Response to Referee 1 on "A fast and unified subglacial hydrological model applied to Thwaites Glacier, Antarctica" by Kazmierczak, Gregov, Coulon & Pattyn.

Dear Referee,

We would like to thank you for the time you have already devoted to reviewing our manuscript. In order to clarify some elements and to enable you to continue your review, we provide hereafter additional comments related to the assumptions made within the original manuscript. Our comments are written in blue. The revised manuscript will be modified so that this information is more complete and clear.

Best regards,

On behalf of the authors,
Thomas Gregov

Response to the Referee's comments

In this work, the authors develop a new hydrological model for Antarctica, and then apply it to some example cases, including modelling the retreat rate of Thwaites Glacier. I am reading the examples as test-cases of the model implementation, rather than fully fledged investigations into the likely future behaviour of Thwaites, and I appreciate that the abstract and conclusions respect this level of preliminarity (although the title might make one think otherwise).

We agree with the Referee – we will change the title in order to be more generic. Thwaites is used as a particularly interesting test case, as it is thought to be composed of both hard and soft regions.

The authors make some interesting modelling assumptions in the setup of the hydrology model, some of which are also found in Gowan et al. (2023), a paper I will admit I was not familiar with. The current manuscript presents itself as not proposing too much beyond simplifications that are already present somewhere in the literature. However, given the number of different hydrological models currently out there, it would be good to compile a clear list of the simplifying assumptions at play in this work, so that future users can quickly assess if their use-case fits in this framework.

We thank the Referee for the suggestion of making the assumptions clearer. As those are not necessarily common within other hydrological models, this should indeed help potential readers identify more easily whether or not our model is suited for specific applications. We will also clarify the differences with the assumptions made in Gowan et al. (2023).

As I read it, the modelling assumptions are

- The hydrological system is always in steady state, i.e. the timescale of basal melt and channel development is fast compared to the timescale of forcing changes - likely a good assumption for Antarctica, less so for seasonal meltwater input in Greenland (so figure 7 seems a bit of a perverse/misleading test case - although here the timescale appears to be thousands of years, so perhaps this is not supposed to investigate seasonality, just a demonstration of the non-monotonicity of figure 5?).

  That is correct. Our model is meant for Antarctica, and should probably not be used for Greenland. Figure 7 is associated with subsection 3.2 ('the efficient to non-efficient switch'), so the role of this figure is indeed to investigate the non-monotonicity of the relation between the flux and the effective pressure, rather than the effect of seasonality.

- Gradients in hydraulic potential are primarily geometric, since N is slowly varying, except at the grounding line, so when converting between Q_w and S using (5a), we can ignore gradients in N. This seems reasonable, but I don't quite understand the paragraph at l.126 - "so we choose not to do this" (do what?). Isn't $\boldsymbol{q}_\mathrm{w}$ being computed directly from (2) without any specification of what gradient it is proportional to? Perhaps the way (2) is solved could be made more explicit - no expression for $\boldsymbol{q}_\mathrm{w}$ is given in the manuscript.

This requires a bit of explanation. Equation (2) is given by

$$\begin{cases} \nabla \cdot \boldsymbol{q}_\mathrm{w} = \dfrac{\dot{m}}{\rho_\mathrm{w}}, \quad \text{in } \Omega, & \text{(R1a)} \\ \boldsymbol{q}_\mathrm{w} \cdot \boldsymbol{n} = 0, \quad \text{on } \Gamma_\mathrm{d}. & \text{(R1b)} \end{cases}$$

By itself, this system of equations alone cannot be used to determine the subglacial water flux $\boldsymbol{q}_\mathrm{w}$. Indeed, if (q_x, q_y) are the components of $\boldsymbol{q}_\mathrm{w}$, then equation (R1a) is explicitly given by

$$\frac{\partial q_x}{\partial x} + \frac{\partial q_y}{\partial y} = \frac{\dot{m}}{\rho_\mathrm{w}}, \quad \text{in } \Omega, \tag{R2}$$

that is, one equation for two unknowns. To determine $\boldsymbol{q}_\mathrm{w}$, we therefore need some additional information. Physically, equation (R1a) constrains how the subglacial water can evolve: in each arbitrary region (and, in particular, in each grid cell), there is an imbalance between the inflow and outflow of subglacial water, the imbalance being caused by the basal-melt term $\dot{m}/\rho_\mathrm{w}$. However, (R1a) does not specify where (i.e., in which direction) the excess of water goes. It is for that reason that we need to specify the direction of $\boldsymbol{q}_\mathrm{w}$.

