Review of “Persistent, Extensive Channelized Drainage Modeled Beneath Thwaites Glacier, West Antarctica - Hager et al.”

Summary of the paper:

The manuscript investigates the possibility of formation of channelized drainage under the Thwaites glacier using subglacial hydrology component of MPAS-Albany Land Ice model. Earlier, Schroeder et al. 2013 reported transition of water system from distributed to efficient drainage system in the interior of Thwaites glacier from specularity content data of bed echo. In this paper, the authors run several numbers of simulations by varying four parameters (sheet conductivity, channel conductivity parameter, Cavity spacing, and bed bump height) and compares the modelled results with the specularity content data of Schroeder et al. 2013 to assess the likelihood of channelization under Thwaites glacier.
The authors discuss their results with different geophysical properties of the Thwaites glaciers, some of which are important for further investigation. A counter-example analysis of channelization is also provided by running some simulations with channelization disabled which is a great addition. It is subject to discussion whether the findings are robust, but the presented results are very well complemented with counter examples, available data and adequate discussions, and that makes it worth getting published.

The paper is overall well written. The introduction nicely covers previous knowledge of subglacial hydrology in general and this region in particular. The results are very well-articulated. The authors complement their results with other observations such as ice shelf basal melt and present some interesting discussions about the findings of the paper, such as effective pressure consideration at the grounding line in sliding laws, etc., which is important for future research. Overall various geophysical aspects of Thwaites glacier are discussed under the findings of this paper, and all these make the content of the paper rich. However, the paper has some loose ends which requires some work. I mention them in the following:

Comments:

1. The method section, especially section 2.4 needs substantial work. This part is very much unclear. The physical basis of choosing the specularity content threshold is not clear to me. Similarly, it is not clear why 6 different Rwt threshold values are used instead of one Rwt (e.g. 0.95). At present it is not easy to understand how different simulations are done with different combinations of parameters. I would suggest to provide a table explaining the simulations. Additionally, it is not obvious how different parameter sweep leads to certain number of simulations.

2. Whereas the parameters choices are very well explained, the choice of thresholds lacks sufficient explanations. There are many thresholds used (flotation, flux steady state, Pressure steady state (all in sect. 2.3), Scrt, Rwt, correlation coefficient> 0.35 (sec. 2.4)) and results of this paper are highly dependent on the choices of these threshold. These choices remain very

subjective, and not enough supported analysis is provided for their choice. I would recommend to provide substantial logic for using those thresholds and have detail discussion around them. The authors need to present some more statistics to support these choices, and it can be included as appendix or as supplementary, if not as main article. That brings me to the question that how does your result are sensitive to the choice of threshold?

3. Comparison with specularity content seemed bit like cherry-picking. However, I do not deny the potential of specularity data in understanding subglacial hydrology. I just feel that these data can be used better/sophisticated way to infer status of subglacial hydrology. Results associated with specularity content are not very robust and presented in very sporadic manner. In my opinion, this is the major area of improvement for the manuscript. The authors should explore better way to have comparison with specularity content data. The choice of threshold of specularity data is not clear. There is no physical basis of it. Furthermore, the description needs to be improved substantially. At present, this part is not completely clear to me. I would recommend to add more detail description with figures for this section to enhance the readability.
Although I do not have any clear suggestions on specularity content data, but the authors should find a better way to compare the specularity data which I think does not require any additional model runs. The present way of representation and analysis is neither very convincing, nor easy to understand.

In addition, I have some major specific questions/comments:

1. Can you please elaborate why two different steady-states criteria are chosen?

2. Please provide justification of considering avg. water pressures of >90% flotation. Using 90% only is very subjective. It would be good to provide supporting result of choosing 90%. For example, show how your results will differ when using 80% or 95% flotation.

3. The effective pressure (N) in the interior seems bit high especially where specularity content is high which is supposed to represent distributed drainage! Can you have some discussions on your derived effective pressure value with effective pressure reported in other studies? You provide good discussion with the discharge from previous studies of Antarctica, but I would recommend to do the same for effective pressure.

4. In addition to above comment on sec. 2.4, I would recommend to add few figures showing different masks derived using thresholds of Scrt or Rwt. If not here, these figures must be provided in appendix or in supplementary.

5. The paper presents nice analysis with parameter sweep and completement their results with ice shelf basal melt rate. I think this part can get more focus as it is an interesting comparison (e.g., Wei et al., 2020).

Minor comments

Line 215: 'majority of the cells' - How many number or cells do you mean here?

L 229-230: Was zero instances of water pressure below 90% outside data-compatible run? I would suggest to include a table here too with these criteria. Otherwise, this line remains bit vague.

L 256- 257: " .. the 50–100 km transition to channelized flow coincides with a substantial increase in basal friction melt rate." - Can you please elaborate on that with some data?

Figures 2,6 - What does the black dots in b and d represents? Are these the locations of significant correlations?

Fig. 5: Does ‘> 5m2s-1’ include >10 m3s-1? or it is >5 and <10?

L 275 -277: " .. pressures near the upper domain boundary, although effective pressures within 300 km of the terminus are in strong agreement with the low-resolution model." - It is not clear to me from the figure.

L561: The author list is incomplete.