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The Cryosphere Discussions

Interactive comment on "Rapidly-changing subglacial hydrology pathways at a tidewater glacier revealed through simultaneous observations of water pressure, supraglacial lakes, meltwater plumes and surface velocities" *by* Penelope How et al.

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We would like to thank the reviewer for their comments and positive response to our manuscript. We have edited our manuscript accordingly, including better definition of frequently-used terms and more detailed descriptions in places. We have also addressed the comment on the inclusion of the GPS velocity data with the figure included in the supplementary material associated with this response. We hope that the re-

viewer finds that all their comments have been carefully considered and thoroughly addressed.

Details of our response to the reviewer's comments are outlined below.

p2, line 8: if such observations are rare, this suggests that there are some. Can you provide reference(s) here?

There are a couple of these studies dotted as citations throughout the manuscript. We have now included these citations in this sentence:

' However, simultaneous measurements of all these manifestations of the subglacial system are rare (e.g., