Response to the Editor on "A fast and simplified subglacial hydrological model for the Antarctic ice sheet and outlet glaciers" by Kazmierczak, Gregov, Coulon & Pattyn (Rev. 2).

Dear Dr. Joe MacGregor,

Once again, we would like to thank you and the reviewers for your comments, which have helped us to improve our article, as well as your handling of our paper.

The detailed description of the changes made to the manuscript can be found in the marked-up version of the revised manuscript, as well as in our responses to the Referees.

Best regards,

On behalf of the authors,
Thomas Gregov

Response to Referee 1 on "A fast and simplified subglacial hydrological model for the Antarctic ice sheet and outlet glaciers" by Kazmierczak, Gregov, Coulon & Pattyn (Rev. 2).

Dear Referee,

We would like to thank you for your review, and in particular for explaining in further details your questions with respect to the model assumptions. This has enabled us to clarify these, and should also allow us to ponder on the limitations of our model, thus paving the way for further interesting research associated with the development of simplified hydrological models.

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

General comment:

Thanks to the authors for their work restructuring and clarifying the paper, which is a very nice and interesting read. Most of my comments below are quite minor. However, I am still a little unhappy that there are two (rather different) assumptions lumped under 'key point 3' in their new section 2.2.1, where the only justification provided in the text is that the assumption 'follows from [their] modelling approach', which seems circular to me.

Thanks for your positive comment and your critical assessment of the hypotheses of the model. We have now separated the third assumption into two separate items:

3. *The drainage density, that is, the number of conduits per grid cell, is uniform.*

4. *The effective-pressure distribution is not calculated at the sub-grid (local) level.*

The other proposed modifications are described below the following paragraphs.

I do appreciate that for practical purposes one must prescribe a single value of N for a given grid cell, to then use in the sliding law. My point was that it is usually the case (per high resolution models) that the effective pressure in a channel [as calculated with something like (5b)] is somewhat higher compared to the average effective pressure in the area surrounding it [which would be more like the N controlling sliding for the whole grid cell]. Admittedly, this effect is likely small, but it would be better to acknowledge and dismiss than to ignore.

We agree with the Referee's remark. We have added the following sentence after the assumptions to acknowledge this effect:

"*(...) However, the effective pressure within a channel may well differ from its value away from the channel, which is something that is not taken into account. Consequently, these last assumptions are the most likely to be debatable.*"

Note that we are very much interested in tackling this issue of attributing a single effective-pressure value in large-scale ice-sheet codes despite the local heterogeneity in the subglacial hydrology, and this something that we wish to work on in the future in our research group.

The assumption that the drainage density is uniform is rather separate. I appreciate the sensitivity tests showing that using different uniform values results in only limited sensitivity of the model, but I was questioning more about whether l_c should change with $\boldsymbol{q}_w$, and therefore would feed more impactfully into the way that N changed with flux. The authors include quite a detailed description around l. 235 about the change in morphology of the patchy film with H, which is exactly why I question that l_c, effectively the distance between separating clasts in this regime, is not also somehow a function of film thickness. Similarly one might consider that the distance between linked cavities is likely quite a bit smaller than the distance between subglacial channels (recorded by eskers). I would really appreciate if the authors spent more time with this assumption, either in section 2.2.1, around equation (4), or when discussing film geometry (∼ l. 235). If nothing else, there is a lot of great discussion in this paper highlighting the need for future investigation, so it would be helpful to highlight that future work could be performed here.

We also agree with this remark of the Referee. Following their suggestion, we have made several changes to the manuscript. Firstly, as mentioned above, a sentence has been added after the assumptions to highlight that the third and fourth assumptions are the ones that are the most tricky ones. Secondly, the following remark has been added after equation (4):

"*However, we acknowledge that this distance is likely to be a function of the drainage system, but leave this to be investigated in future work.*"

Finally, we have modified the model limitations subsection in the Discussion section to include this limitation into account. The beginning of the paragraph has been modified as follows:

"*(...) Our subglacial hydrology models do not include variations of drainage density or of effective pressure below the resolution of the ice-sheet discretization.*"

Specific comments:

Abstract: it could be nice to focus more on the fast aspect of the model.

We have added the following sentence to the abstract:

"*It does not explicitly simulate the details of water conduits at the local scale and assumes that subglacial hydrology is in quasi-static equilibrium with the ice sheet, which makes the computations very fast.*"

Together with the changed title, this should emphasize the fast aspect of the model.

Line 2: Perhaps just 'switch' rather than 'dynamic switch' since the hydrology is quasi-steady?

We have replaced 'dynamic' by 'automatic'. This suggests that the switch is made by the model itself, which is an important feature of the model.

