, the hydrology model effectively sees such a large (in magnitude) negative value for m that any water which flows to, or diffuses to, that location during a time step is immediately removed. *(iv)* The ice-free land and ocean grid points have pressure determined by external factors (e.g. atmospheric or ocean-base pressures).

[16] See method `raw_update_W()` in file `PISMRoutingHydrology.cc` in branch `stable0.6` of the PISM source code.

- *Finally, I felt the paper might be shortened without losing detail; there are a number of places where the discussion of relatively simple points is laboured. Sections that might be reduced include section 2.4, section 4.3, section 6.2, section 7.1, section 9.2.1, (could just reference Aschwanden et al for much of this?), section 9.2.4, and the appendix.*

  With this comment we heartily agree, and we have reduced length by 17%. Regarding the specific recommendations, we have shortened section 2.4, improved 4.3 without much reduction, removed almost all of section 6.2, but mostly-kept 7.1. We have shortened 9.2.1 and 9.2.4, removed Figure 6, and emphasized [1] as a reference on our spinup procedure. We have halved the length of the Appendix. In addition we have made substantial reductions in section 5.2, we have removed sections 6.1 and 7.4 (replaced them by a sentence or two), and we have substantially shortened sections 7.2, 7.5, and 8.

- *Specific comments*
  1. p4708, l3, also throughout - I do not see why the parabolic equation is always described as a 'regularization', which suggests some element of artifice. For the physical system described, the equation is parabolic, and there is no need to treat it as a regularization.

  The reason we call it a "regularization" is that we do *not* have a conserved variable for englacial water amount, namely "W_{eng}" in the language of referee 3. Whether or not the parabolization effect of the englacial term is a "regularization", our englacial model is thus not complete. We use the englacial porosity coefficient as a way to make computations with our explicit scheme faster. Because our pressure equation is parabolic, we can do explicit time-stepping and enforce, by truncation/projection, the inequalities on all the state variables. The reader is allowed to believe there is an element of artifice if she wants, but "regularization" is appropriate.

- *2. p4708, l7 - I'd temper this by saying that till is 'sometimes' observed, as I don't think it is true that it is always observed.*

  Though all reviewers objected to our categorical language here, none offered counter examples. Nonetheless we added an expected weasel word to the revised text.

- *3. p4708, l20 - it is not the inclusion of wall melt in the mass conservation equation that leads to the instability but rather [the] inclusion of wall melt in the kinematic opening-closure equation.*

  In a model where the subglacial aquifer is always full (i.e. "$W = Y$" in our model), there is no distinction between these ideas. The cavity volume is the mass, and both are conserved. Thus the comment also applies to the "normal pressure" case in the model in [20, 24], for example.

- *4. p4711, l9 - given the coupling with PISM, it seems a bit odd to say that you 'accept' the hydrostatic approximation, since you should be calculating P_o*

consistently with the ice flow. As I understand it P_o is always hydrostatic for the level of approximation in PISM, so this would seem a better justification.

It is perfectly reasonable, and clear, to describe this as "accepting" an approximation which we make just for our subglacial hydrology model. Someone using a Stokes ice dynamics model could accept, or not accept, this simplification of the hydrology model.

- *5. p4713, l11, also throughout - I find the repeated reference to the 'advection-diffusion equation' a bit misleading as although it has advection and diffusion terms, it is rather different from what is normally associated with that term, as the velocity depends on the pressure which is evolving simultaneously. Perhaps this is my own connotation of advection-diffusion, but I think it should be emphasized that (12) is not stand-alone and is inherently coupled to more equations.*

  This is mystifying. There is a large literature of numerical advection-diffusion equations, and *all* of it implicitly or explicitly assumes that the numerical recommendations therein should apply both in uncoupled and coupled circumstances. Nobody seriously models advection in a case where the velocity is given by God, though all textbooks start that way. Of course motion comes from other, coupled equation(s).

  We agree that many *proofs* of qualitative properties, or convergence of numerical schemes, only hold in the uncoupled case. But good advice for constructing numerical schemes, coming from careful analysis in the uncoupled case, should be used when working numerically in the coupled case, unless there is a coupled analysis which is more informative.[17] The point of this text, clearly made, is that the advection-diffusion separation allows us to carefully choose numerical schemes for the two terms in the mass conservation equation. Since the same divergence of the same flux appears in the pressure evolution equation, we use the same numeric split there too.

- *6. p4717 - the prescription of a minimum value for N seems a bit arbitrary—could it be explained briefly what this physically represents? (e.g. this is the level at which the till becomes sufficiently deformable that a cavity system is developed and that effectively caps the water pressure?) I would have thought a critical pressure, rather than a critical fraction of overburden, might be more reasonable? ...*

  Broadly speaking, the reviewer seems to be saying that he/she does not have a plan for modeling the manner in which the effective pressure in the till should not reach full overburden pressure. Note that till water pressure has been observed to not reach full overburden pressure in those boreholes through ice sheets which have been drilled to the base.

  We do have a plan, and we explain where it comes from (esp. Tulaczyk), and derive specific equations because otherwise there is no model. The manner in which this "critical pressure" arises is modeled nowhere in the literature of distributed subglacial hydrology systems, that we know of, and the reviewer offers no suggestions either. But we have constructed a highly-simplified, very-few-parameters version which is supported, though indirectly, by comparing its results to observations at the surface of the ice [1].

[17] What a nice thing to imagine! I know of no examples.

- *. . . That aside, I found the prescription of W_{til}^{max}, and subsequent derivation of till thickness η (22) rather odd, since it seems more natural to prescribe the thickness of till η and have W_{til}^{max} derived from that (and δ and P_o). As it is, η varies as the overburden varies (when coupled with ice flow), so that there is implicit redistribution of sediment.*

We are actually quite clear that the pore void ratio and the water amount in the till are proportional; see equation (20). We are then quite clear that, because we are enforcing inequalities on the conserved quantity W_{til}, we prefer to parameterize the maximum capacity of the till by a maximum water amount value. It is trivial to choose to specify the till (mineral part) thickness instead of W_{til}^{max}, if the reader so desires.

Whether or not the reviewer likes our way of parameterizing the lower bound on effective pressure, as scaling with the overburden, in fact equations (18) and (20), which come from in situ observations of till [22], imply a relationship where η varies as the minimum effective pressure varies. Indeed, redistribution of till may be the way a minimum effective pressure arises.

Again, our choice is not asserted to be wrong but "rather odd" and less "natural". It is hard to argue against such criticism.

- *7. p4721, (30) and following sentences - it is a bit confusing to write $P = P_{FC}(W)$ here (and in (29), and similarly in the appendix), as the formula depends upon P_o and therefore space, as well as on W. It'd be clearer to include x as an additional argument here ((30) is not then a clean porous-medium equation).*

Reviewer 1 wants us to emphasize that $P(W)$ in our steady state is actually $P(|\mathbf{v}_b|, W)$, and reviewer 2 wants us to replace $P_{FC}(W)$ with $P_{FC}(P_o, W)$. But our notation for functional dependence is used in the normal way of applied mathematics.

The reviewer believes the phrase "clean porous-medium equation" is reserved for the constant-coefficient case? To avoid this, we have written "generalizes the porous medium equation" now, for the relevant equation.

- *8. p4723, l5 - this sentence reads rather strangely. Aren't most of the parameters 'user-adjustable'? What is meant by temporal 'detail' in the pressure evolution - is it suggesting that $\phi_0 = 0$ is 'correct'? Later that paragraph, what is meant by diffusive 'range', and would it not scale as $\phi_0^{1/2}$?*

Most of the parameters in climate models are only barely user-adjustable; it is standard to require recompilation if there is a parameter change. In PISM this is a runtime adjustment only, through either a command-line option or a configuration file.

In any case, our point is that users of the model in computationally-challenging coupled circumstances (e.g. high-resolution, ice-age-duration simulations of whole ice sheets coupled to GCMs) can choose to lose (i.e. smooth out) temporal resolution in the model but thereby gain performance. In other words, we are emphasizing that the *tradeoff* is user adjustable, not just the parameter. Emphasizing the "user" aspect of this is relevant.

The "correct" value of ϕ_0 is a silly concept; the macroporosity of the near-base parts of ice sheets is, and will remain, nearly unobservable. This is one more reason why we call

the way we include this parameter a "regularization." We have removed the concept of "diffusive range" from that part of the text, as one of many shortenings.

- *9. p4723, l16-22 - this algorithm is certainly a lot more computationally efficient than the method used to solve the elliptic variational problem of Schoof et al (2012), but it should be noted that the schemes are not solving exactly the same problem (at least, for non-steady states, which is where the computational cost lies). Difficulties of Schoof et al's method stemmed notably from discontinuities in W associated with unfilled cavities, which are absent in the current problem.*

This is mystifying. By no means do we assert that [20] is solving "exactly the same" time-dependent problem. There are many points in the paper where we distinguish, most prominently: (i) We say in section 4.2 that we assume full cavities and there draw a contrast with [20]. (ii) We have a large section 4.3 on the englacial regularization, which clearly states that this is a change from [20]. (iii) We then say in 5.2 what specific changes would convert back to the [20] model.

Furthermore we don't say "this algorithm is certainly a lot more computationally efficient than the method used to solve the elliptic variational problem of Schoof et al (2012)." It would be tedious (read "it would be a math paper") to analyze the algorithms so as to perform this comparison, supposing we had enough detail from a 2D implementation of [20] to do so. The comparison would involve the efficiency of the numerical choices used in solving the nonlinear elliptic problem in [20], presumably including Newton solver and iterative linear algebra choices.

What we do show is an actual example at a much larger computational scale, not to mention in 2D, than offered in [20]. However, the differences are from different continuum models *and* different numerics. Because the 2D implementation in [24] does not actually bother with the elliptic variational problem of [20], we can't compare apples to apples at all.

- *10. p4727, l6 - I'm not sure how much we know that the system is close to steady state 'much of the time', so I'd recommend removing this; justification for looking at steady states is probably not required.*

Thanks! We have removed the wordy justification for looking at the steady state equations.

- *11. p4728, l1 - clarify that this statement is for a given discharge?*

This statement was justified by a specific argument in the Appendix, but the whole concern is to subtle. We have removed the statement and its justification to save space.

- *12. p4729, l11 - I am confused by the 'solution' $W = W_r$ to (45). This would only be a solution if the ice surface were a very particular shape?*

The idea that the ODE in question had a constant solution, which was not clear relative to the statement of equation (45) anyway, has been removed as part of our major reduction of the description of the nearly-exact solution.

- *13. Section 6.2 - the discussion of the boundary conditions here seems unnecessarily confusing and it could be much clearer just to state the shape, sliding velocity, and boundary conditions that are used, rather than explaining in generality how the solution works. Note that W_c has only been defined in the appendix so comes out of the blue here. Since $r = L$ is the edge of the domain, the distinction between L_- and L seems pedantic (the definition of variables outside of the domain has not yet been given, and is more of an algorithmic issue).*

  Again, this text has been removed. We agree it, and the material in the Appendix, were not well-factored.

- *14. p4731, (48) - φ_0 is ω_0 ?*

  Again, the relevant text, and the symbol ω_0, have been removed. (And the answer is "yes"; it was a typo.)

- *15. p4731, l7 - presumably the numerical value for W^* given here corresponds to a particular parameter set? It must depend upon k, H_0 etc?*

  Yes, the numerical value of W^* depended upon particular parameter values listed in Table 3. But the text in this section 6.2, and Table 3, have been removed. The nearly-exact solution is described in words and then pictured in Figures 2 and 3.

- *16. p4736, l20 - the right hand column here seems unnecessary?*

  Yes. We have simplified these equations by removing the right-hand column.

- *17. p4739, l25 - The numerical values of timesteps here and on p4732 could be brought together to save space and avoid repetition. The value of ϕ_0 used seems rather large; if a smaller value were used (going towards the elliptic limit) might the timestep restriction become restrictive?*

  The time step restrictions have been brought together, as suggested, in the revised text.

  We agree the value of ϕ_0 is rather large. A smaller time step would cause more expensive computations, which is exactly the point made earlier about the "user-adjustable tradeoff". In the text in question, our point becomes stronger with a smaller value of ϕ_0: the time-step for the pressure equation is controlling for the coupled scheme.

- *18. p4748, l15, and figure 11 - I was a bit confused by the comparison of W and P/P_o; what significance is P/P_o believed to have? Doesn't a lot of this information come just from the steady state relationship between W and P in (A4)?, The caption is a bit confusing when it refers to 'pairs' (W, P), but what is plotted is really P/P_o.*

  Yes, a lot of the information comes from formula (A4), but we want to *show it*, not just state it. Showing a figure with various cases makes the multi-parameter dependence in (A4) clearer to the reader, we believe.

  Because P is always bounded above by P_o, and because creep closure of cavities is a function of the difference $N = P_o - P$, we want to clearly show areas which are close

to overburden. Because the thickness of the Greenland ice sheet, and thus P_o, is highly variable on our 2km grid, the easiest way to show the areas where the pressure is close to overburden is to show P/P_o. We have fixed the caption to refer to $(W, P/P_o)$ pairs.

- *19. p4749, l9 - what is the 'actual diffusivity of the advective flux'? 'diffusive nature of the advective flux' might be clearer.*

  Yes, that is a better rewording, and we now use it.

- *20. p4749, l15 - this statement is rather vague, and I'm not sure what it's trying to say.*

  We have simplified and improved the relevant text. In particular, the two-item un-numbered list on lines 14–19 of page 4749 has become the following single sentence: "The reasonably-comprehensive exposition here also clarifies the relationship among several pressure-determining "closures" (section 4), and it allows us to understand our model as a common extension of several seemingly-disparate published models (section 5)."

- *21. p4751, l17 - something missing from this sentence?*

  Right. It should not have "is that". The totally-rewritten sentence now says "In any case, in the current paper we do not impose a relationship $P = P(W)$ at all, though such a relation emerges in steady state."

Comments by Anonymous Referee #3.

- ***Summary of the manuscript***
  The manuscript (MS) describes a novel subglacial hydrology model implemented as part of the PISM ice sheet model. To my knowledge, this model together with the model of de Fleurian et al. (2014) (Elmer/Ice) are the currently most complex hydrology models included in large scale ice sheet models. The hydrology model consists of a cavity-like layer which can conduct the water horizontally, and two storage components: a till layer and an englacial aquifer. The coupling to ice flow would be through the yield strength of the till, which in terms [sic; "turn"?] depends on the amount of water stored (although no two-way coupled runs are demonstrated). The model performs well on test cases with analytic solutions and on an application to the Greenland ice sheet.
  The MS is very detailed and describes the mathematical model, some analytic solutions, the numerical implementation and some test applications. The MS is suitable for publication in GMD after the comments below are addressed.

  We appreciate this summary but want to make some comments.

  While the de Fleurian et al. (2014) [7] model is included in the ice sheet model Elmer/Ice there is no evidence whatsoever that it applies at "large scale" as implied by the reviewer. The paper [7] itself applies it only to a single mountain glacier, and does not give an estimate of the number of degrees of freedom in the hydrological system; we suspect it is more than an order of magnitude less than in our application.[18]

[18] Scale is not everything, and the application in [7] may be a very good one. But not "large scale".

Next, we do not have "englacial storage," although this is clearly desired by the reviewer (below). As our manuscript states in a variety of ways, we use the regularization effect of an englacial network to make the pressure equation better behaved, but we do not have conserved degrees of freedom describing the englacial water, which would be "W_{eng}" in the reviewer's notation. Thus we do not have as large a space of unknown transfer coefficients as would such a theory.

- ***Mathematical model***
  My main comments are that water in englacial storage is not accounted for, that the statements $0 \le P \le P_o$ and $W = Y$ are inconsistent, and that boundary conditions are omitted. Further, in the mathematical sections it is never explained how in detail the bounds on P and also W till are enforced, although it can be deciphered from the later sections on numerical implementation. Also the authors mention that their pressure regularisation is necessary to allow enforcing $0 \le P \le P_o$ (by projection). Why is this so? Why could this not be done using the elliptic pressure equation?

Yes, the water in englacial storage is not in the model, thus not accounted-for.

The statement $P \le P_o$ and $W = Y$ are of course consistent, and also apply to the model [20], so we presume that the concern is with the combination $0 \le P$ and $W = Y$. In this case, what does "inconsistent" mean? Inconsistent with a physical principle—none is named—or not well-posed in combination with the other equations—we disagree—or just inconsistent with the imagery in [20]? In any case, it is hard to respond to this vague claim.

We have attempted to improve, and bring earlier, the presentation of boundary conditions. In particular, we note the inequality $W \ge 0$ which implies additional free boundaries not addressed in the literature. We handle such free boundaries, and the ones which arise from enforcement of pressure bounds, by a common, documented scheme.

We never assert "pressure regularisation is necessary to allow enforcing $0 \le P \le P_o$ (by projection)". Rather, we enforce these inequalities on the solution of a regularized (thus parabolic) version of the unregularized (elliptic) equation in [20]. These same inequalities are enforced in [20] for that elliptic equation, by restricting admissible functions in a variational inequality formulation. The enforcement of inequalities is thus an independent issue from regularization.

- *Mass conservation (Eq. 1, 34a) should also take into account W_{eng}, the equivalent layer of water stored englacially:*

$$\frac{\partial W}{\partial t} + \frac{\partial W_{till}}{\partial t} + \frac{\partial W_{eng}}{\partial t} + \nabla \cdot \mathbf{q} = m/\rho_w. \qquad (1)$$

  In particular, for the void ratios ($\phi_0 = 0.01$) considered in this MS the W_{eff} [sic; presumably "W_{eng}"] term is important. For instance, a relatively small pressure difference of 10 m water head leads to a change in W_{eff} of $0.1m$ which is on the order of W. In fact, having $\phi_0 = 0.01$ is probably beyond what may be considered a regularisation (i.e. having negligible effect on the solution), and the MS should be updated accordingly.

In our model the conserved mass is $W + W_{til}$, not $W + W_{til} + W_{eng}$, as desired by the reviewer. This reduces the number of parameters and inequalities to which the solution is subjected. Instead we only use the parabolicization effect of englacial porosity to make the pressure equation less stiff. This is very clearly-stated in the paper.

- *If possible, it would be nice to state the bounds on the various state equations more explicitly, e.g.:*

$$\frac{\partial W_{till}}{\partial t} = \begin{cases} m/\rho_w - C_d & \text{if } \dots \\ 0 & \text{otherwise} \end{cases} \qquad (2)$$

Or if that is not possible, state the bounds next to the equations.

This is a good point. We now state the bounds immediately after the evolution equation.

- *For the pressure, according to the numerics outlined in section 7.6, the authors solve Eq.33 on the whole domain for P and then project/update P such that $0 \leq P \leq P_o$ (except where $W = 0$ also $P = P_o$). Therefore $Y = P$ [huh?; sic; presumably "$Y = W$"?] is only true in the so-called "normal-pressure" regions, which should be stated. In the overpressure or underpressure regions the authors instead use the mathematical closures $P = P_o$ and $P = 0$, which should also be stated. Also, it seems that the pressure equation is solved for the whole domain using boundary conditions at the edge of the domain, which is in contrast to Schoof et al. (2012). This difference needs to be discussed in a section about boundary conditions.*

We are not quite sure how to respond to this comment, which seems to ignor what we have written and instead be a re-argument for the model in Schoof et al. (2012) [20], which has never been implemented in 2D. In summary, our cavities are full ($Y = W$), our pressure has bounds ($0 \leq P \leq P_o$), and we believe our model is well-posed and correctly implemented, and the reviewer seems not to be pointing to any evidence to the contrary.

We have improved our text in one related way, namely clearly defining "underpressure" as $P = 0$ and "overpressure" as $P = P_o$ at the first use of these phrases to describe our model results. The underpressure and overpressure regions in the model results are not using new closures as stated by the reviewer. The closure is still "$Y = W$" but the continuum model includes the inequalities. Where there is no water ($W = 0$) we set either $P = 0$ or $P = P_o$—we have clarified this—so as to determine pressure gradients at boundaries numerically, but this is not a "closure", as the water in such locations is absent.

- *Even apart from the storage term (which the authors acknowledge), the presented scheme is not quite equivalent to the one in Schoof et al. (2012): To determine the regions where pressure equation needs to be solved (Eq.34c in this MS) Schoof et al. (2012) uses constraints on W and not on P (see their equations 4.1, 4.7 and 4.11). In the region where the pressure equation is solved, Schoof et al. (2012) uses appropriate boundary condition to link to the adjacent regions. Also in underpressure regions Schoof et al. (2012) solve both for Y and W (their h and h_w).*

As we write above in reply to the previous reviewer, by no means do we assert that Schoof et al. (2012) [20] is solving an "equivalent" model. There are many points in the paper where we distinguish, most prominently: (i) We say in section 4.2 that we assume full cavities and there draw a contrast with [20]. (ii) We have a large section 4.3 on the englacial regularization, which clearly states that this is a change from [20]. (iii) We then say in 5.2 what specific changes would convert back to the [20] model.

The reviewer is actually wrong, however, that "Schoof et al. (2012) uses constraints on W and not on P". It is very clear in [20] that constraints on P are used to define the convex space of admissible functions on which the variational inequality acts, and W only plays the role of an input into the resulting weak problem. In this sense our model is more like [20] than portrayed by the reviewer.

In any case a 2D version of the [20] is not solved by the numerical work which those authors actually pursued, i.e. in [24], because they were apparently unable to do so. Indeed our $Y = W$ assumption is exactly as in [24], but in this case we *do* enforce the pressure bounds that are used in [20].

- *To illustrate the impact of the different models, here [is] a pathological case which (I think) the mathematical model of Schoof et al. (2012) handles fine but the one in this MS less so:*
  Starting with an initial, steady state with a region where $W > W_r$ and $P = P_o$. Decrease input into that region until $P < P_o$, i.e. something like a draining subglacial lake. Now (as far as I understand the equations in the MS) W in that region would evolve according to Eq.13, i.e. shrink by viscous creep (unless $P < 0$ at which point it would again evolve according to Eq.34a). This contrasts to Schoof et al. (2012) which keeps $P = P_o$ until $W \leq W_r$.

Ice creep is sensitive to stress (i.e. pressure differences in this case) not whether the ice is in contact with water. Thus we say *yes*: once $P < P_o$ then the cavities *should* start to close even if the cavity height is greater than the roughness scale W_r. Indeed, none of the models in the literature tie the creep closure rate to the roughness scale. In the absence of other factors, which relate to horizontal gradients in hydraulic potential, and to water inputs, this will have the effect of driving the pressure back up to P_o.

We think the reviewer wants instantaneous action at a distance, a property of the [20] model, to instantly set P to P_o in this $W > W_r$ case.[19] All that the reviewer actually is saying, as far as we can tell, is that $P < P_o$ and $W > W_r$ would violate the equations in [20], which we are not even sure is actually true.

We are not asserting that our model is equivalent to [20]. We think that this "pathological case" may even make our model look better as it does sensible things to initial states which would, apparently, be rejected in [20]. We think we do roughly the same thing to this initial state that would occur in the model of Werder et al (2013) [24].

