The Cryosphere

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Discussions

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

C. Schoof et al.

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We would like to express our thanks for the constructive reviews we received. Individual responses are given below.

1 Referee # 1

- *I would like to see a discussion of the boreholes' storage capacity and influence on the observed variations. Is the storage capacity of the borehole large enough to influence the behavior of the subglacial hydrology system? This seems less likely during phase 1 when there is surface melt, but it could be critical during*

phase 4 when there is no melt. The slow rises and abrupt falls in water level for boreholes A2 and A3 is similar to observations on both South Cascade and Bench glaciers but no good mechanisms has been proposed for this type of behavior. A2 has more of these fluctuations than A3 although the two boreholes sometimes show synchronous drops despite different magnitudes. I wonder if the proximate cause of the drops is related to changes in an active channel/cavity system or if the drops are caused by isolated boreholes both affected by ice motion or some other non-direct hydrological forcing. Over short time scales with poorly connected holes, borehole volume *might* be a significant factor, but bear in mind that at least some of the later oscillations take much longer than a single day (by contrast for instance with the sub-daily oscilations apparent in the Bench Glacier record, e.g. figure 2 of Fudge et al (2005)). Also, while we cannot be certain about the extent to which our boreholes freeze shut, experience with redrilling recent boreholes even within a few days leads us to believe that there is significant freeze-up, and therefore probably not much water storage. We have added the following new paragraph at the end of the modelling section (which, for the sake of clarity, we have turned into a 'Discussion' section):

'The model assumes that the necessary water storage consists primarily of cracks or other storage bodies that can fill and drain easily with water, and in which filling level and therefore storage is an increasing function of water pressure at the bed. It is possible but not very likely that the boreholes themselves constitute storage volume, in which case the act of observation could interfere with the observed system. Two reasons make this unlikely. The first is that the boreholes freeze shut relatively quickly (in at most two to three days) and a few instances in which we have re-drilled boreholes shortly after they have closed off suggests that freezing occurs along most of their lengths. Secondly, even if they remain open, the boreholes have a cross-section of about 0.01 m^2, corresponding to $V_p = 1.6 \times 10^{-3}$ $m^3 Pa^{-1}$ in the model for lake-like drainage above, three orders of magnitude smaller than the storage volumes we have used in our

simulations. With this limited storage capacity, a variation in hydraulic head of 10 m typical of the later pressure oscillations in holes A2–A3 would correspond to a water volume of 0.1 m^3 stored in or discharged from each borehole over an approximately 12-day period. This contrasts with the approximately 200 m^3 we expect to be produced by friction and geothermal heating over the upstream area for the borehole line A1–A6.'

We are aware of slow rises and abrupt falls in the published data from Bench Glacier (for instance figure 2 in the Fudge et al (2005) reference; however, these seem to refer to much shorter (sub-daily) oscillations, which we also see at South Glacier in unconnected boreholes. If we've missed a reference to these osicllations occuring in winter (also on South Cascade Glacier), we would be keen to include them — please point us to the relevant papers!

- *I am also curious if the two-day periodicity during Phase 1 (diurnal fluctuations) was observed in any other boreholes in other years. Of the 60 other boreholes drilled and instrumented, did this occur at any other sites or is it a relatively unique occurrence?*
  We did not find any unambiguous cases of doublets elsewhere. The updated test states in the penultimate paragraph of the 'Data' section that 'we have no clear evidence for two-day "doublets" elsewhere.' For a longer discussion of the larger data set, please see the responses to reviewer # 2 below (which were written earlier than this response, for no particular reason other than that I opened that review first).

- *The question of the borehole storage capacity also affects the modeling. I had difficulty understanding the magnitude of the water input and storage terms in the model. I think a description of how the magnitude of the modeling inputs compare to expectations for South Glacier would be helpful. This may rule out the borehole as a storage component able to influence the subglacial hydrology system.*

