Dear Dr. Kang and Reviewers,

We are very appreciative of your time in providing thorough comments and feedback for improving our manuscript. We are encouraged that both reviewers thought the manuscript was either ready for publication or close to the point of publication. We have made minor revisions to the manuscript in response to our anonymous reviewer's suggestions. The most significant changes are a new paragraph in Section 4.1, as well as the inclusion of two additional figures in Appendix B: 1) a flow chart of our methodology for comparing specularity content with model results (Figure B1), and 2) a plot of the cumulative density function of our specularity data (Figure B2). We also made minor wording changes to improve the manuscript's readability and inserted a few additional statistics regarding our choice of R_{wt} thresholds to improve the credibility of our methods. In cases where we felt a change to the manuscript was not warranted, we provided our justification for our choice. We hope these minor revisions satisfy the reviewer's concerns.

In this document, reviews are given as blue italicized text, and our responses and descriptions of the revisions made are provided in black text. References to specific lines and figures in the manuscript refer to the revised text. All changes are highlighted in the 'track changes' version of the manuscript. Again, we thank you for your time and careful reading of our manuscript.

Sincerely,

Alex Hager

Referee comment on “Persistent, Extensive Channelized Drainage Modeled Beneath Thwaites Glacier, West Antarctica”

I appreciate the detail response of the authors. They have adequately addressed my questions. The addition of the Appendix further enhances the strength of the manuscript, especially the addition of Fig, B1 adds further clarity to the analysis. I must say the authors did good job to improve the readability of the manuscript, but I am sorry to say that it is not sufficient yet. Therefore, I still have some concerns and all of my concerns are basically around the comments that I made last time. Although, I am still not sure how robust this analysis is, but it seems convincing considering the limitations.

1. *Flux-steady state: Where from the assumption comes that "For flux steady-state runs, meltwater production above each transect must equal the total discharge across the transect within 0.5%." Do you take it from some other paper or it's your assumption? Please provide detail justification.*

   The flux steady-state criterion is a metric used for this paper and is not taken from previous studies. However, this is already a more robust and quantitative criterion than what is used in most other subglacial hydrology modeling studies, which either do not define a steady-state criterion or do so qualitatively. Ideally, up-glacier meltwater production would exactly equal discharge across each transect; however, this is never actually the case. Therefore, a cutoff point must be defined to establish when a model run can be categorized as being at a steady state. We include our quantitative criterion in the

text to be as thorough and open about our methods as possible, but this is beyond the norm in subglacial hydrology modeling papers. Appendix A includes an analysis of how much our choice of steady-state criterion affects our results, and the current revision includes an addition clause at lines 561–562 to emphasize the negligible uncertainty it contributes.

2. *The authors have taken an unconventional way to support the channelization beneath Thwaites glacier by comparing specularity content with model derived Rwt to. The approach is commendable, but simultaneously, being it bit unconventional (and ad hoc, as also author mentioned), a step-by-step clarification and discussion is required. I understand the authors have taken very conservative approach to match specularity with model, but the approach is still bit ad hoc and not easy to understand. The comparison conducted between Specularity and Rwt is extensive, and I appreciate it, but it is not clearly reflecting in text. I would strongly recommend the authors to extend this section. Also, include the cdf figure in the appendix (provided in response letter). Explain, what possible Rwt values you may get from the model run and then justify your choice of threshold. Without this progression, it is very confusing. If required, split section 2.4 into subsections and add more figures in appendix.*

   A flow chart illustrating the step-by-step process for comparing specularity content with Rwt was added to Appendix B (Figure B1), along with the CDF figure described by the reviewer (Figure B2). If the steps outlined in Section 2.4 and Appendix B are still unclear, we kindly ask the reviewer to provide specific items that need more clarification.