Line 9: Comma missing after 'itself'.

Corrected.

Line 17: What is meant by 'plasticity' here? In the sense of variability, not in the sense of sediment having a plastic rheology? Perhaps rephrase.

In the sense of variability, but this variability (or lack thereof) could be explained by a plastic rheology of the bed, as mentioned directly after: 'which mainly depends on the bed rheology'.

Line 41: There are no processes listed in the previous sentence? Unless by distribution you mean the flow, rather than the final spatial distribution? Also, please add an example reference for this sentence.

Indeed. We have modified this sentence to the following: "*Furthermore, subglacial processes occur on time scales that can be as small as a few hours (e.g., Clarke, 2005)*".

Line 43: A[nother] limiting factor, rather?

Corrected.

Line 50: 'allows [us?] to dynamically link' word missing.

Corrected.

Line 51: Perhaps state that the hydrology is quasi-static but temporally varying.

Thanks for the suggestion. However, the details of the model follow in the next section. Here we just have a very broad statement on the temporal and spatial variability.

Line 92: Inefficient hydrological systems can still transport large fluxes of water if they need to, they just induce larger pressure gradients to do so. Maybe add 'with low gradients in ϕ' to the end of the sentence.

We agree with the Referee that inefficient hydrological systems can also, in principle, transport large fluxes of water. However, inefficient systems are thought to be unstable when the input of water gets above a threshold. Therefore, it seems to us that it is fair to say that we expect efficient systems to be those that transport large fluxes of water in channels, while inefficient systems are characterized by a distributed transport.

To improve the clarity of the efficient/inefficient distinction, we have modified this sentence to the following:

"*Generally, efficient systems transport large water fluxes and are characterized by localized channelized flow, while inefficient systems take the form of distributed water flow.*"

Line 123: As noted above, split this point into two parts. Also, drainage density has not yet been defined in the paper at this point.

Corrected.

Line 131: You have your own synthetic and real geometries to estimate $\nabla\phi_0$ from, so it could be good to use those values.

This is an interesting suggestion that we had also considered. However, it seemed to us that such a justification could take the form of a circular argument, since we would be relying on the results of our simulations to justify the assumptions of the model. So we preferred to rely on other pre-existing references for estimates.

Line 159: Suggest for clarity 'we choose not to do so as this allows us to decouple the water routing solver from the effective pressure calculation'.

Thanks for the suggestion, which we have followed.

Line 197: Not sure the sentence beginning 'Note that' belongs here.

We have removed the "*Note that*" which was not necessary here.

Line 200: Clarify - drainage rate from where to what aquifer?

This sentence has been modified to the following: "*Indeed, as the drainage rate from the till towards a subglacial aquifer is much smaller than the basal melt rate, (...)*".

Line 212: Can commit to this analysis having been discussed already in 2.2.1 and skip straight to using $\nabla\phi_0$.

Corrected.

Line 222: Weaken to 'and we suggest the effective pressure is approximated over the whole domain by', since the functional form does not formally come from the boundary layer analysis.

Corrected.

Line 306: How are the fluctuations in meltwater forced, since there is (as I understand it) no direct meltwater input in the model?

It is true that there is no direct surface or englacial meltwater input to the model. Here, the subglacial meltwater oscillates thanks to variations in the meltrate $\dot{m}$ that is artificially set to a sinusoidal function (see figure 6a).

Line 448: is split → can be split.

Corrected.

Line 460: Confusingly worded - seems more like the second sentence is restating the first, rather than being implied by it.

Indeed. We have removed the implication: the sentence is now simply given by "*The zone of low effective pressure migrates with a migrating grounding line.*".

Line 464: I don't understand how low $\tilde{\tau}_b$ causes a low N - via ice sheet loss? Or is the implication the other way round and just via (12b)? Please clarify logical flow with a more explicit description of the mechanism.

It seems that there was an error in this sentence, and that the explanation was not very clear overall. We have rewritten it as follows:

"*The zone of low effective pressure migrates with a migrating grounding line. Such migration obviously does not take place when the subglacial hydrological field is kept constant or when subglacial hydrology is not linked to basal sliding (or not considered; NON). For a retreating grounding line, such linkage actually amplifies grounding-line retreat, as the friction stress close to the grounding line is also reduced following this retreat, leading to a positive feedback mechanism. This reduction in* τ_b *stems from a reduction of* $\tilde{\tau}_b$*, but most importantly from a large value in the magnitude of* $\Delta\tilde{\tau}_b$*, which is typically negative.*"

We hope this clarifies the instability mechanism.

Line 465: that → which.

Corrected.

Line 481: Probably should use something less strong than 'On the contrary', such as 'However' or even 'Similarly' since the logic is the same. Then have a paragraph break or contrasting start to the next sentence to introduce the new logic of a spatially variable C.

