Response to Referee 2 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 you detailed review; your numerous constructive comments are much appreciated. We are convinced that your comments helped improving our manuscript significantly. Below, you will find a point-by-point response to your remarks, written in blue. We hope that our responses will be satisfactory.

Best regards,

On behalf of the authors,
Thomas Gregov

Response to the Referee's comments

Overview

This manuscript presents a subglacial hydrology model that represents inefficient and efficient subglacial drainage in the context of hard and soft beds, coupled to an ice dynamics model. The model is demonstrated with application to an idealized setting based on the MISMIP experimental setup and to Thwaites Glacier to investigate the influence of subglacial hydrology on its future behavior.

I am glad to see this work being done, combining different pieces of subglacial hydrology modeling in a way that is more practical for large-scale and long-term ice sheet simulations than many previous efforts. While I am enthusiastic about the paper's topic and findings, it will benefit from some revisions to strengthen it before publishing.

Please see the specific comments below. In general, the model description needs additional work for completeness and clarity, as already highlighted by another reviewer's comments. The structure of the paper could also be improved upon for easy navigation, to clearly indicate where results are presented versus experimental descriptions. The lengthy Discussion section would benefit from being broken up into subsections for each theme addressed within it.

These are valuable suggestions and we thank the Referee for them. Following their suggestion, we have added a new subsection to the Model section. 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, as also requested by another Referee.

Regarding the sections 3 and 4, we now explicitly mention in the title of each subsection whether we are discussing the experimental set-up or results of simulations.

Finally, we have split the Discussion section into several subsections, as follows:

1. Influence of subglacial conditions
2. Hydrological feedback
3. Model limitations

As a final comment, much of the analysis of Thwaites behavior focuses on location and migration of the grounding line. How would this change by considering a grounding zone rather than a distinct grounding line? Some brief mention or discussion about this would be helpful.

In our model we do not consider sub-shelf melting beyond the grounding line, so partially grounded cells are not affected by sub-shelf melting (see Seroussi and Morlighem (2018) for a more profound discussion on this). Of course, extending sub-shelf melting under grounded ice shelves increases the sensitivity of the model, since you are melting away the grounded ice sheet that is already close to floatation, so the latter is enhanced. In a recent paper, Rignot et al. (2024) make observations of a grounding zone and witness pretty high melting rates in that area, especially when all the heat is used for melting. It leads to values of up to 60 meters per year, which is a lot. Definitely, such high melt rates will increase the sensitivity of grounding-line retreat and it is worth looking into this problem in future research. Furthermore, in a just published paper by Bradley and Hewitt (2024), water intrusion underneath the grounding zone could lead to a further instability. Both mechanisms are not considered in our model, but are now discussed.

I look forward to seeing this work being refined to make an impactful publication. It is an important effort to improve representation of subglacial hydrology in large ice-sheet models, and the work presented here is a great contribution toward this aim.

We would like to thank the Referee for their encouraging comment.

Specific Comments

Lines 40-42: It may be helpful to some readers to give example ranges of the typical temporal and spatial scales discussed here, for both hydrology and ice dynamics.

We have added this information in the revised manuscript – for subglacial hydrology, the spatial and temporal scales can be as small as a few meters and a couple of hours, whereas for Antarctic ice-sheet dynamics, areas are hundreds of kilometers wide and the temporal response occurs over spans of several years.

Line 49: Clarify what “various bed types” means.

We have added that this corresponds to the hard/soft distinction.

Figure 1: This figure could be combined with another figure as an inset.

That is a good suggestion. We have included this figure as an inset of the Figure 9(a) of the original manuscript.

Line 89: It would be useful to include a brief description of what you mean by ‘efficient’ and ‘inefficient’ drainage before this.

We have added the following here: “*Here we consider that a hydrological system is efficient if it transports large fluxes of water.*”.

Line 91: How small is the local scale? Order of sub-meter, meter, tens of meters, hundreds of meters?

We have added the following here: “*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)*”.

Note that, in our model, this local scale is not explicitly prescribed. Rather, it is the amount of hydrological components (which we refer to as ‘conduits’ in our manuscript) per grid cell that is prescribed, through the quantity l_c.

Line 99: The SHAKTI model also combines inefficient and efficient drainage, with a continuum approach. Sommers, A., Rajaram, H., and Morlighem, M.: SHAKTI: Subglacial Hydrology and Kinetic, Transient Interactions v1.0, Geosci. Model Dev., 11, 2955–2974, `https://doi.org/10.5194/gmd-11-2955-2018`, 2018.

We have added the reference Sommers et al. (2018) here.

Figure 2: I am slightly confused by this figure and the flow shown. A more thorough description of the coupling in the text would probably help.

