The authors present a new subglacial hydrological dataset. The volume of data is notable. The authors do a solid job of curating complexities of the observations for the reader (Section 2, 3, and 4). Among a wealth of other observations, the authors show that out of the 311 boreholes drilled, 71% showed slow-flow behavior, 3% showed fast flow behavior, and 26% appeared to be hydraulically disconnected. This observation motivates the authors to modify the previously published model by Schoof (2010) in order to account for hydraulically isolated parts of the bed. The modified model introduces a percolation description whereby cavities form a connected system if and only if they exceed a critical dimension. I have listed my questions and comments in the attached outline.

Brad Lipovsky

1. **Additional questions about the observations/interpretation (Sections 2, 3, 4)**
    a. Given the complexity of the spatial patterning, would it be possible to make a movie that plots all the data? I envision the map in Figure 2 with each symbol having a color that is associated with a pressure scale. This should be feasible given the low sampling rate. There's only so much that can be conveyed with words.
    b. How long does drainage of the borehole take upon connection to the bed? This timescale is mentioned only qualitatively in the manuscript. Early work by Kamb and Englehardt used this timescale to estimate properties of subglacial conduits.
    c. Relative amplitude of pressure and temperature. Interquartile ranges (instead of standard deviations) may be more useful given the orders of magnitude variability.
    d. Is it possible to quantify how fast switching events or connection/disconnection occur? For example, on page 27 line 26: "very abruptly in time". What does that mean, exactly? Do transitions ever occur faster than the sampling resolution?
    e. What does the pressure sensor response curve look like with and without the snubbers? Do the snubbers limit the ability of the sensor to measure high-frequency water pressure oscillations?
2. **Questions about the model (Section 5).**
    a. A broader question regarding this type of modeling (i.e., also applicable to Schoof, 2010; Werder et al., 2013): Are conduit models

convergent under grid refinement? Werder et al. (2013) in their Appendix A discuss grid densification. As those authors pointed out, this creates complexities associated with changing the domain geometry. But what refinement is undertaken in such a way that more grid nodes are added only at the midpoints between existing grid nodes. Does the model converge under this narrower sense of grid refinement?

b. What are the smallest scales that must be resolved by the spatial discretization? Do these length scales have practical significance for glacier modeling?
c. Is the model stable to perturbations of all wavelengths? This question is motivated by the observed "very abrupt" pressure changes. Consider, for example, Equation 17 in the supplement to Schoof 2010. The term v_m depends on the effective pressure gradient, which suggests that large effective pressure gradients may change the sign of the term in parentheses, and therefore destabilize flow. Is this analysis correct? If so, at what wavelengths does destabilization occur? How are these related to the wavelengths in the previous point.
d. This line of questioning is based in part on my experience with subglacial hydrology modeling in the paper Lipovsky and Dunham (2015, JGR). In that paper we showed that there is no flow destabilization (at least not at glaciological flow velocities) in a sheet configuration without melting when elastic effects are taken into account (and with other assumptions).
e. Some small points: should the symbol S in Equation 1a be S_{R,ij}? Or is S another quantity? Same with Equation 1b. Also, S_{K0} is not defined in the text.

3. Connections between observation and model

a. I was disappointed by Section 5.2. Up to this point, I was carried along in the narrative of the paper: the reader learns about a dizzying array of new data, their broader interpretation, and then the formulation of a model improvement. But then I'm not sure what I'm supposed to learn from these simulations. Is the fit to data good? Does it capture some of the aspects of the field observations and not others? Given the ambitious scope of the paper, a much more extensive discussion of these topics is warranted.

b. I would strongly recommend the creation of a new "Section 5.3: Discussion of the Simulations". There were so many observations in Section 3 that I had a difficult time keeping track of all of them (see later comment). As written, there is no relationship drawn between Figures 16 and 17 and the main observational results/figures.
c. Near the last line of the paper it is stated, somewhat belatedly, that "However, the ability of the system to fully shut-down requires the incorporation of other physical process that could allow the reactivation of the drainage system during the spring event, something that is probably accomplished by over-pressurization." This should be included earlier, in a potential model discussion section.
d. Is the model capable of describing stage 1, 2, and 3 as defined in Section 4?
e. Does the observed spatial heterogeneity (Section 3) factor into the choice of smoothing length scale?
f. The bottom panels of Figure 17 would be better plotted in terms of water pressure (units equivalent water height) so that they can be easily compared to the rest of the figures in the paper…
g. ...Which of the various observed time series should the reader associate with the four panels Figure 17d-g?

4. Comments on the writing.

a. There are so many important points in Section 3 that I had a difficult time sorting through all of them. I suggest adding a writing device to emphasize the most important ones. This is partially a stylistic choice. One option would be to enumerate the points at the start. Another option would be to align subsection headings with main points.
b. The manuscript, especially Section 3 and 4, would be improved by revision for brevity. There is a lot of repetition, particularly in Section 3. The authors mention at least four times, for example, that clustering is subjective.