Thanks for the suggestion; we have replaced "*On the contrary*" by "*However*".

References

Clarke, G. K. (2005). Subglacial Processes. *Annual Review of Earth and Planetary Sciences*, 33(1):247–276.

Response to Referee 2 on "A fast and simplified subglacial hydrological model for the Antarctic ice sheet and outlet glaciers" by Kazmierczak, Gregov, Coulon & Pattyn (Rev. 2).

Dear Referee,

We would like to thank you again for your detailed review and your numerous remarks.

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

Overview:

The authors of this manuscript have made substantial improvements addressing concerns and questions raised in the first round of reviews. They present a method for simulating subglacial hydrology on large scales (both in space and time) that captures several interesting qualities of potential drainage systems that may arise. The model assumes a steady state hydrological configuration and involves switches between different classifications of drainage types: efficient vs. inefficient, opening by sliding vs. opening by melt, close to the grounding line vs. far from the grounding line, hard bed vs. soft bed. While there are limitations, I think this is a creative approach with demonstrated practical use that has great potential for producing helpful advancements in understanding of the relationship between ice dynamics and subglacial hydrology on broad scales.

The revised manuscript is well organized and well written. I have a few relatively minor comments and questions, listed by line number below, along with a few typographical errors pointed out. With a bit of further refinement, this paper will make a nice contribution to the glaciological literature.

We would like to thank you for your positive assessment of our paper.

Specific Comments:

Title: I like the new title, but suggest "Antarctic Ice Sheet" to follow convention (capitalized when referring to a specific named ice sheet).

Corrected.

Line 41: extra "a" before "small".

Corrected.

Line 42: Some mountain glaciers can surge remarkably fast, advancing significantly in sub-year time scales, and Antarctic ice shelves can break apart rapidly. I suggest adding a qualifier here: "... several orders of magnitude smaller than the typical response time of glaciers and ice sheets..."

Corrected.

Line 65: ice sheet-ice shelf model (replace "-" with "–" for clarity, otherwise this reads as sheet-ice)

To avoid any confusion, we have replaced 'ice sheet-ice shelf' by 'ice sheet/ice shelf'.

Lines 91-92: This is a vague description of efficient criteria – perhaps a more quantitative statement would be better here. What qualifies as a large flux? Or you could describe physical characteristics of an efficient drainage system.

Following your advice, we have modified this sentence to the following: "*Generally, efficient systems transport large water fluxes and are characterized by localized channelized flow, while inefficient systems take the form of distributed water flow.*".

Line 122: This assumption may only be valid if there isn't much water generated. If you have a substantial amount of subglacial water, this won't necessarily be accurate.

Indeed. However, as mentioned later in this subsection, the meltwater supply in Antarctica (which is our primary application) is limited to subglacial meltwater.

Line 123: Does "sub-grid" here refer to your global or local grid scale?

This sentence was indeed ambiguous. We have replaced it by the following: "*The effective-pressure distribution is not calculated at the sub-grid (local) level.*".

Lines 149-150: Under what conditions was the dissipative melt negligible relative to the other melt terms? This can be an important source of basal meltwater depending on geometry etc., and in fact is the melt term usually responsible for triggering the channelization process. So, I don't think the general dismissal of this term is justified by this statement – and you may be missing an important piece in the hydrology physics if you ignore heat from dissipation. This deserves more acknowledgement.

We agree with the Referee that dissipative melt is an essential component in subglacial hydrology as it is a key element for channelization (e.g., Warburton et al., 2024). Dissipative melt of conduits impacts out model in two ways:

(i) In the subglacial water routing model, i.e., in the mass-balance equation (2).

(ii) In the equation for the effective pressure, i.e., in the balance of opening-closing balance equation (5).

In our model, the subglacial water flux is integrated across the entire grid area and distributed to a discrete number of conduits. Equation (4) describes how $\boldsymbol{q}_{\mathrm{w}}$ is converted to Q_{w}. Based on this approach, the location where the water is generated does not influence the calculation of Q_{w}. Since we have determined that $\dot{m}_{\mathrm{w}}$ is relatively small compared to $\dot{m}$, it can be safely ignored in this calculation.

However, the opening due to melt must be included in the opening-closing balance equation (5), as it is the dominant process for channels. Accordingly, we have kept the dissipative melt in that equation.

To clarify a bit that the dissipative melt is ignored only in equation (3), we have modified lines 149-150 to the following:

"*However, we do not include this last term in the computation of the subglacial water in our simulations as it was found to be negligible compared to the other terms.*"

Lines 176-177: It seems inconsistent to assume fully turbulent flow with large fluxes as described here, but neglect heat from dissipation in Eq. (3). This limitation should at least be mentioned and justified.