We have improved the description of the caption of this figure by completing it with additional information. It is now given by the following:
"*Flowchart of the dynamical linkage between the ice sheet and the subglacial hydrology. At each time step, the ice-sheet model provides the basal melt rate $\dot{m}$ and the geometrical potential ϕ_0. Based on these, the effective pressure is computed in three steps: (i) The global distributed subglacial water flux $\boldsymbol{q}_\mathrm{w}$ is computed according to Le Brocq et al. (2009); (ii) a connection between both global and local (conduit) scale is obtained by specifying the distance l_c between the conduits (Gowan et al., 2023), yielding a volumetric water flux Q_w in each conduit; (iii) the effective pressure N is computed for each conduit via a parametrization where $\mathcal{F}(\phi_0/N_\infty) = \mathrm{erf}[(\sqrt{\pi}/2)\phi_0/N_\infty]$ serves as a correction factor for the impact of the grounding line (GL), and where N_∞ is the effective pressure far upstream of the grounding line. This effective pressure is then used by the large-scale ice-sheet model and is the same for all conduits that belong to the same grid cell.*".

Besides, we now clearly mention that the content of the figure is described in further details in the following subsections.

Line 108: How cheap? Give some illustrative value to back up this claim, probably based on domain size, resolution, time step, simulation time, number of processors, wall-clock time.

We have modified this paragraph to the following: "*By contrast, 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*".

Overall, the major computational cost comes from the distributed water flux $\boldsymbol{q}_\mathrm{w}$ calculation, which is done efficiently using the method from Le Brocq et al. (2009), combined to a parametrization (i.e., an explicit formula) for the effective pressure – equation (7).

To give an order of magnitude, the non-forced Thwaites experiments on a homogeneous bed with a hard bed (HARD) take $\sim 15\%$ more computing time compared to the the no-hydrology case (NON).

Line 112: Depth-integrated subglacial water flux?

We are not convinced that 'depth-integrated subglacial water flux' is the right name for $\boldsymbol{q}_\mathrm{w}$. There are several ways to relate this flux to well-known quantities. One of these is to consider that this water flux is evenly distributed over the whole grid cell, giving rise to a water film of depth d_w (Le Brocq et al., 2009). In that case,

$$\boldsymbol{q}_\mathrm{w} = \overline{\boldsymbol{u}}_\mathrm{w}\, d_\mathrm{w}, \tag{R1}$$

where $\overline{\boldsymbol{u}}_\mathrm{w}$ is the depth-averaged horizontal velocity. Hence $\boldsymbol{q}_\mathrm{w}$ has units m^2/s.

Another way is to consider the water flux. Usually, it is defined as the volume of water that crosses a surface per unit of time. The relation between $\boldsymbol{q}_\mathrm{w}$ and the total water flux in each cell

Q_{total} follows the same reasoning to convert $\boldsymbol{q}_{\text{w}}$ and Q_{w}: we have

$$Q_{\text{total}} = \Delta x \, \|\boldsymbol{q}_{\text{w}}\|, \tag{R2}$$

where Δx is the width of the square grid cell. Hence, $\boldsymbol{q}_{\text{w}}$ can be interpreted as a water flux per unit length (hence our name 'distributed subglacial water flux'), but not as a depth-integrated water flux.

Lines 119-120: For completeness, describe how the melt rate due to dissipation ($\dot{m}_{\text{w}}$) is calculated. Do you include this dissipation term everywhere? This is worth clarifying because of the legacy of models that only include it for channel components.

Apologies, the expression for $\dot{m}_{\text{w}}$ was indeed missing from the manuscript. We have modified this paragraph as follows: "*(...), i.e.,*

$$\dot{m} = \frac{G + \boldsymbol{\tau}_{\text{b}} \cdot \boldsymbol{v}_{\text{b}} - q_T}{L_{\text{w}}} + \dot{m}_{\text{w}} \,, \tag{R3}$$

where G is the geothermal heat flux, q_T is the thermal conduction flux, L_{w} is the latent heat for ice, and $\dot{m}_{\text{w}} = |\boldsymbol{q}_{\text{w}} \cdot \nabla\phi|/L_{\text{w}}$ is the water melt rate due to the dissipated energy from the subglacial water conduits. However, we do not include this last term in our simulations as it was found to be negligible compared to the other terms."

Lines 126-129: Intriguing to use the simple routing scheme – I'm interested to see the results that support the claim that ϕ_0 is approximately equal to ϕ over most of the domain. Perhaps pointing to a figure would be good, rather than simply saying "in anticipation of what follows". It seems like a strange thing to want to decouple the water routing from effective pressure when you are interested in modeling subglacial hydrology, given that water flow is driven by gradients in potential, which obviously changes depending on effective pressure.

In the new structure of the description of the model, we tried to improve the description of the hydrological model by stating the essential assumptions prior to the derivation of the model. This allowed us to remove this 'in anticipation of what follows'.

