Review of “A fast and unified subglacial hydrological model applied to Thwaites Glacier, Antarctica” by Kazmierczak and others, submitted to *The Cryosphere*

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.

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.

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.

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.

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

Lines 55-56: Suggest changing wording to avoid using “evaluate” twice in the same sentence.

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

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

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

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.

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.

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.

Line 112: Depth-integrated subglacial water flux?

Lines 119-120: For completeness, describe how the melt rate due to dissipation (m_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.

Lines 126-129: Intriguing to use the simple routing scheme – I'm interested to see the results that support the claim that ϕ_o 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.
Line 132: How is l_c chosen? How sensitive are results to this value?

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

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.

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?

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?

Equation6: Are N_infinity and S_infinity the effective pressure and conduit cross-sectional area far from the grounding line? That's what I infer, but they should be explicitly defined.

Line 181: How close to 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.

Line 204: missing a period

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

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.

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

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

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.

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?

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).

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

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.

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?

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

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).

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?

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

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.

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

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.

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

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