In addition to the text about storage capacity above, we have included the following paragraph immediately preceeding the discussion of storage capacity:
"Our simulations have been based on a particular set of parameter choices that are motivated by the geometry of our field site. Ultimately, many of the parameters in the model are not well constrained, and our model flowline model is highly idealised. It would be unwise to attach much importance to the precise range of unstable water supplies in Fig. 6 as our model results are parameter-dependent and Fig. 6 only explores changes in a a single parameter, namely the storage capacity v_p. If we do take the unstable water supply rates at face value, we find values in panel a of figure 6 in the range 200–30000 m^{3}day^{-1}. The upper end of that range is very unlikely to be realized under wintertime conditions, while the lower end is plausible for wintertime discharge if there is groundwater discharge under the glacier. The borehole line A1–A6 has a total upstream area of about 0.5 km^3 for $f = 0$ and $f = 0.5$ in figure 2 if we include only glacier-covered areas, and about twice that if ice-free valley slopes are included. Assuming a geothermal heat flux of 0.04 Wm^{-2} and with measured ice velocities around 10 ma^{-1} (De Paoli and Flowers, 2009, Flowers et al, in press) and driving stresses around 10^5 Pa, we can estimate basal melt rates of around 2×10^{-5} mday^{-1}, giving an integrated water supply of 10^3 m^{3}day^{-1}, below the unstable range. To explain higher supply rates in excess of 200 m^{3}day^{-1} requires ongoing groundwater draiange, with area-averaged discharge rates (and therefore long-term recharge rates) around 2×10^{-4} mday^{-1}, equivalent to 7 cm per year. This amounts to about 10 % of the glacier-wide accumulation rate estimated in Wheler et al (in press).'

- *I also did not understand the justification for the lower boundary condition in the model- ing. N (effective pressure) is set to 0 at the glacier snout. It seems like a more common boundary condition is that the water pressure is 0 (atmospheric pressure) which comes from the observation that most streams exiting a glacier*

have carved a tunnel that is not completely filled with water. This seems particularly likely at the end of the melt season. Please describe why N=0 at the snout is the appropriate boundary condition or discuss the impact of the choice of lower boundary condition.

$N = 0$ in fact corresponds to atmospheric water pressure if there is no overburden (i.e. the glacier thickness goes to zero). Trying to model partially-filled conduits as suggested *can* be done (see the cited Schoof et al (2012) and Hewitt et al (2012) papers) but makes computations slow and is not the primary objective of our work here, so we have not tried. We have added the following sentence (with 'latter' referring to the $N = 0$ boundary condition): 'The latter corresponds to having both, zero water pressure and zero overburden at the snout. For computational tractability and in keeping with many other similar drainage models (e.g. Werder et al, 2013), we do not impose the upper and lower bounds on water pressure considered in Schoof et al (2012) and Hewitt et al (2012)'

- *I also wonder how applicable this analysis is to the much thicker ice of Greenland and Antarctic outlet glaciers? With faster creep closure rates, does the system work the same except with larger water flux values? Or does the faster creep prevent either channels or cavities from being stable?*
  This is an interesting question, but one which we would like to leave for future work. In terms of modelling, the primary changes would be much longer flow paths with lower hydraulic gradients, and possibly quite different plausible water supply rates.

Specific comments:

1. *Subsections would be helpful in the interpretation and modeling section*
   We have introduced several subsection headings

2. *P5614, L6, "These" is ambiguous*

   Changed 'These' to 'our data'

3. *P5615, L20, delete"also"*
   Done

4. *P5619, L22& 23, do you mean "further" or "farther"* Apparently we mean "further" as in "additional" as opposed to "more distant". That said, in British English (which we checked in the Oxford English Dictionary online, so it must be true), "further" is used to mean both "additional" and "more distant", and "farther" is less commonly used.

5. *P5620,L5, is anything lost by plotting at 3 hour intervals? Seems like it might clip the falls in water during period 4*
   Very little information winds up being lost; a plot that retains higher resolution looks slightly less smooth but adds no new features.

6. *P5620,L15, Have you thought about plotting in height above some datum instead? You indicate that there are consistent offsets between boreholes. This could be sensor calibration, but it could also be differences in the bedrock elevation (borehole height) that if corrected might make the boreholes match.*
   As described in the text, we do not have inclinometry data for the boreholes so we actually do not know the base elevation. As a result, we have not attempted to plot relative to a datum.

7. *. P5621,L21-23,"Initially, all three." sentence is hard to follow.*
   Deleted "ongoing but"

8. *P5624,L18, this sentence makes is sound like you could re-calibrate but didn't, but really its that you can't get the transducer back*
   Changed to '. . . that generally cannot be determined after the fact.'