Specifically, the following discussion has been added to the new 'Simplifying assumptions' subsection:

"*The key simplifying assumptions are given by the following:*

1. *There is limited temporal melt variability so that the hydrological system is in a quasi-static equilibrium with respect to the ice-sheet geometry. Therefore, changes in ice geometry will be the main driver for changes in subglacial water variability (both spatial and temporal).*

2. *A few kilometers upstream of the grounding line, the hydraulic gradient is approximated by the geometric gradient.*

3. *The drainage density is uniform and the effective pressure is not calculated at a sub-grid level.*

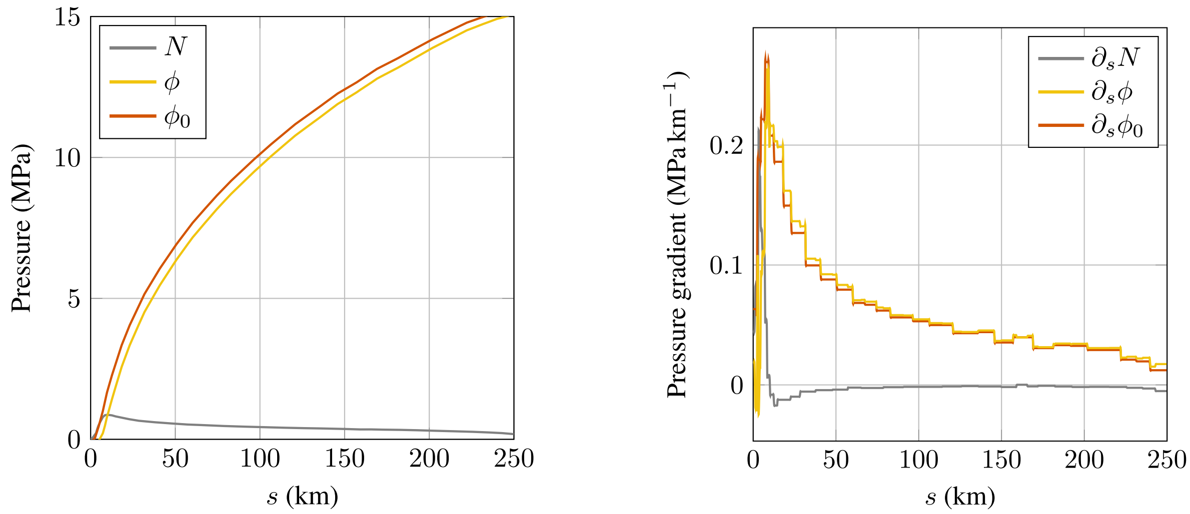
The first assumption is based on several studies of subglacial hydrology in Antarctica (Le Brocq et al., 2009; Pattyn, 2010; Kazmierczak et al., 2022), among others, that demonstrate that — contrary to the Greenland ice sheet— there is limited surface meltwater infiltration. Hence, changes in hydrology 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. The second assumption is motivated by a scaling analysis through an estimation of the dimensionless ratio $\eta := [\nabla N]/[\nabla\phi_0]$, where $[\nabla N]$ is the scale of the spatial gradients for the effective pressure and $[\nabla\phi_0]$ is the characteristic scale for the geometric potential gradient. For the former we take $[\nabla N] = [N]/[x]$, with $[N] = 1\,MPa$ and $[x] = 10^3\,km$. For the latter we take

$[\nabla\phi_0] = 5 \times 10^{-2}\,MPa\,km^{-1}$, *which is a plausible value for ice sheets (Hewitt, 2011). This results in* $\eta = 2 \times 10^{-2} \ll 1$, *suggesting that* $\|\nabla N\| \ll \|\nabla\phi_0\|$ *and* $\nabla\phi \approx \nabla\phi_0$. *We further note that profiles obtained with a high-resolution subglacial hydrology model suggest that* $\nabla\phi$ *and* $\nabla\phi_0$ *have a correlation of at least* $\sim 80\%$ *for a region that is several kilometers upstream of the grounding line (see Supplementary Material S1). Finally, the third assumption follows from our modeling approach, where we do not describe the effective pressure at the sub-grid scale and where we assume the same number of conduits in each grid cell, similar to Gowan et al. (2023).*"

Note that saying that $\|\nabla N\| \ll \|\nabla\phi_0\|$ is not the same as saying that $N = 0$; rather, we are saying that N varies much more slowly in space compared to ϕ_0.

The Supplementary Material S1 refers to the assessment of the assumption that $\nabla\phi \approx \nabla\phi_0$ outside the vicinity of the grounding line based on data. Here is the content of this addition to the supplementary materials:

"*Here, we provide additional data to underpin the validity of the assumption that* $\nabla\phi \approx \nabla\phi_0$ *outside the range of influence of the grounding line, which is a few kilometers from it. Since there are no direct observations of the effective-pressure field in Antarctica, we have to rely on high-resolution models. A first test case comes from Lu and Kingslake (2023) who uses a high-resolution model that couples ice-sheet dynamics and subglacial hydrology for hard beds. Potential limitations of that study is that it considers a flow line and a smooth bedrock. The assumption that* $\nabla\phi \approx \nabla\phi_0$ *a few kilometers upstream of the grounding line is confirmed numerically (Figure R1).*



(a) Numerical results.

