Response to Referee 3 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 spent reading our paper, and for your comments on it. Your questions should help us to clarify certain points and improve our paper.

You will find below, in blue, our responses to your comments.

Best regards,

On behalf of the authors,
Thomas Gregov

Response to the Referee's comments

In this paper, the authors present a novel subglacial hydrology model that includes efficient and inefficient drainage systems on bed rheologies that range continuously from soft to hard. The model can be run on the same spatial scales as ice flow, which makes the model computationally efficient. The model is applied to Thwaites Glacier in Antarctica and the response to present day climate forcings is analyzed for hard, soft, and mixed beds for efficient and inefficient drainage systems.

I think the development of a model with these capabilities is an exciting and important step in modeling coupled ice flow and subglacial hydrology on the ice sheet scale. There are a few things that I believe should be clarified and adapted in the writing before publication.

We would like to thank you for your encouraging comment. You will find the responses to your remarks hereafter.

General comments:

Although assuming hydrologic equilibrium are most likely appropriate for Antarctica, it should be made clear that this model would not be appropriate for modeling places with more highly variable water fluxes such as mountain glaciers or the Greenland ice sheet (or at least the margins) where the subglacial hydrological system is often not in equilibrium due to external meltwater input which varies on much shorter timescales than changes in ice geometry.

Indeed, our model is clearly primarily meant for Antarctica. To clarify this, we have made the following changes to the manuscript:

- The title has been changed to "*A fast, simplified, and unified subglacial hydrological model for the Antarctic ice sheet and outlet glaciers*".
- We have modified the Model section in order to highlight the key assumptions of our model. It is now structured as follows:
  1. Ice-flow model
  2. Hydrological model
     2.1. Simplifying assumptions
     2.2. Subglacial water routing
     2.3. Subglacial effective pressure
     2.4. Bed rheology

  The additional subsection, 'Simplifying assumptions', should clarify our hypotheses. In it, we specify that the equilibrium assumption is based on the study of subglacial hydrology in

> Antarctica (Le Brocq et al., 2009; Pattyn, 2010; Kazmierczak et al., 2022), where there is limited surface water infiltration. Thus, changes in hydrology are primarily due to changes in ice geometry. Since the time scales associated with water flow are much smaller than those associated with ice flow, subglacial hydrology automatically adapts to any change in ice geometry and reaches the associated equilibrium.
>
> In this additional subsection, we also discuss the simplification of the hydraulic gradient (the assumption that $\|\nabla N\| \ll \|\nabla \phi_0\|$ far from the grounding line, so that $\nabla \phi \approx \nabla \phi_0$ in that region), and the conduits distribution (uniform drainage density with no sub-grid resolution).

There could be an argument that this model could be applied to the Greenland Ice Sheet on long timescales (that neglect short term changes resulting from subglacial hydrology), but this would need to be thoroughly addressed and justified. Add to the abstract and conclusion that this model is appropriate for modeling subglacial hydrology where changes in water flux are on the same timescales as changes in ice geometry. Does the thermomechanical ice flow model determine where there is basal melting? If so, does this evolve in time? How is water routed through areas with a frozen bed? Is there refreezing? Elaborate more on this, perhaps in section 2.1.

We agree with the referee that the current model cannot be applied to the Greenland ice sheet, since short-time variations in meltwater supply and subglacial water pressure are important and neglected in the current description. As mentioned in the previous response, we have modified the title and added a new subsection to highlight this assumption and its range of validity.

The thermomechanical ice flow determines where basal melting occurs and these regions may evolve over time (Pattyn, 2017). In our idealized simulations, the bed is entirely temperate, and for our simulations over Thwaites, the vast majority of the domain is also temperate, since the geothermal heat flow underneath the West Antarctic ice sheet is rather high and most of the bed remains temperate.