[19] This is the way differential-algebraic systems like [20] work. We point out in the paper the numerical difficulties of differential-algebraic systems, but one could also note the physical undesirability of pressure "wave" propagation at infinite speed.

- *Not getting this and other corner cases right is not bad and still results in a great subglacial hydrology model, in particular for the application intended here. However, Schoof et al. (2012) gets them right(er) (as far as I understand) and thus the authors' claims that they successfully solve that problem should be a bit more qualified (see line-comments below).*

As above, we can disagree that we get this corner case wrong.

We don't claim to "successfully solve" the model in [20]. We do claim that our model is a "common generalization" of four models, of which [20] is one, and we precisely clarify what we mean by this phrase. But readers do not expect that changed equations give the same solutions.

- *Other comments*
  The manuscript is quite lengthy and could do with some streamlining. Among others, Section 4.1 and 5.2 should be merged, Section 9.2.1 should be shortened and Fig. 6 removed.

We agree that the manuscript should be shortened, and we have done so. However, merging 4.1 and 5.2 is undesirable because our explanation of what closure we *put in* to the model (section 4) is very different from our text on what behavior *appears from* our relatively-comprehensive model (subsections 5.2 and 5.3). Section 9.2.1 has been shortened, and Figure 6 has been removed, as suggested.

- *It would help if the authors would state the unknown variables at the beginning of the mathematical description.*

This will be greatly helped by the placement of Table 2 in the published version (if that happens). We have moved the first mention of Tables 1 and 2 to the start of section 2.

- *The authors mention frozen conditions but never go into details about them. What happens to the cavity sheet and till layer when input is negative? What does the water pressure do? What do the cavities and thus W do? In fact, the evolution equation for Y does not contain a melt/freeze term so $Y > 0$ even when frozen. How does this link to setting $P = P_o$ when $W = 0$ (p.4742 l.4). This should warrant at least a paragraph.*

"Frozen conditions" can only be handled by considering conservation of energy. We (appropriately) cite [2] for the two-phase conservation of energy model in PISM, which, in particular, determines the basal melt rate under an ice sheet. We furthermore note that, unlike other work, can handle a negative basal melt rate and thus we must actively enforce $W \geq 0$ for the distributed system water thickness.

In summary, there is no separate thermodynamic variable for the temperature of the till. Since the till (i.e. not cavities) makes up the vast majority of the base of the ice sheet, by area, adding such a variable would be the primary way in which the model could made more complicated and physical. The equations we present for hydrology modeling are independent of thermodynamics except through their dependence on the (signed) value of the melt rate m and the meaning of the inequalities $W \geq 0$ and $W_{til} \geq 0$. Our formula for

till yield stress implies that un-saturated till (i.e. with small values of W_{til}) is quite strong; this is the content of the model in [22], which we cite. We believe we have already supplied adequate information on these points to the reader in the revised text.

- ***Comments by page and line number*** *(add 4700 to the page number):*
  p.6 l.8 State how many parameters are used

  Other papers do not do this. We will not either, because the number of parameters can be made small or large by deciding on the meaning of "parameter".[20] We prefer that the reader see Table 1, which can be compared to similar tables in other papers, if the reader wants a sense of the relative number of parameters between models.

  Far more relevant than the number requested here is the content of section 5.2, where we show that by setting particular parameters to particular values we get specific published reduced models. This gives a reader a clear, practical sense of the "number of parameters."

- *p.6 l.8 Instead of "We use englacial porosity as a regularization, and we preserve physical bounds on the pressure." write "We use englacial porosity as a regularization to impose physical bounds on the pressure." But in fact, I am not sure this statement is right, as bounds on the pressure are enforced by projecting it onto* $0 \le P \le P_o$.

  In fact we do *not* use "englacial porosity as a regularization to impose physical bounds on the pressure". We have changed the relevant sentence in the abstract to say simply "We preserve physical bounds on the pressure." The separate technical fact that we have regularized the elliptic variational inequality model of [20] to a parabolic model, using notional englacial porosity as a regularization constant, is now not mentioned in the abstract, though it is fleshed-out and clear in the text. The reviewer gets to the correct fact—in our explicit scheme the bounds on pressure are enforced by projecting—and we have made this clearer in the revised text.

- *p.6 l.21 reword "reasonable"*

  We have written "Any continuum-physics-based dynamical model" to replace "Any reasonable dynamical model".

- *p.8 l.4-6 This is not quite right, see my Section above.*

  The sentence in question *is* quite right. It is simply true that "The subglacial water pressure solves an equation which is a parabolic regularization of the distributed pressure equation given in elliptic variational inequality form by [20]." Compare equations (32) and (33) in the till-free case.

[20] Is the acceleration of gravity a parameter? It is adjustable in PISM, because Mars ice models are encouraged, but [7] does not list it as a parameter with a value while [24] does. In our paper symbols "e_0" and "C_c" appear, but only their ratio e_0/C_c appears in our actual model (and in [22]); do we count this observation-based value as one parameter or two?

- *p.8 l.19-24 Maybe this paragraph should be moved to start at line 7.*

  We have done so.

- *p.8 l.29 Whilst no mathematical proven [sic; "proof"] of convergence of grid-based models is available, they do seem to converge under grid refinement in a statistical sense (see appendix of Werder et al. (2013)). Also, their parameters are independent of the grid. Thus automatic grid-resolution determination should be possible.*

  The Appendix of [24] in question is wrong. It contains only evidence for the opposite of what it attempts to sell.

  It is started by the correct sentence "The solution produced by a numerical scheme for solving partial differential equations should converge to the true solution under mesh refinement." The second sentence is "However, for the presented model convergence in this sense will not be satisfied as the mesh forms part of the solution by restricting potential channel locations ..." Now that there is no PDE, we don't know what they are looking for as "convergence;" no meaning of that word is given though it is then used.

  Indeed, they show next that they have *not* made progress toward a PDE because their evidence shows their model *does not* converge as would a solution to a PDE, even statistically. Their figure of results (Figure A1) shows that as the mesh distance decreases, and so the number of channels increases, the maximal discharge of any channel converges to around 100 m^3/s, the level against which they then report a "convergence rate" of "$O(\Delta x^2)$".

  But convergence of edge fluxes to a non-zero amount is not a property of a numerical solution of a 2D PDE model. When the number of mesh edges increases, in such a model in which fluxes are along edges, the maximum flux must decrease to zero. Specifically, for a 2D, flux-conservation PDE problem solved by a structured-grid method as here, the number of edges crossing a fixed line[21] is proportional to $1/\Delta x$ as $\Delta x \to 0$. The total flux across that line should converge to whatever amount is given by the continuum solution, so the flux through each edge should converge *to zero* at rate $O(\Delta x)$.[22] The evidence given is actually adequate to make this statement: The parameters in the model do not currently scale so as to generate a PDE limit.

  Thus we are in a bad situation, made worse by additional text in the same Appendix and now by the reviewer's assertions. First, the final sentence of the Appendix is an evidence-free claim of prospective performance: "This variability should decrease further once real topography is used and is unlikely to be larger than the errors of field measurements." Second, the reviewer now implies that what is missing from [24] is merely a proof of convergence,[23] but this is apparently only indirection; we never come *close* to saying it was a lack of a "proof" of convergence that caused us to not implement conduits. Third, the claim by the reviewer of additional prospective "automatic" numerical performance, without evidence, is unfortunate given the available evidence.

[21] I.e. "at $x = 5\text{km}$".

[22] If the PDE solution is irregular then the maximum flux though any edge might converge at an even slower rate like $O(\Delta x^{1/2})$, but if it does not converge to zero then there is no PDE solution.

[23] "Whilst no mathematical proven of convergence ..."

Reviewers # 1 and # 3 want us to buy into this idea of using a 2D lattice model of conduits in a scalable ice sheet model, and the only offered evidence of this even being possible, *much less the right modeling choice for a model to be applied at every point of an ice sheet*, is actually evidence for the opposite view.

- *p.9 l.8 "closures" here and elsewhere can be confused with "creep closure," reword.*

  While we don't think this is a likely reader confusion, we have put quotes around "closure" here, so that the reader looks to the referenced section 4 for the meaning of the word.

- *p.9 l.19 It would help to briefly introduce which processes will be described and in particular which are the unknown variables (or major variables as the authors call them later).*

  We have moved the first mention of Table 2, which categorizes major variables into "state", "input", and "output," forward to this point. The introduction states what processes are involved.

- *p.10 Eq.1 add a term $\partial W_{eng}/\partial t$*

  No. As noted, we do not conserve water held englacially.

- *p.10 l.9 it is not quite clear what "the two-dimensional subglacial layer" is. Presumably it is the layer which has thickness W.*

  We model the base of the glacier as a two-dimensional surface. Furthermore, in common with the literature, we have a *thickness* for subglacial water, which makes the model two-dimensional. The conserved quantity $W + W_{til}$ is a thickness, the flux follows the ice base (instead of going into the ice or into the ground), and the equations for all variables only involve x and y derivatives; these are all the usual meanings of "two-dimensional".

- *p.10 l.18 Specify that the pressure P is at the top of the water layer too.*

  No. This is not what we mean, nor what is meant by the pressure variable in other literature (e.g. p_w in equation (1) of [24]). In such 2D pressure equation models one can regard the pressure variable as a vertical average of the pressure, but note it does not increase with thickening of the layer alone, which it would if it had the meaning implied by the reviewer. This is why we add "$\rho_w g W$" to our formula for the hydraulic potential, because (in common with all the literature) we have no vertical profile of pressure in the distributed system, but (not in common with the literature) we want subglacial lakes to spread-out as they physically would.

- *p.16 l.8 write "and $N_{til} = P_o - P_{til}$ is the effective pressure of the overlying ice on the saturated till . . . "*

  We don't feel we need to introduce the new symbol P_{til} only to eliminate it in this way. The cited literature adequately defines the phrase "the effective pressure of the overlying ice on the saturated till."

- *p.16 l.10 Should be "previous section" but specify section number instead.*

  Yes. Fixed.

- *p.16 l.19 I find N_0 confusing. The very similar looking subscript "o" in P_o refers overburden but the "0" is something else. Maybe N_r or N_{ref}?*

  We do not use symbol "N_o" or other things that could be confused. We are using here the notation from [22], and we would like to keep that correspondence.

- *p.17 l.8-16 What follows in this part is unclear. Reformulate of [sic] this introductory sentence "On the other hand we will describe the maximum capacity of the till by specifying . . . " to prepare the reader that instead of working with δ you change to W_{til}^{max}.*

  The sentence in question now says "On the other hand we specify a maximum W_{til}^{max} on the water layer thickness, . . . ", to bring forward the symbol W_{til}^{max}.

- *p.17 l.10 Should this not just be $W_{til} < W_{til}^{max}$. The lower bound is never used, or is it?*

  The lower bound $0 \le W_{til}$ is most-certainly used. We observe that almost all of the literature, in situations like this, fails to note this inequality. In fact, if the source terms can be negative in a mass conservation equation for which the conserved variable is a *thickness* then the model must actively enforce the fact that the thickness is nonnegative. Within our paper we have also made this is made clearer with respect to the thickness W_{til}.

- *p.19 l.10 For this section the Y equation is not needed/decoupled. That should be mentioned.*

  This is precisely what the sentence on lines 11–12 says: "We first consider two simple closures which appear in the literature but which do not use cavity evolution Eq. (13) or similar physics." Equation (13) is the $\partial Y/\partial t$ equation.

- *p.20 l.22 comma after "consider."*

  The sentence in question now says "At an almost opposite extreme, our second simplified closure makes the water pressure a function of the amount of water." This avoids the issue.

- *p.22 l.10 Expand here (or maybe elsewhere) on how $P \le P_o$ is enforced.*

  We now state how this inequality is enforced in both sections 5.1 and 6.5 (new numbering).

- *p.23 l.15-22 This paragraph is a bit misplaced in this section. Maybe the enforcement of the various constraints, including $0 \le P \le P_o$, warrants its own section. Which is where this paragraph would belong.*

  This paragraph belongs here because explicit time-stepping is only possible in equations without algebraic constraints. That is, only after we state our regularized pressure evolution equation can we make a basic point: because of the $\partial P/\partial t$ term in our equation, a time-stepping solution *can* be explicit. Now we don't have to follow [20] and use an elliptic

variational inequality form to incorporate the bounds $0 \le P \le P_o$.[24] This paragraph, and the following paragraph about Clarke's model, are indeed about consequences and meaning of the englacial porosity regularization.

- *p.23 l.19-22 These two sentences suggest that the authors have solved the "prohibitively expensive" problem of Werder et al. (2013). But as discussed above, they only solve a simplified version of Schoof et al. (2012) without channels. Reword.*

The reviewer is not paying attention to what Werder et al. (2013) [24] actually says:

> *Note that we impose no restrictions on the values that the water pressure can attain. This is in contrast to the model in Schoof et al. [2012] and Hewitt et al. [2012] which assumes that an air/vapor gap forms when the pressure drops to zero, and instantaneous ice uplift occurs when pressure exceeds overburden. However, the numerical procedure used in those studies is prohibitively expensive to use in 2-D.*

This makes it copiously clear that it is the "numerical procedure used in those studies", including in Schoof et al. (2012) which has no conduits, which is expensive. And that it is is a numerical procedure to "impose ... restrictions on the values that the water pressure can attain." We have indeed offered an alternative, and demonstrated without question that it ours is not "prohibitively expensive".

The reviewer claims this issue is about "channels" (conduits). It isn't. We stand by what we said, "This variational inequality problem is asserted to be 'prohibitively expensive' by Werder et al. (2013) when solved in two dimensions at each step of a time-stepping model.".

- *p.24 Sec.5.1 I like this summary. One suggestion: write the equations in Eq.34 all as "time derivative of unknown = something." Add the boundary conditions.*

We have added text about boundary conditions to subsection 5.1.

However, we do not take the suggestion that we write as "time derivative ... = something" for a well-known reason. When there are constraints, ODE systems are often written

$$M\frac{d\mathbf{u}}{dt} = A\mathbf{u}$$

and not

$$\frac{d\mathbf{u}}{dt} = M^{-1}A\mathbf{u},$$

even when M is invertible. The reasons are subtle, but they apply here: we want the minimum well-posed statement of evolution in a model in which there are constraints on the variables. In our case our system is PDEs, but (appropriately-interpreted) our M is triangular and invertible. It ceases to be invertible as $\phi_0 \to 0$, as we state in comparing our model to that of [20], which has a differential-algebraic version of our problem.

[24] We could follow [20] in this respect, but we have an easier-to-implement option which is numerically consistent and (we believe) convergent.

- *p.25 l.1-8 Either be specific about which functions are what type or leave the paragraph away.*

  Table 2 makes it copiously clear which functions are of which type. We have pointed the reader to that Table earlier in the paragraph. This paragraph is key for explaining coupling to ice dynamics.

- *p.25 Sec.5.2 This section should be merged with section 4.1, probably at this location in the MS.*

  We believe that the existing structure of sections 2–5 is very important in this regard.

  In sections 2 and 3 we point out physical principles with which readers will not disagree.[25] Then we start section 4 with the key statement "The evolution equations listed so far . . . can be simplified to three equations in the four major variables . . . We do not yet know how to compute the water pressure P . . . " Subsection 4.1 then collects closures which are for this purpose (i.e. determining the water pressure) but which are scattered all over the literature. Only with this structure can we *then* make clear why our choice (i.e. simply $W = Y$) is reasonable and how it fits in, and can we also regularize the resulting pressure-determining equation (subsections 4.2 and 4.3). At that point (i.e. section 5.1) we can summarize our whole model. Then we can help the actual user of our model by showing in what parameter limits we get some simpler or different models (section 5.2).

- *p.25 l.11-19 as stated above, I dont think this is quite the Schoof et al. (2012) model.*

  This is indeed the Schoof et al. (2012) [20] model. Equations (2.8)–(2.10) in [20] give our equation (36a). Equation (2.12) in [20] is our equation (36b).

- *p.27 l.23 write "layer thickness" instead of "amount"*
  p.28 l.1 write "layer thickness" instead of "amount" (and other places in the MS)

  We somewhat agree. First, our exposition of this particular list of observations has been simplified so that the issue does not arise.

  Throughout the manuscript, when we refer to W or W_{til} in particular we generally use "thickness". But "water amount" is an appropriate phrase when we want to contrast with other water properties, especially "water pressure."

- *p.37 l.15 What happens when $W < 0$ should probably be discussed in the mathematical section too.*

  We have added some discussion of the appearance of free boundaries, from the enforcement of inequalities including $W \geq 0$, to subsection 5.1 on the continuum model.

[25] I.e. disagree fundamentally. Details are subject to disagreement, of course.

- *p.39 l.14 For a mountain glacier porosity seems to be around* 0.01 *(Bartholomaus et al., 2011). Porosity for an ice sheet may be more on the order of* 10^{-4}.

  Yes. (Or rather, we don't doubt that perhaps someday a careful survey will reveal that to be so.) The role of our englacial porosity in regularizing the solution, and in smoothing-out modeled temporal detail if it is too large, is already adequately covered.

- *p.40 l.13 What is the "active subglacial layer"?*

  That ambiguous phrase has been removed. We now say "There are also special cases at the boundaries of the region where $W > 0$ . . . "

- *p.42 l.17 is this connected to the statement on p. 37, l.15? How?*

  Yes, it is connected. Because literature including [11, 20, 24] is modeling water input into mostly-temperate mountain glaciers, mostly from surface melt, that literature misses the point that $W \geq 0$ must be enforced. That is, it must be enforced when there is refreeze, a known case under ice sheets. Thus there are more-diverse free boundaries, and potential for conservation errors, in our physical model for ice sheets than in that literature.

- *p.45 l.20 write "The spin-up grid sequence..."*

  Done.

- *45 Sec.9.2.1 This section is too long and detailed considering this is not about ice flow modelling. Is this spin-up different from others used before? Also in a similar vein, Fig. 6 could be removed.*

  This section has been shortened, and Figure 6 removed, but nonetheless this material defines the meaning and quality of the input data into the subglacial hydrology model. It cannot be removed entirely.

- *p.48 l.5-7 The till is either completely full or empty. If I understand the dependence of sliding on the till hydrology correctly, this means either fully slippery on not at all. So, is there no dependence of sliding on hydrology? Maybe this point could be briefly discussed.*

  There is dependence of sliding on hydrology. While basal shear stress only directly depends on the amount of water in the till, not in the transport network, the model in its two-way coupled form can "turn on" and "turn off" ice streams for the correct (e.g. at least from what is understood for Kamb ice stream) thermodynamically-determined reasons.[26]

  It is also true that when there is sufficient basal melt then the till in the model will be fully-saturated. Is this asserted to be wrong?

  Finally it is very important to note that the velocity of sliding is *not*, by any means, a local function of basal shear stress, much less hydrology. PISM solves a nontrivial stress balance to determine where sliding occurs, implying high-quality results (in the sense of comparison to observed surface velocities in outlet glaciers [1]).

[26] While none of the reviewers seem to note this aspect of subglacial hydrology, it is probably our highest priority, as revealed by all the papers on whole ice sheet modeling which we cite . . . all of which are ignored.

- ***Comments for tables and figures***
  Tab. 3 Why is W_r so much higher here?

  Table 3 itself is removed. The particular value of a parameter in a verification test is rarely the issue. We think both $W_r \approx 0.1$ m and $W_r \approx 1$ m are reasonable given the roughness seen in deglaciated areas.

- *Fig. 2 Label R_1 and L.*

  The presentation of the nearly-exact solution has been made briefer and clearer, and the labels are not (now) needed.

- *Fig 2 & 3 they could be combined.*

  Perhaps as subplots, but our arrangment allows separate, and clearer, captions. We want to show W and P separately.

- *Fig. 6 could be left away*

  Done.

- *Fig. 8 [sic; 9?] & 11 mention what model run this is for*

  The Figure 8 caption is clear on which model. The Figure 9 caption is clear that it is the same model as in Figure 8. The Figure 11 caption (now Figure 10) has been corrected to make it clear it is `distributed`.

- *Fig. 11 Add a label to the colour-scale. Also, I think there is a inconsistency between the caption and the text (p.48, l.18), one says ice thickness one says sliding speed.*

  The caption already says that the color scale is for ice thickness. We are not sure what was the "inconsistency between the caption and the text". We have re-written the caption to make it as clear as possible.

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Manuscript prepared for Geosci. Model Dev.
with version 5.0 of the LaTeX class copernicus.cls.
Date: 8 February 2015

Mass-conserving subglacial hydrology in the Parallel Ice Sheet Model version 0.6

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Abstract. We describe and test a ~~distributed~~ two-horizontal-dimension subglacial hydrology model which combines ~~a pressurized, plastic~~ till with a distributed system of water-filled, linked cavities which open through ~~sliding-generated cavitation~~ sliding and close through ice creep. The addition of this sub-model to the Parallel Ice Sheet Model accomplishes three specific goals: (1) conservation of the mass of ~~two-phase (solid/liquid) waterin the ice sheet~~water, (2) simulation of spatially- and temporally-variable basal shear stress from physical mechanisms based on a minimal number of free parameters, and (3) convergence under ~~two-horizontal-dimensional grid refinementof the subglacial water amount and pressure~~grid refinement. The model is a common generalization of ~~at least~~ four others: (i) the undrained plastic bed model of Tulaczyk et al. (2000b), (ii) a standard "routing" model used for identifying locations of subglacial lakes, (iii) the lumped englacial/subglacial model of ~~(Bartholomaus et al., 2011)~~ Bartholomaus et al. (2011), and (iv) the elliptic-pressure-equation model of Schoof et al. (2012). We ~~use englacial porosity as a regularization, and we~~ preserve physical bounds on the pressure. In steady state ~~the model generates a local~~ a functional relationship between water amount and pressure emerges. We construct an exact solution of the coupled, steady equations ~~which is used~~ and use it for verification of our explicit time-stepping, parallel numerical implementation. We demonstrate the model at scale by five year simulations of the entire Greenland ice sheet at 2 km horizontal resolution, with one million nodes in the hydrology grid.

1 Introduction

Any ~~reasonable~~ continuum-physics-based dynamical model of the liquid water underneath and within a glacier or ice sheet has at least these two elements: the mass of the water is conserved and the water flows from high to low values of the modeled hydraulic potential. Beyond that there are many variations considered in the literature. Modeled aquifer geometry might be a system of linked cavities (Kamb, 1987), conduits (Nye, 1976), or a sheet (Creyts and Schoof, 2009). Geometry evolution processes might include the opening of cavities by sliding of the overlying ice past bedrock bumps (Schoof, 2005), the creation of cavities by interaction of the ice with deformable sediment (Schoof, 2007), closure of cavities and conduits by creep (Hewitt, 2011), or melt on the walls of cavities and conduits which causes them to open (Clarke, 2005). Water could move in a macro-porous englacial system ~~(Bartholomaus et al., 2011; Harper et al., 2010)~~ (Bartholomaus et al., 20 it could be stored in a porous till (Tulaczyk et al., 2000a).