9. *P5625,L28, what effect does a new snow layer have in terms of reflecting solar energy and buffering surface melt water?*
The meteorological data at the site does not indicate that the first instance of extended freezing at night corresponds to the formation of a persistent snow cover; in fact, net ablation occurs throughout Auguust. The installation of a time-lapse camera system in 2012 confirms this pattern for the summer season subsequent to that in which the data presented here.

10. *P5628,L20,"channel-like"*
Corrected.

11. *P5628,L25, delete"also"*
Done.

12. *Figure 3: No need to subscript borehole names, A1 Figure 6: superscript values on x-axis; are the lower mL values realistic? Figure 9: superscript values on x-axis*
Figures corrected; see more detailed response above (dealing with the effect of borehole storage) for 'realistic' discharge data and the need to be cautious when taking the range of water supplies leading to instability at face value.

2 Referee # 2

- *P5617, L14-17. From the world-view imagery (or field observations), are there any moulins draining in to the glacier upstream of the study area? This is worth adding/discussing given your summary of the surface hydrology? It is also relevant to your subsequent discussion – you say (p5625, l29) that "Due to the absence of moulins in or above the study area" but are you sure there are no moulins above the study area that could be driving your diurnal cycles rather*

than from drainage through crevasses and narrow cracks? After all, the glacier is polythermal (and presumably pretty cold in the upper layers given Nov, Dec, Jan temps) so water will likely find it hard to get to the bed through narrow cracks as it will refreeze.
The third paragraph of the 'Field site and methods section' says:
'Between 2008 and 2011, 76 boreholes were drilled to the glacier bed in the upper ablation area, as shown in Fig. 1. At present, ice velocities in the study area measured in situ using Global Positioning System receivers are around 10–30 $\mathrm{m\,yr^{-1}}$, with ice thicknesses ranging from about 45 to 100 m. *There are numerous crevasse fields both inside and upstream of the study area, but no moulins.* Two major surface streams originate within the study area, which has significant surface topography, but none enter it from above.'
We hope that covers the question of moulins. On-the-ground observations, the satellite imagere as well as time lapse photography from the ridges surrounding the glacier have shown no evidence of moulins either inside or upstream of the study area. There are many crevasses both inside and upstream of the study area, severylu limiting the catchment any surface stream could have. This probably accounts for the lack of moulins, as melt drains locally into crevasses. There are several moulins closer to the glacier snout, downstream of the study area.

- *P5627, l4 – this ongoing flow could be groundwater but why also do you not mention melt generated from geothermal heat and frictional heat (basal sliding) which will pre- sumably provide a pretty constant flux throughout the year as it does at other glaciers? This issue crops up in other places (e.g. p5627, l26; p5636, l13-14) and needs clarify- ing.*
We have rephrased this line as follows:
'This input could take the form of ground water, slow englacial drainage through the temperate ice that exists in the upper glacier (**?**), or melt generated by geothermal heat or frictional dissipation'

and later, P5627 l25,

'This may be fed by ongoing, low-level but relatively steady water supply to the glacier bed from englacial sources, geothermal or frictional heating or, possibly more likely, from ground water flow after the end of the melt season.'

It is worth noting however that frictional dissipation and geothermal heating may generate only limited amounts of water, which is why we did not include them as possiblities originally. A back-of-the-envelope calculation with a geothermal heat flux of 0.05 W m^{-2}, density of 1000 kg m^{-3} for water and latent heat of fusion of 3.35×10^5 J kg^{-1} gives melt rates of 10^{-5} m day^{-1}, and a volume of 50 m^3 per day for the whole glacier, neglecting any upward conduction into the ice. For a driving stress of 10^5 Pa and measured wintertime velocities around 10 metres per year, the dissipation rate is about 0.04 W m^{-2}, comparable to geothermal heat flux, and therefore generating similar volumes of water.