There are a few places in the manuscript where references are made to literature with different timescales and rates of change being discussed than in this paper. In this model, the assumptions around hydrologic equilibrium (melting balances closure in Eq. 5b) imply that the changes in time in the subglacial hydrologic system are small enough to neglect ($\mathrm{d}S/\mathrm{d}t = 0$). However, the manuscript contains references to Schoof (2010) and Iken (1981) which are specifically referring to when $\mathrm{d}S/\mathrm{d}t$ is not zero. In line 267, there is a reference to Schoof (2010) and the importance of the meltwater variability rather than meltwater input, but these processes are time-dependent changes that occur on timescales of hours to days, at most months. From Schoof (2010), “Further acceleration must then be driven instead by **short-term temporal variability** in water supply.” In this model, it is assumed to be in steady state which means that these short term increases in water pressure due to varying water flux on short timescales that result in conduit growth are inherently neglected. The same is true for the reference in line 292 in the manuscript, which I believe is referring to the following statement in Iken (1981). “It has been seen that the effect of a water pressure p_w on the sliding velocity is largest at the instant of separation and then gradually decreases **until steady cavities have formed**.” This is again talking about when the system is not in steady state, unlike the model presented here. To be clear, I think the assumption of steady-state is reasonable given the context of Antarctica, where changes in water flux occur on long-timescales, but these references and associated statements (specifically noted in the line-by-line comments) are not applicable in this context.

It is true that the assumptions in Schoof (2010) apply to the Greenland ice sheet, with, in particular, a strong melt supply variability. Here our model fails to cope with such changes. However, an important aspect of that paper is the ability of water channelization to slow down the ice. Fundamentally, this channelization mechanism is linked to the form of the $Q-N$ curve, and is more specifically due to the fact that $\partial N/\partial Q > 0$ for channels. While the hydrological system in our manuscript is considered at equilibrium, it does evolve *with respect to the ice-sheet geometry*. Nevertheless, we have removed the reference to Schoof (2010) on Line 267; we agree that it was misleading as it was associated with shorter time scales.

Moreover, the comparison with Iken (1981) remains in our opinion legitimate: while the hydrological system is assumed to be at equilibrium at each time, this does not exclude its evolution over time. In particular, cavities are allowed to grow over time, provided the hydrological system stays in quasi-static equilibrium with respect to the ice-sheet geometry.

Line-by-line comments:

Line 5: This is worded strangely. Perhaps, "We find that accounting for subglacial hydrology in the sliding law accelerates the grounding line retreat of Thwaites Glacier under present-day climatic conditions." or something similar.

Thanks for your suggestion, which has been corrected accordingly.

Line 13: 'behavior' since using American spelling elsewhere.

Corrected.

Fig 1. Figure could be made part of Fig. 9 or moved to before Fig. 9, but I don't feel strongly about this.

Following your suggestion, we have included this figure as an inset of Figure 9.

Line 71: introduce variable before introducing product of variables "... N is the effective pressure, C is a friction coefficient limiting the shear stress to a maximum plastic value CN, ..."

Corrected.

Line 90: Replace "that is much smaller than the global one" with " that is on the order of meters" or what the scale actually is.

We have modified this sentence to the following: "*We define two spatial scales: a global scale, which is the same as the one used for the ice-sheet model and that is typically of the order of kilometers, and a local scale, associated with a water conduit, and that is much smaller than the global one; observations suggest that channels are meters to at most a few hundreds meters wide, that maximal width being reached close to the grounding line (Drews et al., 2017).*".

Line 93: The term 'conduit' may be confusing, as it is often synonymous with channels and efficient drainage in the subglacial hydrology literature. Although, I don't have a better suggestion...

We are not entirely happy with this word either, but we could not find a better alternative. However, we now emphasize that it is not synonymous with channels. Specifically, we have added at Line 96 the following sentence: "*In particular, we do not use 'conduit' as a synonym for 'channel', as a conduit can correspond to other types of hydrological elements.*".

Lines 105-106: Bueler and van Pelt (2015) ran the model efficiently on the Greenland ice sheet, but these statements are generally true.

It is true that the implementation described in Bueler and van Pelt (2015) is computationally efficient, as it has been optimized for large-scale simulations. However, it represents a significant computational cost when the subglacial hydrology is coupled to the ice flow, since the equations described in Bueler and van Pelt (2015) take the form of coupled PDEs.

On the contrary, our model is computationally cheap, as the hydrological model boils down to one simple algebraic expression (Equation 7) that does not require the solution to a differential equation.

Therefore, we have added a sentence at the end of the paragraph: "*Finally, due to their high spatial and temporal resolution they are often computationally demanding. The latter may limit their application to drainage basins or single glaciers on time scales of a few years. However, our model is computationally cheap, with the computational time associated with the subglacial hydrology calculation representing only a small fraction of the computational time associated with the ice-sheet model. This allows us to study the impact of subglacial hydrology on ice dynamics on a large scale and at a limited computational cost, while at the same time keeping the essential features of complex subglacial hydrology models.*".