~~Successful models~~ Models have combined subsets of these different morphologies and ~~processes—for examples see Flowers and Clarke (2002a); Hewitt (2013); van der Wel et al. (2013); W~~ ~~It is not, however, always true that adding more processes makes a better model. Especially when used to understand variations in ice flow and sliding, which is a goal hereprocesses~~ (Flowers and Clarke, 2002a; Hewitt, 2013; Hoffman and Price, 2014; va However, the completeness of the modeled processes ~~should~~ must be balanced against the number of uncertain model parameters and the ultimate availability of observations with which to constrain them.

This paper describes a carefully-selected model for a distributed system of linked subglacial cavities, with ad-

ditional storage of water in the pore spaces of subglacial till. ~~The mass conservation equation in our model describes the evolution of the sum of the transportable water in the distributed system and the water stored in the till.~~ Water in excess of the capacity of the till passes into the ~~transport system, and in~~ distributed transport system. In this sense the model could be called a "drained-and-conserved~~plastic bed~~" extension of the "undrained~~plastic bed"~~ " plastic bed model of Tulaczyk et al. (2000b).

The ~~goals of the current work are the implementation, verification, and practical demonstration of this two-dimensional subglacial hydrology model. It must also be parallelizable, apply at a wide variety of spatial and temporal scales, exhibit convergence of solutions under grid refinement, and have as few parameters as practical. The result is a sub-model of a comprehensive three-dimensional ice sheet model, the open-source Parallel Ice Sheet Model (PISM; pism-docs.org). The submodel can be used in any PISM run by a simple run-time option.~~

~~The~~ cavities in our modeled distributed system open by sliding of the ice over bedrock roughness and ~~they~~ close by ice creep~~.~~ . These two physical processes ~~which~~ combine to determine the relationship between water amount and pressure. Pressure is thereby determined non-locally over each connected component of the hydrological system. No functional relation between subglacial water amount and pressure is assumed (compare Flowers and Clarke, 2002a). The ~~subglacial water~~ pressure solves an equation which is a parabolic regularization of the distributed pressure equation given in elliptic variational inequality form by Schoof et al. (2012).

In cases where boreholes have actually been drilled to the ice base, till is often observed (Hooke et al., 1997; Tulaczyk et al., 2000a; Truffer et al., 2000; Truffer and Harrison, 2006). Laboratory experiments on the rheology of till (Kamb, 1991; Hooke et al., 1997; Tulaczyk et al., 2000a; Truffer et al., 2001) generally conclude that its deformation is well-approximated by a Mohr-Coulomb relation (Schoof, 2006b). For this reason we adopt a compressible-Coulomb-plastic till model when determining the effective pressure on the till as a function of the amount of water stored in it (Tulaczyk et al., 2000a). Existing models which combine till and a mass conservation equation for the subglacial water are rather different from ours, as they either have only one-horizontal dimension (van der Wel et al., 2013) or have a pressure equation which directly ties water pressure to water amount, which generates a porous medium equation form (Flowers and Clarke, 2002a; de Fleurian et al., 2014).

~~Wall melt in the linked-cavity system can be calculated diagnostically from the modeled flux and hydraulic gradient. If included as a contribution to the mass conservation equation, however, the addition of wall melt generates an unstable distributed system (Walder, 1982) , though such a system can be stabilized to some degree by bedrock bumps (Creyts and Schoof, 2009) . In this model, wall melt is not added into the mass conservation equation~~The major goals here are to implement, verify, and demonstrate this two-dimensional subglacial hydrology model. The model is applicable at a wide variety of spatial and temporal scales but it has relatively-few parameters. It is parallelized and it exhibits convergence of solutions under grid refinement. It is a sub-model of a comprehensive three-dimensional ice sheet model, the open-source Parallel Ice Sheet Model (PISM; pism-docs.org); the sub-model can be added to any PISM run by a simple run-time option.

Conduits are ~~also not includedin our model. While the pressure and amount of water in conduits could evolve by physical processes, the existing theory~~ not included. Existing theories of conduits apparently ~~requires~~ require their locations to be fixed a priori ~~(~~Schoof~~,~~ 2010b~~; ?;~~ Hewitt et al.~~,~~ 2012~~;~~ Hewitt~~,~~ 2013~~;~~ Werder et al.~~,~~ 2013 Such lattice models have no known continuum limit in the map plane. ~~Because~~ By contrast with conduits, linked-cavity models do not put the cavities at the nodes of a pre-determined lattice, exactly because the continuum limit of such a lattice model is known (Hewitt, 2011) , namely partial differential equation (PDE) (13) in the current paper. Regarding lattice models, because all PISM usage involves a run-time determination of grid resolution, ~~which varies from 40 km to 10 mm in the applications documented in the PISM User's Manual (~~PISM authors~~,~~ 2013~~) ,~~ all parameters must have grid-spacing-independent meaning. Lattice or other ~~fixed-grid~~ input-grid-based models are therefore not acceptable as components of PISM.

Wall melt in the linked-cavity system, which is believed to be small (Kamb, 1987) , is not added into the mass conservation equation in our model. (It can be calculated diagnostically from the modeled flux and hydraulic gradient, however.) If included in mass conservation, the addition of wall melt can generate an unstable distributed system (Walder, 1982) , though such a system can be stabilized to some degree by bedrock bumps (Creyts and Schoof, 2009) .

The structure of the paper is as follows: Section 2 considers basic physical principles, culminating with a fundamental advection-diffusion form of the mass conservation equation. Section 3 reviews what is known about till mechanical properties, water in till pore spaces, and shear stress at the base of a glacier. In section 4 we compare ~~closures~~ "closures" which directly or indirectly determine the subglacial water pressure. Based on all these elements, in section 5 we summarize the new model and the role of its major fields. In this section we also show how the model extends several published models, ~~and~~ we note properties of its steady states ~~;~~ (see also Appendix A~~. In section ?? we compute an exact~~), and we compute a nearly-exact steady solution in the map-plane, a useful tool for verification. In section 6 we present ~~all~~ the numerical schemes, with particular attention to time step restrictions and the treatment of advection~~. Section ?? documents~~ , and we document the PISM options and parameters seen by ~~a user~~users. Section 7 shows numerical results

from the model, ~~including~~ starting with convergence under grid refinement in the verification case~~, and a demonstration of~~. We then demonstrate the model in five year runs on a 2 km grid covering the entire Greenland ice sheet.

2 Elements of subglacial hydrology

2.1 Mass conservation

We assume that liquid water is of constant density ρ_w; see Table A1 for constants. Thus the thickness of the layer of ~~laterally-transportable (mobile)~~ laterally-moving water, denoted by $W(t,x,y)$, determines its mass; see Table A2 for variable names and meanings. In addition there is liquid water stored locally in the pore spaces of till (Tulaczyk et al., 2000b) which is also described by an effective thickness $W_{\text{til}}(t,x,y)$. Such thicknesses are only meaningful compared to observations if they are regarded as averages over a horizontal scale of ~~tens to thousands~~ meters to hundreds of meters (Flowers and Clarke, 2002a).

~~The~~ Thus the total effective thickness of the water at map-plane location (x,y) and time t is $W + W_{\text{til}}$. This sum is the conserved quantity in our model. In two map-plane dimensions the mass conservation equation is (compare Clarke, 2005)

$$\frac{\partial W}{\partial t} + \frac{\partial W_{\text{til}}}{\partial t} + \nabla \cdot \mathbf{q} = \frac{m}{\rho_w} \tag{1}$$

where ~~$\nabla\cdot = (\partial/\partial x) + (\partial/\partial y)$~~ $\nabla\cdot = \partial/\partial x + \partial/\partial y$ denotes divergence, $\mathbf{q}$ is the ~~(vector)~~ vector water flux (~~units~~ $\text{m}^2\,\text{s}^{-1}$), and m is the total input to the subglacial hydrology (~~units~~ $\text{kg}\,\text{m}^{-2}\,\text{s}^{-1}$)~~and ρ_w is the density of fresh liquid water; see Table A1 for this an other physical constants~~. Note that the water flux $\mathbf{q}$ is concentrated within the two-dimensional subglacial layer.

The water source m in equation (1) includes both melt on the ~~lower surface~~ base of the glacier and drainage to the bed from the glacier surface~~if that occurs~~. In portions of ice sheets with cold surface conditions, such as Antarctica and the interior of Greenland, the basal melt rate part of m is ~~determined~~ dominated by the energy balance at the base of the ice (Aschwanden et al., 2012)~~. The~~, and the Greenland results in section 7 use only that basal melt for m. Drainage from the surface has also been added to m in applications of our model (van Pelt, 2013), but modelling such drainage is outside the scope of this paper.

2.2 Hydraulic potential and water pressure

The hydraulic potential $\psi(t,x,y)$ combines the pressure $P(t,x,y)$ of the transportable subglacial water and the gravitational potential of the top of the water layer (Goeller et al., 2013; Hewitt et al., 2012),

$$\psi = P + \rho_w g\,(b + W). \tag{2}$$

Here $z = b(x,y)$ is the bedrock elevation.

We have added the term "$\rho_w g W$" to the standard hydraulic potential formula $\psi = P + \rho_w g b$ (Clarke, 2005; Shreve, 1972) because differences in the potential at the *top* of the subglacial water layer determine the driving potential gradient for a fluid layer. ~~The W term in makes the mass conservation equation diffusive, regardless of the action of other diffusive mechanisms; see subsection 5.3.~~ When the water depth becomes substantial ($W \gg 1$ m), as it would be in a subglacial lake, this term keeps the modeled lakes from being singularities of the water thickness field ~~. Indeed, subglacial lakes of infinitesimal extent and infinite depth form at local minima of the hydraulic potential if this term is absent (Le Brocq et al., 2009)~~ (compare Le Brocq et al., 2009) .

Ice is a viscous fluid which has a stress field of its own. The basal value of the downward normal stress, ~~traditionally~~ called the overburden pressure, is denoted by P_o. We accept the shallow approximation that ~~it~~ this stress is hydrostatic (Greve and Blatter, 2009)~~:~~ ,

$$P_o = \rho_i g H, \tag{3}$$

where H is the ice thickness. ~~Because~~

Overpressure $P > P_o$ has been observed in ice sheets, but only for short durations (Das et al., 2008) . In our model and others (Schoof et al., 2012) , however, because the condition $P > P_o$ is presumed to cause the ice to lift and thus reduce the pressure back to overburden $P = P_o$~~(Schoof et al., 2012) , it follows that~~ , the pressure solution is subject to inequalities

$$0 \le P \le P_o. \tag{4}$$

~~In temperate glaciers a similar upper bound applies because water rising to the surface through efficient englacial conduits is free to flow at the surface, ensuring $P \le (\rho_w/\rho_i) P_o$, at least if supraglacial geysers are regarded as exceptional (Bartholomaus et al., 2011; Bueler, 2014) .~~

2.3 Darcy flow

~~Transportable~~ Subglacial water flows from high to low hydraulic potential. The simplest expression of this is a Darcy flux model for a water sheet,

$$\mathbf{q} = -K W \nabla\psi \tag{5}$$

where the hydraulic conductivity K is a constant (Clarke, 2005). More generally Schoof et al. (2012) suggests a power law form

$$\mathbf{q} = -k W^{\alpha} |\nabla\psi|^{\beta-2} \nabla\psi \tag{6}$$

for $\alpha \ge 1$, $\beta > 1$, and a coefficient $k > 0$ with units that depend on α and β (see Table A1). ~~The power-law form is justified as an instance of a Manning or Darcy-Weisbach law~~

(Schoof et al., 2012) . Clarke (2005) suggests $\alpha = 1$ and $\beta = 2$, to give (5) above, Creyts and Schoof (2009) use $\alpha = 3/2$ and $\beta = 3/2$, Hewitt (2011, 2013) uses $\alpha = 3$ and $\beta = 2$, and Hewitt et al. (2012) suggest $\alpha = 5/4$ and $\beta = 3/2$. The current paper implements law (6) generally but uses the Clarke (2005) and Hewitt et al. (2012) exponents in an exact solution and in numerical experiments, respectively. When we use we We call $K = kW^{\alpha-1}|\nabla\psi|^{\beta-2}$ the *effective* hydraulic conductivity, so that equation (5) applies formally throughout.

2.4 Advection-diffusion decomposition

Combining (2) and (6), and separating the term proportional to ∇W, we get the flux expression

$$\mathbf{q} = -kW^{\alpha}|\nabla\psi|^{\beta-2}\nabla(P+\rho_w g b) - \rho_w g k W^{\alpha}|\nabla\psi|^{\beta-2}\nabla W. \tag{7}$$

The second term with ∇W acts diffusively in the mass conservation equation . On the other hand, because which suggests a mix of mechanisms. If P generally scales with the overburden pressure P_o, and if $|\nabla(H+b)| \gg |\nabla W|$, then the first flux term in (7) will dominatein the common situation $|\nabla H| \gg |\nabla W|$. The second term with ∇W acts diffusively in the mass conservation equation (1). We will see in subsection 5.3 that in near-steady-state circumstances the part of the transport velocity which is proportional to where there is significant sliding, the first term with ∇P is also significantly diffusive in the mass conservation equation (subsection 5.3). In conditions far from steady state, however, the direction of ∇P is presumably different from the direction ∇W.

We will construct our conservative numerical scheme based on decomposition (7). To simplify the model expression slightly, the small thickness approximation $W \approx 0$ is made inside the absolute value signs in (7), namely

$$|\nabla\psi| \approx |\nabla(P+\rho_w g b)|. \tag{8}$$

This simplification, which makes no change in the $\beta = 2$ case (see subsection 2.3), lets us define redefine the effective hydraulic conductivity as

$$K = kW^{\alpha-1}|\nabla(P+\rho_w g b)|^{\beta-2}. \tag{9}$$

In terms of K we define a velocity field and a diffusivity coefficient:

$$\mathbf{V} = -K\nabla(P+\rho_w g b), \qquad D = \rho_w g K W, \tag{10}$$

Now so that (7) is a clean advection-diffusion decomposition,

$$\mathbf{q} = \mathbf{V}W - D\nabla W. \tag{11}$$

From equations (1) and (11) we now have an advection-diffusion-production equation for the evolution of the water amountconserved water amount $W + W_{til}$:

$$\frac{\partial W}{\partial t} + \frac{\partial W_{\text{til}}}{\partial t} = -\nabla\cdot(\mathbf{V}W) + \nabla\cdot(D\nabla W) + \frac{m}{\rho_w}. \tag{12}$$

There are distinct numerical approximations (section 6) for the advection term $\nabla\cdot(\mathbf{V}W)$ and the diffusion term $\nabla\cdot(D\nabla W)$, and they impose with time-step restrictions of different magnitudes . We will see that equation (section 6). Equation (12) is often advection-dominated in the sense that $|\mathbf{V}W| \gg |D\nabla W|$, but the and numerical schemes for advection and diffusion must be tested in combination . (We measure convergence of the combined numerical schemes in section 7.)

As is well known (Clarke, 2005) , the flux q depends significantly on the ice surface slope because the ice overburden pressure dominates the subglacial water pressure. The model in this paper also generates pressure fields with this property in some circumstances, but the directions of hydraulic potential and surface gradients are significantly different in general because the pressure depends on physical mechanisms for the opening and closing of cavities(section 7).

2.5 Capacity of a linked-cavity distributed system

The rate of change of the area-averaged thickness of the cavities in a distributed linked-cavity system can be described as is the difference of opening and closing rates (Hewitt, 2011). This thickness Y, also called the bed separation"bed separation" (Bartholomaus et al., 2011), has generic evolution equation

$$\frac{\partial Y}{\partial t} = \mathcal{O}(|\mathbf{v}_b|, Y) - \mathcal{C}(N, Y) \tag{13}$$

where $\mathbf{v}_b$ is the ice base (sliding) velocity and $N = P_o - P$ is the effective pressure on the cavity system. Denoting $X_+ = \max\{0, X\}$, we choose an a nonnegative opening term based on cavitation only:

$$\mathcal{O}(|\mathbf{v}_b|, Y) = c_1|\mathbf{v}_b|(W_r - Y)_+. \tag{14}$$

Here c_1 is a scaling coefficient and W_r is a maximum roughness scale of the basal topography (Schoof et al., 2012); see Table A1. The closing term models ice creep only (Hewitt, 2011; Schoof et al., 2012):

$$\mathcal{C}(N, Y) = c_2 A N^3 Y, \tag{15}$$

where c_2 is a scaling coefficient and A is the softness of the ice. We have used Glen exponent $n = 3$ for concreteness and simplicity. By the opening term $\mathcal{O}$ is nonnegative, and the The closing term $\mathcal{C}$ in (15) is also nonnegative because our modeled pressure P satisfies bounds $0 \le P \le P_o$.

The physical intuition behind a model which combines with mass conservation and a Darcy flux relation like is

~~as follows. If the cavity is larger than local water sources can immediately fill then the pressure should drop. Lower pressure encourages water inflow and, by , it speeds cavity closure, bringing the pressure back up. Conversely, if local water sources exceed capacity then increased pressure should push water out of the area and slow creep closure.~~

3 Till hydrology and mechanics

Till with pressurized liquid water in its pore spaces ~~can be~~ is expected to support much of the ice overburden~~in areas where the ice base is not frozen~~. When present, such saturated till is central to the complicated relationship between the amount of subglacial water and the speed of sliding. Our model includes storage of subglacial water in till ~~, potentially everywhere under the ice sheet,~~ both because of its role in conserving the mass of liquid water and its role in determining basal shear stress.

We will assume throughout that liquid water or ice fills pore spaces in the till, and that there are no air- or vapor-filled pore spaces. ~~We suppose that when $m = 0$ and~~ When $W_{\text{til}} = 0$ ~~then~~ in the model, the pore spaces in the till are regarded as filled with ice and the basal shear stress is ~~correspondingly-high~~correspondingly high. When W_{til} ~~is small the till will generally hold both liquid water and ice. Only when W_{til}~~ attains sufficiently large values~~is the till conceived-of as entirely melted, at which point~~ , however, the till is regarded as saturated with liquid, and a drop in effective pressure becomes possible (subsection 3.2 below).

3.1 Evolution of till-stored water amount layer thickness

~~While the thickness W in describes the amount of water in subglacial cavities, and in the connections between cavities (Kamb, 1987) , the~~ The water in till pore spaces is much less mobile than that in the linked-cavity system because of the very low hydraulic conductivity of till ~~(Lingle and Brown, 1987; Tulaczyk et al., 2000a; Truffer et al., 2001)~~ (Lingle and Brown, 1987; Truffer et al., 2001). Therefore we choose an evolution equation for W_{til} ~~for simplicity (Bueler and Brown, 2009)~~ without horizontal transport for simplicity (Bueler and Brown, 2009; Tulaczyk et al., 2000a) , namely

$$\frac{\partial W_{\text{til}}}{\partial t} = \frac{m}{\rho_w} - C_d. \tag{16}$$

Here $C_d \geq 0$ is a fixed rate that makes the till gradually drain in the absence of water input~~. Equation is the same as equation (2) in Tulaczyk et al. (2000b) . In practice~~ ; we choose $C_d = 1$ mm/a, which is small compared to typical values of m/ρ_w. Refreeze is also allowed, as a negative value for m. ~~Note that any water removed from the till~~

As in (Bueler and Brown, 2009) , we constrain the layer thickness by

$$0 \leq W_{\text{til}} \leq W_{\text{til}}^{\max}. \tag{17}$$

Any water in excess of the capacity of the till, i.e. $W_{\text{til}}^{\max}$, "overflows" the till and enters the transport system; it is conserved. Because the source term m in equation (16), or the whole right side, can be negative, the lower bound in (17) must be actively-enforced. The upper bound in (17) also relates to the effective pressure on the till, as we explain next.

3.2 Effective pressure on the till

~~There is extensive evidence that deformation~~ Deformation of saturated till is well-modeled by a plastic (Coulomb friction) or nearly-plastic rheology (Hooke et al., 1997; Truffer et al., 2000; Tulaczyk et al., 2000a; Schoof, 2006b). ~~The~~ Its yield stress τ_c ~~of such till~~ satisfies the Mohr-Coulomb relation

$$\tau_c = c_0 + (\tan\varphi) N_{\text{til}} \tag{18}$$

where c_0 is the till cohesion, φ is the till friction angle, and N_{til} is the effective pressure of the overlying ice on the saturated till (Cuffey and Paterson, 2010). (~~The~~ Note that the effective pressure $N = P_o - P$ used in ~~the next section~~ section 2.5 for modeling cavity closure is distinct from N_{til} in (18). This distinction is ~~justified~~ again explained by the very low hydraulic conductivity of till.)

Let $e = V_w/V_s$ be the till void ratio, where V_w is the volume of water in the pore spaces and V_s is the volume of mineral solids (Tulaczyk et al., 2000a). From the standard theory of soil mechanics and from laboratory experiments on till (Hooke et al., 1997; Tulaczyk et al., 2000a), a linear relation exists between the logarithm of N_{til} and e,

$$e = e_0 - C_c \log_{10}(N_{\text{til}}/N_0). \tag{19}$$

Figure A1(a) shows a graph of (19). Here e_0 is the void ratio at a reference effective pressure N_0 and C_c is the coefficient of compressibility of the till. Equivalently ~~to~~ (19), N_{til} is an exponential function of e, namely $N_{\text{til}} = N_0 10^{(e_0-e)/C_c}$ (van der Wel et al., 2013, equation (15)). ~~Note that in ,~~ , so N_{til} is nonzero for all finite values of e.

While ~~equations~~ (19) ~~suggest~~ suggests that the effective pressure could be any positive number, in fact the area-averaged value of N_{til} under ice sheets and glaciers has limits. It cannot exceed the overburden pressure for any sustained period. Furthermore, once the till is close to its maximum capacity then the excess water will be "drained" into a transport system. We suppose this occurs at a small, fixed fraction δ of the overburden pressure. Thus we assume bounds

$$\delta P_o \leq N_{\text{til}} \leq P_o \tag{20}$$

where $\delta = 0.02$ in the experiments in this paper.

The void ratio e and the effective water layer thickness W_{til} are describing the same thing, namely the amount of liquid water. In fact, if Δx, Δy are the horizontal dimensions of a rectangular patch of till with (mineral-portion) thickness η then $V_w = W_{\text{til}}\,\Delta x\,\Delta y$ and $V_s = \eta\,\Delta x\,\Delta y$ ~~where η is the thickness of the mineral portion of the till. Because $e = V_w/V_s$ it follows that~~ . Thus

$$e = \frac{W_{\text{til}}}{\eta}. \tag{21}$$

On the other hand we ~~will describe the maximum capacity of the till by specifying a maximum~~ specify a maximum $W_{\text{til}}^{\max}$ on the water layer thickness ~~(Bueler and Brown, 2009) , that is,~~

$$0 \le W_{\text{til}} \le W_{\text{til}}^{\max}.$$

, in bounds (17). The minimum $N_{\text{til}} = \delta P_o$ of the effective pressure occurs at ~~the maximum~~ maximum values of void ratio e and ~~at maximum~~ effective thickness W_{til}. ~~But then~~ , so equations (19) and (21) ~~combine~~ allow us to express the solid-till thickness η in terms of our preferred parameters ~~and the overburden pressure,~~ $W_{\text{til}}^{\max}$, δ, e_0, N_0, and C_c:

$$\eta = \frac{W_{\text{til}}^{\max}}{e_0 - C_c \log_{10}(\delta P_o/N_0)}. \tag{22}$$

From (19), (21), and (22), the effective pressure N_{til} can now be written as the following function of W_{til}:

$$\hat{N}_{\text{til}} = N_0 \left(\frac{\delta P_o}{N_0}\right)^s 10^{(e_0/C_c)(1-s)} \tag{23}$$

where $s = W_{\text{til}}/W_{\text{til}}^{\max}$. However, as noted above, N_{til} is bounded ~~, so the form we use~~ :

$$N_{\text{til}} = \min\left\{P_o, \hat{N}_{\text{til}}\right\}. \tag{24}$$

This function is shown in Figure A1(b) ~~:~~

$$N_{\text{til}} = \min\left\{P_o, \hat{N}_{\text{til}}\right\}.$$

.

