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:

We thank the reviewer for their thorough review and for providing useful suggestions for improving the manuscript. We have addressed all concerns or provided further explanations to our choices of methodology. As most of the reviewer's concerns regarded our method for comparison between specularity content and model output, we have provided detailed additional information explaining our choice of comparison method that we hope will be satisfactory to the reviewer.

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.

For each set of bed roughness parameter combinations, we sampled and expanded the conductivity parameter space at consistent intervals until runs either failed to reach steady-state or had average water pressures below 90% flotation (lines 163–165). Useable conductivity parameter space varied with different sets of bed roughness parameters, so we were not able to conduct the same number of runs for every set of bed roughness parameters. The authors would be open to including a table of all of our runs in a supplement, or conversely, include supplementary figures analogous to Figure 3 for all 6 sets of roughness parameters (see response to RC2, comment on Section 3.1.1). These figures would similarly illustrate average water pressures, correspondence to specularity content, and which runs reached what steady-state criteria. Please see response to comment #3 for a discussion on specularity content and R_{wt} thresholds.

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?

Establishing steady state criteria inherently involves defining a cutoff threshold for acceptable noise remaining in the model. For our pressure-steady runs, effective pressure at each cell is allowed to fluctuate 0.5% of its value on average. This equates to an allowable fluctuation of roughly 1 kPa where effective pressure is lowest (∼200 kPa) and 10 kPa where effective pressure is highest (∼2000 kPa). For flux steady-state runs, meltwater production above each transect must equal the total discharge across the transect within 0.5%. Total melt production above the grounding line is roughly 155 m^3/s, so our steady-state criteria require that we know the total grounding line discharge within 0.8 m^3/s. Given this analysis, we believe the choices of steady-state thresholds are fairly strict and do not meaningfully influence our results. No data-compatible runs had average water pressures below 91% flotation, so our requirement that acceptable runs have water pressures >90% does not influence our results (please see response to specific comment #2). Please see response to the following comment for discussion about the thresholds used in comparison between specularity content and R_{wt}.

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.

High specularity content and high R_{wt} both unequivocally represent broad, flat areas of pooled water, yet the two are governed by independent processes and likely do not covary when their values are low. This makes comparing the two difficult, and a simple spatial correlation unlikely to work as a comparison method. Comparisons between the two quantities should instead rely on spatial point patterns (such as our binary masks) that map where specularity content and R_{wt} are high. Unfortunately, this method does require choosing critical thresholds of what is considered "high" for each quantity. We address this problem by creating a population of masks for each variable, each using a different critical threshold within a reasonable range (see below for determination of "reasonable values"), and comparing all 66 combinations of specularity content and R_{wt} masks. Data-compatible runs only have to match one mask combination, which makes our comparison less sensitive to our choices of critical thresholds.

Absolute values of specularity depend on the geometry of ice thickness, survey geometry, radar processing, and subglacial water geometry (Schroeder et al., 2013; Schroeder et al., 2015; Young et al., 2016; Haynes et al., 2018). As a result, the relative specularity can be interpreted as a measure of the relative "amount of bed covered by flat subglacial water bodies" within the glacier. For this we set a threshold based on the cumulative distribution of specularity within the particular survey/glacier (Figure RC1a). Comparison with the model is sensitive to the choice of threshold, so the comparison is repeated using a range of different thresholds between 0.15 – 0.25, which selects for the greatest ~5 – 15% of our specular data. This is a conservative and deliberatively empirical approach focused on comparing the water transition expressed in the specularity in Schroeder et al. (2013) with our modeling. There is potential for finer-scale local analysis of specularity signals by adapting and expanding the electromagnetic modeling approach in Mark Haynes et al. (2018) to the glacier-catchment scale, which is exciting but beyond the scope of this paper (which is focused on investigating and understanding transitions in subglacial system configuration in ice sheet modeling and comparing that to broad-scale patterns in specularity data).

As with our choice of S^{crt}, there is a range of R_{wt} that could simulate specularity in our model (this occurs near R_{wt} = 1.0), and our comparison results are sensitive to this choice. To address this, we again create multiple masks of R_{wt} using different thresholds ranging between 0.95 and 1.0 and require only one of these masks match one specularity mask. This approach allows us to minimize the sensitivity of our analysis to our choices of thresholds.

Matching specularity and R_{wt} masks is essentially a comparison between two spatial point patterns. Such a comparison is challenging as it requires a global statistic that can recognize local patterns of point clusters. We explored many alternative methods of mask comparison, including calculating the spatial similarity index described in Andresen (2009; 2014), which segments the domain into multiple areal units and uses a Monte Carlo approach to determine an overall similarity statistic across all areal units. However, this method is highly sensitive to the size, shape, and location of areal units, and was largely unsuccessful at identifying similar specularity and R_{wt} masks.

The method developed in the current paper shares the concept of areal units by defining four physically-based zones within which we assess similarity between the two masks. These zones are intentionally chosen to loosely encompass regions of specularity or non-specularity, which allows for some spatial variability between masks and decreases the sensitivity to the zonal boundaries. We then require the two specularity and R_{wt} masks to match at 50% or more of grid points within each zone. While this criterion does well by itself in selecting positive matches, it also selects many false positives. This occurs when the R_{wt} mask is almost entirely non-specular and over 50% of the cells in each zone is non-specular in the specularity mask. It is therefore necessary to include a second criterion that can remove these false positives, which we do by requiring an overall correlation coefficient of $r \geq 0.35$. Again, correlation by itself does a fair job at identifying positive matches, but it also identifies false positives when the R_{wt} mask is overly specular. As the two criteria fail for opposing reasons, they can check and balance each other if the thresholds are tuned appropriately. We acknowledge this comparison method is sensitive to multiple choices of thresholds, so we attempt to make our criteria for selecting data-compatible runs as generous and inclusive as possible while still removing runs that clearly do a poor job at resembling observations. We empirically determined that requiring ≥ 50% of cells in each zone to agree and $r \geq 0.35$ works well at identifying positive matches and is sufficiently general to allow a reasonable variety of R_{wt} masks to pass this filtering process.