Line 111: Elaborate on what these conditions are. "This domain evolves over time according to internal and external conditions."

We have modified this sentence to the following: "*This domain evolves over time according to both internal conditions (e.g., changes in ice velocity) and external conditions (e.g., changes in sub-shelf melt).*".

Eq 3: L_w is confusing with L being the length of conduit. Use cursive L or something else to denote latent heat.

We changed L_w to $\mathcal{L}_\mathrm{w}$ to avoid any confusion.

Line 120-123: This assumption is only reasonable in Antarctica and locations where changes in water flux are only through changes in ice geometry which happen on long time scales. Make this more clear in the text.

We have modified the title of the paper to "*A fast, simplified, and unified subglacial hydrological model for the Antarctic ice sheet and outlet glaciers*". Furthermore, the hydrological model description (subsection 2.2) now starts with a discussion of the simplifying assumptions, in particular this steady-state hypothesis.

Line 128: Delete "anticipating what follows".

The assumption that $\nabla\phi \approx \nabla\phi_0$ far from the grounding line is now explicitly described in the additional subsection 'Simplifying assumption' of the Model section. Hence, we have removed this statement.

Fig 4: This is how the channel might look in theory. In the caption of Fig 4, it should also be stated that this is what you are trying to capture and not the actual geometry you have described in your model (which is square channel?).

Indeed, the conduits shown in Figure 4 are schematics. We do not make a distinction between a square and circular channel. This is why we write '$L \sim H$', in the scaling of Table 1: the width of the channel is similar to its height, but there could be a $\mathcal{O}(1)$ factor here. In other words, what matters is the functional relation, i.e., that L increases linearly with H. In a second step, we prescribe the relation between L and H through $H = \sqrt{S}$, which is the exact solution for a square channel, but approximates a circular channel as well, given all other uncertainties in our approach.

Line 220-224: Elaborate on how this relates to theory and observations. Are your model results what we expect for these cases?

This role of this paragraph is to provide an intuitive explanation behind the divergence between the hard-bed and soft-bed trends for efficient systems as shown in Figure 5. It can indeed be observed on this figure that for large water flux values, the effective pressure is an increasing function of the water flux for hard beds (i.e., for channels), while it is a decreasing function of the water flux for soft beds (i.e., for canals). To provide this explanation, we solely rely on the theory, i.e., on the equation (5b).

We agree with the Referee that a comparison with observations would be most welcome. However, such observations are still fairly limited for Antarctica. Nevertheless, there exists interesting studies in the recent literature that aim at validating hydrological models against observational data, e.g., Hager et al. (2022), which we refer to in our Discussion section. Overall, and as mentioned in the conclusion of our paper, it is clear that further research should address this comparison between numerical results and observations.

Fig 5:

Wouldn't we expect much higher effective pressures for that high of water flux in an efficient system? Or is this because it is the average of effective pressure over a larger scale than the channel? This could be made more clear either in the text or in the figure caption. On first glance at the figure, I find this result surprising.

We agree with the Referee that this result might be surprising. However, two reasons may eventually explain this behavior:

- The plot has been made based on standard values for A, $\|\boldsymbol{v}_\mathrm{b}\|$, and $\|\nabla\phi_0\|$, whose values are mentioned in the caption. However, the effective pressure depends quite strongly on those variables (specifically, it depends quite significantly on $\|\nabla\phi_0\|$), so modifying these values can lead to both larger and lower values, respectively, in terms of the effective pressure N_∞ far from the grounding line.
- For large flux values, N_∞ is an increasing function of the flux, but the growth is rather slow: we have $N_\infty \propto Q_\mathrm{w}^{1-1/\alpha}$. For $\alpha = 5/4$, this yields $N_\infty \propto Q_\mathrm{w}^{1/5}$.

On a related note, do you ever observe this high of water fluxes in channels far from the grounding line in the model? How exactly is the water fluxes on the local scale in the efficient system related to the effective pressure which is an average over a larger area presumably?