It follows from equations (18), (23), and (24) that the yield stress τ_c ~~can be determined from the water amount~~ is determined by the layer thickness W_{til}. Regarding the parameters in this relation:

(*i*) Experiments on till suggest small values for cohesion c_0 in (18), $0 \le c_0 \lesssim 1$ kPa (Tulaczyk et al., 2000a), and we choose $c_0 = 0$ for concreteness.

(*ii*) ~~Observed~~ Measured till friction angles φ are in a $18°-40°$ range (Cuffey and Paterson, 2010). Simulations of the whole Antarctic (Martin et al., 2011) and Greenlandic (Aschwanden et al., 2013) ice sheets have been based on a hypothesis that the till friction angle φ ~~can depend~~ depends on bed elevation ~~,~~ so as to ~~accommodate~~ model the submarine history of ~~some~~ low-elevation sediments.

(*iii*) The ratio e_0/C_c can be determined by laboratory experiments on till samples (e.g. Hooke et al., 1997; Tulaczyk et al., 2000a). Values for the dimensionless constants e_0 and C_c used ~~in this paper~~ here (Table A1) are from till samples from ice stream B in Antarctica ~~, namely $e_0 = 0.69$ and $C_c = 0.12$ in Figure 6 of Tulaczyk et al. (2000a) , thus~~ (Tulaczyk et al., 2000a) , and they give $e_0/C_c = 5.75$ in (23).

(*iv*) The till capacity parameter $W_{\text{til}}^{\max}$ could be set in a location-dependent manner from in situ (Tulaczyk et al., 2000a) or seismic reflection (Rooney et al., 1987) evidence, but for simplicity we set it to a constant 2 meters.

3.3 Sliding law

~~The~~ Observe that the ice sliding velocity $\mathbf{v}_b$ is an input into the subglacial hydrology model we are building, because of equation (14). On the other hand, the yield stress τ_c is an output of the till-related part of the hydrology model (subsection 3.2). In an ice dynamics model like PISM, $\mathbf{v}_b$ is determined by solving a stress balance in which the vector basal shear stress $\boldsymbol{\tau}_b$ appears either as a boundary condition (Schoof, 2010a) or as a term in ~~the~~ a vertically-integrated balance (Schoof, 2006a; Bueler and Brown, 2009). In PISM ~~the scalar yield stress , τ_c determines the basal shear stress~~ and $\mathbf{v}_b$ combine to determine $\boldsymbol{\tau}_b$ through a sliding law

$$\boldsymbol{\tau}_b = -\tau_c \frac{\mathbf{u}}{|\mathbf{u}|^{1-q} u_0^q} \frac{\mathbf{v}_b}{|\mathbf{v}_b|^{1-q} v_0^q}. \tag{25}$$

where ~~$\mathbf{u}$ is the sliding velocity of the base of the ice,~~ $0 \le q \le 1$ ~~, and u_0~~ and v_0 is a threshold sliding ~~velocity (Aschwanden et al., 2013) .~~ speed (Aschwanden et al., 2013) .

Power law (25) generalizes, and includes as the case $q = 0$, the purely-plastic (Coulomb) relation ~~$\boldsymbol{\tau}_b = -\tau_c \mathbf{u}/|\mathbf{u}|$~~ $\boldsymbol{\tau}_b = -\tau_c \mathbf{v}_b/|\mathbf{v}_b|$. At least in the $q \ll 1$ cases, under (25) the till "yields" and the magnitude of the basal shear stress becomes nearly independent of ~~$|\mathbf{u}|$ as $|\mathbf{u}| \gg u_0$~~ $|\mathbf{v}_b|$, when $|\mathbf{v}_b| \gg v_0$. Equation (25) could also be written in generic power-law form ~~$\boldsymbol{\tau}_b = -\beta |\mathbf{u}|^{q-1}\mathbf{u}$ with coefficient $\beta = \tau_c/u_0^q$~~ $\boldsymbol{\tau}_b = -\beta |\mathbf{v}_b|^{q-1}\mathbf{v}_b$ with coefficient $\beta = \tau_c/v_0^q$; in the linear case $q = 1$ we have ~~$\beta = \tau_c/u_0$~~ $\beta = \tau_c/v_0$.

4 Closures to determine pressure

The evolution equations listed so far, namely (12), ~~, and~~ (13), and (16), can be simplified to three equations in the four ma-

jor variables W, W_{til}, Y, and P. We do not yet know how to compute the water pressure P or its rate of change $\partial P/\partial t$ given the other variables and data of the problem. A closure is needed.

4.1 Simplified closures without cavity evolution

We first consider two simple closures which appear in the literature but which do not use cavity evolution equation (13) or similar physics. ~~These simplified closures differ in their physical motivation and the form of their mass conservation equations.~~ We list them because the resulting simplified conservation equations emerge as reductions of our more complete theory. For simplicity we present them without till storage ~~, that is, with~~ ($W_{\text{til}}^{\max} = 0$~~in previous equations. We state only~~) and only in the constant conductivity case ($\alpha = 1$ and $\beta = 2$~~in equation~~).

Setting the pressure equal to the overburden pressure is the simplest closure (Le Brocq et al., 2009; Shreve, 1972):

$$P = P_o. \tag{26}$$

This model is sometimes used for "routing" subglacial water under ice sheets so as to identify subglacial lake locations ~~(Livingstone et al., 2013; Siegert et al., 2009)~~ (Goeller, 2014; Livingstone et al., 2013; Siegert et al., 2009). Straightforward calculations using equations (1), (6), and (26) show that the advection-diffusion form (12) has an ice-geometry-determined velocity ~~,~~

$$\frac{\partial W}{\partial t} = -\nabla\cdot\left(\tilde{\mathbf{V}}\,W\right) + \nabla\cdot(\rho_w g k W \nabla W) + \frac{m}{\rho_w}$$

~~where~~

$$\tilde{\mathbf{V}} = -\rho_w g k\left[\frac{\rho_i}{\rho_w}\nabla h + \left(1-\frac{\rho_i}{\rho_w}\right)\nabla b\right].$$

$\tilde{\mathbf{V}}$,

$$\frac{\partial W}{\partial t} = -\nabla\cdot\left(\tilde{\mathbf{V}}\,W\right) + \nabla\cdot(\rho_w g k W \nabla W) + \frac{m}{\rho_w}, \tag{27}$$

$$\tilde{\mathbf{V}} = -\rho_w g k\left[\frac{\rho_i}{\rho_w}\nabla H + \left(1-\frac{\rho_i}{\rho_w}\right)\nabla b\right].$$

Because the approximation $W \ll H$ is usually accepted, so that the hydraulic potential is insensitive to the water layer thickness, i.e. $\psi = P_o + \rho_w g b$ (Le Brocq et al., 2009), the diffusion term ~~$\nabla\cdot(\rho_w g k W \nabla W)$ on the right of~~ in (27) is usually not included. With this common simplification, equation (27) becomes ~~a pure advection with a velocity $\tilde{\mathbf{V}}$ which is independent of W~~ an advection equation with a source term. It therefore possesses characteristic curves~~(?) which are the *a priori* known~~, trajectories of the water flow ~~. These trajectories~~ or "pathways" (Livingstone et al., 2013) , which are determined by ice sheet geometry.

However, without ~~a diffusion term~~the diffusion term, equation (27) ~~also~~ exhibits continuum solutions with infinite water concentration at every location where the simplified potential $\psi = P_o + \rho_w g b$ has a minimum. Applications ~~using the simplified potential~~ therefore only compute the characteristic curves ~~(i.e. "pathways", Livingstone et al., 2013)~~ themselves. We ~~therefore~~ prefer equation (27) as stated, *with* the diffusion term, because it is well-posed for positive initial and boundary values on W (compare Hewitt et al., 2012), so that numerical solutions can converge under sufficient grid refinement.

At an almost opposite extreme~~in terms of the mathematical form, the~~ , our second simplified closure ~~we consider assumes that~~ makes the water pressure ~~is locally determined by~~ a function of the amount of water. Specifically, Flowers and Clarke (2002a) propose

$$P_{FC}(W) = P_o\left(\frac{W}{W_{\text{crit}}}\right)^{7/2}, \tag{28}$$

~~For Trapridge glacier~~where, for Trapridge glacier, Flowers and Clarke (2002b) use $W_{\text{crit}} = 0.1$ m. Thus no separate pressure evolution equation needs to be solved~~(??) . One obvious~~ .

One concern with form (28) is that $P_{FC}(W)$ can be arbitrarily larger than overburden pressure (Schoof et al., 2012). ~~In the flat bedrock case $\nabla b = 0$, we can derive an equation from~~ In any case, ~~, and~~ (28) ~~, namely~~

$$\frac{\partial W}{\partial t} = \nabla\cdot(k W \nabla P_{FC}(W)) + \frac{m}{\rho_w}.$$

~~Equation is~~ is used in equations (1) and (6) to yield a nonlinear diffusion which generalizes the porous-medium equation $\partial W/\partial t = \nabla^2(W^\gamma)$ ~~(Schoof et al., 2012; Vázquez, 2007)~~ (Vázquez, 2007) . The main idea in such a nonlinear diffusion is that the direction of the flux is $-\nabla W$. ~~Physically, however, it would seem that~~ However, a Darcy-type model $\mathbf{q} \sim -\nabla\psi$ ~~would give~~ like (6) normally gives flux directions different from $-\nabla W$ in many cases, especially in rapidly-evolving hydrologic systems, if the pressure is determined by a more physical closure. We consider such a closure next.

4.2 Full-cavity closure

~~Requiring~~ Simply requiring the subglacial layer to be full of water is ~~a closure for the subglacial pressure P. Following Bartholomaus et al. (2011) , we adopt it in our model~~also a closure (Bartholomaus et al., 2011) , which we adopt:

$$W = Y. \tag{29}$$

The consequences of this closure are ~~actually~~ explored at some length by Schoof et al. (2012), Hewitt et al. (2012),

and Werder et al. (2013), ~~where they~~ who describe the full-cavity case as the "normal pressure" condition~~(e.g. equation (4.13) in Schoof et al. (2012))~~.

Equation (29) obviously allows us to eliminate either W or Y as a state variable. We choose to eliminate Y because W is part of the conserved mass $W + W_{\text{til}}$. ~~Using equations , , and we can then derive~~

$$\mathcal{O}(|\mathbf{v}_b|, W) - \mathcal{C}(N, W) + \frac{\partial W_{\text{til}}}{\partial t} + \nabla \cdot \mathbf{q} = \frac{m}{\rho_w}.$$

In the zero till storage case~~(set $W_{\text{til}}^{\max} = 0$ so $W_{\text{til}} = 0$), equation is exactly the~~ , equations (1), (13), and (29) imply

$$\mathcal{O}(|\mathbf{v}_b|, W) - \mathcal{C}(N, W) + \nabla \cdot \mathbf{q} = \frac{m}{\rho_w}. \tag{30}$$

which is exactly elliptic pressure equation (2.12) of Schoof et al. (2012). They ~~solve in one dimension with pressure boundary conditions at the lateral edges of the subglacial hydrologic system to determine the pressure P, and they~~ argue that a model based on (30) should accommodate the possibility of partially-empty cavities with $W < Y$ ~~and at zero pressure~~ when $P = 0$. ~~Direct evidence for such vapor/air-filled cavities does not exist for tidewater glaciers or ice sheets, though of course subglacial hydrology is poorly-observed generally. In any case~~ However, like Werder et al. (2013) who implement the model in two dimensions, we accept a potential loss of model completeness by using a full-cavity model.

~~Overpressure $P > P_o$ has been observed in ice sheets (Das et al., 2008; Bartholomaus et al., 2011, for example) , but only for short durations. Our modelled pressure satisfies $P \le P_o$; compare Werder et al. (2013) .~~

4.3 ~~Notional englacial~~ Englacial porosity as a pressure regularization

Englacial systems of cracks, crevasses, and moulins have been observed in glaciers (Fountain et al., 2005; Bartholomaus et al., 2008; Harper et al., 2010, for example), and these have been included in combined englacial/subglacial hydrology models (Flowers and Clarke, 2002a; Bartholomaus et al., 2011; Hewitt, 2013; Werder et al., 2013). The englacial system is generally parameterized as having macroporosity $0 \le \phi < 1$. If the englacial system is efficiently-connected to the subglacial water then the amount of englacial water is equivalent to the subglacial pressure~~. Subglacial pressure~~ , which is reflected by an englacial "water table" in such models.

Bueler (2014) shows that ~~an~~ a distributed extension of the lumped englacial/subglacial model in Bartholomaus et al. (2011) ~~to the distributed case~~ gives an equation similar to (30)~~, but with the crucial difference~~ . The crucial difference from (30) is that the equation is parabolic for the pressure and not elliptic (compare Hewitt et al. (2012)). Based on this analysis, ~~we use a parabolic equation with constant notional englacial porosity $\phi = \phi_0$~~our model uses a parabolic regularization of (30) which has constant (notional) englacial porosity ϕ_0:

$$\frac{\phi_0}{\rho_w g}\frac{\partial P}{\partial t} = -\nabla \cdot \mathbf{q} + \frac{m}{\rho_w} + \mathcal{C}(N, W) - \mathcal{O}(|\mathbf{v}_b|, W) - \frac{\partial W_{\text{til}}}{\partial t}. \tag{31}$$

Compare equations (7) in ~~Hewitt (2013)~~ (Hewitt, 2013) and (24) in ~~Werder et al. (2013)~~ (Werder et al., 2013) . Unlike Werder et al. (2013) , however, we do not add an englacial water amount variable to the conservation equation, and in this sense the porosity only serves to regularize the pressure equation.

~~Addition of~~ Using englacial porosity as a regularization, as in (31), allows a user-adjustable trade-off between temporal detail in the pressure evolution versus computational effort (van Pelt, 2013). If the englacial porosity ϕ_0 is small ~~, so that~~ then there is a nearly impermeable "cap" on the subglacial system ~~, as would occur under a thick ice sheet, then~~ and equation (31) is stiff (Ascher and Petzold, 1998)~~and indeed~~ . Equation (31) is then similar, in terms of numerical solution, to ~~an elliptic equation . If~~ elliptic equation (30). Indeed, if elliptic equation (30) is used instead of (31) then the coupled hydrological equations system is differential-algebraic (Ascher and Petzold, 1998) , and hardest to solve numerically. By contrast, if ϕ_0 is ~~relatively large~~ larger then equation (31) causes local changes in subglacial pressure P to be damped in the speed and range of ~~their influence~~influence, on other parts of the connected subglacial hydrologic system~~. In fact, the diffusive range of equation is proportional to ϕ_0. If the elliptic equation is used instead of then the system is differential-algebraic in time (Ascher and Petzold, 1998) and hardest to solve numerically~~, and the numerical solution is easier.

Schoof et al. (2012) show that the ~~time-independent~~ mathematical problem encompassing (30), constraints (4), and appropriate pressure boundary conditions can be written as an elliptic variational inequality (Kinderlehrer and Stampacchia, 1980). ~~This~~ Solving this variational inequality problem in two dimensions, at each time step, is asserted to be "prohibitively expensive" by Werder et al. (2013)~~when solved in two dimensions at each step of a time-stepping model~~. Our adaptive explicit time-stepping scheme (section 6), by contrast, ~~satisfies~~ solves (31), while satisfying constraints (4), at demonstrably-reasonable computational cost (section 7).

Stiffness ~~of pressure equation~~ in these pressure equations ultimately follows from the incompressibility of water and the relative non-distensibility (i.e. hardness) of the ice and bedrock. Clarke (2003) addresses this in a physically-different ~~way by including~~ manner from englacial porosity. He includes a relaxation (damping) parameter "β" which is based on the small compressibility of water, but which is more than two orders of magnitude larger than the physical value. Clarke's parameter β appears in his equation exactly

as ~~the englacial porosity~~ ϕ_0 appears in equation (31), multiplying the pressure time derivative.

5 ~~A~~ The new subglacial hydrology model in PISM

5.1 Summary of ~~equations and symbols~~ the model

The major evolution equations for the model are mass conservation (12), till-stored water ~~amount~~ layer thickness evolution (16), and pressure evolution (31). ~~Recalled~~ Collected here for clarity they are:

$$\frac{\partial W}{\partial t} + \frac{\partial W_{\text{til}}}{\partial t} = -\nabla\cdot(\mathbf{V}W) + \nabla\cdot(D\nabla W) + \frac{m}{\rho_w}, \tag{32}$$

$$\frac{\partial W_{\text{til}}}{\partial t} = \frac{m}{\rho_w} - C_d,$$

$$\frac{\phi_0}{\rho_w g}\frac{\partial P}{\partial t} + \frac{\partial W_{\text{til}}}{\partial t} = -\nabla\cdot(\mathbf{V}W) + \nabla\cdot(D\nabla W) + \frac{m}{\rho_w} + c_2 A (P_o - P)^3 W - c_1 |\mathbf{v}_b| (W_r - W)_+.$$

~~using~~ Also recall these definitions:

$$D = \rho_w g K W \qquad \text{diffusivity of } W \text{ diffusivity,}$$

$$K = k W^{\alpha-1} |\nabla(P + \rho_w g b)|^{\beta-2} \qquad \text{effective conductivity effective conductivity,}$$

$$P_o = \rho_i g H \qquad \text{overburden pressure } s = W_{\text{til}}/W_{\text{til}}^{\max} W_i \text{ relative to capacity overburden pressure, and}$$

$$\mathbf{V} = -K\nabla(P + \rho_w g b) \qquad \text{velocity of } W \text{ velocity.}$$

Equations (32) are coupled to ice dynamics by Mohr-Coulomb equation (18) and till effective pressure equations (23), (24).

The model includes these bounds on major variables: ~~$0 \le W$, $0 \le W_{\text{til}} \le W_{\text{til}}^{\max}$, $0 \le P \le P_o$. The model is also coupled to ice dynamics by Mohr-Coulomb equation and till effective pressureequation , namely~~

$$\tau_c = c_0 + (\tan\varphi)\, N_{\text{til}},$$

$$N_{\text{til}} = \min\left\{P_o, N_0 \left(\frac{\delta P_o}{N_0}\right)^s 10^{(e_0/C_c)(1-s)}\right\}.$$

$$0 \le W, \quad 0 \le W_{\text{til}} \le W_{\text{til}}^{\max}, \quad 0 \le P \le P_o. \tag{33}$$

As a result of inequalities (33), free boundaries arise in the domain at locations where, in particular, m is sufficiently negative to drive W to zero or where the pressure P goes to zero or overburden.

~~The functions used above~~ A coupled weak formulation of equations (32) and constraints (33) would be a mathematically-rigorous unified description of the free boundary conditions, but this paper takes a more pragmatic approach, as follows. First, PISM uses a periodic domain for whole ice sheet computations (section 7), so the computational domain has no classical boundary. Second, inequalities (33) are enforced in our coupled explicit scheme by truncation/projection (section 6). Third, at ice-free land and ocean (i.e. ice shelf or ice-free ocean) grid locations, pressure P is determined by atmospheric or ocean pressure, respectively. Fourth and finally, at ice-free land and ocean grid locations the mass conservation equation effectively have m sufficiently negative so that water which flows or diffuses into that grid location during a time step is fully removed and thus $W = 0$ and $W_{\text{til}} = 0$; see the "mask" variables in section 6.

As in Table A2, the functions in the model can be categorized into *state* functions, which must be provided with initial values~~and which evolve according to the model~~, *input* functions, which are either supplied by observations or by other components of an ice sheet model~~(e.g. the stress balance in an ice dynamics model will provide $|\mathbf{v}_b|$)~~, and *output* functions which are supplied to other components of the ice sheet model~~(e.g. the yield stress τ_c is fed back to the stress balance); see Table A2~~. In two-way coupling the ice dynamics model passes H, m, and $|\mathbf{v}_b|$ to the subglacial hydrology model, and τ_c is ~~passed the other way~~returned.

5.2 Reduction to existing models

Four reductions (limiting cases) of model (32) can now be stated precisely:

(i) The zero till storage ($W_{\text{til}}^{\max} = 0$) and zero englacial porosity ($\phi_0 = 0$) case of (32) is essentially the model described by Schoof et al. (2012)~~, recalling~~. Recalling that $\mathbf{q} = -KW\nabla\psi$, the equations are

$$\frac{\partial W}{\partial t} = -\nabla\cdot(KW\nabla\psi) + \frac{m}{\rho_w}, \tag{34}$$

$$0 = \nabla\cdot(KW\nabla\psi) + \frac{m}{\rho_w} + c_2 A (P_o - P)^3 W - c_1 |\mathbf{v}_b| (W_r - W)_+.$$

The bounds $W \ge 0$ and $0 \le P \le P_o$ are unchanged. Model (32) is a parabolic ~~regularization~~ version of (34)~~based on~~, regularized using a notional connection to porous englacial storage, and with coupling to ~~additional~~ till storage.

(ii) The $P = P_o$ limit of (32), in which ~~physical processes for the evolution of pressure are~~ the evolution equation for pressure is ignored, is essentially the model for "routing" water to subglacial lakes under cold ice sheets ~~in~~ used by Siegert et al. (2009) and Livingstone et al. (2013). ~~Assuming again that till storage is removed ($W_{\text{til}}^{\max} = 0$) then the model has only W as a state variable, the single evolution equation is~~

$$\frac{\partial W}{\partial t} = -\nabla\cdot(\mathbf{V}W) + \nabla\cdot(D\nabla W) + \frac{m}{\rho_w}.$$

along with the bound $W \geq 0$ and further-simplified definitions $K = kW^{\alpha-1}|\nabla(P_o + \rho_w g b)|^{\beta-2}$ and $\mathbf{V} = -K\nabla(P_o + \rho_w g b)$. As noted in section 4, the $W_{\text{til}}^{\max} = 0$ and $\alpha = 1$ case of this model routes water with a velocity which is determined entirely by ice and bedrock geometry. This reduced model is mostly an advection, but, because of for the hydraulic potential, which implies some diffusion, model has continuous solutions for W.

(iii) The non-distributed "lumped" form of (32), in which, in particular, $\nabla \cdot \mathbf{q} = (q_{out} - q_{in})/L$ where L is the length of a one-dimensional the glacier and q_{out}, q_{in} are given by observations, is the model of Bartholomaus et al. (2011); see Bueler (2014).

