Review of A fast and unified subglacial hydrological model applied to Thwaites Glacier, Antarctica, Kazmierczak et al.

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

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

As I read it, the modelling assumptions are

- The hydrological system is always in steady state, i.e. the timescale of basal melt and channel development is fast compared to the timescale of forcing changes - likely a good assumption for Antarctica, less so for seasonal meltwater input in Greenland (so figure 7 seems a bit of a perverse/misleading test case - although here the timescale appears to be thousands of years, so perhaps this is not supposed to investigate seasonality, just a demonstration of the non-monotonicity of figure 5?)
- Gradients in hydraulic potential are primarily geometric, since N is slowly varying, except at the grounding line, so when converting between Q_w and S using (5a), we can ignore gradients in N. This seems reasonable, but I don't quite understand the paragraph at l.126 - "so we choose not to do this" (do what?). Isn't $\boldsymbol{q}_{\mathrm{w}}$ being computed directly from (2) without any specification of what gradient it is proportional to? Perhaps the way (2) is solved could be made more explicit - no expression for $\boldsymbol{q}_{\mathrm{w}}$ is given in the manuscript.

  Note there is an extra factor of S_∞ in (6a), but I assume this is just a typo, since the plots of N_∞ in figure 5 show the correct behaviour from (5b).
- Close to the grounding line, N must go to zero, so by eye, the authors pick an error function to approximate this transition. Per appendix B, this is not the solution to any local inner form of the ODE, but just a function that has the right gradient at the grounding line.
- Drainage density, regardless of the nature of the basal hydrology, is constant in space and time, and thus the flux through a drainage element is some constant, large, multiple of the flux through the area it represents. This one I find harder to wrap my head around, particularly since inefficient drainage is often imagined as slow flow everywhere (so what even is a drainage element in this case?) and models such as GlaDS and SHAKTI show dynamically evolving channel networks and drainage densities over time. This really is a big simplification, and the one that allows for the shift in scale, and I'm saddened that it is not discussed further (the choice of value for l_c, the drainage density, is not discussed at all).
- Effective pressure within the drainage elements (a small proportional of the domain) is equal to the effective pressure everywhere else - despite how strongly models that resolve the channels show them as being local lows in the hydraulic potential. (Not discussed)
- Specific choices about how H, L, and S depend on the type of bed, which are well-discussed and clear.

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

I'm also confused about the basal melt production. No expression for the $\dot{m}_w$ in equation (3) is given, the term driving feedback between routing and meltwater production. The channelised version of the expression is given in (5b), but it's not clear if/how this is included in the routing algorithm. This feedback also seems to be missing in (B1b), with the meltwater input to the channel assumed constant (scaled to 1 in B4b) and not dependent on local melt.

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