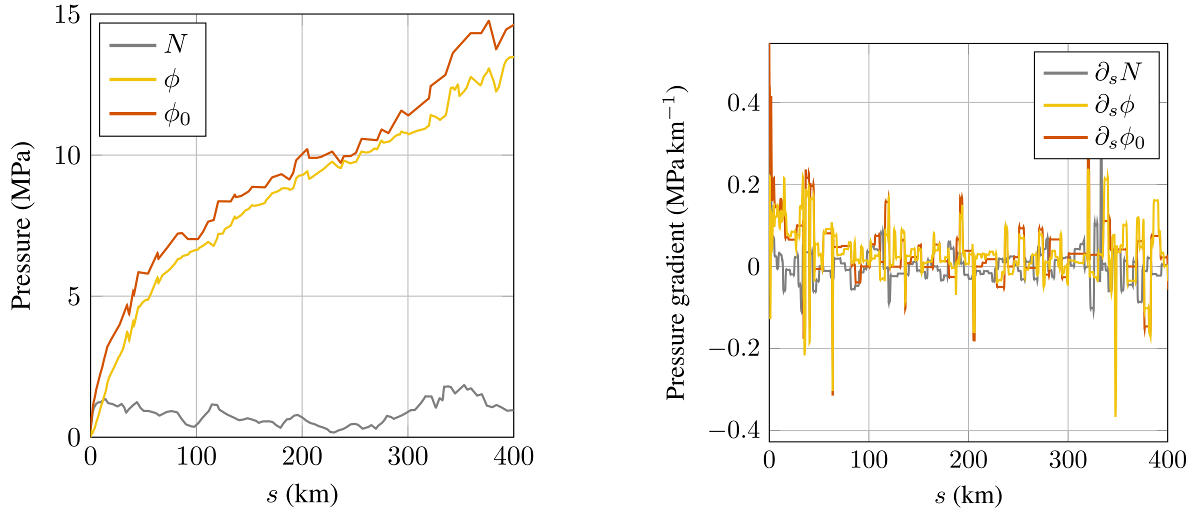
(b) Computed gradients.

Figure R1: Data derived from Figure 4 of Lu and Kingslake (2023).

A second test case comes from Hager et al. (2022) who applied the high-resolution model MALI (Hoffman et al., 2018) to Thwaites Glacier. They also consider a hard-bed hydrology. The computed effective pressures along a center-line transect are shown in Figure R2. Note that the signals are much more noisier compared to the first test case. This noise can be attributed to the model resolution, but also to the presence of localized hydrological features that cross the center-line transect at which the effective pressures are evaluated, therefore resulting in very localized variations. However, we observe a good correlation between $\partial_s\phi$ *and* $\partial_s\phi_0$ *out of the vicinity of the grounding line (Figure R2):* $\sim 80\%$ *over the range* $[10, 400]$ *km, suggesting that the assumption that* $\nabla\phi \approx \nabla\phi_0$ *is valid in this region.*"

Line 132: How is l_c chosen? How sensitive are results to this value?

That is a good question and apologies that we did not specify this more clearly in the manuscript.

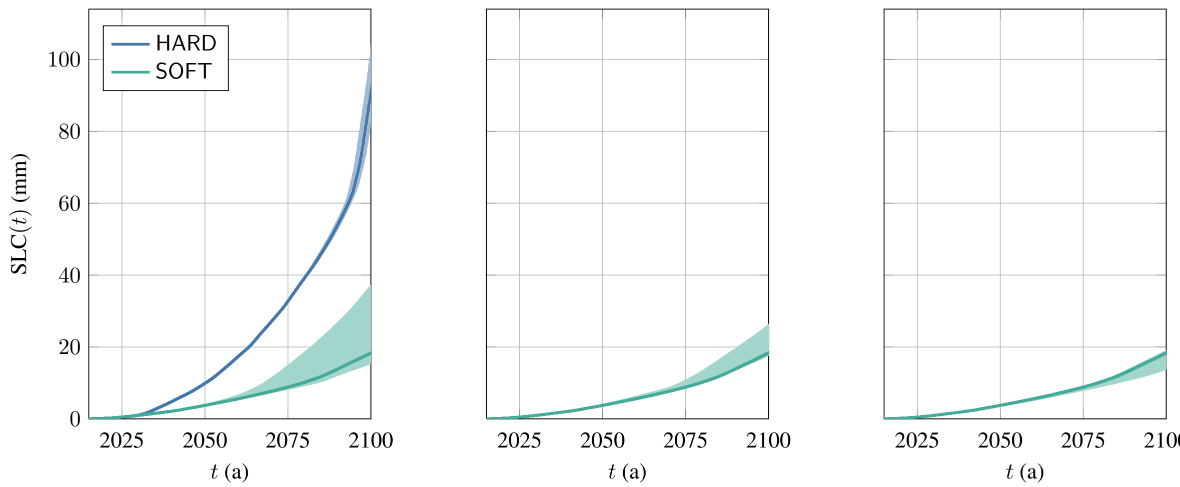


(a) Numerical results. (b) Computed gradients.

Figure R2: Data derived from Figure 8 of Hager et al. (2022).