(iv) The undrained plastic bed (UPB) model of Tulaczyk et al. (2000b) arises as the $W = 0, \mathbf{q} = 0, \phi_0 = 0$ reduction of (32). This model depends on friction-heating feedback to keep W_{til} bounded, which is not effective in a membrane-stress-inclusive theory in which if local friction heating is a non-local function of changes in till strength. Bueler and Brown (2009) therefore enforce $W_{\text{til}} \leq W_{\text{til}}^{\max}$ by non-conservatively removing water above the capacity $W_{\text{til}}^{\max}$, giving a minimal non-conservative, but "drained, which is a minimal "drained" version of the UPB model.

The above list does not imply that all possible subglacial hydrology models are subsumed in reductions of ours. For example, the subglacial hydrology model of Johnson and Fastook (2002) is a variation on idea **(ii)** above but it is not a reduction. The Flowers and Clarke (2002a) model mentioned in subsection 4.1 is also not a reduction, although though a significant connection is explained in the section on steady states below. Most significantly, Appendix.

Two-dimensional models which include conduits (Schoof, 2010b; ?; Hewitt et al., 2012, among others) (Schoof, 2010b) are not reductions of our model. Conduit evolution is numerically-straightforward to implement in one-dimensional hydrology models (?Hewitt et al., 2012; van der Wel et al., 2013) (Hewitt et al., 2012; van der Wel et al., 2013), but when extended to two-horizontal dimensions all existing models (Schoof, 2010b; Hewitt, 2013; Werder et al., 2013) become "lattice" models without a known continuum limit. 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.

5.3 Steady states

The steady states of equations are of physical modelling importance because the subglacial system can be close to steady state much of the time, but also because physical processes become decoupled in steady state, which helps us understand the model. Specifically, the The steady form of model (32), with stated using $\alpha = 1$, $\beta = 2$, and $W_{\text{til}}^{\max} = 0$ for simplicity, can be written as follows in terms of $\mathbf{V}, \mathbf{q}, W, P$:

$$\mathbf{V} = -k\nabla(P + \rho_w g b), \tag{35}$$

$$\mathbf{q} = \mathbf{V}W - \rho_w g k W \nabla W, \tag{36}$$

$$0 = -\nabla \cdot \mathbf{q} + \frac{m}{\rho_w}, \tag{37}$$

$$0 = c_2 A (P_o - P)^3 W - c_1 |\mathbf{v}_b| (W_r - W)_+. \tag{38}$$

Steady state equations –are These steady state equations are also stated in the one-dimensional case by Schoof et al. (2012)model, where the decoupling is also noted; see equations (5.8) and (5.10) in (Schoof et al., 2012) , where traveling-wave exact solutions are also found. Observe that the equations describing mass conservation (37) and cavity opening/closing processes (38) have become decoupled.

We can make four specific make three observations about solutions to (35)–(38), which we find are useful in understanding the time-dependent model at longer time-scales also:

(*i*) from (38) there is a functional relationship $P = P(W)$which determines the pressure given the water amount,

(*ii*) by (35) and (38), the apparently advective flux "$\mathbf{V}W$" in (36) actually acts diffusively, if sliding is occurring and if the water amount is either small or comparable to the roughness scale, the water amount W generally scales inversely with the conductivity, and

(*iii*) exact radial nearly-exact solutions can be constructed.

In Appendix A we detail points (*i*) , (*ii*), and (and (*iiiii*). Observation (*iviii*) is addressed in the nextsectionnext.

6 An exact steady state solution

5.1 Radial equations A nearly-exact steady state solution

Steady equations –are the basis on which we now build a For the purpose of verifying numerical schemes we have built a two-dimensional, nearly-exact solution for W and Pin the map-plane, in a case with nontrivial overburden pressure and ice slidingspeed. This solution is useful for verifying numerical schemes. It depends on the numerical solution of a scalar first-order ordinary differential equation (ODE) initial value problem, something we can do with high accuracy. Traveling-wave exact solutions in one horizontal dimension appear in Schoof et al. (2012) .

Consider We solve the flat bed case ($b = 0$) . Assuming dependence only on the radial coordinate $r = \sqrt{x^2 + y^2}$,

from angularly-symmetric case of coupled equations (35)–one may eliminate the velocity to get

$$q = -kW\left(\frac{dP}{dr} + \rho_w g \frac{dW}{dr}\right),$$
$$\frac{1}{r}\frac{d}{dr}(rq) = \frac{m}{\rho_w}.$$

In the case of constant water input $m = m_0$, we can integrate from 0 to r and use symmetry ($q(0) = 0$)to get

$$q(r) = \frac{m_0}{2\rho_w} r.$$

Suppose $h(r)$ is given so that $P_o(r)$ is also determined. Assume that the scaled sliding speed $s_b(r)$ has a bounded derivative and that the solution $W(r)$ satisfies conditions $W_c < W < W_r$; these properties can be verified for the constructed solution. By combining , (38). By assuming spatially-constant water input ($m = m_0$), , and we can eliminate q and P to find

$$\omega_0 r = -W\left[\frac{dP_o}{dr} - \frac{ds_b}{dr}\left(\frac{W_r - W}{W}\right)^{1/3} + \left(\frac{s_b W_r}{3W^{4/3}(W_r - W)^{2/3}} + \rho_w g\right)\frac{dW}{dr}\right]$$

where $\omega_0 = m_0/(2\rho_w k)$.

Equation is a a parabolic ice thickness profile in the radial coordinate r, and a particular profile of sliding—namely a function $|\mathbf{v}_b|(r)$ with onset of sliding at location $r = 5$ km, about one-fourth of the ice cap radius $r = 22.5$ km—the equations reduce to a single first-order ordinary differential equation (ODE) for $W(r)$. To put it in the standard form expected by a numerical ODE solver we solve it for dW/dr.

5.2 A nontrivial solution

Though equation has a constant solution $W(r) = W_r$, to generate a nontrivial exact solution we will choose a positive thickness of ice at the margin (a cliff) so that $P_o(L^-) > 0$. At the ice margin $r = L$ we have water pressure $P = 0$ so $W(L) = W_c(L^-)$ is the boundary condition for the ODE. We assume that at the margin there is some sliding so that $s_b(L^-) > 0$, and by we require that $s_b(L^-)W_r > P_o(L^-)^3 W(L)$. The condition at $r = L$ also satisfies $W(L) < W_r$. Then we integrate ODE in r for the water thickness $W(r)$. The pressure $P(r)$ is then determined from $r = L$ to $r = 0$. The central water thickness value $W(0)$ is determined as part of the solution.

It is useful to have an ice cap geometry in which the surface gradient formula is simple so that dP_o/dr in is also simple, so we choose a parabolic profile

$$H(r) = H_0\left(1 - \frac{r^2}{R_0^2}\right)$$

where $H(0) = H_0$ is the height (thickness) at the center of the ice cap. It follows that $dP_o/dr = -Cr$ where $C = 2\rho_i g h_0 R_0^{-2}$. We choose $L = 0.9R_0$ and we note that $H(L) = 0.19h_0$ is the size of the cliff.

The sliding speed could be determined by a model for stresses at the ice base and within the ice (Greve and Blatter, 2009) , but for hydrology model verification we simply choose a well-behaved sliding speed function which has no sliding near the ice cap center, until a radius $r = R_1$ at which sliding increases:

$$|\mathbf{v}_b|(r) = \begin{cases} 0, & 0 \le r \le R_1, \\ v_0\left(\frac{r - R_1}{L - R_1}\right)^5, & R_1 < r \le L. \end{cases}$$

It follows from and that ds_b/dr in is bounded and continuous on $0 \le r \le L$$W(r)$ by the functional relationship (A3) which arises in steady state (Appendix A).

Now we solve ODE with initial condition $W(L)$ and the specific values in Table **??**. We To compute the nearly-exact solution we use adaptive numerical ODE solvers, both a Runge-Kutta 4(5) Dormand-Prince method and a variable-order stiff solver, with relative tolerance 10^{-12} and absolute tolerance 10^{-9}. The two solvers gave essentially identical results . Modest stiffness (Ascher and Petzold, 1998) of ODE is observed at $r \approx R_1$identical results to more than six digits. The result $W(r)$ is shown in Figure A2.

Because equations and imply a pressure functional relation $P = P(W, r)$ from , we can also show in Figure A2 , which also shows the regions of the r, W plane which correspond to overpressure ($P = P_o$ in our model), normal pressure ($0 < P < P_o$), and underpressure . We see that $W(r)$ ($P = 0$). Figure A3 shows the corresponding pressure solution $P(r)$. Starting at the margin, we see that the solution is in the normal pressure region as r decreasesfrom $r = L$ to $r = R_1$, but at $r = R_1$ the function $W(r)$, until the onset of sliding ($r = 5$ km). At that location it switches into the overpressure case because there is no sliding. Figure A3 shows the corresponding pressure solution $P(r) = P(W(r))$ from .

The reason for stiffness near R_1 is that as the sliding goes to zero the cavitation rate goes to zero. Because creep closure balances cavitation in steady state, effective pressure also goes to zero ($P \to P_o$) . The remaining active mechanisms in the model are the variable overburden pressure and the rate of water input, and they must exactly balance. In this case reduces to the simpler form

$$\frac{dW}{dr} = -\frac{\varphi_o r W^{-1} + \frac{dP_o}{dr}}{\rho_w g}.$$

Though we have not derived it this way, Equation is the steady radial form of the mass conservation equation under the "$P = P_o$" closure, namely equation .

In equation we see that $dW/dr = 0$ if W satisfies $W = -\omega_0 r/(dP_o/dr)$. In our case with geometry this

reduces to a constant value $W^* = 0.21764$ m because dP_o/dr is linear in r. Both numerical ODE solvers used here confirm that $W(r)$ is asymptotic to this constant value W^* as $r \to 0$, and that $W(r) \approx W^*$ within about 1on all of $0 \le r \le R_1$. This is seen in Figure A2Verification results using the nearly-exact solution appear in section 7. The numerical methods (next section) use a cartesian (x, y) grid unrelated to the radial nearly-exact solution. Thus numerical error comes from generic relationships between exact solution features and the grid.

6 Numerical schemes

All the numerical schemes described in this section are implemented in parallel using the PETSc library (Balay et al., 2011) .

6.1 Mass conservation: time-stepping

The mass conservation equation The equations in model (32) will be discretized by an explicit , conservative finite difference methodare discretized by explicit finite difference methods (Morton and Mayers, 2005) . A centered, second-order scheme will be is applied to the diffusion part . Two of the mass conservation equation in (32), but two upwind-type schemes for the advection part will be are compared, namely first-order upwinding "donor cell" upwinding (LeVeque, 2002) and a higher-order flux-limited upwind-biased method (Hundsdorfer and Verwer, 2010) .

We first consider stable time steps. Stability for the advection schemes occurs with a time step $\Delta t \le \Delta t_{\text{CFL}}$ where

$$\Delta t_{\text{CFL}} \left(\frac{\max|u|}{\Delta x} + \frac{\max|v|}{\Delta y} \right) = \frac{1}{2}.$$

Because of the additional diffusion process, for stability the time step should also satisfy $\Delta t \le \Delta t_W$ where (Morton and Mayers, 2005)

$$\Delta t_W \max D \left(\frac{1}{\Delta x^2} + \frac{1}{\Delta y^2} \right) = \frac{1}{4}.$$

The condition $\Delta t \le \min\{\Delta t_{\text{CFL}}, \Delta t_W\}$ is sufficient for stability and convergence of the scheme. (We show this for the first-order upwind scheme, but standard theory suggests the same conclusion for the higher-order flux-limited advection scheme (Hundsdorfer and Verwer, 2010) .)

We can understand the scale of these restrictions better by considering an example using the parameter values in Table A1. We ran the model on a $\Delta x = \Delta y = 250$ m grid to approximate steady state for the subglacial hydrology of (van Pelt, 2013) . We used a hypothesized water input distribution with average value about 1 m a^{-1}, and a glacier-wide constant sliding rate of 50 m a^{-1}. The result is that the maximum computed water speed $|\mathbf{V}|$ is about 0.2 m s^{-1} so the advective restriction is $\Delta t_{\text{CFL}} \approx 300\,\text{s} \approx 10^{-5}$ a. Computed diffusivity $D = \rho_w g K W$ has a maximum value that varies significantly in time, $0.1 \le \max D \le 5\,\text{m}^2\,\text{s}^{-1}$. Diffusive restriction using value $\max D = 1\,\text{m}^2\,\text{s}^{-1}$ is $\Delta t_W \approx 8000\,\text{s} \approx 2.5 \times 10^{-4}$ a. Thus in this simulation $\Delta t_W \approx 25 \Delta t_{CFL}$.

This example suggests that, unless both the global peak velocity is unusually slow, and deep subglacial lakes develop so that D is large, the diffusive time scale is significantly longer than the CFL time scale for a 250 m grid. The scaling $\Delta t_W = O(\Delta x^2)$ versus $\Delta t_{CFL} = O(\Delta x^1)$ makes it clear that under sufficient spatial grid refinement Δt_W is the controlling restriction, but we suppose that Δt_{CFL} is controlling for $\Delta x \gg 100$ m. We will see below, however, that the time step restriction associated to an explicit time-stepping method for the pressure equation is typically shorter than either of $\Delta t_W, \Delta t_{CFL}$, and it scales as $O(\Delta x^2)$ like Δt_W.

If implicit time-stepping is used for the pressure equation, which requires variational inequality treatment to preserve physical pressure bounds (Schoof et al., 2012) , then the time scales $\Delta t_W, \Delta t_{CFL}$ addressed here are the only restrictions. The time step restriction Δt_W could also be removed by implicit steps for the mass-conservation equation, though it would seem this requires another variational inequality formulation because of the lower bound $W \ge 0$. Our observation that $\Delta t_{CFL} \ll \Delta t_W$ for practical ice sheet grids suggests that implicit time-stepping for the mass-conservation equation is not beneficialAll the numerical schemes are implemented in parallel using the PETSc library (Balay et al., 2011) .

6.1 Mass Discretization of the mass conservation : spatial discretization equation

To set notation, suppose the rectangular computational domain has $M_x \times M_y$ gridpoints (x_i, y_j) with uniform spacing $\Delta x, \Delta y$. Let $W^l_{i,j} \approx W(t_l, x_i, y_j)$, $(W_{\text{til}})^l_{ij} \approx W_{\text{til}}(t_l, x_i, y_j)$, and $P^l_{i,j} \approx P(t_l, x_i, y_j)$ denote the numerical approximations.

We will compute velocity components and flux components at the staggered (cell-face-centered) points, shown in Figure A4using , from centered finite difference approximations of equations (10) and (11). We use "compass" indices such as $u_e = u_{i+1/2,j}$ for for such staggered values, so that, for example, the "east" staggered value of u and $v_n = v_{i,j+1/2}$ for the and "north" staggered value of v. Similarly we use compass indices for staggered grid values of the water layer thickness, water layer thicknesses are computed by averaging regular grid values:

$$W_e = (W^l_{i,j} + W^l_{i+1,j})/2,$$

$$W_n = (W^l_{i,j} + W^l_{i,j+1})/2.$$

$$W_e = (W^l_{i,j} + W^l_{i+1,j})/2, \quad W_n = (W^l_{i,j} + W^l_{i,j+1})/2. \tag{39}$$

The nonlinear effective conductivity K from (9) is also needed at staggered locations. As a notational convenience define $R = P + \rho_w g b$ and define these staggered-grid values (compare Mahaffy, 1976):

$$\Pi_e = \left|\frac{R_{i+1,j} - R_{i,j}}{\Delta x}\right|^2 + \left|\frac{R_{i+1,j+1} + R_{i,j+1} - R_{i+1,j-1} - R_{i,j-1}}{4\Delta y}\right|^2,$$

$$\Pi_n = \left|\frac{R_{i+1,j+1} + R_{i+1,j} - R_{i-1,j+1} - R_{i-1,j}}{4\Delta x}\right|^2 + \left|\frac{R_{i,j+1} - R_{i,j}}{\Delta y}\right|^2.$$

Thereby define

$$K_e = k W_e^{\alpha-1} \Pi_e^{(\beta-2)/2}, \quad K_n = k W_n^{\alpha-1} \Pi_n^{(\beta-2)/2}. \tag{40}$$

The velocity components (u, v) of the water velocity $\mathbf{V}$ are then found by differencing:

$$u_e = -K_e \left(\frac{P_{i+1,j} - P_{i,j}}{\Delta x} + \rho_w g \frac{b_{i+1,j} - b_{i,j}}{\Delta x}\right),$$

$$v_n = -K_n \left(\frac{P_{i,j+1} - P_{i,j}}{\Delta y} + \rho_w g \frac{b_{i,j+1} - b_{i,j}}{\Delta y}\right).$$

~~Similarly for diffusivity we have~~

$$u_e = -K_e \frac{R_{i+1,j} - R_{i,j}}{\Delta x}, \quad v_n = -K_n \frac{R_{i,j+1} - R_{i,j}}{\Delta y}. \tag{41}$$

For diffusivity we simply have

$$D_e = \rho_w g K_e W_e, \quad D_n = \rho_w g K_n W_n. \tag{42}$$

We get the remaining staggered-grid quantities by shifting indices~~:~~

$$u_w = u_e\big|_{(i-1,j)}, \quad K_w = K_e\big|_{(i-1,j)}, \quad D_w = D_e\big|_{(i-1,j)},$$

$$v_s = v_n\big|_{(i,j-1)}, \quad K_s = K_n\big|_{(i,j-1)}, \quad D_s = D_n\big|_{(i,j-1)}.$$

~~Now we define~~ Define $Q_e(u_e)$, $Q_w(u_w)$, $Q_n(v_n)$, and $Q_s(v_s)$ as the face-centered (staggered-grid) normal components of the advective flux $\mathbf{V}W$~~. These quantities are described in more detail~~ ; more detail is given in the next subsection. ~~They use only the staggered velocity component but there is upwinding to determine which W value, or combination of W values, is used.~~

The grid values of $\mathcal{D} = \nabla \cdot \mathbf{q} = \nabla \cdot (\mathbf{V}W) - \nabla \cdot (D \nabla W)$ using (41) and (42) now become:

$$\mathcal{D}_{i,j} = \frac{Q_e(u_e) - Q_w(u_w)}{\Delta x} + \frac{Q_n(v_n) - Q_s(v_s)}{\Delta y} - \frac{D_e(W^l_{i+1,j} - W^l_{i,j}) - D_w(W^l_{i,j} - W^l_{i-1,j})}{\Delta x^2} - \frac{D_n(W^l_{i,j+1} - W^l_{i,j}) - D_s(W^l_{i,j} - W^l_{i,j-1})}{\Delta y^2}. \tag{43}$$

~~To ensure conservation ,~~ Local conservation is ensured by using $Q_e(u_e)$ ~~used~~ in computing $\mathcal{D}_{i,j}$ ~~must be the same as~~ equal to $Q_w(u_w)$ used in ~~computing~~ $\mathcal{D}_{i+1,j}$, and ~~similarly for "north" and "south" staggered fluxes; our formulas have these properties~~so on.

~~Now our~~ Our scheme for approximating mass conservation equation (12) is

$$\frac{W^{l+1}_{i,j} - W^l_{i,j}}{\Delta t} + \frac{(W_{\text{til}})^{l+1}_{i,j} - (W_{\text{til}})^l_{i,j}}{\Delta t} = -\mathcal{D}_{i,j} + \frac{m_{ij}}{\rho_w}. \tag{44}$$

The updated value of W_{til}, which appears on the left side of (44), is computed by trivial integration of equation (16), ~~namely~~

$$(W_{\text{til}})^{l+1}_{i,j} = (W_{\text{til}})^l_{i,j} + \Delta t \left(\frac{m_{ij}}{\rho_w} - C_d\right). \tag{45}$$

The ~~right-hand-side value~~ given value W^{l+1}_{til} is used if it is in the closed interval $[0, W^{\max}_{\text{til}}]$, but otherwise the bounds $0 \le W_{\text{til}} \le W^{\max}_{\text{til}}$ are enforced. Once W^{l+1}_{til} is computed, the value of W^{l+1} can be updated by (44) in a mass-conserving way.

Assuming no error in the flux components Q, the local truncation error (Morton and Mayers, 2005) of scheme (44) would be $O(\Delta t^1 + \Delta x^2 + \Delta y^2)$ as an approximation of (12). The actual truncation error depends on the ~~nature of the approximation which generates~~ approximation of the discrete fluxes, addressed next.

6.2 Discrete advective fluxes

We test two ~~flux discretization schemes, namely~~ flux-discretization schemes, namely a first-order upwind scheme and the Koren flux-limited third-order scheme (Hundsdorfer and Verwer, 2010). Both schemes achieve non-oscillation and positivity, but with different local truncation error and complexity of implementation. The third-order scheme is best explained as a modification of ~~the better-known~~ our conservative ("donor cell"; LeVeque (2002)) first-order upwind scheme~~we use~~.

~~In fact~~For a flux-limited scheme, the following formulas apply in the cases $u_e \ge 0$, $u_e < 0$, $v_n \ge 0$, and $v_n < 0$, re-

spectively:

$$Q_e(u_e) = u_e\left[W_{i,j} + \Psi(\theta_i)(W_{i+1,j} - W_{i,j})\right], \tag{46}$$
$$Q_e(u_e) = u_e\left[W_{i+1,j} + \Psi\left((\theta_{i+1})^{-1}\right)(W_{i,j} - W_{i+1,j})\right],$$
$$Q_n(v_n) = v_n\left[W_{i,j} + \Psi(\theta_j)(W_{i,j+1} - W_{i,j})\right],$$
$$Q_n(v_n) = v_n\left[W_{i,j+1} + \Psi\left((\theta_{j+1})^{-1}\right)(W_{i,j} - W_{i,j+1})\right].$$

~~The~~ where the subscripted θ quotients are ~~as follows:~~

$$\theta_i = \frac{W_{i,j} - W_{i-1,j}}{W_{i+1,j} - W_{i,j}}, \qquad (\theta_{i+1})^{-1} = \frac{W_{i+2,j} - W_{i+1,j}}{W_{i+1,j} - W_{i,j}},$$
$$\theta_j = \frac{W_{i,j} - W_{i,j-1}}{W_{i,j+1} - W_{i,j}}, \qquad (\theta_{j+1})^{-1} = \frac{W_{i,j+2} - W_{i,j+1}}{W_{i,j+1} - W_{i,j}}.$$

$$\theta_i = \frac{W_{i,j} - W_{i-1,j}}{W_{i+1,j} - W_{i,j}}, \qquad \theta_j = \frac{W_{i,j} - W_{i,j-1}}{W_{i,j+1} - W_{i,j}}.$$

The first-order upwind scheme simply sets $\Psi(\theta) = 0$ in formulas (46). The Koren scheme ~~"limits"~~ limits its third-order and positive-coefficient correction to the upwind scheme by using this formula (Hundsdorfer and Verwer, 2010):

$$\Psi(\theta) = \max\left\{0, \min\left\{1, \theta, \frac{1}{3} + \frac{1}{6}\theta\right\}\right\}. \tag{47}$$

When using the Koren flux-limiter the stencil in Figure A4 is extended because regular grid neighbors $W_{i+2,j}$, $W_{i-2,j}$, $W_{i,j+2}$, $W_{i,j-2}$ are also involved in updating $W_{i,j}$. The flux-correction-limited Koren third-order scheme bypasses the first-order limitation of positive linear finite difference/volume schemes imposed by Godunov's barrier theorem (Hundsdorfer and Verwer, 2010, section I.7.1) by ~~having~~ using a nonlinear correction formula~~, i.e. the combination of and above~~. Though the Koren scheme is ~~usually~~ third-order where smoothness allows, it reverts to first-order at extrema and ~~other non-smooth areas~~ jumps where $\theta \gg 1$ or $\theta \ll 1$.