We do not observe water fluxes in our results that are that important, especially far from the grounding line. The maximum water fluxes that we observe are of the order of a few tens of cubic meters per second, and these are obtained close to the grounding line (see Figure S1(d) in the supplementary materials). However, one should realize that our subglacial water routing is of course an approximation of the real subglacial system and we may not necessarily capture all the details of that system.

Again, the plot shown in Figure 5 is obtained for specific values of several parameters, so the effective pressure value could deviate from it, especially if the gradient in the geometric potential, $\|\nabla\phi_0\|$, varies spatially.

In our model, we do not explicitly compute a spatial average of the effective pressure within each grid cell. Once the volumetric water flux Q_w has been obtained, the value for the effective pressure is computed, using equation (7). This value for the effective pressure is the one that is sent back to the ice-sheet code. The fact that we do not compute explicitly the spatial distribution of the effective pressure within in each grid cell is an inherent limitation of our model. There is no simple way to avoid it, as we explicitly do not want to resolve the dynamics at a sub-grid scale.

To make this limitation more clear, it is now explicitly mentioned in the additional 'Simplifying assumption' subsection which is at the beginning of the Model section. This limitation is also briefly mentioned in the Discussion section ("*Our subglacial hydrology models do not include variations of effective pressure below the resolution of the ice-sheet discretization. This is a clear limitation, as we have shown that the spatial variability plays an important role in the numerical experiments.*"). Nonetheless, we have improved the structure of the Discussion section, which is now divided in three subsections:

1. Influence of subglacial conditions
2. Hydrological feedback

3. Model limitations

This should help to highlight the limitations of the model.

On what scale are these calculated?

The effective pressure is computed using equation (7), and the value obtained is sent back to the ice-sheet code. The Thwaites simulations are done with a 2 km uniform resolution.

Line 226-229: You mean efficient/inefficient, not effective/ineffective. The system still transports water, so it isn't ineffective. It just doesn't transport it efficiently.

Corrected.

A discussion about how effective pressure behavior differs near the grounding line and far from the grounding line would be helpful.

This is explained a bit earlier in the text (around Lines 181-187).

Line 236: Does this mean the whole bed is temperate in this experiment since melting is uniform?

Yes, the whole bed is assumed to be temperate.

Fig 6b. Why does the effective pressure go down by 2 MPa in both the soft and hard bed cases at $x = 0$?

This comes from the fact that for low water flux values, the effective pressures in hard-bed and soft-bed systems are similar (see, e.g., Figure 5). Because there is a uniform melt supply, the subglacial water flux increases linearly with x. In particular, close to $x = 0$, it is very small so that we are indeed in an inefficient regime in which hard and soft beds leads to similar results.

The value observed at $x = 0$ (around 2 MPa) is a result of the sampling used for the display of the plot, the discretization error in the numerical simulation, and the way $\boldsymbol{v}_{\mathrm{b}}$ behaves near the origin. Close to $x = 0$, the water flux is rather small, so that the hydrological system is in an inefficient regime, with an effective pressure given by:

$$N = \left(\frac{\|\boldsymbol{v}_{\mathrm{b}}\| h_{\mathrm{b}}}{2n^{-n} A S_{\infty}}\right)^{\frac{1}{n}} \tag{R1a}$$

$$= \left(\frac{\|\boldsymbol{v}_{\mathrm{b}}\| h_{\mathrm{b}}}{2n^{-n} A K^{-\frac{1}{\alpha}} \|\nabla \phi_0\|^{\frac{1-\beta}{\alpha}} Q_{\mathrm{w}}^{\frac{1}{\alpha}}}\right)^{\frac{1}{n}}, \tag{R1b}$$

which is the particularization of equation (7) for an inefficient regime. Here, we have considered hard beds, soft beds being very similar (the only difference is that there is an additional factor F_{till}, but this does not change the reasoning).

As $x \to 0$, both Q_{w} and $\|\boldsymbol{v}_{\mathrm{b}}\|$ go towards zero because both quantities must vanish at that position (these are prescribed boundary conditions). Therefore, the value taken by N depends on the way both quantities converge towards zero, i.e., how fast they do so. The flux Q_{w} depends on linearly with respect to x as the melt rate is uniform. Hence, the value taken by N depends on the way $\|\boldsymbol{v}_{\mathrm{b}}\|$ goes towards zero. It seems to us that it does so with a rate that is at least linear, which would suggest that $N \to 0$ as $x \to 0$. As N reaches a small but non-zero value, we speculate that this difference comes from either the sampling used to display the plot, or numerical errors in the simulations. Note that the value taken by N at this position is not really important for our simulations as those are focused around grounding-line dynamics.