We have added the following after its introduction: "*We take $l_c = 10$ km, which is similar to the value considered in Gowan et al. (2023) based on observations of distances between eskers formed under the Laurentide Ice Sheet (Storrar et al., 2014).*"

Furthermore, we have added an appendix with a simple sensitivity analysis with respect to l_c, Q_c, and F_{till} (Figure R3).



(a) Effect of l_c. (b) Effect of Q_c. (c) Effect of F_{till}.

Figure R3: Sensitivity analysis of the results with respect to the parameters l_c, Q_c, and F_{till}. The set-up is the same as the one described in the forcing experiments over Thwaites (subsection 4.2, Subglacial hydrology on homogeneous beds), except that different values of these parameters are chosen. The shaded areas correspond to the ranges $l_c \in [5, 15]$ km, $Q_c \in [0.5, 1.5]$ m^3/s, and $F_{till} \in [1, 2]$, and the lines correspond to the nominal values considered in the original experiment.

It can be observed that l_c has only a limited effect for hard beds, while it has a more pronounced impact for soft beds. From equation (4), a change in l_c results in a change in the water flux Q_w, which will be important if water flow transitions from an efficient to an inefficient flow (or the reverse). However, for hard beds, the entirely efficient or inefficient cases yield similar results (Figure 9b). On the contrary, for soft beds, the difference between the entirely efficient or inefficient cases is more pronounced (Figure 9b), and it follows that there is a stronger dependence with respect to l_c. For Q_c and F_{till}, the impact is limited. Finally, it can be noted the spread in the results increase over a time. This figure and discussion have been added to the additional appendix.

Line 143: Do you always assume turbulent flow in the model?

Yes, although one could argue that in practice the flow could also be laminar. In our manuscript, we have chosen a turbulent parametrization for the Darcy's flow equation; this fixes both the exponents α and β, as well as the value of the conductivity parameter K. In this way, we follow the analysis of Schoof (2010) and Gowan et al. (2023).

However, we now discuss the possibility of having a laminar flow of water in the revised version of our manuscript, together with relevant references from the hydrological literature, in particular Hill et al. (2023).

Lines 149-150: Is the opening by sliding over obstacles the same for hard and soft beds? It isn't clear from this sentence whether the model treats these the same or differently, or if this means that the physical interpretation is simply different.

We treat them similarly; this is now clarified in the revised manuscript, where we have written the following : "*The bed obstacles correspond to bed protrusions if the bed is hard, and to clasts if the bed is soft, and our model treats these cases the same.*".

Lines 150-151: Why isn't melt opening associated with both inefficient and efficient drainage systems? Similarly to the previous comment, is this sentence purely commentary on physical interpretation, or describing a coded switch in melt equations applied to different parts of the model domain?

Physically, we associate melt opening with an efficient drainage system, as this is the system in which it will be the dominant term (as it is proportional to the water flux). However, in general, opening can occur by different mechanisms; in our model we consider both opening by melting and opening by sliding over bed protrusions. By default, both mechanisms (efficient and inefficient) operate in our model, as both terms are included. In particular, there is no switch between the two in the code.

However, for some simulations in the paper we considered either one of them to test the sensitivity.

Lines 175-176: It would be helpful to justify the assumption that effective pressure is "fairly constant" far from the grounding line, perhaps with a plot either in the main text or in a supplement or appendix. How far from the grounding line?

For the "fairly constant" assumption, we refer to our response to the comment of Lines 126-129.

Far from the grounding line, $N \approx N_\infty$, while close to it, $N \approx \phi_0$. As a first approximation, the switch between the two regimes therefore appears when $\phi_0 \approx N_\infty$, which is consistent with our equation (7). Numerically, for the simulations over Thwaites, this corresponds to a few kilometers from the grounding line.

Equation 6: Are N_∞ and S_∞ the effective pressure and conduit cross-sectional area far from the grounding line? That's what I infer, but they should be explicitly defined.

Yes, that is the case. This is now clarified in the revised manuscript: we introduce equation (6) as follows: "*In that case, we obtain algebraic equations for the effective pressure and the cross-sectional area far from the grounding line, N_∞ and S_∞:*".

Line 181: How close to the grounding line?

This corresponds to the region where $\phi_0 \approx N_\infty$, which is typically a few kilometers from the grounding line.

Section 2.2.3: How sensitive are results to these geometric assumptions (the relationships between L, H, and S, also the value of F_{till})? These are nicely explained here, but are still mostly unconstrained by observations and are somewhat arbitrary, so it would be more thorough to consider their influence on model results.

This is an important point. It is evident from our analysis that the results are very sensitive to these geometric relations (as there is a large variability in the results when comparing hard and soft beds; see e.g. Figures 5 and 9). Overall, this highlights the necessity for more data on the bed rheology of the Antarctic ice sheet, as well as additional studies to compare this data with the results of numerical models.

For the value of F_{till}, we have taken $F_{\text{till}} > 1$ because we physically expect a lower effective pressure for soft beds, compared to hard beds. We have added in the Appendix a sensitivity on the choice of F_{till} (Figure R3), and results suggest a relatively limited effect.