For either ~~the first-order or Koren schemes~~scheme, if the water input m is negative then we must actively enforce, by truncation, the positivity of the water thickness W. ~~Positivity~~ In fact, positivity of the source-free advection-diffusion scheme~~is~~, a desirable property ~~but it~~ which we can show by standard methods (Hundsdorfer and Verwer, 2010), does not ensure positivity of the solution if there is ~~actual water removal~~(water removal, i.e. if ~~$(m/\rho_w) - \partial W_{\text{til}}/\partial t < 0$~~). ~~Therefore we project (reset) W to be nonnegative at the end of each time step.~~ $m/\rho_w - \partial W_{\text{til}}/\partial t < 0$.

6.3 Mass conservation: positivity and stability

~~Explicit numerical scheme~~

6.3 Discretization of the pressure equation

Pressure evolution equation (31) is a nonlinear diffusion with "reaction" terms from the opening and closing of cavities. However, our numerical scheme for this equation is similar to the scheme for the mass conservation ~~PDE , combined with the first-order upwind case of formulas , is sufficiently simple so that we can analyze its stability properties. For this scheme we now sketch a maximum principle argument which shows stability (~~Morton and Mayers, 2005~~). The argument also shows positivity (~~Hundsdorfer and Verwer, 2010~~) as long as the total water input is nonnegative, but here only the case $m = 0$ and $W_{\text{til}}^{\max} = 0$ case is shown. Also we consider only the upwinding case where the discrete velocities at cell interfaces are nonnegative: $u_e \geq 0$, $u_w \geq 0$, $v_n \geq 0$, $v_s \geq 0$.The other upwinding cases can be handled by similar arguments.~~equation (section 6.1) because the spatial derivatives are actually the same in each equation, namely $\nabla \cdot \mathbf{q}$. Thus we reuse the computation of those derivatives, namely scheme (43), which gives $\mathcal{D}_{i,j}$.

~~Define $\nu_x = \Delta t/\Delta x$, $\nu_y = \Delta t/\Delta y$~~Let $\mathcal{O}_{ij}$, $\mathcal{C}_{ij}$ be the gridded values of the zeroth-order (i.e. without spatial derivatives) opening and closing rates; see equations (14), ~~$\mu_x = \Delta t/\Delta x^2$, and $\mu_y = \Delta t/\Delta y^2$. Collecting terms in to write the new value as a linear combination of the old values, we get~~

$$\begin{aligned} W_{i,j}^{l+1} &= (\nu_x u_w + \mu_x D_w) W_{i-1,j}^l + (\mu_x D_e) W_{i+1,j}^l \\ &\quad + (\nu_y v_s + \mu_y D_s) W_{i,j-1}^l + (\mu_y D_n) W_{i,j+1}^l \\ &\quad + \Big[1 - \nu_x u_e - \nu_y v_n \\ &\quad - \mu_x(D_e + D_w) - \mu_y(D_n + D_s)\Big] W_{i,j}^l \\ &= \tilde{A} W_{i-1,j}^l + \tilde{B} W_{i+1,j}^l + \tilde{C} W_{i,j-1}^l \\ &\quad + \tilde{D} W_{i,j+1}^l + \tilde{E} W_{i,j}^l. \end{aligned}$$

~~Because of our assumption about nonnegative velocities, and noting that the diffusivities are nonnegative, we see that coefficients $\tilde{A}, \tilde{B}, \tilde{C}, \tilde{D}$ are all nonnegative. Only $\tilde{E}$ could be negative, depending on values of ν_x, ν_y, μ_x, and μ_y~~(15). Define the sum of all zeroth-order terms:

$$Z_{ij} = \mathcal{C}_{ij} - \mathcal{O}_{ij} + \frac{m_{ij}}{\rho_w} - \frac{(W_{\text{til}})_{ij}^{l+1} - (W_{\text{til}})_{ij}^l}{\Delta t}. \tag{48}$$

Using (43) for the flux divergence, the scheme for pressure equation (31) is

$$\frac{\phi_0}{\rho_w g} \frac{P_{i,j}^{l+1} - P_{i,j}^l}{\Delta t} = -\mathcal{D}_{i,j} + Z_{ij}. \tag{49}$$

Because equation (48) uses the updated value $(W_{\text{til}})_{ij}^{l+1}$, equation (45) must be applied before (49) can be used to

update P. There are also special cases at the boundaries of the region where $W > 0$; see subsection 6.5.

~~Requiring $\tilde{E}$ in to be nonnegative is a sufficient stability condition (Morton and Mayers, 2005) , which we generate based on an equal split between advective and diffusive parts. First there is a CFL restriction for the advection terms, namely $\nu_x\alpha_e + \nu_y\beta_n \le \frac{1}{2}$, which is~~

6.4 Stability of time-stepping

A sufficient condition ~~when generalized to all upwinding cases. The second is a time-step restriction on the diffusion , namely $\mu_x(D_e + D_w) + \mu_y(D_n + D_s) \le \frac{1}{2}$, which is condition . If both and hold then the coefficient $\tilde{E}$ in is nonnegative~~for stability of mass-conservation scheme (44) comes from combining sufficient conditions for stability of the advection and diffusion parts. For the advection part we first define Δt_{CFL}, after the well-known Courant-Friedrichs-Lewy restriction for advection schemes (Morton and Mayers, 2005) , by

$$\Delta t_{\text{CFL}} \left(\frac{\max|u|}{\Delta x} + \frac{\max|v|}{\Delta y} \right) = \frac{1}{2}, \tag{50}$$

where $\mathbf{V} = (u, v)$ is the velocity of the water in the distributed system. For the diffusion part we define Δt_W by

$$\Delta t_W \max D \left(\frac{1}{\Delta x^2} + \frac{1}{\Delta y^2} \right) = \frac{1}{4}. \tag{51}$$

The condition $\Delta t \le \min\{\Delta t_{\text{CFL}}, \Delta t_W\}$ is sufficient for stability and convergence of scheme (44) if $\mathbf{V}$, D, and m were all externally-provided functions, i.e. in the case where the equations of (32) are decoupled. We can show this by maximum principle arguments for the first-order upwind advection choice (Morton and Mayers, 2005) , but standard theory at least suggests the same conclusion for the higher-order flux-limited advection scheme (Hundsdorfer and Verwer, 2010) .

~~Because the coefficients in linear combination also add to one, as the reader may check, it follows from and that the scheme is stable (Morton and Mayers, 2005) . It also follows from~~ These time-step restrictions can be understood by considering an example. We ran the model on a $\Delta x = \Delta y = 250$ m grid to approximate steady state for the subglacial hydrology of Nordenskiöldbreen(van Pelt, 2013) . We used realistic inputs for H, b, and ~~that if $W_{i,j}^l \ge 0$ for all i, j then gives $W_{ij}^{l+1} \ge 0$, in this $m = 0$ and $W_{\text{til}}^{\max} = 0$ case, which is our positivity claim. Thus, under conditions~~ m, but a spatially-constant ice sliding rate of $|\mathbf{v}_b| = 50$ m a^{-1}; other parameter values were from Table A1. The result is that the maximum computed water speed $|\mathbf{V}|$ is about 0.2 m s^{-1} so (50) ~~and , scheme is stable and positivity-preserving~~gives $\Delta t_{\text{CFL}} \approx 300$ s. Computed diffusivity $D = \rho_w g K W$ has a maximum value that varies significantly in time, $0.1 \le \max D \le 5\,\text{m}^2\,\text{s}^{-1}$. Using a typical value $\max D = 1\,\text{m}^2\,\text{s}^{-1}$ in (51) gives $\Delta t_W \approx 8000$ s. Thus in this simulation $\Delta t_W \approx 25 \Delta t_{CFL}$. This example suggests that, unless both the maximum speed $|\mathbf{V}|$ is unusually low, and deep subglacial lakes develop so that $\max D$ is large, the diffusive time scale is significantly longer than the CFL time scale. The scaling $\Delta t_W = O(\Delta x^2)$ versus $\Delta t_{CFL} = O(\Delta x^1)$ makes it clear that under sufficient spatial grid refinement Δt_W is controlling, but we suspect that Δt_{CFL} is controlling for $\Delta x > 100$ m.

6.5 Discretization of the pressure equation

~~The pressure evolution equation is a nonlinear diffusion with additional "reaction" terms associated to opening and closing~~However, the time step restriction from the pressure equation scheme is typically shorter than either Δt_W or Δt_{CFL}. The time step restriction for ~~our explicit pressure scheme~~ scheme (49) is comparable to Δt_W, though the proof above for the stability of the mass conservation scheme does not suffice to prove stability. ~~That is, if~~ Nonetheless we define Δt_P by

$$\Delta t_P \left(\frac{2 \max D}{\phi_0} \right) \left(\frac{1}{\Delta x^2} + \frac{1}{\Delta y^2} \right) = 1. \tag{52}$$

If the time step ~~satisfies $\Delta t \le \Delta t_P$, where~~ is set by

$$\Delta t_P \frac{2 \max D}{\phi_0} \frac{1}{\Delta x^2} + \frac{1}{\Delta y^2} = 1 \min\{\Delta t_{\text{CFL}}, \Delta t_W, \Delta t_P\}. \tag{53}$$

then we ~~assert that, and~~ observe in practice that ~~, the scheme~~ the coupled scheme consisting of (44), (45), and (49) is stable. ~~From~~

Recalling (51)~~the resulting time step~~ , however, Δt_P is actually a fraction of Δt_W~~:~~

$$\Delta t_P = 2\phi_0 \, \Delta t_W.$$

~~We can again be quantitative in a particular example. Consider the same 250 m simulation of the hydrology of~~ , namely $\Delta t_P = 2\phi_0 \, \Delta t_W$. If we return to the above example for Nordenskiöldbreen~~as earlier. With~~ , with $\phi_0 = 0.01$ we have ~~Δt_P which is 50 times smaller than Δt_W and half of Δt_{CFL}:~~

$\Delta t_W \approx 8000$ s	from (51),
$\Delta t_{\text{CFL}} \approx 300$ s	from (50),
$\Delta t_P \approx 160$ s	from (**??**).

~~This analysis suggests that the numerical scheme for pressure diffusion, given next, may often have~~ $\Delta t_W \approx 8000$

s, $\Delta t_{\text{CFL}} \approx 300$ s, and $\Delta t_P \approx 160$ s. In this case the pressure scheme has the shortest time step, but it ~~may be~~ is comparable to CFL. ~~Note that $\Delta t_{\text{CFL}} = O(\Delta x)$ while Δt_W and~~ Because Δt_P ~~are~~ is $O(\Delta x^2)$~~. The time step restriction~~, the pressure scheme restriction is certainly controlling for sufficiently-fine grids. However, the time step Δt_P ~~scales with the adjustable regularizing~~ also scales with porosity ϕ_0, so we can make it more or less severe by adjusting that parameter.

~~The scheme we use~~ If implicit time-stepping were instead used for the pressure equation~~is similar to the scheme we have just presented for the mass continuity equation . Denote $\psi^l_{i,j} = P^l_{i,j} + \rho_w g(b^l_{i,j} + W^l_{i,j})$. Let $\mathcal{O}_{ij} = c_1 |\mathbf{v}_b|_{i,j} \left(W_r - W^l_{i,j}\right)_+$ and $\mathcal{C}_{ij} = c_2 A \left(\rho_i g H_{i,j} - P^l_{i,j}\right)^3 W^l_{i,j}$ be the gridded values of the cavitation-opening and creep-closure rates. Also define the sum of all zero order (i.e. without spatial derivatives) terms~~

$$Z_{ij} = \mathcal{C}_{ij} - \mathcal{O}_{ij} + \frac{m_{ij}}{\rho_w} - \frac{(W_{\text{til}})^{l+1}_{ij} - (W_{\text{til}})^l_{ij}}{\Delta t}.$$

~~Using for the flux divergence, the scheme for pressure equation is now~~

$$\frac{\phi_0}{\rho_w g} \frac{P^{l+1}_{i,j} - P^l_{i,j}}{\Delta t} = -\mathcal{D}_{i,j} + Z_{ij},$$

~~or, in explicit update form,~~

$$P^{l+1}_{i,j} = P^l_{i,j} + \frac{\rho_w g \, \Delta t}{\phi_0} \left(-\mathcal{D}_{i,j} + Z_{ij}\right).$$

~~Because equationuses the updated value $(W_{\text{til}})^{l+1}_{ij}$, equation must be applied before can be used to update P to the new time t_{l+1}.~~

~~There are special cases at the boundaries of the active subglacial layer: *(i)* where there is no ice $H_{i,j} = 0$ and land $(b_{i,j} > 0)$ we set $P^{l+1}_{i,j} = 0$, *(ii)* where the ice is floating we set $P^{l+1}_{i,j} = (P_o)_{i,j}$, and *(iii)* where there is grounded ice $(H_{i,j} > 0)$ and no water $(W^l_{i,j} = 0)$ we set $P^{l+1}_{i,j} = (P_o)_{i,j}$ if there is no basal sliding and $P^{l+1}_{i,j} = 0$ if there is sliding (because of cavitation; see equation)~~, which requires overt variational inequality treatment to preserve physical pressure bounds (Schoof et al., 2012) , then the time scales $\Delta t_W, \Delta t_{CFL}$ addressed here are the only restrictions. The time step restriction Δt_W could also be removed by implicit steps for the mass-conservation equation, though again this requires a variational inequality formulation because of the lower bound $W \geq 0$. Our observation above that $\Delta t_{CFL} \ll \Delta t_W$ for practical ice sheet grids suggests that implicit time-stepping for the mass-conservation equation is not beneficial.

6.5 One time step of the model

Mathematical model (32) evolves the fields W, W_{til}, and P. Here we describe one time step of the fully-discretized ~~evolution. For convenience we treat~~ coupled evolution.

For convenience only we denote the ice geometry, bed geometry, and sliding speed ~~as fixed, and so $h_{i,j}$~~(i.e. $H_{i,j}$, $b_{i,j}$, $(P_o)_{i,j}$, and $|\mathbf{v}_b|_{i,j}$~~are all denoted as~~) as though they were all time-independent.

~~The ice~~ The geometry may be quite general, with ice-free land~~or floating ice~~, floating ice shelf, or ice-free ocean allowed at any location (x_i, y_j). ~~The ice geometry~~ In fact, the geometry data determines boolean "masks" ~~for gridcell state~~ ~~(~~on the grid, based on zero as the sea level elevation~~):~~:

$$\texttt{icefree}_{i,j} = (h H_{i,j} \geq 0) \,\&\, (h_{i,j} = b_{i,j} \geq 0),$$

$$\texttt{float}_{i,j} = (\rho_i (H_{\text{float}})_{i,j} < -\rho_{sw} b_{i,j}),$$

~~Here we take a sea-water density~~ where $\rho_{sw} = 1028.0$ ~~and define $H_{\text{float}} = h_{i,j}/(1-r)$ as the thickness of the ice if it is floating, where $r = \rho_i/\rho_{sw}$. Note that~~ is sea-water density. Note $\texttt{float}_{i,j}$ is ~~also true in ice-free ocean~~ true both where there is floating ice shelf and where the ocean is ice-free. The subglacial hydrology model exists only for grounded ice, that is, only if both flags $\texttt{icefree}$ and $\texttt{float}$ are false. ~~The other mask cases provide boundary conditions when they are neighbors to grounded ice-filled cells.~~

One time step follows this algorithm:

(i) Start with values $W^l_{i,j}$, $(W_{\text{til}})^l_{i,j}$, $P^l_{i,j}$ which satisfy ~~the~~ bounds $W \geq 0$, $0 \leq W_{\text{til}} \leq W^{\max}_{\text{til}}$, and $0 \leq P \leq P_o$.

(ii) Get $(W_{\text{til}})^{l+1}_{i,j}$ by (45). Enforce $0 \leq W_{\text{til}} \leq W^{\max}_{\text{til}}$. If $\texttt{icefree}_{i,j}$ or $\texttt{float}_{i,j}$ then set $(W_{\text{til}})^{l+1}_{i,j} = 0$.

(iii) Get W values averaged onto the staggered grid from (39), staggered grid values of the effective conductivity K from (40), velocity components u, v at staggered grid locations from (41), and staggered grid values of the diffusivity D from (42).

(iv) Get time step ~~$\Delta t = \min\{\Delta t_{\text{CFL}}, \Delta t_W, \Delta t_P\}$ using criteria , , and~~ Δt from (53).

(v) Using (46) and a ~~particular~~ flux-limiter $\psi(\theta)$, compute the advective fluxes $Q_e(\alpha_e)$ ~~at all staggered-grid points $(i+1/2, j)$~~ and $Q_n(\beta_n)$ at all staggered-grid points~~$(i, j+1/2)$~~.

(vi) Get flux divergence approximations $\mathcal{D}_{i,j}$ ~~of the flux divergence~~ from (43). ~~For each direction (i.e. x- and y-directions), do not compute the divided-difference contribution to the flux divergence in if either neighbor is $\texttt{icefree}$ or $\texttt{float}$.~~

(vii) If $\texttt{icefree}_{i,j}$ then set $P_{i,j}^{l+1} = 0$. If $\texttt{float}_{i,j}$ then set $P_{i,j}^{l+1} = (P_o)_{i,j}$. If $W_{i,j}^{l} = 0$and , and if $\texttt{icefree}_{i,j}$ and $\texttt{float}_{i,j}$ are both false, then either set $P_{i,j}^{l+1} = (P_o)_{i,j}$. Then use to compute values for (no sliding) or $P_{i,j}^{l+1} = 0$ (any sliding). Otherwise use (49) to compute $P_{i,j}^{l+1}$ at the remaining locations.

(viii) If $P_{i,j}^{l+1}$ does not satisfy bounds $0 \le P \le P_o$ then reset (project) truncate/project into this range.

(ix) If $\texttt{icefree}_{i,j}$ or $\texttt{float}_{i,j}$ then set $W_{i,j}^{l+1} = 0$. Otherwise use (44) to compute values for $W_{i,j}^{l+1}$.

(x) If $W_{i,j}^{l+1} < 0$ then reset (project) truncate/project to get $W_{i,j}^{l+1} = 0$.

(xi) Update time $t_{l+1} = t_l + \Delta t$ and repeat at *(i)*.

This recipe goes with a reporting scheme for mass conservation. Note that in steps *(ii)* and *(ix)* water is lost or gained at the margin where either the ice thickness goes to zero on land (margins), or at locations where the ice becomes floating (grounding lines). Because such loss/gain may be the modeling goal—users want hydrological discharge—these amounts are reported. This reporting scheme also tracks the projections in step *(x)*, which represent a mass conservation error which goes to zero under in the continuum limit $\Delta t \to 0$.

7 PISM options for hydrology models

In this section we document the runtime options for the PISM hydrology model (PISM authors, 2013) . There are three choices of model equations, namely `distributed`, `routing`, and `null`. The first of these is the complete model described in this paper. The other two are reductions; we list them in order of decreasing complexity

6.1 Run-time options for hydrology models

Option `-hydrology NAME`, where `NAME` is one of the three headings below, chooses the model equations.

6.2 `distributed`

This most-complete PISM hydrology model is chosen by runtime option `-hydrology distributed`. It is `distributed`: This model is governed by the full set of equations (32) in section 5; see also Tables A1and A2. The full set of parameters (Table A1) and variables (Table A2) are active in this model.

6.2 `routing`

This model is chosen by option `-hydrology routing`. It is governed by a subset of equations , with the equation for evolution of pressure P removed, and with the replacement $P \mapsto P_o = \rho_i g H$ in defining K, $\mathbf{V}$, and ψ. Thus the equations simplify to:

$$\frac{\partial W}{\partial t} + \frac{\partial W_{\text{til}}}{\partial t} = -\nabla \cdot (\mathbf{V} W) + \nabla \cdot (D \nabla W) + \frac{m}{\rho_w},$$
$$\frac{\partial W_{\text{til}}}{\partial t} = \frac{m}{\rho_w} - C_d,$$

along with `routing`: In this reduced model the equation for pressure evolution is replaced by $P = P_o$. The evolution equations for the state variables W and W_{til}, and the bounds $0 \le W$ and $0 \le W_{\text{til}} \le W_{\text{til}}^{\max}$. The determination of N_{til} and τ_c is , are unchanged.

6.2 `null`

This non-conserving model is chosen by option `-hydrology null`. It is the default hydrology model in PISM`null`: This further-reduced model is non-conserving. It has only the state variable W_{til} . The equations are the same as in except that there is no "W" and the first of equations is gonewhich is subject to bounds $0 \le W_{\text{til}} \le W_{\text{til}}^{\max}$ and evolves by equation (16).

6.2 Configurable constants

All of the constants in Table A1 are configurable parameters in PISM. The correspondence between PISM parameters names and the symbols the notation in this paper is and PISM's configurable parameters is shown in Table A3. These parameters can be changed set at runtime by using the parameter name as an option, or by setting a `pism_overrides` variable in a NetCDF file which is read with the `-config_override` option . See (PISM authors, 2013) . File `src/pism_config.cdl` for determines the default values and units.

7 Results

7.1 Verification of the coupled model

By using the coupled, steady-stateexact solution constructed in section ?? we can verify , nearly-exact solution (subsection 5.1) we verified most of the numerical schemes described above. (Verification is the process of measuring and analysing the errors made by the numerical scheme, especially as the numerical grid is refined (Wesseling, 2001; Bueler et al., 2005) .

We initialize (Wesseling, 2001) .) To do this we initialized our time-stepping numerical scheme with the exact

nearly-exact steady solution and we measure measured the error relative to the steady exact values after one model-month. The continuum time-dependent model (32) would cause no drift away from steady state, so any drift is error.

For the verification runs we use the values in Table ??. We do numerical error. We did runs on grids decreasing by factors of two from 2 km to 125 m. Figure A5 shows the results based on first-order upwinding for the fluxes.

This convergence evidence suggests that we have implemented the numerical schemes in section 6, for the coupled advection-diffusion-reaction equations for W and Phave correctly-implemented numerical schemes, correctly. The rate of convergence in this verification case is roughly linear (i.e. about $O(\Delta x^1)$) because the largest errors arise at locations of low regularity of the exact solution, including the radius $r = R_1$ $r = 5$ km where P abruptly quickly drops from P_o, and at the ice sheet margin $r = L$ where there is a jump in the water thickness W to zero.

