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Interactive comment on "Oscillatory subglacial drainage in the absence of surface melt" *by* C. Schoof et al.

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Below I post comments received just after the discussion period deadline by Reviewer 3. I ask the authors to consider these constructive comments in their revision. -IMH

—General Comments—

The submitted paper "Oscillatory subglacial drainage in the absence of surface melt" by Schoof, et al. presents borehole water pressure observations from a valley glacier in the Yukon, highlighting the existence of multi-day oscillations after the termination of surface melt inputs. The authors apply a one-dimensional subglacial hydrology model that can describe both cavity-like and channel-like flow to an idealized version of the study glacier, and conclude that these oscillations are driven by interactions between

the subglacial drainage system and subglacial or englacial storage. This mechanism of distributed storage is compared to the well-described Jokulhaup instability based on localized storage. Additionally, the distributed storage mechanism is offered as a plausible explanation for observations of a two day period in water pressure superimposed on the expected diurnal signal.

The paper is very well written and nicely combines observations and modeling. The observational and modeling components of the paper are largely separate but are complementary. The observational section is perhaps a bit lengthy with unneeded detail beyond what is required to set up the modeling application, but given this is the first paper describing this borehole drilling campaign on this glacier, perhaps the authors understandably want to firmly set the context for this, and possibly future, papers. The interpretation of observations put forth seems plausible, and the authors clearly make an effort to justify their interpretation. I have described below one place where perhaps more care could be taken. The modeling section uses a variation on an existing, well-documented model and is thorough in describing the model application. It is not entirely clear how comparable the very idealized experiments are to the actual glacier, but given the general uncertainty in knowing subglacial conditions, that does not particularly bother me; the modeling puts forth a plausible and parsimonious explanation for the observed oscillations. Overall, my concerns are all relatively minor, and I look forward to the final version of this paper.

—Primary Specific Comments—

On p. 5620-5621 the interpreted four phases of the borehole records are described. This approach is useful to the reader in simplifying a complex dataset. However, the approach may also oversimplify a complex dataset. I am particularly interested in the period July 25–July 30 when the diurnal signals in boreholes A1-A3 are absent. As the defining feature of Phase 1 is “strong diurnal pressure cycles”, would it not be more appropriate to consider this time period as Phase 2 (“disappearance of dominant diurnal signal, and a gradual drop in water pressure”)? I am not necessarily suggesting

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that the authors adjust their categorization, but at least this inconsistency should be addressed. (The discussion in the last paragraph of p.5622 may be relevant here?)

—Other Specific Science Comments and Technical Corrections—

p5615/9: "The presence of strong diurnal cycles in water input . . . and possibly playing a key role in ice flow speed-up (Schoof, 2010; Hewitt, 2013)." What role is this?

p.5615/21: A reference to the recent paper by (Tedstone et al., 2013) would also be appropriate here. (Though I am not entirely convinced that Greenland hydrology is relevant to the introduction for this paper.)

p.5616/23: "Aereal" should be "Aerial"

p.5617/21: "was" should be "were"

p.5619/12-16: I believe it would be more accurate as this paragraph is written to say that f=0 corresponds to zero water pressure everywhere, and f=1 corresponds to zero effective pressure everywhere (or water pressure at the floatation pressure). In terms of the routing algorithm described (that uses hydraulic potential *gradients*), the current explanation of the meaning of f=0 and f=1 is accurate, but in reference to actual hydraulic potential as calculated by Eq. 1, the current explanation is somewhat confusing.

p.5619/23: "150" should (presumably) be "150 m".

p.5624/4: Clarifying "peak temperatures" as "peak air temperatures" would be useful here.

p.5624/15: "errors due damage" should be "errors due to damage"

p.5626/1: "crevasses" should be "crevasses".

p.5626/19-21: This sentence is worded to sound like water pressures cannot begin rising until the conduits have re-equilibrated to a smaller size. However, the water

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pressure presumably will rise as the conduits are re-equilibrating.

p.5627/5-10: The effort gone to confirm this is a true signal is appreciated.

p.5628/20: “channel-lie” should be “channel-like”.

p.5633/11-14: This sentence should state that these limiting values are for the range of storage used. Also, it would help the reader while discussing these values to refer to Figure 6, and specifically the transition from circle to rhombus shapes and the transition from empty to filled markers.

p.5633/17: It would be good to start a new paragraph here; the remainder of the existing paragraph describes a new model formulation.

p.5634/10: Is this result (that localized storage only leads to instability for channel-like conduits) universal? The text in the parentheses sounds like this figure is the proof, but that certainly does not exclude the possibility that a different result would arise with different parameter values.

p.5634/22-25: Can the period length also be longer than two days? If so, is there a reason why two days is special? (Perhaps the more detailed description at p.5635/10-11 could come here.)

p.5635/1: According to Figure 6, the onset of oscillations occurs only below a critical water supply rate but **above** some other limiting supply rate. I think that distinction is worth making here – if the supply rate is reduced enough, these oscillations will not occur.

p.5637/2: No comma needed after ‘both’.

Fig 1 / 2: It would help in comparing these two figures if they both used the same coordinate system (either lat/long or UTM).

Fig. 2: “is likely straddle” should be “is likely to straddle” or “likely straddles” Fig. 3: “(a) Temperature time series shown (black line) and specific surface mass balance”. It

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seems the word “shown” should be removed from this statement.

Fig 3: “(g) And (h) have different **vertical** axis scalings from (b–f)”

Fig 3: Last sentence of caption: “corrspond” should be “correspond”

Fig. 3: It might eliminate potential confusion if the caption also mentions that the light gray background shading differentiates the various phases.

Fig. 4: Highlighting the interpreted four phases with labels and light gray shading (as in Fig. 3) would be helpful here.

Fig. 6: It would be very helpful to see the locations of the initial conditions used for the examples shown in Figures 7 and 8 marked on this figure.

—References Cited— Tedstone, A. J., Nienow, P. W., Sole, A. J., Mair, D. W. F., Cowton, T. R., Bartholomew, I. D., & King, M. a. (2013). Greenland ice sheet motion insensitive to exceptional meltwater forcing. Proceedings of the National Academy of Sciences of the United States of America, 110(49), 19719–24. doi:10.1073/pnas.1315843110

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