Line 204: missing a period.

Corrected.

Line 207: How is the critical water flux value Q_c selected?

We added the following sentence: "*In our simulations, we took $Q_c = 1\,m^3\,s^{-1}$ which corresponds to the scale of the water flux considered in Walder and Fowler (1994).*". This value is also of the same order of magnitude as the value of the flux for which the regime transitions from an efficient regime (in which the sliding-over-protrusions opening term dominates) to an efficient regime (in which the melt-opening term dominates) for a hard bed (see Figure 5).

Its sensitivity is further gauged in the Appendix (Figure R3).

Line 209: I am curious as to how confident we can be in prescribing which regions are hard bedded and which soft bedded, particularly as these can be highly heterogeneous spatially. Maybe this is coming later in the application to Thwaites.

That is a good point and, actually, a motivation for our work. If our results were similar over a hard, a soft, and a mixed bed, then the question of the bed type would not be particularly important for ice-sheet simulations. But our results (which indeed are in the Thwaites section) precisely show the opposite: the type of bed is a key parameter that strongly affects ice flow, together with the flow efficiency. Nonetheless, the vast majority of hydrological models consider water flow to take place over beds that are similar to what we describe as 'hard beds', with a mix of cavities and channels (i.e., no canals). This suggests that: (i) ice-sheet models should be able to incorporate water flow over both hard and soft beds and (ii) additional research efforts should address the characterization of the bed, specifically for basins that are susceptible to be retreating in the next centuries. Since the spatial distribution of hard and soft bed is poorly known, our study advocates for the importance to improve observational constraints.

Line 226: Do you mean "entirely efficient" (instead of "entirely effective")?

Yes – this is now corrected.

Line 229: Similarly, should this be "entirely inefficient"?

Yes – this is now corrected too.

Line 229: It is not clearly justified why the dissipation term should be removed in the inefficient system. Is this based on similar earlier models that needed this for numerical stability? Is this necessary in your model formulation? I'm not convinced that it makes sense physically to ignore the dissipative contribution to melt if you can help it.

We do not remove the dissipation term for a physical or numerical reason, but for a testing purpose, to assess the sensitivity of the system with respect to its different opening mechanisms.

Note that by default, we consider both opening terms: the 'inefficient' one, associated with sliding over bed protrusions/clasts, and the 'efficient' one, associated with melting. It is only in our additional tests that we consider entirely efficient and entirely inefficient cases in which we artificially remove one of the opening terms. We have slightly modified this paragraph so that it is more clear (see response to your next comment).

Lines 225-230: This section about switches imposed in the model needs to be clarified. It's great to represent inefficient and efficient systems and systems that don't fall cleanly into either category. But it is not entirely clear from reading what the thresholds are for selecting different forms of the equations. Are these manually set based on preference of the modeler and the problem of interest? Or are there criteria that automatically trigger these switches?

We have modified the beginning of this paragraph as follows: "*Besides soft, hard, and mixed beds, we also consider entirely efficient and inefficient drainage systems to gauge the sensitivity of both separately, independent of the subglacial water flux. By default, our model is such that the subglacial system naturally transitions from one to another depending on the subglacial water flux. This transition happens because the melting term, which is proportional to the water flux, becomes dominant over the sliding term in the left-hand side of (5a) as the water flux increases. To obtain an entirely efficient system, the opening term associated with the sliding over obstacles, $\|\boldsymbol{v}_\mathrm{b}\|h_\mathrm{b}$, is removed from equation (5a), as well as from the parametrization (6a). We also set $Q_\mathrm{c}=\infty$, which guarantees that the conduit geometry is the one of an inefficient system for soft beds. To obtain an entirely inefficient system, we remove the efficient component, $Q_\mathrm{w}\|\nabla\phi\|/\rho_\mathrm{i}L_\mathrm{w}$, from (5b), together with the condition that $Q_\mathrm{c}=0$.*"

This should make it more clear that in the model, the 'switch' between the efficient and inefficient regimes is naturally included in the equations as a function of subglacial water flux.

Lines 237-238: It would be helpful to comment on why Weertman was selected as the sliding law, and why a uniform value for the friction coefficient, and why that value. (You have to make some choices, just curious about the rationale behind these selections).

We have revised the section on model initialization, as we felt it was not clear enough. In particular, it seemed to us that the initialization procedure would be easier to understand if it were described in words rather than equations. Here is the new version:

"*On this bed topography a marine ice sheet is developed with a spatial resolution of* 500 m*, following the set-up described in the EXP1 of the MISMIP experiments (Pattyn et al., 2012, see Figure 6a). The steady state obtained with these conditions is considered to be the 'reference state'.*

In our experiments, we use a regularized Coulomb friction law combined with hydrological models, while the reference state has been obtained with a Weertman friction law. To guarantee that the thickness and velocity fields obtained in the reference state are still compatible with a steady state, we modify the friction coefficient at each position, following the method of Brondex et al. (2017, 2019). In practice, an iterative nudging method is used so that the basal friction matches the basal friction obtained in the reference state. Here, the subglacial hydrologies are generated with a uniform basal melt rate underneath the grounded ice sheet of $\dot{m}/\rho_\mathrm{w}=5\times10^{-3}\,\mathrm{m\,a^{-1}}$, which corresponds to the mean basal melt rate of the Antarctic ice sheet (Pattyn, 2010). By construction, this method yields initial states that are steady states and in which both the geometry and the velocity field are identical for each type of hydrology, allowing a direct comparison between them. The initial ice-sheet effective-pressure profiles are shown in Figure 6b."

