Referee comment on ‘Persistent, Extensive Channelized Drainage Modeled Beneath Thwaites Glacier, West Antarctica’

Doug Brinkerhoff January 2022

1 Summary

In this section, the authors make the case for significant channelization in the Thwaites catchment, which may have substantial implications for its evolution due to both the modification of sub-shelf circulation by melt plumes and also a relatively higher upstream basal shear stress due to efficient evacuation of water (and thus a reduction in water pressure). This argument primarily relies upon the comparison of a numerical model of drainage development with putative radar-based observations of a proxy for drainage type. A moderately large ensemble of model configurations are tested, leading to several configurations that are consistent with observations, all of which involve the development of substantial channelization.

I find this paper to be well written and compelling. I particularly appreciate the ensemble approach to the modelling, which greatly improves the robustness of the results. I do not have any major comments; however I do have a handful of minor comments, clarifications, and technical corrections that I would like to see addressed.

We thank Dr. Brinkerhoff for his thoughtful and detailed review our paper, and for providing useful suggestions for improving our manuscript. We have addressed all concerns and have provided clarifying explanations where necessary.

Line-by-line comments

L92 I suggest mentioning that the implications of neglecting the pressure change term are discussed in the discussion.

A sentence mentioning this will be added to the next draft of the manuscript.

Eq. 7 Where are the channels? Are they defined on the edges of the Voronoi mesh, or between center points, or something else entirely?

Channels segments connect the center points of neighboring cells, and channel velocities and fluxes are computing at the edge midpoints between two cells. A few sentences describing where variables are computed on the Voronoi mesh can be added to the manuscript, although this has been done in detail in Hoffman et al. (2018).

L101 I suggest language like ‘time derivatives are discretized using an explicit forward Euler method ...’.

This language will be revised in the manuscript to improve clarity.

L104 What are these ‘transients’ mentioned here? Numerical instabilities? Usually this word refers to non-physical changes induced by initial conditions that are far out of balance with the governing equations, in which case, temporal averaging wouldn’t seem to do much.

The "transients" described here are minor, stable oscillations similar to the limit cycles described in Kingslake (2014) and Schoof (2020), and result from pressure fluctuations caused by the englacial storage term or bedrock cavity space. Taken as a multi-year average, these small perturbations fluctuate around a steady-state, and are not consequential to our model results. The authors are open to any language changes here if it would improve clarity.

L117 'surfface' → 'surface'

This will be changed in the next version of the manuscript.

L123 I don't understand the notion of varying hydropotential values: isn't sea level just sea level? Depth wouldn't have anything to do with it.

Hydropotential in ocean cells is calculated as $\phi_o = \rho_w g Z_b - \rho_o g Z_b$, where ρ_w is the density of fresh water, g is gravity, Z_b is the bed elevation, positive above sea level, and ρ_o is the density of ocean water. Thus, the freshwater subglacial hydrologic system at the bed 'feels' the hydrostatic pressure of the overlying ocean water column at the grounding line boundary, which varies spatially with changes in bathymetry.

L123 Why suppress backflow from the ocean boundary? That would seem to be something that could easily happen physically.

Backflow into the subglacial system from the ocean is theoretically likely to occur up to several kilometers inland from the grounding line in both the distributed and channelized systems (Wilson et al., 2020; Robel et al., 2022). However, the theory describing this process is not fully developed, nor is it currently implemented in two-dimensional subglacial hydrology models. Saltwater intrusions are thought to propagate into the subglacial system as a salt wedge beneath outflowing subglacial freshwater that is likely influenced by a combination of turbulent mixing, double diffusive instabilities, tidal pumping, spatially-varying bed lithologies, and other complicating factors (Wilson et al., 2020; Robel et al., 2022). Without the ability to resolve this complex two-layer exchange flow, our model can form unrealistic instabilities near the grounding line akin to a Jökulhlaup draining from the ocean into the glacier interior when effective pressure gets very low. It is rare in the model, but due to fixed sea level, when it does occur, it results in unchecked exponential channel growth and instability. Suppressing backflow from the ocean is therefore the most realistic choice for our model; however, we acknowledge this as a potential limitation and future avenue for model improvement.

Sec. 2.3 Maybe this comment isn't specific to this section, but generally there has been a fair bit of work in recent years on inferring the parameters of subglacial hydrologic models from indirect observations, and some of these would be worth citation, for example Irarrazaval, 2021 and Koziol, 2017 (and references therein).

The next version of the manuscript will reference other studies, such as those provided by the reviewer, that used alternate approaches to infer unknown subglacial hydrology parameters.

L162 I don't understand the strategy of holding either W_r or c_s constant while varying the other, which seems quite ad hoc. Why not do a proper grid search or pseudorandom sample?

While a full sweep of the conductivity parameter space is done for each bed roughness combination, the determination of bed roughness parameters is closer to a sensitivity analysis (varying one parameter at a time). This is done intentionally in part to decrease the number of necessary model runs, and to ease the complexity of a four-dimensional parameter sweep. This choice is made because real physical constraints exist for conductivity parameters (lines 136 – 156), while bed roughness parameters are theoretical quantities approximating the broad, general characteristics of the bed that only have indirect physical corollaries. Therefore, it makes sense to explore all of realistic parameter space for conductivity values, yet undertake a sensitivity analysis (deviating from typical values in literature) for bed roughness parameters that lack real physical constraints. We acknowledge our approach is a bit ad hoc and lacks the rigor of a proper uncertainty quantification.