The rates of convergence for average errors are nearly identical for the higher resolution higher-resolution flux-limited (Koren) scheme and for the first-order upwinding scheme (not shown). Because our problem is an advection-diffusion problem in which both the advection velocity and the diffusivity are solution-dependent, it is difficult to separate the errors arising from numerical treatments of advection and diffusion. The first-order upwinding scheme for the advection has much larger numerical diffusivity but this diffusivity is masked by the physical diffusivity. Based on our verification evidence it is reasonable to choose the simpler first-order upwinding for applications. It also , as it requires less interprocess communicationin a parallel implementation like ours.

7.2 Application of to the model at Greenland ice sheet scale

We now apply our mass-conserving hydrology models to the entire Greenland ice sheet at 2 km grid resolution. This non-trivial example demonstrates the model at large computational scale using real ice sheet geometry, with one-way coupling from ice dynamics for a realistic distribution of sliding giving a realistic distributions of overburden pressure, ice sliding speed, and basal melt rate.

7.2.1 Spun-up initial state

The PISM dynamics and thermodynamics model (Bueler and Brown, 2009; Winkelmann et al., 2011; Aschwanden et al., 2012), using the non-mass-conserving `null` hydrology model (section ??subsection 6.1), was applied by grid sequencing used to compute a consistent and nearly-steady model of the ice sheet, a "spun-up" initial state. Model choices for ice dynamics, including enhancement factor, sliding law power, and till friction angle, follow Aschwanden et al. (2013) . The steady , following the procedures in Aschwanden et al. (2013) . Our model uses no spatially-variable parameter values, such as basal shear stresses, found by inversion of surface velocities. The bed elevations and present-day climate of the ice sheet, especially surface mass balance and surface temperature temperature and surface mass balance (Ettema et al., 2009), were from the SeaRISE data set for Greenland (Bindschadler et al., 2013). The

The spin-up grid sequence was to run 50 ka on a 20 km grid, 20 ka on a 10 km grid, 2 ka on a 5 km grid, and finally 200 a on a 2 km grid. All model fields were bilinearly interpolated , with bilinear interpolation at each refinement stage. This whole spinup used 2800 processor-hours on 72 processors on a linux cluster with 2.2 GHz AMD Opteron processors, a small computation for modern supercomputers.

The final 2 km stage, on a horizontal grid of 1.05 million grid points, used uniform 10 m vertical spacing so that the ice sheet flow was modelled on a structured 3D grid of 460 million grid points(e. g. locations where ice temperature and velocity were computed)velocity/temperature points. This whole spin-up used 2800 total processor-hours on 72 2.2 GHz AMD Opteron processors, a small computation for modern supercomputers.

The results of this spin-up were validated by comparing results to present-day observations. In the last 100 a of the final stage this run the ice sheet volume varied by less than 0.04 percent. Other more active measures showed stability during the last 100 a at the level of less than one percent (e.g. the area of temperate base and the maximum ice velocity over the whole sheet) to at most a few percent (the floating ice area).

The results of this whole-ice-sheet spinup were validated by comparing results to present-day observations. (Though , so the model is in nearly steady state, though the actual Greenland ice sheet may not be as close to steady.) The spun-up ice sheet volume of $3.094 \times 10^6\,\mathrm{km}^3$ is close to the present-day volume of $3.088 \times 10^6\,\mathrm{km}^3$ computed from the SeaRISE data on the same grid. However, in describing more careful validation measures for similar 2 km PISM model runs, Aschwanden et al. (2013) observe that volume aloneis inadequate for model validation. A Compared to volume alone, a better evaluation of dynamical quality is shown in Figure ??, which compares to compare the modeled and observed surface speed. We see that the extent of the Northeast Greenland ice stream is smaller than observed, and the distribution of flow in Western Greenland outlet glaciers differs from the observed pattern. However, our model uses no spatially-variable parameter values such as basal shear stresses found by inversion of surface velocities (Aschwanden et al., 2013) (Joughin et al., 2010) surface speed, with a very similar result to the comparison described in Aschwanden et al. (2013) .

The spun-up initial state includes, in particular, modelled ice thickness H, basal melt rate m, and sliding velocity $|\mathbf{v}_b|$; the latter two fields are shown in Figure A6. ~~We note that the areas~~ Areas of sliding roughly coincide with areas of basal melt because ~~modeled basal resistance~~ heat-producing (modeled) basal drag comes from the yield stress parameterized in section 3.

7.2.2 Experimental setup and model runs

We used fields H, m, $|\mathbf{v}_b|$ from the spun-up state as steady data in five model-year runs of ~~our mass-conserving hydrology~~ the `routing` and `distributed` ~~models. Because these fields were fixed,~~ hydrology models; see subsection 6.1 for model descriptions. Thus only one-way coupling was tested: a steady ice dynamics model fed its fields to an evolving subglacial hydrology model. The hydrology model was initialized with the W_{til} values from the spun-up state, but with $W = 0$ initial values ~~and~~ for both models, and also $P = 0$ initial values ~~(~~for `distributed`~~)~~.

~~These runshad 1.05 million subglacial hydrology grid points at which~~ In the runs, variables W, W_{til}, and P were recomputed at each time-step~~according to the numerical model described in section 6~~, at each of 1.05 million subglacial hydrology grid points, using parameter values from Table A1. In both `routing` and `distributed` models the ~~modelled~~ hydrological system became ~~quite~~ steady after the first three model years.

~~The adaptively-determined~~ Adaptively-determined time-steps ~~for the hydrology model~~ reached a steady level of about 4 ~~model hours~~ model-hours for the `routing` model based on maximum subglacial water speeds $|\mathbf{V}|$ of 0.05 $\mathrm{m\,s^{-1}}$ and maximum diffusivity D of 10.6 $\mathrm{m^2\,s^{-1}}$. For the `distributed` model the time steps were actually slightly longer, primarily because `routing` concentrates large water amounts and fluxes along steepest-descent paths~~. The~~ ; the time steps were about 6 ~~model hours based on~~ model-hours based on maximum speeds $|\mathbf{V}|$ of 0.03 $\mathrm{m\,s^{-1}}$ and much smaller maximum diffusivities D of about 0.25 $\mathrm{m^2\,s^{-1}}$. ~~(Higher water velocities $\mathbf{V}$ were seen in the 250 m grid resolution case mentioned in section 6, based on additional simulated surface water input added to the thermodynamically-generated basal melt rate (~~van Pelt, 2013~~) , and the pressure time steps in that case were shorter than the mass time steps.)~~ These hydrology-only runs used much less computation than the spin-up: 14.7 processor-hours for the `routing` run and 14.2 for `distributed`.

7.2.3 `routing` results

The final ~~values of~~ W_{til} and W ~~for~~ fields from the `routing` run are shown in Figure A7. ~~We see that the~~ The till is fully saturated ($W_{\text{til}} = 2$ m) in essentially all areas where basal melt occurs. In the outlet glacier areas the transportable water W concentrates along curves of steepest descent of the hydraulic potential; ~~this effect is seen in~~ see detail in Figure A8. The location of the pathways is determined primarily by the bedrock elevation detail provided by the SeaRISE data set, which is limited. Furthermore, the grid resolution of 2 km, while very high for ~~contemporary ice dynamics~~ whole ice sheet models, still ~~represents a significant~~ causes spatial "smearing" of the flow pathways. ~~Specifically, though relatively few areas have $W > 1$ m, the~~

The continuum limit of the model would ~~be expected to have $W \gg 1$ m in~~ have concentrated pathways of a few meters to tens of meters width.

~~This model~~ These concentrated pathways could be regarded as ~~a~~ minimal "conduit-like" ~~description~~ features of the subglacial ~~flow, because of these concentrated pathways~~hydrology. As noted in the introduction, however, our model has no "R-channel" conduit mechanism, in which dissipation heating of the flowing water generates wall melt-back. ~~The location of pathways/conduit here is determined primarily by the bedrock elevation detail provided by the SeaRISE data set, which is limited; the results are especially suspect in the Eastern outlet glaciers in Figure A8.~~

7.2.4 `distributed` results

The final values of W and the relative water pressure P/P_o for the five model-year `distributed` run are shown in Figure A9. ~~Again the~~ The till is full ($W_{\text{til}} = 2$ m) in essentially all areas where basal melt occurs, ~~and indeed~~ so, as W_{til} is ~~not shown because it is identical to that in~~ nearly-identical to the `routing` ~~model in this one-way coupled case~~result, it is not shown.

Recall that $|\mathbf{v}_b|$ determines the pressure drop caused by ~~cavitation~~sliding-generated cavities. The effect is to spread out the water W relative to the `routing` model, as clearly seen in Figure A9. There is now no strong concentration of W along curves of steepest descent of the hydraulic potential~~. This result depends, however, on the~~ , but the spreading depends on opening and closing parameters in the `distributed` model, especially parameters c_1, c_2, ϕ_0, W_r~~;~~ ~~see Tables A1 and A3~~. Darcy flux model parameters α, β, k are also important. Parameter identification using observed surface, in situ, basal-reflectivity, discharge, and other data, though needed, is beyond our current scope.

We can examine the local relationship between water ~~amount~~ layer thickness W and pressure P in the `distributed` results. Though the model is near steady state, the basal melt rate, sliding speed, and overburden pressure all show ~~the large spatial variationswhich are characteristic of a real ice sheet. Figure A10 shows that if~~ realistically-large spatial variations. In Figure A10 we "bin" pairs (W, P) by relatively-narrow sliding ~~velocity ranges , as shown in each scatter plot, then there is usually a rough~~ speed ranges (each sub-plot) and color the points by the ice thickness. There is an increasing relationship between W

and the relative pressure P/P_o ~~. At~~ in each bin. While in the fast-sliding ~~locations the water amount~~ case W is often comparable to the bed roughness scale W_r~~. For low sliding velocities~~ , for slow sliding we see generally lower water amounts ($W \lesssim W_r/10$) but a full range of pressures. In thick ice the pressure P is close to overburden even if there is fast sliding. Locations with high sliding, high water amount, and low pressure ~~also~~ always have low ice thickness. ~~Note Figure A10 would show even more scatter if the run were not close to steady state, for example if there were time-varying surface melt input into the subglacier (~~van Pelt, ~~2013) .~~

8 Conclusions

This paper documents additions made to the Parallel Ice Sheet Model in its 0.6 version released February 2014. It describes and demonstrates a subglacial hydrology model which is novel in having these features:

- a 2D parallel implementation of a coupled till-and-linked-cavities model~~(sections 2–6)~~,
- ~~an englacial porosityregularization which allows a practical numerical model in which physical bounds~~ a pressure-equation regularization, using notional englacial porosity, which eases implementation and improves numerical performance,
- a scheme for maintaining physical pressure bounds ($0 \le P \le P_o$~~ hold~~) at all times~~(sections 4 and 6)~~,
- ~~an analysis of steady states (subsection 5.3 and Appendix A), describing the actual diffusivity of the advective flux in that case, an exact~~ verification using a nearly-exact solution of the coupled ~~mass~~ mass-conservation and pressure equations, in the steady radial case~~(section ??), leading to verification (section 7)~~, and
- demonstration at high resolution and whole ice-sheet scale on a million-point hydrology grid~~(section 7)~~.

~~The comprehensive treatment here of certain subjects is also important. We have clarified the relationship of several~~

Furthermore, the comprehensive exposition here clarifies the relationship among several pressure-determining "closures" ~~which turn morphological ideas about the subglacial aquifer into concrete pressure equations~~ (section 4), and ~~created and implemented~~ it allows us to understand our model as a common extension of several ~~seemly-disparate~~ seemingly-disparate published models (section 5). ~~Adeliberate limitation in scope of the current paper is that we show only~~ Additional analysis (Appendix A) shows that in that in steady state a functional relationship "$P = P(W)$" arises between pressure and water layer thickness. This analysis reveals the diffusive nature of the apparently-advective part of the steady-state flux.

The current paper only demonstrates one-way coupling~~. In this paper~~ , 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.

9 Code availability

The source code for all versions of PISM is available through host website https://github.com/pism/pism. Extensive PDF and searchable browser documentation for PISM is contained both in the source code and online through PISM homepage http://www.pism-docs.org/. PISM is licensed under the GNU General Public License (version 3).

Acknowledgements. Comments by editor Dan Goldberg, reviewer Tim Bartholomaus, and two anonymous reviewers improved the focus and quality of the paper. Detailed comments by Andy Aschwanden and Martin Truffer were much appreciated. Constantine Khroulev helped with the PISM implementation. The first author was supported by NASA grant #NNX13AM16G. This work was supported by a grant of high-performance computing resources from the Arctic Region Supercomputing Center. ~~Constantine Khroulev helped with the PISM implementation. Detailed comments by Andy Aschwanden, Tim Bartholomaus, and Martin Truffer were much appreciated.~~

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Appendix A

Analysis of steady states

Relative to the time-dependent ~~form~~ model equations (32), steady-state equations (35)–(38) have separate balances between the divergence of the flux and the water input~~(equation)~~, and the opening and closing processes~~(equation).~~

~~Equation~~ . In particular, equation (38) allows us to write the pressure $P = P(W)$ in steady state as a continuous function of the ~~water amount~~ layer thickness W. ~~Steady~~ However, steady state is only possible if a condition holds:

$$c_1|\mathbf{v}_b|(W_r - W)_+ \le c_2 A P_o^3 W. \tag{A1}$$

~~That is,~~ This condition says that the maximum closing rate $\mathcal{C}(N,W)$, which occurs at zero water pressure, must ~~match the~~ equal or exceed the sliding-generated opening rate $\mathcal{O}(|\mathbf{v}_b|,W)$~~, which is pressure-independent. Define the following~~ .

We define a scaled basal sliding speed which has units of pressure; it is a scale for the pressure drop from cavitation:

$$s_b = \left(\frac{c_1|\mathbf{v}_b|}{c_2 A}\right)^{1/3}. \tag{A2}$$

Then (A1) is equivalent to

$$W \ge W_c := \frac{s_b^3}{s_b^3 + P_o^3} W_r.$$

~~If or holds~~ the condition $W \ge W_c$, where $W_c = W_r s_b^3/(s_b^3 + P_o^3)$ is a critical water thickness. If $W \ge W_c$ then

$$P(W) = P_o - s_b \left(\frac{(W_r - W)_+}{W}\right)^{1/3}. \tag{A3}$$

~~Note that in~~ Formula (A3) ~~we have $P(W_c) = 0$~~applies even if $W \ge W_r$, in which case $P = P_o$. Underpressure ($P = 0$) with subcritical water amount ($W < W_c$) does not occur in steady state, though it can occur in nonsteady conditions~~. Formula may apply even if $W \ge W_r$, in which case the water pressure takes the overburden value $P = P_o$.~~

; note $P(W_c) = 0$. Figure A11 shows the function $P(W)$ ~~from~~ for different values of sliding speed $|\mathbf{v}_b|$, and Figure A12 shows ~~the function for~~ it for different values of overburden pressure P_o. ~~We see that as the water amount reaches the roughness scale ($W \to W_r$) the pressure rises rapidly to overburden ($P(W) \to P_o$). At the other extreme, we see that $P(W) \to 0$ as $W \to W_c$. The curves $P(W)$ in Figures A11 and A12 do not include the underpressure cases $0 \le W < W_c$ wherein is violated.~~

~~Recall that~~ Flowers and Clarke (2002a) propose function $P_{FC}(W)$~~for~~ —see equation (28)—for both steady and nonsteady circumstances. Both functions $P(W)$ ~~in~~ and $P_{FC}(W)$

~~in are increasing. They both relate the water pressure~~ are increasing, and both relate P to the overburden pressure P_o. However, ~~while~~ in (A3) the relation of P to P_o is additive, while in (28) ~~it is a multiplicative scaling. The power~~ they are proportional. Power law form (28) is not justified by the physical reasoning which led to equation (A3), even in steady state. It would appear that any functional relationship $P(W)$ should also depend on the sliding velocity, as it does here, if cavitation influences the water pressure. ~~Also, the $W > W_{\text{crit}}$ casegives $P_{FC}(W) > P_o$~~ In any case, in ~~, but this condition does not arise in .~~ ~~In~~ the current paper ~~is that~~ we do not ~~set~~ impose a relationship $P = P(W)$ at all, ~~even~~ though such a relation emerges in ~~runs with steady stateinputs~~steady state.

We now consider how the steady state water velocity $\mathbf{V}$, and the associated flux $\mathbf{q}$, depends on other quantities. ~~Because $\mathbf{V}$ depends on ∇P, according to~~ First, from equations (35) and (A3), in steady state we have

$$\frac{\partial P}{\partial W} = \frac{s_b W_r}{3W^{4/3}(W_r - W)^{2/3}} \tag{A4}$$

if $W_c < W < W_r$. If $W \le W_c$ then $\partial P/\partial W$ is undefined, and if $W > W_r$ then $\partial P/\partial W = 0$. ~~Note that the condition $W_c < W < W_r$ corresponds to the pressure condition $0 < P < P_o$ in steady state.~~ Formula (A4) and Figures A11 and A12 agree that $\partial P/\partial W \to \infty$ as ~~$W \to W_r$. Equations $W \nearrow W_r$.~~

Now note that equations (35), (A3), and (A4) imply a formula for the velocity in steady state:

$$\mathbf{V} = -k\left[\nabla\psi_o - \left(\frac{W_r - W}{W}\right)^{1/3}\nabla s_b + \frac{s_b W_r}{3W^{4/3}(W_r - W)^{2/3}}\nabla W\right], \tag{A5}$$

where $\psi_o = P_o + \rho_w g b$.

~~Formula helps us understand the advective flux "$\mathbf{V}W$" in $\mathbf{q}$. The~~ Thus the direction of water velocity $\mathbf{V}$ is determined by a combination of a geometric direction ($\nabla\psi_o$), a direction derived from spatial variations in the sliding speed (∇s_b), and a diffusive direction (∇W). ~~Thus~~ Indeed, a portion of the advective flux $\mathbf{V}W$ is diffusive in steady state, in addition to the *a priori* diffusive flux $-D\nabla W$; recall equation (11) in subsection 2.4. In fact~~we can write the whole flux as a linear combination of gradients,~~

$$\mathbf{q} = -kA_1\nabla\psi_o + kA_2\nabla s_b - kA_3\nabla W,$$

~~with coefficients~~

$$A_1 = W,$$
$$A_2 = (W_r - W)^{1/3}W^{2/3},$$
$$A_3 = \frac{s_b W_r}{3(W_r - W)^{2/3}W^{1/3}} + \rho_w g W.$$

~~The first two coefficients A_1, A_2 go to zero as $W \to 0$, but A_3~~ , because the coefficient of ∇W in (A5) remains large when $W \to 0$, as long as sliding is occurring ($s_b > 0$)~~. Thus~~ , then for low water amount and sustained sliding we should think of the water as diffusing in the layer. ~~When~~ On the other hand, when the water thickness is ~~greater, namely if it is almost~~ almost at the roughness scale ($W \lesssim W_r$), then ~~A_3 is~~ the same coefficient is also large in sliding cases ($s_b > 0$); again the effect is diffusive.

~~In steady state the water amount W roughly scales with $1/k$ where k is the hydraulic conductivity. In fact, if we combine equation with and rearrange slightly then we find~~

$$-\nabla\cdot(A_3\nabla W) = \frac{m}{k\rho_w} + \nabla\cdot(A_1\nabla\psi_o) - \nabla\cdot(A_2\nabla s_b)\,.$$

~~One may regard as a non-linear elliptic equation for W. In fact, in the case where H, b, and $|\mathbf{v}_b|$ are all spatially-uniform, so that $\nabla\psi_o = \nabla s_b = 0$, equation is of the form $-\nabla\cdot(A_3(W)\nabla W) = m/(k\rho_w)$ where $A_3(W) = A_3$ is given in . If W is both bounded away from zero and bounded away from the roughness scale W_r (i.e. there is $\epsilon > 0$ so that $\epsilon < W < W_r - \epsilon$) then this equation is uniformly elliptic. Thus a maximum principle applies (?) . This means that the maximum of W will equal or exceed the maximum of W along the boundary of that region, so the graph of W is concave down. Thus the values of W will scale with $1/k$. Indeed, for the simpler equation $-\nabla\cdot(D_0\nabla W) = m_0/(k\rho_w)$, with D_0, m_0 positive constants, on a disc of radius L, and zero boundary values, the solution has maximum value $W(0)$ which precisely scales as $1/k$. As seen in numerical results, the solution W of will also scale with $1/k$ if $\nabla\psi_o$ and ∇s_b are not too large. (However, if $W \approx 0$ or $W \lesssim W_r$ then the diffusivity coefficient $A_3(W)$ will be large and so the values of W away from the boundary will be flattened-out by the resulting fast diffusion.)~~

~~Constants used in constructing the exact solution. **Name Value Units Description**α 1 power in flux β 2 power in flux H_0 500 m center thickness k $0.01/(\rho_w g)$ $\text{m}^3\,\text{s}\,\text{kg}^{-1}$ hydraulic conductivity L 22.5km cliff at $r = 0.9R_0$ m_0 $0.2\rho_w$ $\text{kg}\,\text{m}^{-2}\,\text{a}^{-1}$ water input rate R_0 25 km ideal ice cap radius R_1 5 km sliding starts v_0 100 $\text{m}\,\text{a}^{-1}$ sliding speed scale W_r 1 m roughness scale~~

~~To evaluate the result of the 2 km grid spun-up ice dynamical model we compare modelled ice speed at the ice surface (left; $\text{m}\,\text{a}^{-1}$) to satellite observations (right; $\text{m}\,\text{a}^{-1}$).~~

Table A1. Physical constants and model parameters. All values are configurable in PISM; see Table A3.

Name	Default	Units	Description
A	3.1689×10^{-24}	$\mathrm{Pa}^{-3}\,\mathrm{s}^{-1}$	ice softness (Huybrechts et al., 1996)
α	$5/4$		power in flux formula (Schoof et al., 2012)
β	$3/2$		power in flux formula (Schoof et al., 2012)
c_0	0	Pa	till cohesion (Tulaczyk et al., 2000a)
c_1	0.5	m^{-1}	cavitation coefficient (Schoof et al., 2012)
c_2	0.04		creep closure coefficient
C_c	0.12		till compressibility (Tulaczyk et al., 2000a)
C_d	0.001	$\mathrm{m\,a}^{-1}$	background till drainage rate
δ	0.02		N_{til} lower bound, as fraction of overburden pressure
e_0	0.69		reference void ratio at N_0 (Tulaczyk et al., 2000a)
ϕ_0	0.01		notional (regularizing) englacial porosity
g	9.81	$\mathrm{m\,s}^{-2}$	acceleration of gravity
k	0.001	$\mathrm{m}^{2\beta-\alpha}\mathrm{s}^{2\beta-3}\mathrm{kg}^{1-\beta}$	conductivity coefficient (Schoof et al., 2012)
N_0	1000	Pa	reference effective pressure (Tulaczyk et al., 2000a)
ρ_i	910	$\mathrm{kg\,m}^{-3}$	ice density (Greve and Blatter, 2009)
ρ_w	1000	$\mathrm{kg\,m}^{-3}$	fresh water density (Greve and Blatter, 2009)
W_r	0.1	m	roughness scale (Hewitt et al., 2012)
$W_{\text{til}}^{\text{max}}$	2	m	maximum water in till (Bueler and Brown, 2009)

Table A2. Functions used in subglacial hydrology model (32).

