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Discussions

Interactive comment on "Subglacial drainage characterization from eight years of continuous borehole data on a small glacier in the Yukon Territory, Canada" *by* Camilo Rada and Christian Schoof

Anonymous Referee #1

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The manuscript "Subglacial drainage characterization from eight years of continuous borehole data on a small glacier in the Yukon Territory, Canada" by Rada and Schoof, describes and interprets an eight year dataset of observations from dozens of boreholes on a small mountain glacier in the Yukon. The dataset is extremely rich and shows complexity at diurnal and seasonal temporal scales at both local and regional spatial scales. The boreholes show support for the standard model of subglacial hydrology of an inefficient distributed system punctuated by an efficient channelizing system of limited spatial extent. However, the data also provide strong evidence for discon-

nected regions of the bed that change in time. The authors present a novel extension to an existing subglacial drainage model that adds a physical representation of the evolution of these disconnected areas. The model is capable of qualitatively reproducing both the traditional distributed and channelized regions as well as the complexity observed in the disconnected regions of the bed.

This study adds South Glacier as a new benchmark location in the pantheon of subglacial hydrology borehole studies by providing an exhaustive description of the long record there beyond the description in Schoof et al. (2014). The study makes a convincing case for many of the "anomalous" features of borehole studies (rapid switches in behavior, out of phase boreholes, water pressure above overburden, etc.) by demonstrating features that are widespread both spatially and temporally and cannot be written off as a fluke of limited sampling. The modeling component of this paper is equally significant. It builds on a recent renewed interest in the disconnected or weakly connected parts of the bed by presenting the first model that allows these regions to evolve on their own. The model results are striking in their ability to reproduce diverse behavior over short length scales that qualitatively matches the complex borehole record. This significant achievement may pave the way for advanced subglacial drainage models that are finally able to reproduce the diversity of behavior seen in the field, as well as simulate the annual cycle.

While the paper has many strong points, that also leads to its limitations - it is a long, dense paper and some of the key points get lost a bit among the thorough observational descriptions. In particular, the important modeling section reads like a bit of an afterthought, which is unfortunate. At times it reads like a chapter from a dissertation, and it might more naturally form two shorter papers. The interpretation of the model results and comparison to existing literature feels a bit incomplete, as explained below.

General Comments ————

1. My primary concern with the paper is the somewhat abbreviated model interpreta-

tion and incomplete comparison with previous work. What is there is very interesting, but the rich model record could be compared a bit more thoroughly to the borehole record (here and elsewhere). There is also no consideration for which model parameters this enhanced model might be sensitive to (though I think a detailed sensitivity study is well beyond the scope of this paper). I did appreciate the model technical detail discussion in the SI, and it would be nice to see those topics mentioned in the main text to encourage readers to look into the SI. My biggest concern in this area is what feels like an incomplete comparison to previous work. The authors acknowledge previous investigations of the disconnected (or weakly connected) system, and their model is an elegant extension of previous, simpler attempts to model it (Hoffman et al., 2016). However, the interpretation in this paper is that the subglacial drainage system becomes increasingly fragmented and disconnected as the summer progresses, while previous studies suggest summer brings *increased* connectivity (Gordon et al., 1998; Hoffman et al., 2016; Iken and Truffer, 1997; Murray and Clarke, 1995). This difference in interpretation should be discussed, and, if possible, reconciled.

2. It would be nice to see a bit more acknowledgement of the possibility of borehole behavior being governed by the presence of subglacial till. This is discussed briefly in a few places, but the paper would benefit from additional consideration of it, or a stronger justification for a dominantly hard bed interpretation. For example, on p. 27 there is discussion about localized diffusive systems with limited flow, and it seems like till would fit the bill.

3. p18, lines 2-3: How much does snow cover or ice albedo change? It is a questionable assumption that the degree day factor remain does not change significantly as surface conditions change over the summer. The interpretation in Figure 11c seems rather tenuous.

4. The title is fine as it is, but it is worth considering the title somehow including something about the importance of the disconnected/weakly connected system in the interpretation, as this is a primary result.

5. As mentioned above, the borehole results section is quite long.

Specific Comments ————

Abstract seems a little short given the length of the paper, but it does hit the most significant highlights of the paper.

1, 22: basal "slip" may be considered the preferred term here (Cuffey and Paterson, 2010), to acknowledge the ice is not sliding differentially from the substrate at its sole.

2, 6: No comma here.

2, 6-11: Interactions between subglacial hydrology and ice motion could be mentioned here as well (e.g., Hoffman and Price, 2014). And Gordon.

2, 29: missing "a" -> "to provide a less efficient"

2, 33: "do" should be removed.

2, 32-34: (Creyts and Schoof, 2009) could be an appropriate additional reference for this topic.

3, 5: I believe you meant for the second "channels" on this line to be "conduits".

3, 9-15: This is a nice summary of the complexity in borehole observations. A few suggestions for additional references: "widespread areas of high water pressure during winter": (Ryser et al., 2014; Wright et al., 2016) "large pressure gradients": (Fudge et al., 2008) "sudden reorganizations": (Gordon et al., 1998) "anti-correlated temporal pressure variations": (Andrews et al., 2014; Ryser et al., 2014)

3, 31: "in-deep" -> "in-depth"

5, 33: Could mention that many authors refer to this as "hydraulic head".