- *P5628, l14 – with reference to water stored in "other smaller water pockets", why not mention subglacial cavities as an obvious possibility (linking to Kamb's theoretical work or the mapping of exposed proglacial bedrock that reveals large cavities (e.g. work of Hallet and others at Blackfoot Glacier or Sharp and others at Tsanfleuron); especially as you then go on to mention linked-cavity drainage systems on l19 of the same page.*

The reason why we do not mention subglacial cavities here is that storage in these cavities presumably evolves on the same time scale as the size of the cavity. This is unlike the proposed storage of water in cracks and crevasses: for the latter, a change in water (or effective) pressure at the bed will very quickly translate into a change in filling level in the crack or moulin and therefore into a change in stored water. For a subglacial cavity, a change in water (or effective) pressure will have no instantaneous effect on water storage. Instead, it will change the creep closure rate of the cavity, which will over time permit a change in water storage.

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Both modes of water storage have been considered previously in the context of drainage models of the type considered here in e.g. Schoof (2010, cited in the paper) and Werder et al (2013, *Journal of Geophysical Research*, doi:10.1002/jgrf.20146). Because of the slow response time of cavity evolution relative to filling or draining pre-existing cracks, storage in cavities turns out to have a minimal effect when compared to storage in cracks; for instance, storage in cavities is insufficient to dampen the effect of diurnal water input variations into channels with distance away from the channel as discussed briefly in section 3.4 of Werder et al., and the effect of water input variations would be felt instantaneously essentially across the entire connected draiange system. One of us (CS) is currently preparing a separate manuscript with Ian Hewitt on this issue; for the present paper, we believe that a detailed exploration of the differences between storage modes is probably too technical to be helpful.

- *P5633, l11-13 Given your estimates of mL, it would be interesting to know what up- stream contributing area would be required to deliver these sorts of volumes of water purely from geothermal and frictional melt (in other words – do these volumes make sense for the upstream catchment area that you have derived from your Shreve style analyses (Fig. 2)?*

The meaning of the sentence may have been a little misleading, and we have rephrased it as

'Based on the above definition, we find that the change from cavity- to channel-like in our conduit occurs between $mL = 10^3$ m^3 day^{-1} and $mL = 2 \times 10^3$ m^3 day^{-1} for the parameter values in Table 1, while oscillatory behaviour persists down to $mL = 250$ m^3 day^{-1}'

The point is that the critical values for changes in behaviour depend on the parameter choices we make in the model, and these are not well constrained. We have chosen parameters for which unstable drainage is possible with net water supply rates as low as $mL = 10^2$ m^3 day^{-1}. For an upstream area of 0.5 km^3 would

C3388

correspond to a basal melt rate $\times 2 \times 10^{-4}$ mday^{-1} — greater than what we have estimated for geothermal and frictional heating by between an order of magnitude but potentially feasible for groundwater flow especially if the wider catchment outside of the glacier (which is ignored in the Shreve-style calculation) is included, which increases the upstream area to around 1 km^2; a recharge/discharge rate of 0.2 mmday^{-1} for an groundwater aquifer averaged over a year is probably not unrealistic, corresponding to 7 cm per day. That said, a different set of transitions from cavity to channel can be obtained by changing the cavity opening parameter v_o, and not too much should be read into specific numerical values stated in the paper.

We have included the following paragraph reflecting the discussion above:
'Our simulations have been based on a particular set of parameter choices that are motivated by the geometry of our field site. Ultimately, many of the parameters in the model are not well constrained, and our model flowline model is highly idealised. It would be unwise to attach much importance to the precise range of unstable water supplies in Fig. 6 as our model results are parameter-dependent and Fig. 6 only explores changes in a a single parameter, namely the storage capacity v_{p}. If we do take the unstable water supply rates at face value, we find values in panel a of figure 6 in the range 200–30000 m^{3}day^{-1}. The upper end of that range is very unlikely to be realized under wintertime conditions, while the lower end is plausible for wintertime discharge if there is groundwater discharge under the glacier. The borehole line A1–A6 has a total upstream area of about 0.5 km^3 for $f = 0$ and $f = 0.5$ in figure 2 if we include only glacier-covered areas, and about twice that if ice-free valley slopes are included. Assuming a geothermal heat flux of 0.04 Wm^{-2} and with measured ice velocities around 10 ma^{-1} (De Paoli and Flowers, 2009, Flowers et al, in press) and driving stresses around 10^5 Pa, we can estimate basal melt rates of around 2×10^{-5} mday^{-1}, giving an integrated water supply of 10^3 m^{3}day^{-1}, below the unstable range. To explain higher supply rates in excess of 200 m^{3}day^{-1} requires ongoing groundwater