Fig 7: Are the light pink and red areas in (a)-(c) related to the colors in (d)? If so, it is not clear how. Add something to the caption about this (or remove from the background?).

Yes, they are related. We now mention in the caption that the light pink (resp. dark pink) areas in the background of the sub-figures (b), (c), and (d) correspond to regions in which the efficient/inefficient system is an inefficient (resp. efficient) regime.

Fig. 9a: It would be helpful to have a sense of scale in the 2D either by adding a scale bar or axis.

Indeed, thanks for the suggestion. We have added a scale bar.

Line 267: This does not apply on the timescales you are analyzing. The theory from Schoof (2010) on meltwater variability is referring to time-dependent changes that occur on timescales of hours to days, at most months. In this model, it is assumed to be in steady state which means that these short term increases in water pressure due to varying water flux are inherently neglected.

We have removed this reference to Schoof (2010).

Line 282: What do you mean by “the latter”. If referring to a smaller ice sheet results in grounding line retreat, replace “the latter” with “consequently” or similar. Both slower velocities and a smaller ice sheet can result in grounding line retreat.

We were referring to the applied perturbation, i.e., the reduction in surface accumulation. To make it more clear, we have therefore replaced this sentence by the following: “*This reduction also results in a slight grounding-line retreat (NON in Figure 7).*”.

Line 318: This is not a unit of mass, but a unit of length. Rephrase to say something like, ”Note that the mass loss for the NON experiment results in sea level rise on the order of 10 mm by 2100...”

Corrected.

Line 341: All of the models result in the collapse of Thwaites within how many years?

This depends on the subglacial hydrology considered but all models lead to a collapse before 2500. These collapses are displayed in the supplementary videos of our manuscript.

Line 392: As mentioned before, I have questions about how the assumption of steady state allows you to relate to the time variability of other work such as in the comment that follows. “This observation aligns with the work of Iken (1981), specifying that the highest velocities is not observed where effective pressures is lowest, but rather when cavities enlarge due to an increase in subglacial water pressure.” I think this statement should be removed.

As mentioned in our response to your general comment, it seems to us that this comparison can be included as our model allows cavities to evolve over time, the hydrological system being in a quasi-static equilibrium with respect to the ice-sheet configuration which itself evolve over time.

Line 396: How does a lower effective pressure in the soft bed system slow down grounding line retreat? Maybe I am missing something.

When the grounding line retreats in the region where the drainage system is inefficient, it tends to slow down as the region near the grounding line experiences an effective pressure that is larger to the one it was previously experiencing. This is because of the form of the relation between the effective pressure and the water flux in an inefficient system: $\partial N/\partial Q < 0$ for such a system (see Figure 5).

Because the effective pressure in an inefficient system for a soft bed is slightly inferior to that of a hard bed (thanks to the parameter $F_{\text{till}} > 1$), this increase in effective pressure experienced by the region close to the grounding line is reduced. As a consequence, this slows down the grounding-line retreat.

Line 434: add "(retrograde)".

Done.

Line 440: "(prograde)".

Done.

Line 465: add 'considering' or 'modeling' so that it reads "considering subglacial hydrology enhances the ice-sheet response to sliding".

Corrected; we now write "*considering subglacial hydrology enhances the ice-sheet response to sliding*".

Line 467: This reference to Schoof (2010) is appropriate.

Ok.

Line 470: Making the connection to changes when the system is explicitly not in steady state ("when basal cavities are growing") does not make sense here since you assume steady state and therefore neglect time-dependent changes in cavity growth.

Cavities are allowed to grow in our hydrological model. As mentioned in our response to the Referee's general comment, we have added an additional subsection in the Model section, called 'Simplifying assumptions', that aims at describing the assumptions more explicitly, and prior to the actual model description. In it, we emphasize that, because we assume that it is assumed that there is limited melt variability, we consider that the water flow is in a quasi-static equilibrium with respect to the ice-sheet geometry. In other words, the hydrological system is assumed to automatically adapt to each change in the ice-sheet geometry, reaching the corresponding equilibrium position.

We hope that this clarifies the assumptions made in our model, and the our reference to evolving cavities.

References

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