Reviewer comments on “Persistent, Extensive Channelized Drainage Modeled Beneath Thwaites Glacier, West Antarctica”

General comments

This paper supports the existence of stable subglacial channels beneath Thwaites Glacier, and suggests that existing observations are incompatible with a distributed-only drainage system. The authors generate an ensemble of simulation results by sweeping through plausible parameter values, and then filter out results that are incompatible with both observed data as well as a number of physical constraints.

The configuration of the subglacial drainage system has consequences for drainage efficiency (and water pressures), submarine melting, and basal friction. Understanding each of these processes is vital if we are to understand the future evolution of Thwaites Glacier. This make the work presented in the paper particularly important.

A key part of this paper is the discussion of a methodology used to select data-compatible parameter values and subsequently drainage configurations. Generally, I think the method employed is sensible, but my general comments are on the consequences of some of the decisions made in this method.

How much of the observed behaviour is imposed (compared to emerging from the results) by the matching criteria? For example, criterion 1 (Line 212) compares zones, thus introducing a “special” line in between the lower and upper specular zones (zones 2 and 3) at the transition zone of Schroeder et al. 2013. Given that zone classification is discrete, it seems like any zone transition is likely to mark/impose a transition in the mode of drainage. Have some of the conclusions (such as the transition between drainage modes) been imposed based on the choice of selection criteria?

To give another example, if the specularity is a strong indicator of channelisation, then some of the observations in the selected runs will necessarily match with the specularity; namely, the extent of the channels. This is not a criticism of the criteria or methodology, but rather I think it would be good to distinguish the observations that can be directly inferred using the imposed data and criteria from those that emerge by incorporating the model. For the latter, I think insight about the nature of the channels (i.e. the number and size of channels) and the set of parameters values compatible with the observations demonstrate the benefit of using this methodology to interpret observations.

To finish, I wonder how a less-discrete compatibility criteria would compare to this method. For example, if you were to use the L2 error between the normalised S and R_{wt} fields. I imagine that this would resemble criteria 2, but would not require choosing critical thresholds. I am not suggesting the authors include this at all in their paper, I am just making a general comment.

In summary, the authors present a sensible methodology for making inference from some observed data (in conjunction with other physical constraints). As a consequence, they suggest that there may be significant channelisation beneath Thwaites Glacier. The existence of stable channels beneath Thwaites will have significant impact on the future of the glacier.

Overall I thought this paper was well written, and the conclusions well reasoned.

Specific comments

1) Given the importance of correlation as a measure of similarity I think it is important to say exactly how the correlation between masks of R_{wt} and S is calculated.

2) In 3.1.1 you state how many runs remained after eliminating unsteady runs that don't satisfy criteria 1 and 2. However, for the remainder of the paper you only use runs data compatible runs, which also have sufficient water pressure. Did this additional criteria eliminate any of the 20/14 steady state runs that satisfy the comparison criteria? If so, it would be interesting to state here how many of your runs in total were data compatible. If not, is this condition (water pressure) at all necessary to include?

3) Section 2.4 was a bit unclear. It wasn't until the start of 3.1.1 that I knew how the criteria were applied. I think the second half of 2.4 should be re-thought to clarify the methodology. Particularly because I think the methodology is key to this paper. I think it is important to highlight that for each simulation there is 66 specularity—R_{wt} combinations to compare and that if one of these combinations satisfy the criteria then the simulation is deemed realistic. I think Lines 226 and 227 say what needs to be said at the end of 2.4 (rather than line 215 which is too vague).

4) Did the specularity—R_{wt} combinations suggest any particular, consistent values of critical S or R_{wt} ? Presumable S^{crt} is an important parameter by which we can interpret specularity results?

Technical corrections

In equation 6: is $\mathbf{q_c}$ a scalar value? If so, it would be clearer if it was not in bold. And if so, how is it calculated? Discharge is a vector so it isn't clear what the "discharge in the distributed system within a distance l_c" means. Presumable there it involves some integration of a dot product taken with respect to a direction. (If it is a vector, the absolute value of a vector should probably be clarified to mean the L_2 norm of the vector.) As it stands, more information about $\mathbf{q_c}$ is required to understand equation (6).

Line 117 : surfface -> surface

Figure 2 is first referenced on line 206. The caption for Figure 2 refers to FSS (flux steady-state) before this abbreviation is introduced in the text. It is not until line 228 that that FSS is defined to mean flux steady-state. Maybe just say flux steady-state rather than FSS in the caption of figure 2?