C3389

draiange, with area-averaged discharge rates (and therefore long-term recharge rates) around 2×10^{-4} mday^{-1}, equivalent to 7 cm per year. This amounts to about 10 % of the glacier-wide accumulation rate estimated in Wheler et al (in press)'

- *The borehole water pressure results would seem to suggest that the subglacial channel is closer to boreholes A1-3 than A4-5 while the Shreve reconstructions put them closer to áĹijA5. Does varying f ever move the predicted channel west to align more closely with boreholes A1-3?*
  The 'channel' location remains pretty much where it is regardless of changes in f, but it shouldn't be surprising that the predicted location differs at least slightly from that suggested by observation. There are multiple reasons why the application of Shreve's analysis should only be seen as providing a guide to like drainage routings rather than having accurate predictive power, and it is probably not worth pursuing it to try to predict water routing to within the nearest 50 metres.

The first reason is that the actual hydraulic potential will not take the form given by equation (1) in the paper with a spatially uniform factor f. More generally (assuming hydrostatic normal stress), Φ would be of the form

$$\Phi = \rho_w g b + \rho_i g(s - b) - N$$

where the effective pressure N must be found from a dynamic drainage model (a plausible example being that in Werder et al (2013). N is in general a function of position and time, so differences in water routing from that predicted by Shreve should be expected, especially at small spatial scales.

The second reason why Shreve's analysis may not give the right routing in practice relates to the computational methods involved. Effectively, Shreve's analysis gives a hyperbolic partial differential equation. Specific upstream area h at a point (x, y) is the upstream area δA draining into an infinitesimal line segment δs_0 that

C3390

is centered on (x, y) and perpendicular to the flow path (i.e. parallel to the local contour of Φ); this definition at least is independent of pixel size and orientation.

The flow direction is given by the unit vector $\hat{\mathbf{v}} = -\nabla\Phi/|\nabla\Phi|$ in the direction of the negative hydraulic potential gradient. h then satisfies the 'conservation law'

$$\nabla \cdot (h\hat{\mathbf{v}}) = 1 \tag{1}$$

Essentially, h is the magintude of water flux that would result from a spatial uniform water supply rate (given by unity) and a prescribed direction $\hat{\mathbf{v}}$ for the flow.

This being a hyperbolic equation, even localized errors in input data will propoagate infintely far downstream from the location where the error occurs. The input data here consists of the hydraulic gradient $-\nabla\Phi$, derived from DEMs for glacier surface and base. The glacier base is however reconstructed from discrete radar survey lines (see the Wilson 2012 reference) and has been interpolated between those, introducing errors that are likely to have a significant effect on the water routing even if Shreve's model did hold exactly. These errors are likely to be amplified when differentiating bed elevations to obtain the water routing direction $\hat{\mathbf{v}}$

The third reason is that even if Shreve's model was exact *and* bed elevation was known exactly, accurate hyperbolic solvers are non-trivial to construct and it is not clear how accurately water routing algorithms really 'solve' equation 1. The Tarboton (1997) D_∞ method is widely used but, as described in the paper, is not directly based on a solver for the partial differential equation equation 1 but formulated explicitly for a pixel-based DEM. It is not immediately clear to what extent this introduces errors; figure 4F in Tarboton's paper certainly indicates a slight bias intrudoced by grid orientation.

We have included a brief derivation of equation 1 for completenes; skip this paragraph if you like. Specific upstream area then satisfies a 'conservation law'. Take a closed loop C on the bed somewhere, encircling some area A. Assume C has