In this new version, it should be more clear that the choice of a Weertman friction law to obtain the reference state stems from the use of the standard MISMIP set-up (Pattyn et al., 2012).

Line 238: should be "upper boundary condition" (singular, not plural for grammatical agreement).

Thanks for spotting this mistake. As we have changed the presentation of the initialization method, it no longer appears.

Line 241: This is confusing about the spatially variable friction coefficient used here, when it was just stated in the previous paragraph that a uniform friction coefficient was used.

We have revised the paragraph on the initialization method (see response to comment on Lines 237-238). We hope this information is now more clear.

Line 246: So did N change throughout the inversion here? A brief description of that would help clarify this, i.e. what initial distribution of N was assumed, and how was it altered through the iterative nudging process?

Yes, N is allowed to change during the inversion: it is updated at each iteration of the nudging procedure. The initial effective-pressure field is the one that is obtained from the model with the initial guess for the sliding coefficient. Note that in the revised version of this paragraph, we have chosen not to include too many technical details about this procedure (i.e., not too many equations), as it seemed to us that these might be rather confusing to the reader.

Line 257: Some observations have suggested low effective pressure in the interior. Can you comment on this here or elsewhere?

Indeed, and this is why we think (and demonstrate) that the HAB parameterization that is widely used in ice sheet models, fails. In the discussion section we mention that our results are qualitatively similar to those of Hager et al. (2022) for hard beds.

Line 263: This statement about the default switch between efficient and inefficient drainage equations would be helpful to include above (see comment about Lines 225-230).

We have included this statement in the paragraph of Lines 225-230.

Line 279: With what size time step is the ice sheet model run for 20,000 years? Is the hydrology model also run for 20,000 years?

The time step for the idealized simulations is 5 years, while for the Thwaites experiments it is 0.05 years. These time steps are now explicitly mentioned in the text.

The question of the time step of the hydrological model is an important one. This is explained in more details in the Appendix C. The key result is that the hydrological model should be updated at a frequency that is at least of the same order as the one used for the ice-sheet model (i.e., with a time step that it smaller to or similar to the one used by the ice-sheet model).

Line 280: It would be helpful to include a brief reminder of how the hydrology and ice sheet models are coupled here.

We have added the following sentence here: "*The hydrological model is updated at each time step (see also Appendix C).*".

Figure 7: Why is the sliding velocity for entirely inefficient not included in panel c? It would also be useful to plot the effective pressure response for entirely efficient and entirely inefficient in panel b to help strengthen your points about the importance of the switching behavior.

Following your suggestion, we have added the sliding velocity for the entirely-inefficient case in panel (c), as well as the effective pressures corresponding to the entirely-efficient and entirely-inefficient case in panel (b).

Lines 341-342: What is the criterion to be considered a collapse?

We have precised what we meant by 'collapse' by modifying the Line 336, which is the first time this word appears, to the following: "*This is in line with large-scale model experiments (Coulon et al., 2023) showing that Thwaites Glacier may collapse, i.e., that it will continue to retreat at an accelerated rate even if the forcing is completely stopped, under present-day climatic conditions on time scales of several centuries*".

Sections 4.1-4.3: The structure of this section needs some improvement. The titles of 4.2 and 4.3 may be renamed to make clear that the first section is the experimental description for Thwaites, and the second and third sections are presenting results. I was a bit confused by this structure while reading. The information about the threshold for hard-to-soft transition in line 328 seems to be repeated in line 352. We also seem to be missing information in the experimental setup on Thwaites about model resolution(s) and time step size(s), which would be interesting to know.

We now make it clear that subsections 4.2 and 4.3 correspond to results – we have renamed these "*Results: subglacial hydrology on homogeneous beds*" and "*Results: subglacial hydrology on heterogeneous beds*", respectively. We also clarified that the limit in line 352 is a repetition of the one in line 328, so that it is clear to the reader that it is not an oversight and that we are simply repeating this information for clarity. We now also mention the resolution (2 km) and the time step (0.05 years).

Discussion: I recommend separating this into some subsections with corresponding headings.

Thanks for the suggestion. We have followed it; it also seemed to us that the Discussion section was rather long and could benefit from further structuring. It is now divided in three subsections:

1. Influence of subglacial conditions
2. Hydrological feedback
3. Model limitations

Tables A1 and A2 could be combined into a single table.

Agreed. However, we have chosen to use two different tables: by separating the Greek and Latin alphabets, we are able to obtain a table that is no longer than one page. If we put them together, we would get a table that would extend over two pages.