Type	Description (symbol, units, meaning)		
state	W	m	transportable water thickness
	W_{til}	m	till-stored water thickness
	P	Pa	transportable water pressure
input	b	m	bedrock elevation
	φ		till friction angle
	H	m	ice thickness
	m	$\mathrm{kg\,m}^{-2}\,\mathrm{s}^{-1}$	total melt water input
	$\lvert\mathbf{v}_b\rvert$	$\mathrm{m\,s}^{-1}$	ice sliding speed
output	N_{til}	Pa	till effective pressure
	τ_c	Pa	till yield stress

Table A3. Correspondence between PISM parameter names and symbols in this paper ~~and PISM configuration parameter names. Alphabetical by parameter name~~(Table A1). All ~~of these~~ are used in the `distributed` model, with ~~the~~ indicated subsets ~~also~~ used in the `routing` and `null` models.

PISM ~~configuration~~ parameter name	**Symbol**	**`routing`**	**`null`**
`fresh_water_density`	ρ_w	×	×
`hydrology_cavitation_opening_coefficient`	c_1		
`hydrology_creep_closure_coefficient`	c_2		
`hydrology_gradient_power_in_flux`	β	×	
`hydrology_hydraulic_conductivity`	k	×	
`hydrology_regularizing_porosity`	ϕ_0		
`hydrology_roughness_scale`	W_r		
`hydrology_thickness_power_in_flux`	α	×	
`hydrology_tillwat_decay_rate`	C_d	×	×
`hydrology_tillwat_max`	$W_{\text{til}}^{\text{max}}$	×	×
`ice_density`	ρ_i	×	×
`ice_softness`	A		
`standard_gravity`	g	×	×
`till_c_0`	c_0	×	×
`till_compressibility_coefficient`	C_c	×	×
`till_effective_fraction_overburden`	δ	×	×
`till_reference_effective_pressure`	N_0	×	×
`till_reference_void_ratio`	e_0	×	×

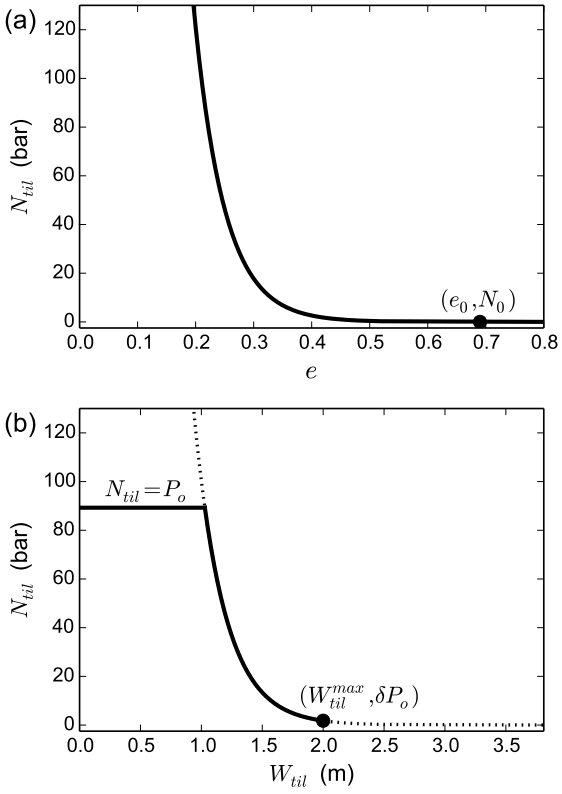

Fig. A1. (a) Equation (19) determines the effective pressure N_{til} as a function of the void ratio e~~, as shown here. Reference~~; reference values ~~of~~ e_0 ~~and~~ ,N_0 are indicated. **(b)** The same curve, but with N_{til} as a function of W_{til}, ~~and bounded~~ with bounds above by overburden pressure P_o and below by a fixed fraction δ of P_o~~(~~; the solid curve ~~),~~ is used in our model. The case shown ~~is for~~ has 1000 meters ice thickness.

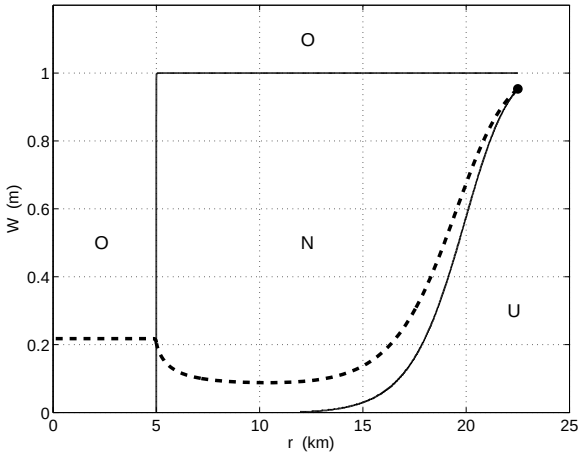

Fig. A2. ~~An exact~~ A nearly-exact radial, steady solution for water thickness $W(r)$ (dashed). In r-versus-W space the overpressure (O), normal pressure (N), and underpressure (U) regions (solid curves) are determined by ice geometry and sliding velocity~~(solid curves; see text)~~, because this is steady state.

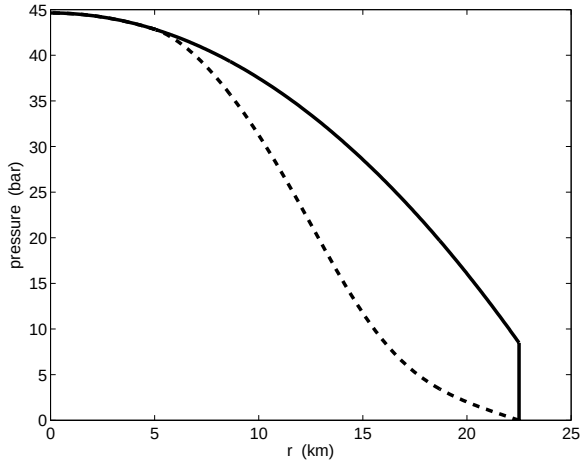

Fig. A3. ~~An exact~~ A nearly-exact radial, steady solution for pressure $P(r)$ (dashed) and overburden pressure P_o (solid).

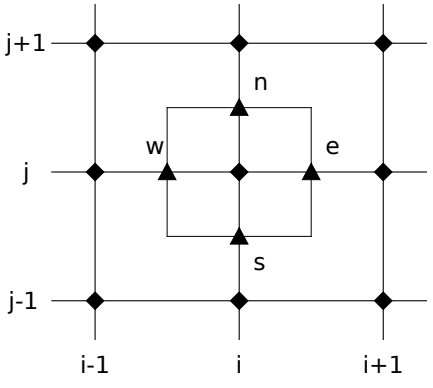

Fig. A4. Numerical schemes (44) and (49) use a grid-point-centered cell. Velocities, diffusivities, and fluxes are evaluated at staggered grid locations (triangles at centers of cell edges) denoted by compass notation e, w, n, s~~)~~. State functions ~~W, P~~ W, W_{til}, P are located at regular grid points (diamonds).

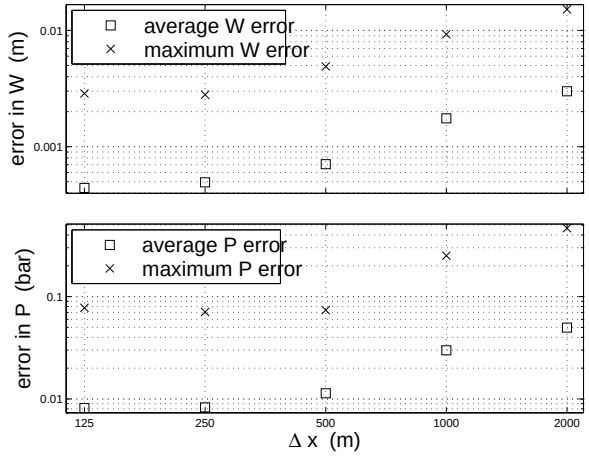

Fig. A5. Average water thickness error $|W - W_{exact}|$ decays as $O(\Delta x^{0.91})$, and average pressure error $|P - P_{exact}|$ decays as $O(\Delta x^{0.92})$, for grids with spacing $250 \le \Delta x = \Delta y \le 2000$ m.

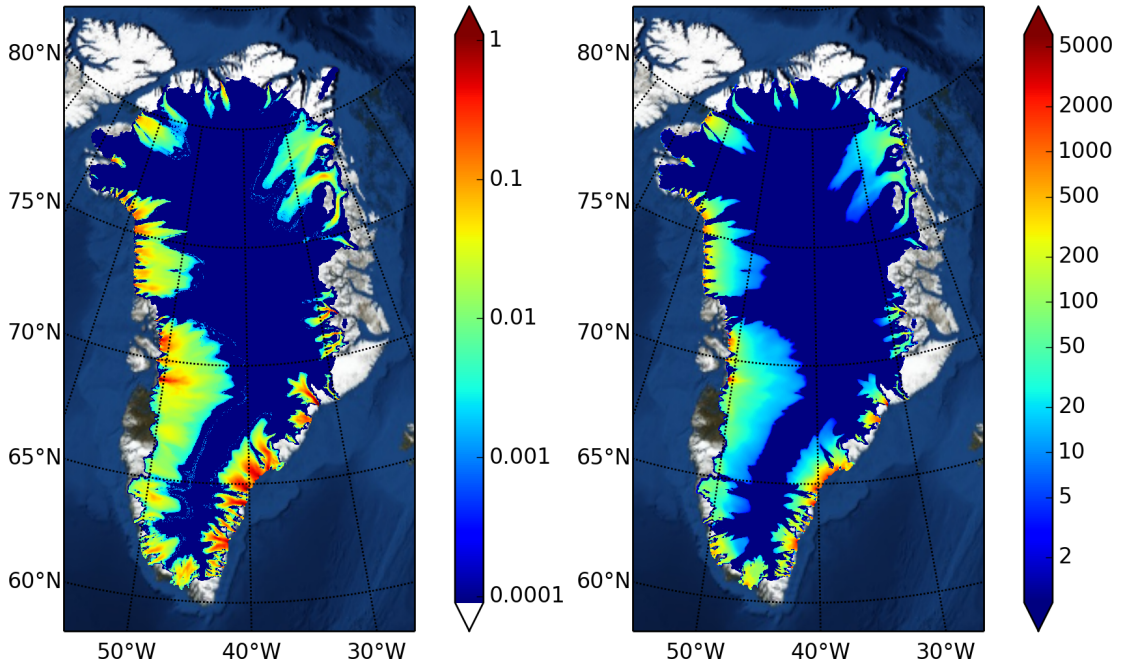

Fig. A6. The inputs to the hydrology model are the modeled basal melt rate m/ρ_w (left; $\mathrm{m\,a^{-1}}$) and sliding speed $|\mathbf{v}_b|$ (right; $\mathrm{m\,a^{-1}}$) from the spun-up ~~model~~state.

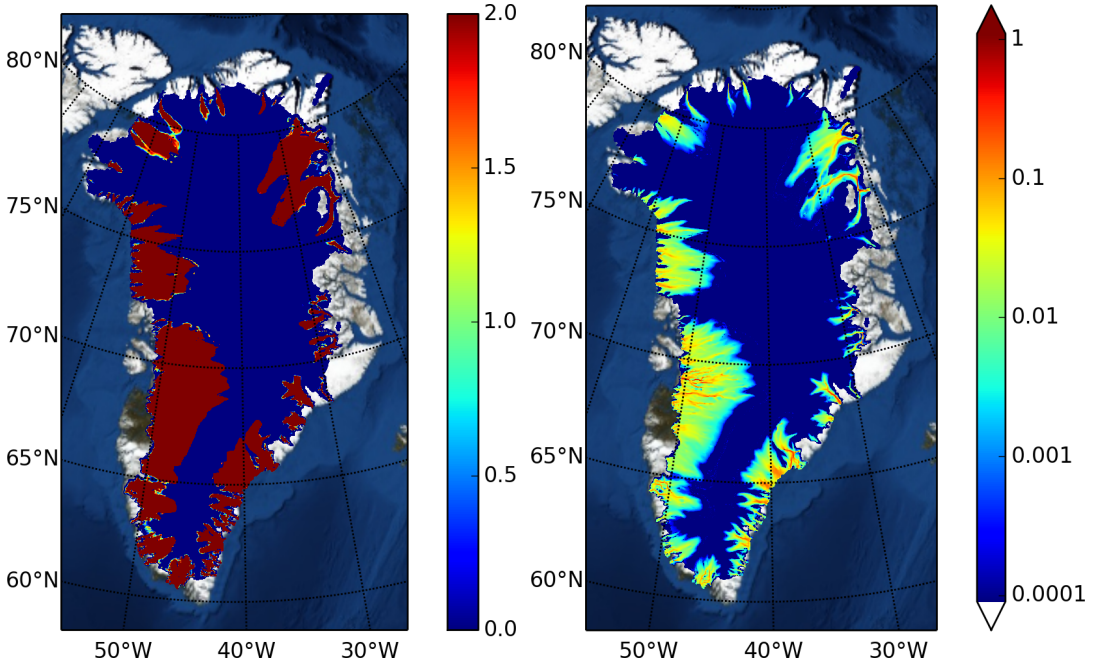

Fig. A7. Outputs from the `routing` hydrology model are the modelled till-stored water layer thickness W_{til} (left; m) and modelled transportable water layer thickness W (right; m).

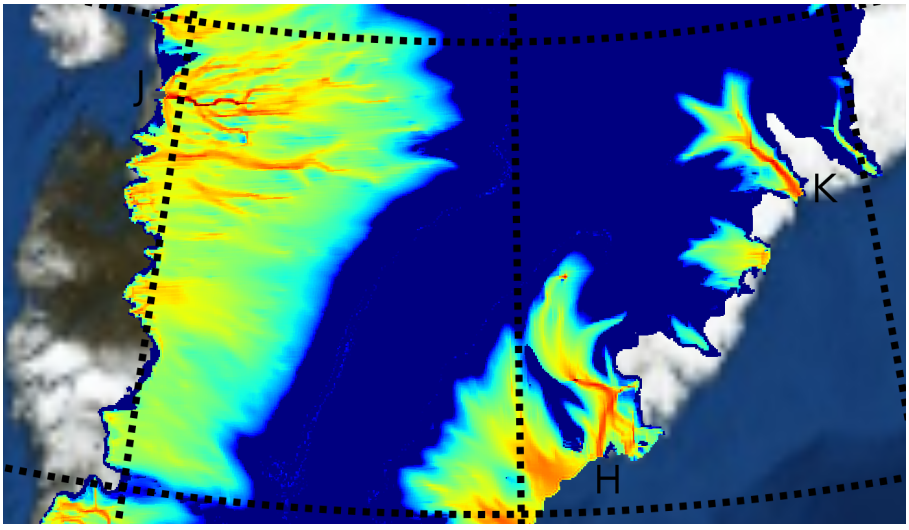

Fig. A8. Detail of transportable water W plotted in Figure A7, covering Jakobshavn (J), Helheim (H), and Kangerdlugssuaq (K) outlet glaciers

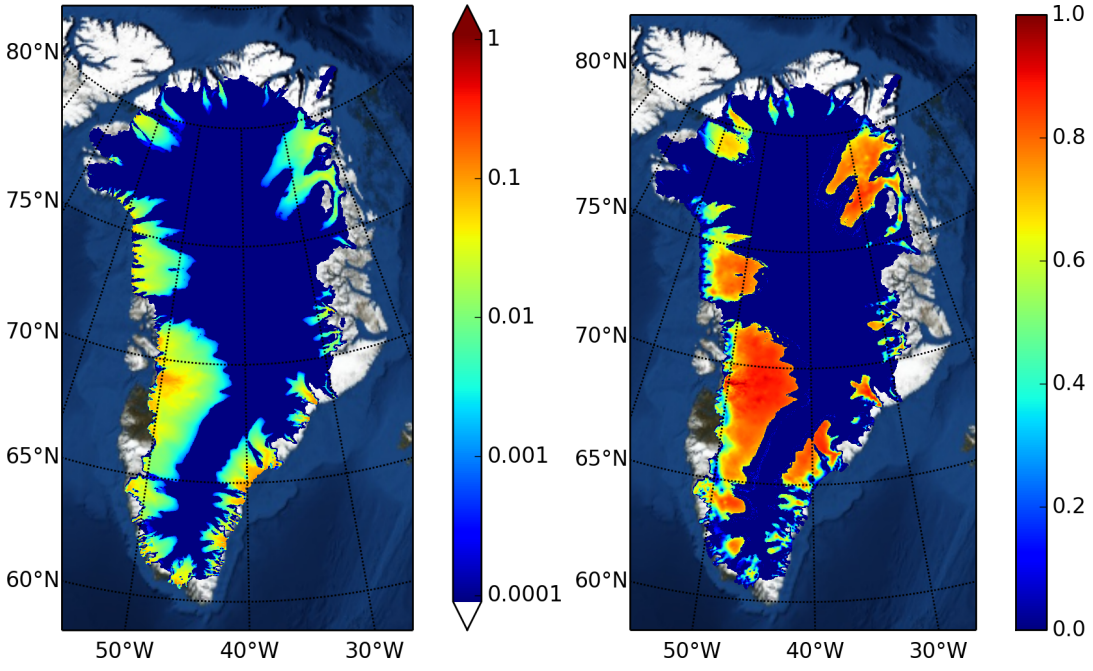

Fig. A9. Outputs from the `distributed` hydrology model include the modelled transportable water layer thickness W (left; m), and the modelled transportable water layer pressure P, shown relative to overburden pressure ~~P/P_o~~ (i.e. P/P_o; right).

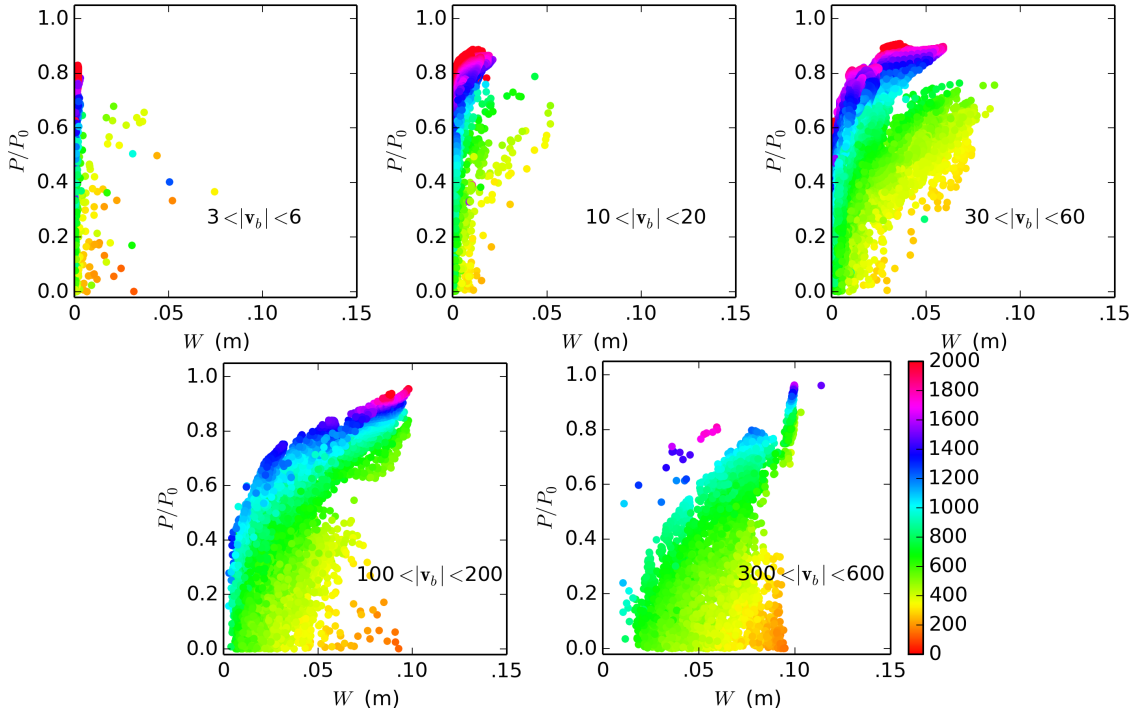

Fig. A10. Scatter plots of ~~(W,P)~~ $(W,P/P_o)$ pairs for all cells ~~at end of a 5 model year steady-input simulation on a 2 km grid for~~ from the ~~whole Greenland ice sheet using~~ `distributed` model run, which used roughness scale $W_r = 0.1$ m. Each ~~scatter plot~~ sub-plot only shows ~~the~~ pairs ~~for a select~~ from the indicated range of ice sliding speeds~~, as indicated~~. Points are colored by ice thickness using a common scale shown beside last figure.

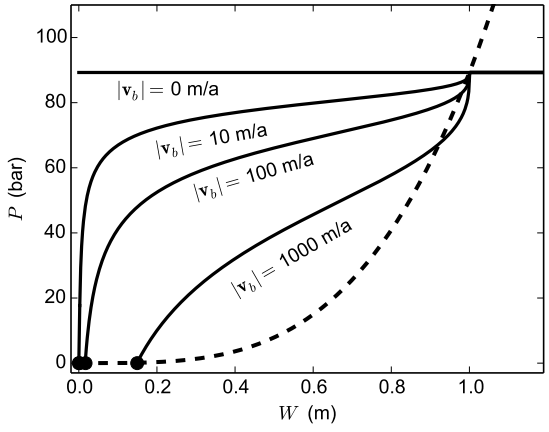

Fig. A11. The steady state function $P(W)$ defined by equation (A3)~~depends on the sliding speed $|\mathbf{v}_b|$. Four cases are shown. All use~~, using $W_r = 1$ m and ~~a uniform ice thickness of~~ $H = 1000$ m (solid curves). Values of W_c are indicated by black dots at $P = 0$. ~~Relation~~ For comparison, Flowers and Clarke (2002a) relation (28) ~~(dashed black)~~ is shown with $W_{\text{crit}} = 1$ m ~~for comparison~~(dashed black).

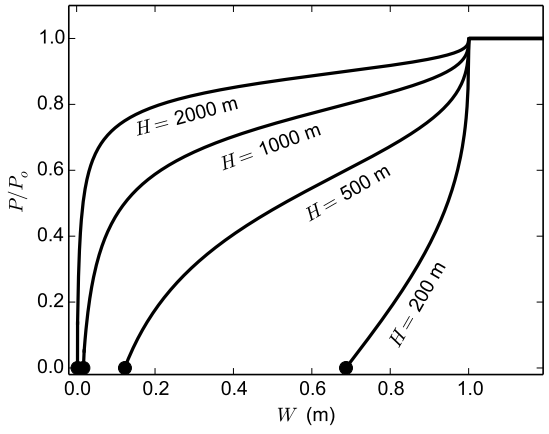

Fig. A12. The graph of $P(W)$ defined by (A3) also depends on overburden pressure $P_o = \rho_i g H$~~. We fix~~, shown using $|\mathbf{v}_b| = 100$ m/a and $W_r = 1$ m~~and consider four cases of uniform thickness H~~.