C3391

two halves, C_{up} where streamlines of the hydraulic gradient enter A and C_{down} where they leave. The difference between the upstream areas for C_{up} and C_{down} must be the encircled area A. To compute the upstream area for C_{up} and C_{down} from the specific upstream area h, split these curves into line elements δs. These are typically oblique to $\hat{\mathbf{v}}$, so the upstream area for each δs is given by projecting it onto the direction normal to flow. If $\hat{\mathbf{n}}$ is outward-pointing to A, the we have

$$\delta a = h\delta s \cos(\theta) = \begin{cases} -h\delta s\hat{\mathbf{n}} \cdot \hat{\mathbf{v}} & \text{on } C_{up} \\ h\delta s\hat{\mathbf{n}} \cdot \hat{\mathbf{v}} & \text{on } C_{down} \end{cases}$$

where $\cos(\theta)$ is the projection angle, and the different signs for C_{up} and C_{down} reflect that the outward-pointing unit normal points upstream (so in the opposite direction to $\hat{\mathbf{v}}$ on C_{up}, and downstream on C_{down}. Summing over these contributions, we have specific upstream areas of $\int_{C_{up}} -h\hat{\mathbf{v}} \cdot \hat{\mathbf{n}} \mathrm{d}s$ and $\int_{C_{down}} hat\mathbf{v} \cdot \hat{\mathbf{n}}\mathrm{d}s$, and the difference between the two is the area enclosed, so

$$\int_C h\hat{\mathbf{v}} \cdot \hat{\mathbf{n}}\mathrm{d}s = \int_{C_{down}} h\hat{\mathbf{v}} \cdot \hat{\mathbf{n}}\mathrm{d}s - \int_{C_{up}} -h\hat{\mathbf{v}} \cdot \hat{\mathbf{n}}\mathrm{d}s = A = \int_A 1\mathrm{d}A.$$

Using the divergence theorem,

$$\int_A \nabla \cdot (h\hat{\mathbf{v}}) - 1\mathrm{d}A = 0,$$

and as A is arbitrary, the integrand must be zero, so equation 1 holds.

- *You drilled 76 boreholes in the upper ablation area but present results from just three which really show clear instabilities in water pressure during the winter period. It would be very useful to know whether these are in fact rather odd/atypical events (which are nevertheless interesting) or whether the behaviour is actually rather characteristic of a lot of your sites as the winter drainage system develops.* This is a good point.

C3392

The first thing to say is that our drilling campaign evolved and was initially not optimally designed to capture details of the drainage system; call it learning by mistake. The layout of boreholes in figure 1 will attest to this: At first, a relatively widely spaced grid of boreholes was drilled, which missed many details of the drainage system. It became clear that boreholes in this grid would often have qualitiatively very different water pressure variations but still show evidence of diurnal cycling and therefore of connection to the glacier surface, indicating significant underresolution (perhaps not surprising in view of the literature on the subject). We switched to drilling more closely spaced boreholes, guided by a Shreve-type upstream area calculation. It however transpired that the digital elevation model (DEM) we were using was not accurate enough. You will see many of the closely-space boreholes around hole C in figure 1. We expected to find a major drainage axis in this region. However, the updated DEM used in figures 1 and 2 resulted in a different predicted drainage routing. The result is that many of the 76 boreholes drilled until the end of 2011 do not lie near predicted major drainage axes, and many show evidence of either no connection to a drainage system, or of more ephemeral connection. The region around hole C also has steep surface slopes, suggesting large hydraulic gradients likely to favour concentration of drainage into narrowly-defined channel-like features.

It is therefore not clear how 'typical' our obsrevations are. Of the 76 holes, only a handful are near major drainage axes in figures 1 and 2; primarily holes A1–A6 as well as hole D and three or four others. We do find wintertime behaviour in hole D similar to A1–A3, with long-period oscillations. We did not include this record in the paper initially because it comes from a single instrument that could have been corrupted. In response to the review, we have now extended the time series, and introduced the following paragraph at the end of the 'Data' section:

'It is unclear whether the qualitative behaviour of phase 4 is replicated in any of the other time series recorded at South Glacier. The strongest indication of

C3393

similar behaviour in another borehole comes from hole D during the 2009–2010 winter. During the equivalent of phase 3, the record from hole D includes two prolonged water pressure spikes in mid to late October 2009 that have no equivalent in the A1–A3 record. These pressure spikes coincide with a prolonged period of above-melting surface temperatures around the same time period. The spikes are followed by period of relatively steady water pressure from late October 2009 to the end of December. Subsequently, hole D exhibited an abrupt drop in water pressure similar to holes A1—A3 in early January 2010, followed by an increase in water pressure and the subsequent onset of oscillations lasting about a week each. While these features are similar to phase 4 as seen in holes A1–A3 in 2011–2012, the record from hole D comes from a single instrument and is not replicated elsewhere, so we cannot rule out that the transducer could have been corrupted.'