The authors acknowledge that some of the above information was not included Section 2.4, yet would be helpful in making a convincing argument for our comparison method. We will adjust section 2.4 or provide a supplement to better explain the justification for the two criteria and how they complement each other. We can also provide supplementary figures illustrating various combinations of specularity and R_{wt} masks together with their comparison statistics (ie. Figures 2b, 2d) so that the reader can see how our two criteria identify positive and negative matches.

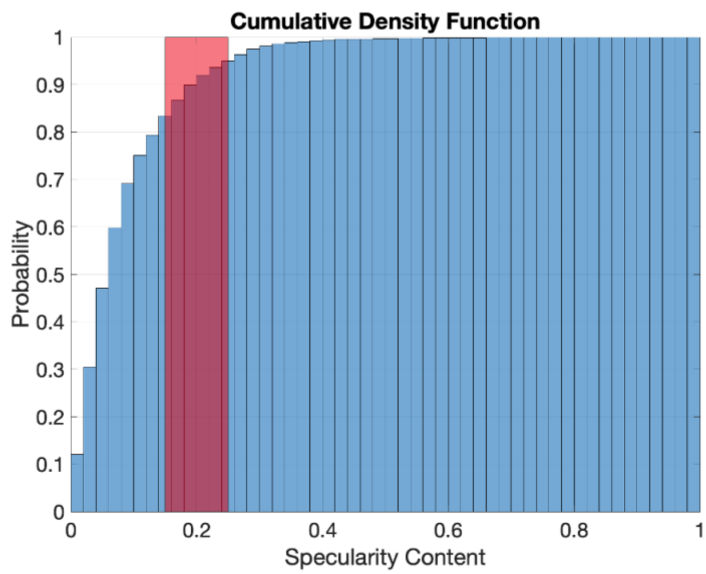

Figure RC1a: Histogram of the cumulative density function of specularity content from Schroeder et al. (2013). Red band indicates the range of S^{crt} thresholds used for binary masks.

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

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

   Please see response to RC2, comment2 L169–180.

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.*

   The standard of ≥ 90 % flotation was used as a first-order criterion for determining when runs may be unrealistic and when to stop expanding parameter space. This choice is consistent with observed and modeled water pressures near flotation observed at Ice Stream B (Engelhardt and Kamb, 1997) and Pine Island Glacier (Gillet-Chaulet et al., 2016), respectively. We built upon this constraint by imposing our steady-state criteria and specularity comparison standards. No data-compatible runs had average water pressures below 91% flotation, so this choice does not affect our results. We acknowledge the wording surrounding this criterion is confusing, and it will be addressed in a later version of the manuscript.

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.*

   Modeled effective pressure is lowest where specularity content is high, which is in agreement with the presence of a distributed system at these locations. One of the novelties of this study is that it provides estimates of effective pressure beneath part of the West Antarctic Ice Sheet, which is largely unknown to date. To the authors' knowledge, the only direct observations of effective pressure in West Antarctica were measured via a borehole at Ice Stream B (Engelhardt and Kamb, 1997). Those authors report effective pressure ranging between -30 – 160 kPa, similar to average effective pressures of 200 – 500 kPa in the highly specular zones in our data-compatible runs. As the observations of Engelhardt and Kamb (1997) were measured at one point in an ice stream, we would expect their reported effective pressures to be less than those at upper Thwaites Glacier, where ice flow is slower. We can incorporate this comparison into section 4.2.2, as well as any other suggestions of observational constraints of effective pressure in West Antarctica.

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.*

   Please see response to above comment on section 2.4.

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).*

   Section 4.2.1 is devoted to the impact of subglacial channels on ice shelf basal melt rates. A comparison to Wei et al. (2020) would fit nicely in this section and will be included in a later version of the manuscript.

Minor comments

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

Greater than 50% of the cells within each zone had to agree between the specularity and R_{wt} masks. The exact number of cells varies by zone, as each zone has a different number of cells.

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.

Please see above response to major specific comment #2

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?

A new figure depicting the sliding velocity, basal traction, and basal friction melting can be added to the methods section of the paper so the reader can compare the primary model forcing to the results in Figure 4.

Figures 2,6 - What does the black dots in b c3and d represents? Are these the locations of significant correlations?

The black dots in Figures 2 and 6 are the binary masks of specularity content and R_{wt}, as described in section 2.4 and in the figure captions. The authors are open to wording suggestions to make the captions clearer to the reader.

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

Q > 5 m^3/s includes Q > 10 m^3/s, and does not mean 5 – 10 m^3/s.

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.

The comparison here is between Figure 4b and Figure 4c. The authors feel these subplots accurately depict the sentence in question, but are open to suggestions about how to make this more clear to the reader. One possibility would be to add a centerline transect to Figure 7a from the high-resolution model.

L561: The author list is incomplete.

This will be fixed in the next version of the manuscript

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