References

Bradley, A. T. and Hewitt, I. J. (2024). Tipping point in ice-sheet grounding-zone melting due to ocean water intrusion. *Nature Geoscience*.

Brondex, J., Gagliardini, O., Gillet-Chaulet, F., and Durand, G. (2017). Sensitivity of grounding line dynamics to the choice of the friction law. *Journal of Glaciology*, 63(241):854–866.

Brondex, J., Gillet-Chaulet, F., and Gagliardini, O. (2019). Sensitivity of centennial mass loss projections of the Amundsen basin to the friction law. *The Cryosphere*, 13(1):177–195.

Coulon, V., Klose, A. K., Kittel, C., Edwards, T., Turner, F., Winkelmann, R., and Pattyn, F. (2023). Disentangling the drivers of future antarctic ice loss with a historically-calibrated ice-sheet model. *EGUsphere*, 2023:1–42.

Drews, R., Pattyn, F., Hewitt, I. J., Ng, F. S. L., Berger, S., Matsuoka, K., Helm, V., Bergeot, N., Favier, L., and Neckel, N. (2017). Actively evolving subglacial conduits and eskers initiate ice shelf channels at an antarctic grounding line. *Nature Communications*, 8(1).

Gowan, E. J., Hinck, S., Niu, L., Clason, C., and Lohmann, G. (2023). The impact of spatially varying ice sheet basal conditions on sliding at glacial time scales. *Journal of Glaciology*, 69(276):1056–1070.

Hager, A. O., Hoffman, M. J., Price, S. F., and Schroeder, D. M. (2022). Persistent, extensive channelized drainage modeled beneath Thwaites Glacier, West Antarctica. *Cryosphere*, 16(9):3575–3599.

Hewitt, I. J. (2011). Modelling distributed and channelized subglacial drainage: the spacing of channels. *Journal of Glaciology*, 57(202):302–314.

Hill, T., Flowers, G. E., Hoffman, M. J., Bingham, D., and Werder, M. A. (2023). Improved representation of laminar and turbulent sheet flow in subglacial drainage models. *Journal of Glaciology*, page 1–14.

Hoffman, M. J., Perego, M., Price, S. F., Lipscomb, W. H., Zhang, T., Jacobsen, D., Tezaur, I., Salinger, A. G., Tuminaro, R., and Bertagna, L. (2018). MPAS-Albany land ice (MALI): a variable-resolution ice sheet model for earth system modeling using voronoi grids. *Geosci. Model Dev.*, 11(9):3747–3780.

Kazmierczak, E., Sun, S., Coulon, V., and Pattyn, F. (2022). Subglacial hydrology modulates basal sliding response of the antarctic ice sheet to climate forcing. *The Cryosphere*, 16(10):4537–4552.

Le Brocq, A., Payne, A., Siegert, M., and Alley, R. (2009). A subglacial water-flow model for West Antarctica. *Journal of Glaciology*, 55(193):879–888.

Lu, G. and Kingslake, J. (2023). Coupling between ice flow and subglacial hydrology enhances marine ice-sheet retreat. *EGUsphere [preprint]*.

Pattyn, F. (2010). Antarctic subglacial conditions inferred from a hybrid ice sheet/ice stream model. *Earth and Planetary Science Letters*, 295(3–4):451–461.

Pattyn, F., Schoof, C., Perichon, L., Hindmarsh, R. C. A., Bueler, E., de Fleurian, B., Durand, G., Gagliardini, O., Gladstone, R., Goldberg, D., Gudmundsson, G. H., Huybrechts, P., Lee, V., Nick, F. M., Payne, A. J., Pollard, D., Rybak, O., Saito, F., and Vieli, A. (2012). Results of the Marine Ice Sheet Model Intercomparison Project, MISMIP. *The Cryosphere*, 6(3):573–588.

Rignot, E., Ciracì, E., Scheuchl, B., Tolpekin, V., Wollersheim, M., and Dow, C. (2024). Widespread seawater intrusions beneath the grounded ice of Thwaites Glacier, West Antarctica. *Proceedings of the National Academy of Sciences*, 121(22).

Schoof, C. (2010). Ice-sheet acceleration driven by melt supply variability. *Nature*, 468(7325):803–806.

Seroussi, H. and Morlighem, M. (2018). Representation of basal melting at the grounding line in ice flow models. *The Cryosphere*, 12(10):3085–3096.

Sommers, A., Rajaram, H., and Morlighem, M. (2018). SHAKTI: Subglacial Hydrology and Kinetic, Transient Interactions v1.0. *Geoscientific Model Development*, 11(7):2955–2974.

Storrar, R. D., Stokes, C. R., and Evans, D. J. (2014). Morphometry and pattern of a large sample (>20, 000) of Canadian eskers and implications for subglacial drainage beneath ice sheets. *Quaternary Science Reviews*, 105:1–25.

Walder, J. S. and Fowler, A. (1994). Channelized subglacial drainage over a deformable bed. *Journal of Glaciology*, 40(134):3–15.