In addition, the 13th paragraph of the 'Interpretation' section now ends with an additional two sentences:
'...are the result of ongoing drainage activity. With no obvious external driver for these oscillations, we interpret them as being the result of an unforced, internal mechanism. *Note that we also expect hole D to lie near a different major drainage axis, and it also exhibits similar behaviour. Although the observations were made during a different year, this suggests a common physical process at work.*'

As for the couplet structure of the summer signal in holes A1–A3, we have no clear evidence of this in other boreholes, but this may again not be surprising — the only other record from a borehole near a major drainage axis that shows evidence of strong diurnal cycling was hole D, active from late July 2009 to the spring of 2010. The summer of 2009 was anomalously hot at the site, and the summertime drainage signal may not have been typical. The updated test states in the penultimate paragraph of the 'Data' section that 'we have no clear evidence for two-day "doublets" elsewhere.'

C3394

Figures:

1. *Fig 1. The map in Figure 1a really needs to be bigger – it is too small to see anything at normal size. Some labelled contours would also help put the glacier morphology into context.*
   Now displayed as separate box in the figure

2. *Fig 2. Delete the second "the" in the 4th line of Fig 2 caption and add "TO straddle" or "likely straddles" in line 6.*
   Done.

3. *Fig 2/3. Bit odd using UTM in one and Lat/long in the other - is there a reason for this?*
   No good reason. Both use UTM now.

4. *Fig 3. The vertical dotted lines need to be darker to be visible.*
   Done.

5. *Fig 3. Use lines across the top of the plot to delimit your four phases as described in the text (as opposed to just simply having the numbers).*
   We now use much stronger shading (in blue) to separate the phases,

6. *Fig 4 and 5. Add that it is also a time series of air temp.*
   Done

Minor points:

1. *Give (expected) instrument error range when stating that the transducers"generally conformed to factory calibration"*
   Changed to '...and generally conformed to factory calibration (fractional errors of a few percent) ...'

C3395

2. *It would be useful here to simply state that f=0 is the equivalent to the routing of flow at atmospheric pressure* Added '...for instance equal to atmospheric pressure ...'

3. *Give the date of the Phases in brackets after each time the phase is first mentioned to aid clarity.*
   The sentences that introduce phases 1 and 4 already give dates, and repeating dates in brackets produces odd sentences. We have put the relevant dates in brackets for phases 2 and 3, prefaced by 'approximately' as not all boreholes behave alike. This results in slightly awkward phrasing such as 'Phase 2 (approxiamtely August 10– ...) ...A4 and A5 experienced an earlier increase, starting on 7 August' but hopefully putting the dates in parentheses (the dates given correspond to the markings in figure 3) is more helpful than it is confusing.

4. *grammar awkward – replace the second"continued" with "ongoing".*
   Done.

5. *P5622, l2 – delete "also"*
   Done.

6. *–until âĹijmid (not late) September i.e. 18 sept for diurnal cycles*
   Done.

7. *channel-liKe*
   Done.

8. *conduitS*
   Done.

9. *delete comma after "both"*
   Done.

C3396

3 Reviewer # 3

We appreciate the comments regarding the level of detail in describing our observational set-up; the hope is indeed that this will set the stage for future publications that can be more succinct.

- *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 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?)*

  This is a good point. Where we discuss the 'phase 1' data, we now include the sentence:

  'All three boreholes immediately displayed a strong and almost identical diurnal cycle, except for a constant offset (Fig. 3). This continues until there is a hiatus in diurnal cycling between 24 and 29 July, after which water pressure once more exhibits pronounced daily peaks.' In the comparison of borehole with water pressure data, we also write

  'The initial hiatus in diurnal cycling during phase 1 (24 to 29 July) occurs during a period of reduced surface melt and less pronounced daily temperature variations, but temperatures remain above the melting point until the night of 25 July, after which significant diurnal temperature variatons recommence.'