Figure 2: I recognize that showing so many different symbols and colors is challenging, but it is difficult to differentiate some of them. Perhaps removing the 3d shading on the symbols would help. In particular, the red symbols are hard to make out. Maybe put a

circle around them or something to make them easier to see. Also, the black and blue lines are difficult to tell apart. Finally, the concept of "upstream area" from Schoof et al., (2014) should be briefly elaborated on (either in the caption or the text).

Figure 3: The figure is a bit small in my printout. In particular, the green dots are hard to see.

7, 8: Consider changing "Fast-Flow" to "Fast Water Flow". When I first read this I interpreted this to be a region where ice velocity is fast.

8, 31: bummer

Figure 4: The colors are a bit difficult to match to the map. Again, perhaps removing the 3d shading of the symbols on the map would help.

Figure 4 caption: in part c), it says two sensors were installed here, but I only see one line in the plot. Clarify if they are plotted on top of each other or if only one is plotted and, if so, which and why.

Figure 5: Is correlation to temperature calculated for panel c here as it was for Figure 4? If not, mention that in the caption.

11, 18: Should "ice" be "water" here?

13, lines 1, 2, 3, 5, 7, 9: Include figure number with each panel reference.

Figure 8: Caption for g) refers to six digital sensors included in panel b, but panel b is the temperature record.

15, 20: The second comma should be removed.

Figure 10: Panel c is pretty hard to make out details of. Perhaps this figure could be reorganized into two columns, or panel c could somehow be made a bit larger.

17, 3: Fig. 8b must be an incorrect reference - do you mean Fig. 10c?

Figure 13: Consider putting earlier on, perhaps with Figure 3.

Section 3.6: The data quality section would be more natural in section 2 (methods), than late in the results section.

22, 10: I think you mean "120% of overburden", not "above".

22, 11: Consider replacing "and" with "however" or "yet" to make it clear you are arguing against sensor drift being able to explain these observations.

23, 12-14: This text would flow better with this sentence in parentheses.

25, 5: (Hubbard et al., 1995) could be an additional appropriate reference here.

25, 14: An aside: water pressure in nearby moulins/crevasses would be useful here. Something to consider if this field campaign continues.

25, 21: Some discussion of bridging stresses leading to isolation of low pressure channels would be good here (Hewitt, 2011; Lappegard et al., 2006).

25, 23: Also, Figure 3.

25, 29: Mention that high up-stream areas means a likely water flow accumulation path (see comment above about introducing the significance of this upstream area).

26, 1-7: This discussion would benefit from inclusion of (Meierbachtol et al., 2016).

26, 28: "fragment into subsystem" -> "fragmented into subsystems"

27, 3-7: Interesting discussion. I think the quotes around "phase lag" should be removed.

27, 21: Is the distance long enough relative to channel flow speed for a phase lag to be expected? The other complication is there could be additional inputs of water from the surface that help to "lock" the channel phase to the surface phase even in the presence of diffusion within the subglacial system.

27, 27-29: Consider (Meierbachtol et al., 2016) again here.

27, 32: There is an alternative hypothesis as well of passive cavity opening due uniform basal sliding (Bartholomaus et al., 2011; Hoffman and Price, 2014; Iken and Truffer, 1997).

28, 8: These island sound like the system described by (Murray and Clarke, 1995).

28, 19: Wouldn't disconnected areas act as *slippery* spots since they maintain high water pressure?

28, 22-26: This is a significant result and well-stated here.

30, 3: "differential motion between ice and till": If basal slip is primarily due to till deformation, then there will not be differential motion between ice and till.

31, 11: Please define n_c.

31, 11: Is there a significance to the designation 'K' or is it just an arbitrary letter choice?

31, 11: Consider adding "along that edge" after "n_c-1 'K' conduits" to emphasize that this treatment is per edge.

Eq 1: It seems odd to use lettered sub-equations rather than a new number for each equation.

31, 21: Also define Psi here.

Eq. 1d/e: A minor quibble: It would seem more intuitive if the threshold size also contributed to flow once the threshold is reached (which is not the case in 1d/1e). However I doubt the choice of how to treat that affects the results in a qualitative way, so either approach is defensible.

Eqn. 1f/1g: Mention this is describing mass conservation to aid the reader. Also, this is a single equation so there should be a single label.

32, 17: This is a run-on sentence. How about ending it at "nodes" and starting a new

sentence with "We".

33, 7: Is (Dow et al., 2015) meant here?

5.2 It would be clearer to call this section "Model Results".

36, 1: "eventually" should be "eventual".

37, 11: The word "a" should be removed.

Data availability: What about model and model configuration and output? Mention it is included in the SI.

Supplemental Material ——————————

* paper_movie.mpg does not play for me.

* It would be more natural to switch the order of sections 1 and 2 to match the order these topics were presented in the main text.

* The SI material, particularly the modeling part, has some very useful information. I would like to see the main text refer to the SI in more places, with brief descriptions of what is found there.

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