See our response to the previous comment.

Lines 219-220: Another interpretation of boundary conditions at the grounding line applies when it is actually grounded ice. Rather than $N = 0$ as is appropriate for floating ice, in the grounded case the subglacial water pressure could be equal to the pressure of the overlying water where it is discharged. This may be a small difference, but should ideally be tested or at least mentioned here.

We agree with the Referee that this boundary condition should also be considered. However, given that our paper specifically targets marine-terminated ice sheets we decided to keep only the 'floating ice' boundary condition. We have modified the description of the equation $N = 0$ as follows:

"*Finally, the third equation comes from the equality between the subglacial water pressure and the sea-water pressure at the grounding line (Drews et al., 2017), which holds because we are considering marine-terminated ice sheets*".

It would be interesting to see how our model would cope with this change in the boundary condition. It seems that in this case a boundary layer still exists (see e.g. Fowler, 2011, end of page 662), so it is likely that our model could be adapted for such a case. We leave this for possible future work.

Lines 260-261: Melt from dissipation was indicated to be neglected above (text following Eq. 3), so how does large flux lead to higher melt in your model? This is not clear.

As mentioned previously, while melt from dissipation is ignored for the computation of the subglacial water, it is not ignored in the opening-closing balance equation (equation (5)). We hope this clarifies your question.

Line 262: Clarify that this is only because of the steady-state condition that ice creep must increase to counter increased melt with increased flux. In a dynamic system that is not at steady state, these opening and closing rates are not necessarily in balance.

Indeed. In the introduction to the system of equations (5) we now explicitly state that these equations are associated with a quasi-static regime. Since the explanation in line 262 is related to equation (5), this should now be more clear.

Line 270: I suggest rewriting slightly to clarify that these are modifications that may be imposed to force the model to produce an entirely efficient or entirely inefficient system. As it is currently written, it can lead to confusion about what is included in the model. Suggested rephrasing: "By removing the opening term associated with the sliding over obstacles, $\|\boldsymbol{u}_\mathrm{b}\| h_\mathrm{b}$, from equations (5b) and (6a), it is possible to force the model to produce an entirely efficient drainage system. In this case, we also set $Q_\mathrm{c} = \infty$, which guarantees that the conduit geometry is the one of an inefficient system for soft beds. Similarly, to force an entirely inefficient system, the efficient component, $Q_\mathrm{w} \|\nabla \phi\| / \rho_\mathrm{i} \mathcal{L}_\mathrm{w}$, can be removed from (5b), together with the condition that $Q_\mathrm{c} = 0$".

Thanks for the suggestion, which we have followed.

Lines 281-282: Why use a different sliding law with the hydrology simulations (regularized Coulomb) than what was used for generating the reference state (Weertman)? I imagine you could justify this because the effective pressure varies spatially in the hydrology simulations, while in the reference state spin-up you likely assumed some spatially uniform fraction of overburden or something. Please explain.

We used a regularized Coulomb siding law in our simulations because it allows for both a viscous and a plastic behavior as a function of the sliding velocity (see equation (1)). However, to generate the initial state, we used a Weertman sliding law. For the idealized experiments, this is motivated by the fact that we rely on the MISMIP set-up which considers a Weertman sliding law to get the initial state. We have slightly modified our manuscript to make this 'choice' more clear; the lines 281-282 have been modified to the following:

"*In our experiments, we use a regularized Coulomb friction law combined with hydrological models, while the reference state from the MISMIP set-up has been obtained with a Weertman friction law.*"

For the Thwaites experiments, we also used a Weertman sliding law. Here, we chose this law for efficiency of the spin-up procedure, as it is also run without subglacial hydrology. Nevertheless, this has no impact on the results of the simulations that follow, given our scaling approach that is explained in the manuscript.

Lines 301-303: Suggested rephrasing for enhanced clarity: "We also compare the impact of the drainage efficiency, by comparing the cases where only efficient (eff) or inefficient (ineff) systems are allowed to develop. Note that, by default, the switch between both systems (efficient/inefficient) is determined based on the subglacial water flux magnitude".

Thanks for the suggestion, which we have followed.

Line 309: "correspond", not "corresponds".

Corrected.

Line 518: "when basal cavities are growing" should be "where basal cavities are growing" (your model is considering steady-state configurations, so this should be a spatial description, not temporal).

Corrected.

References

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).

Fowler, A. (2011). *Mathematical Geoscience*. Springer London.

Warburton, K., Meyer, C., and Sommers, A. (2024). Numerical and physical instability of subglacial water flow. *EarthArXiv preprint*.