L169 This is a bit of a weird sentence: stability should not, a priori, influence experimental design. Of course, there is a de facto dependence when simulations don't converge, but this shouldn't enter the reasoning when designing which experiments to run.

Please see response to next comment.

L169–180 I don't understand these criteria for steady state and why they are used for filtering simulations, and this section should be developed more substantially. The inputs to this model are not time-dependent, so why is there a problem for achieving steady state with respect to either flux or effective pressure? Why not just run longer? Does a lack of steady-state indicate numerical instability? How do you know that it's an instability and not a limit cycle? Why are simulations that achieve the flux steady state somehow more reliable than the other?

Runs failed to reach steady state for two main reasons: 1) A local numerical instability developed in the channel model, or 2) the domain became over-pressurized so that the adaptive timestep failed to progress. Instabilities occurred when discharge in a channel was unrealistically high (normally this occurred because the channel conductivity was too high or the distributed conductivity was too low) so that an unstable feedback cycle developed where unrealistic channel discharge increased channel dissipative melting, which in turn led to higher channel discharge and more dissipative melting. This normally occurred locally at a few neighboring grid cells, so that the water pressure and thickness elsewhere in the domain was unaffected. Hence, we developed the pressure steady state criteria so that these runs could still yield some useful information about aspects of the subglacial drainage system other than discharges. In some cases, instabilities could be avoided by changing the englacial porosity, which acts as a buffer between meltwater production and the subglacial system but does not affect the steady state configuration.

Runs became over-pressurized at very low distributed conductivities where the distributed system could not compensate the net input of meltwater, and the adaptive timestep became impractically small to meet the pressure CFL condition. Runs that failed to reach flux steady-state did not represent steady systems where the subglacial discharge realistically balanced the production of meltwater, and so it was not possible to accurately assess the relative fraction of channel discharge to distributed system discharge. As our goal was to explore as much of parameter space as possible, runs were continually restarted until either reaching flux steady state, forming an unpreventable numerical instability, or becoming computationally untenable to keep running. A more detailed description of this process and the reasoning behind it can be added to section 2.3 in the manuscript or to a supplement.

Eq. 12 A single uniform W_r is a nice numerical parameterization, but it seems unlikely that such a thing acutally exists, and that real bump heights are randomly distributed. This deserves a comment*, because this notion of simulated specularity would seem to rely on actual uniformity in W_r.*

The authors agree that a spatially uniform W_r likely does not actually exist, and that this deserves a comment here in the manuscript. It an underlying assumption of MALI and similar subglacial hydrology models (ie., Schoof, 2010; Hewitt, 2011; Werder et al., 2013) that this parameterization of bed roughness broadly captures sub-grid cell bed characteristics, and it remains the best approximation for bed properties *in lieu* of very high-resolution (< meter scale) observations of the bed beneath the Antarctic Ice Sheet. However, this does not change the reasoning behind Equation 12, which would still simulate specularity if W_r varied spatially.

L205 I am quite skeptical of the justification for trying many combinations of model and data specularity thresholds as capturing transitional behaviors. It seems like a more reasonable justification is that it's not immediately obvious how to compare these proxies for subglacial conditions, and by doing a grid search for high correlations, you're doing a sort of ad hoc maximum likelihood estimator for the parameters of the model relating the two.

The transitional behavior is an important reason that comparison between modeled and observed specularity is not straightforward; however, the authors agree that the reviewer's justification for this comparison method is convincing. This section can be revised to combine both justifications, as well as address the concerns of the other reviewers' comments (see response to RC1, comment #3).

L232 'This range ...' which range is being referred to?

This sentence is referring to the range of k_q values that were seen in data-compatible runs. This distinction will clarified in the next draft of the manuscript.

Sec. 3.1.1 I think that reporting the ranges of parameters independently that agree with data (i.e. a marginal posterior distribution) is of limited utility because of the high likelihood of posterior correlations (e.g. only simulations that have a high k_Q and low k_q or vice versa might make it through the filter. Such a pattern has been seen in other works that infer parameters of subglacial hydrologic models). I think that a useful figure would be a plot where all four parameters are plotted pairwise, with both initial and filtered simulations presented (but colored differently). This would elucidate (at least) pairwise posterior dependencies between parameters. This is already done a bit in Figure 3, but I think it would be useful to extend it to other parameters as well.

The authors agree this could be helpful; however, because we use consistent values for each parameter, the plots described by the reviewer would result in many overlapping points and would be difficult to interpret. Instead, we can include versions of Figure 3 for each bed roughness parameter combination in a supplement. We hope this will address the reviewer's concerns.

L315 Here and elsewhere, it would be helpful to define what is meant by 'below capacity'.

A definition of "capacity" can be included at line 193, where the word is first used.

L327 It would be helpful to see a figure illustrating this lake filling a draining. I don’t actually see much of a mechanism in the model that allows for subglacial lakes to form to begin with: what does this look like in the model, and are its physics really capable of simulating such a thing?

As briefly discussed in lines 330 – 335, our model lacks the proper physics to accurately capture realistic subglacial lake filling/draining behavior, and the behavior seen in our high-resolution is likely akin to the limit cycles of lake filling and draining described in Kingslake (2014) and Schoof (2020). A figure or video illustrating this behavior this could be added to a supplement. Conversely, the authors are also open to omitting this paragraph, as this discussion point does not impact our primary results and is still somewhat speculative.

References

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