This can be made more explicit by considering an integral version of (R1a): integrating this equation over a grid cell ω of the mesh, one gets

$$\psi_\mathrm{out} = \psi_\mathrm{in} + \int_\omega \frac{\dot{m}}{\rho_\mathrm{w}} \, \mathrm{d}\omega, \tag{R3}$$

in which ψ_in and ψ_out are the inflow and outflow integrated scalar fluxes, respectively. Explicitly, these are given by

$$\psi_\mathrm{in} = -\int_{\partial\omega_\mathrm{in}} \boldsymbol{q}_\mathrm{w} \cdot \boldsymbol{n} \, \mathrm{d}l \quad \text{and} \quad \psi_\mathrm{out} = \int_{\partial\omega_\mathrm{out}} \boldsymbol{q}_\mathrm{w} \cdot \boldsymbol{n} \, \mathrm{d}l, \tag{R4}$$

with $\partial\omega_\mathrm{in}$ (resp. $\partial\omega_\mathrm{out}$) the part of cell boundary associated to an inflow of subglacial water, i.e., $\boldsymbol{q}_\mathrm{w} \cdot \boldsymbol{n} < 0$ (resp. $\boldsymbol{q}_\mathrm{w} \cdot \boldsymbol{n} > 0$). Note that, by construction, ψ_in and ψ_out are non-negative. Equation (R3) can then be iteratively solved by determining the value of the scalar flux in a cell, and then 'propagating' the outgoing flux to neighboring cells that are in the direction of $\boldsymbol{q}_\mathrm{w}$. This is the method we follow to solve this equation; specifically, we use the method of Le Brocq et al. (2009), as stated in the original manuscript. This method takes the form of a routing algorithm, and is based on earlier developments (Budd and Warner, 1996; Le Brocq et al., 2006). It has been used in the context of the computation of subglacial water flow in other studies, e.g., in Pattyn (2010) and in Kazmierczak et al. (2022).

It remains to clarify the question of the direction of $\boldsymbol{q}_\mathrm{w}$. Physically, this direction is the same as the direction of the hydraulic potential gradient $\nabla\phi$, as water flows from high to low values of the hydraulic potential ϕ. We also have

$$\nabla\phi = \nabla\phi_0 - \nabla N, \tag{R5}$$

in which, at this point in the manuscript, $\nabla\phi_0$ is known, but ∇N is not. However, we anticipate the result that $\nabla\phi \approx \nabla\phi_0$ in most of the domain. Therefore, we state that the direction of $\boldsymbol{q}_\mathrm{w}$ is the one of $\nabla\phi_0$, and not the one of $\nabla\phi$. We therefore choose not to include the term ∇N in the computation of the direction of the flow.

Note there is an extra factor of S_∞ in (6a), but I assume this is just a typo, since the plots of N_∞ in figure 5 show the correct behaviour from (5b).

Indeed, thanks!

- Close to the grounding line, N must go to zero, so by eye, the authors pick an error function to approximate this transition. Per appendix B, this is not the solution to any local inner form of the ODE, but just a function that has the right gradient at the grounding line.

  It is true that the error function is not the analytical solution to the inner problem. Nonetheless, it is reasonably close to the numerical solution to that problem (see figure B1(b)). Hence, its use has the advantage of leading to a practical closed-form expression: the error function is easy to compute and available in most scientific computing libraries. At the same time, its use leads to a relatively small error.

- Drainage density, regardless of the nature of the basal hydrology, is constant in space and time, and thus the flux through a drainage element is some constant, large, multiple of the flux through the area it represents. This one I find harder to wrap my head around, particularly since inefficient drainage is often imagined as slow flow everywhere (so what even is a drainage element in this case?) and models such as GlaDS and SHAKTI show dynamically evolving channel networks and drainage densities over time. This really is a big simplification, and the one that allows for the shift in scale, and I'm saddened that it is not discussed further (the choice of value for l_c, the drainage density, is not discussed at all).

  We agree with the Referee that this is a strong assumption in the model, and we apologize for not discussing it further.

  In our model, we took $l_\mathrm{c} = 10\,\mathrm{km}$, which is similar to the value used in Gowan et al. (2023). Although the choice of taking a value of l_c that is both uniform and constant is made for simplicity, the value chosen in Gowan et al. (2023) is based on some observations of the distance between eskers. The goal of our study is to provide a model that is capable of representing the essential physics of different types of hydrological components (efficient/inefficient, hard/soft) at a relatively large scale, i.e., at a resolution that is of the order of at least a few kilometers. In that sense, we use a 'lumped-element' approach, i.e., we parametrize complex and distributed flows using simplified relations that aim to reproduce the overall behavior of the system. With that in mind, it seems to us that developing models that are able to incorporate these different types of hydrological components is particularly important, which is why we have focused our study on including these, rather than prescribing or tuning parameters. This is corroborated by our results, which are noteworthy; they suggest that, even for a a relatively simple model (e.g., with a constant and uniform value for l_c), (i) including subglacial hydrology that is coupled to ice flow greatly increases the sensitivity of marine ice sheets to external forcings and (ii) there is a strong dependency on the efficiency of the system and on the type of bed.

