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.

2 Line-by-line comments

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

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

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

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.

L117 'surfface' → 'surface'

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.

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

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).

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?

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.

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?

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.

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.

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

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.

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

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?

References

[1] Irarrazaval, I., Werder, M., Huss, M., Herman, F., Mariethoz, G. (2021). Determining the evolution of an alpine glacier drainage system by solving inverse problems. Journal of Glaciology, 67(263), 421-434. doi:10.1017/jog.2020.116

[2] Koziol CP and Arnold N(2017) Incorporating modelled subglacial hydrologyinto inversions for basal drag.The Cryosphere11(6), 2783–2797. doi:10.5194/tc-11-2783-2017.