  We now define phase 2 as the 'final' disappearance of the diurnal signal, to contrast it with the hiatus identified for 24 to 29 July: 'Phase 2 (approximately August

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5–10) corresponds to the final disappearance of the dominant diurnal signal'

Specific points:

1. *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?*

   We have demoted this to 'a role' rather than a 'key role'. The argument would be that the daily modulation of water input gives a succession of water pressure peaks and troughs, and the speed-up created by water pressure peaks outweighs the slow-down caused by water pressure troughs, so the effect should be a net speed-up when averaging over the diurnal cycle. That argument is rehearsed in Schoof (2010) so we do not repeat it here.

2. *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.)*

   We have included the suggested reference.

3. *"Aereal" should be "Aerial"*

   Corrected

4. *"was" should be "were"*

   Corrected

5. *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 cur- rent explanation of the meaning of f=0 and f=1 is*

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accurate, but in reference to actual hydraulic potential as calculated by Eq. 1, the current explanation is somewhat confus- ing.

The explanation in the paper would seem to be correct as is — a zero effective pressure is a spatially uniform effective pressure, and a zero water pressure is a spatially uniform water pressure? To say that the effective pressure is spatially uniform is slightly more general than saying it is zero (which seems pretty unlikely as a floating glacier ought to experience no basal friction other than large scale form drag), ditto for saying water pressure is spatially uniform versus zero.

6. *150 should (presumably) be "150 m".*
   Corrected.

7. *Clarifying "peak temperatures" as "peak air temperatures" would be useful here*
   Done

8. *"errors due damage" should be "errors due to damage"*
   Corrected

9. *"crevasses" should be "crevasses"*
   Corrected

10. *. 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 pressure presumably will rise as the conduits are re-equilibrating.*
    Good point. Re-phrased to say 'The subsequent increase in water pressure in holes A1–A3 during phase 3 is also consistent with the conduits re-equilibriating to a smaller size. As they do so, water pressure can rise again'

11. *"channel-lie" should be "channel-like".*
    Corrected

12. *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.*

    Rephrased as 'Based on the above definition, we find that the change from cavity-to channel-like in our conduit occurs between $mL = 10^3 \, \mathrm{m^3 \, day^{-1}}$ and $mL = 2 \times 10^3 \, \mathrm{m^3 \, day^{-1}}$ for the parameter values in Table 1 while oscillatory behaviour persists down to $mL = 250 \, \mathrm{m^3 \, day^{-1}}$ (note that the transition from channel-like to cavity like corresponds to the change from circular to rhombus-shaped markers, while oscillations occur for empty markers).'

13. *It would be good to start a new paragraph here; the remainder of the exist- ing paragraph describes a new model formulation.*
    Done.

14. *Is this result (that localized storage only leads to instability for channel-like con- duits) 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.*
    It is a general result — but the demonstration of that result will have to wait for a separaete paper (in preparation, as stated in the Discussion section).

15. *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.)*
    The phenomenon here is effectively a 'period-doubling', a phenomenon that can occur across a wide class of oscillatory dynamical systems. The original idea behind this goes back to Jonathan Kingslake's PhD thesis, which contains much more detailed discussion and I understand the relevant material is being prepared

for publication. We did check with Jonathan that he was happy for us to mention the doublet structure in our data as possibly being linked to his theoretical work, but don't wish to pre-empt the journal publication of that work.

It might be possible that longer periods or even more complicated behaviour could be induced by we have no evidence for this as yet. We have kept the result / discussion separation as in the original paper, as there is now a separate 'Discussion' section

16. *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.*
    We now state under 'Third' that
    'Third, oscillations occur only above a lower critical value for water supply rate. For distributed water storage, that lower threshold for oscillatory behaviour is small enough that the conduit can be cavity-like in the sense of Schoof (2010)'

17. *p.5637/2: No comma needed after 'both'*
    Corrected

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

19. *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 seems the word "shown" should be removed from this statement.
    Done.

20. *Fig 3: "(g) And (h) have different **vertical** axis scalings from (b–f)"*
    Corrected in revised manuscript

21. *Fig 3: Last sentence of caption: "corrspond" should be "correspond"*
    Corrected.

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

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

24. *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.*
    Done

Interactive comment on The Cryosphere Discuss., 7, 5613, 2013.