  Nonetheless, we acknowledge that the drainage density, in general, should not be a constant in space or in time. As such, it could be particularized to the type of drainage system or the type of bed, e.g., by tuning it against a high-resolution hydrological model such as GlaDS or SHAKTI. We leave this for future work. We still want to emphasize that the value of l_c does not change the fundamental dynamics that govern water flow, in the sense that, for example, the effective pressure is an increasing or decreasing function of the subglacial water flux is mainly unchanged if the value of l_c is modified. In the end, friction coefficients (and, possibly, other parameters), will be tuned so that some computed quantities fit observations. Again, with that in mind, it seems more important to obtain the right dynamical relations between the variables of the system rather than determining exactly one of the parameters, as many choices of parameters can potentially lead to a good fit with observations. By contrast, fixing correctly parameters does not guarantee the correct relation between the effective pressure and the flux, for example.

This discussion should also, hopefully, respond to the Referee's question about the nature of an inefficient drainage component: it is a component that is such that the relation between N and Q in a grid cell is the one prescribed by an inefficient system (i.e., equation (6) with $H \sim \sqrt{S}$); no more, no less.

- Effective pressure within the drainage elements (a small proportion of the domain) is equal to the effective pressure everywhere else - despite how strongly models that resolve the channels show them as being local lows in the hydraulic potential. (Not discussed)

  We agree with the Referee's comment that our model is not able to include any spatial variation of the effective pressure at a scale that is smaller than the grid scale. That is an inherent limitation of the approach that we have pursued. Although this has been mentioned in the discussion section, we will discuss it in more detail in the revised manuscript.

- Specific choices about how H, L, and S depend on the type of bed, which are well-discussed and clear.

  Ok.

- Specific choices about how Q_w depends on S and $\nabla\phi$, which have quite a lot of precedent in the literature, although I might have expected a non-turbulent parametrisation for the inefficient system, and it's not clear why K should be the same for all geometries.

  Here we have followed Schoof (2010) and Gowan et al. (2023). We recognize that there is quite an important missing portion of the literature that we have omitted in our paper, and will include the relevant references in the revised version.

  Fundamentally, there is no reason why K should be the same for all geometries. Our rationale is the same as the one described earlier for the choice of values for l_c: in our paper, we focused on the right relations rather than the right parameters, although we acknowledge that the choice of parameters could, and should, be studied in more details in future work.

I'm also confused about the basal melt production. No expression for the $\dot{m}_\mathrm{w}$ in equation (3) is given, the term driving feedback between routing and meltwater production. The channelised version of the expression is given in (5b), but it's not clear if/how this is included in the routing algorithm.

Indeed, the expression for $\dot{m}_\mathrm{w}$ is missing from the manuscript. It is given by

$$\dot{m}_\mathrm{w} = \frac{|\boldsymbol{q}_\mathrm{w} \cdot \nabla\phi|}{L_\mathrm{w}} = \frac{1}{l_\mathrm{c}} \frac{Q_\mathrm{w}\|\nabla\phi\|}{L_\mathrm{w}} = \frac{1}{\Delta x}\left[\frac{\Delta x}{l_\mathrm{c}} \frac{Q_\mathrm{w}\|\nabla\phi\|}{L_\mathrm{w}}\right]. \tag{R6}$$

Here, $\Delta x/l_\mathrm{c}$ is the number of conduits per cell, and $Q_\mathrm{w}\|\nabla\phi\|/\rho_i L_\mathrm{w}$ is the melt production per conduit. In our numerical experiments, the term $\dot{m}_\mathrm{w}$ was found to be relatively small in the total melt production.

This feedback also seems to be missing in (B1b), with the meltwater input to the channel assumed constant (scaled to 1 in B4b) and not dependent on local melt.

Indeed, we have assumed a constant meltwater input. In fact, it turns out that the local melt is not an important contribution to the total melt in a channel. We refer to Lu and Kingslake (2023) – in which the authors describe a model analogous to ours – that show through a scaling analysis that this term can be dropped at leading order for marine ice sheets.

I have not read too closely into the experiments, and model results, nor provided specific linebyline minor comments, because I would like more clarity on the model setup first. I hope this is ok. I do think this is potentially quite an interesting approach to modelling Antarctic hydrology, but I would like to see more justification from the authors for the assumptions of their model.

We thank the Referee for their encouraging comment, and hope that the justifications provided here will allow them to continue their review.

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