   As defined by Equation 12, values of Rwt can be any positive quantity, but should rarely exceed 1, as this would require the distributed system to be above its capacity and effective pressures to be zero. Rwt must be near or above 1 to resemble a flat, highly specular surface, although there should be no difference in specularity between Rwt = 1 and Rwt > 1, because both would create a perfectly flat, specular surface. Thus, the only discretion we exert in choosing Rwt thresholds is to determine a lower bound. Comparison success rates exponentially increased with higher Rwt, with only 4% of successful matches occurring with a Rwt threshold of 0.95 (as opposed to a success rate of 34% for a threshold of 1.0). The few runs that had successful matches with a Rwt threshold of 0.95 also had successful matches using higher Rwt thresholds, indicating this choice of lower bound does not influence our results. This information was added to the results at lines 267–270 to improve the credibility of our methods.

3. *Another confusing part of the manuscript is absolute values of specularity and relative specularity. This is not clear what do you mean by these two specularity term. In addition, in the response letter, figure RC1, you showed a figure with specularity value up to 1. Is this absolute or relative specularity. In either way, this needs to be clarified. This is one of the important parts of the paper, but this severely lacks clarity.*

   Absolute values of specularity content depend on survey-specific factors, such as survey geometry and radar processing, and thus specularity should only be compared relative to other data within a specific survey. Therefore, values that may qualify as high specularity

in a survey of Glacier A may not be deemed high specularity in a separate survey of Glacier B. There is no difference in the values of absolute and relative specularity, just the classification of high or low specularity. We acknowledge the wording in this section is confusing, and we made minor wording changes to lines 235–238 to clarify this distinction. The CDF figure mentioned above was used to illustrate our determination of "high" specularity content within our dataset, and is now Figure B2 in the revised text.

4. *The discussion part of the manuscript is very rich with various geophysical aspect, but except the discussion about Specularity and Rwt. I think there is ample opportunity to discuss about Rwt values, what can be its possible physical meaning, what they infer, whether this can be compared with other data, etc. The reason I am emphasizing on this is because this is a new approach and that's why it needs detail analysis and discussion. So far, it is vaguely convincing and incomprehensible.*

   Following the reviewer's suggestion, we inserted a paragraph into the discussion at lines 264–372 connecting the regions of high Rwt and specularity to our reconciled framework of Thwaites Glacier subglacial hydrology. Here, we interpret these regions as the "pooling of broad, flat water bodies in a distributed system at or near its capacity." This agrees with the physical definition of Rwt provided in Figure 2, lines 219–221, lines 225-–227, and in the Figure 7 caption. If this is not sufficient to meet the reviewer's concerns, we kindly ask the reviewer to provide specific lines from the manuscript where it could be improved and an explanation of what is unclear.

   Rwt was developed here specifically as a corollary to radar specularity content, and we refrain from speculating what other observational data it may be compared to, as this is outside the scope of the paper.

5. *One technical question: What do you change in the model to disable channelization? Can you provide a distributed Rwt figure for distributed only run, like Fig. 3c? Do you use same threshold of Rwt of 0.95-1 for distributed only run for comparison with specularity content? It is not clear there what threshold is used (Figure 7).*

   The model is written with an option to disable channelization before initiating the run. Should this option be enacted, the model proceeds without defining channels between cells and Equations 4–6 are ignored.

   Comparison between specularity content and Rwt is consistent for channel-enabled and distributed-only runs, including the thresholds used for defining the Rwt and specularity masks. Following the reviewer's suggestion, we have altered Figure 7 to include maps of Rwt (panels a–c), as well as the corresponding Rwt masks (panels d–f). To encompass the full range of Rwt patterns in distributed-only runs, we chose model runs for this figure with three different k_q values and masks with three different Rwt thresholds. The values of k_q and Rwt used in each is provided in panels d–f. In general, Rwt in distributed-only runs resembled one of these three patterns, and none resembled the data-compatible Rwt pattern shown in Figure 3.

I do not have any further comments on the rest of the manuscript. The paper has all the things to get it published, but only after the above concerns are adequately addressed and incorporated in the manuscript.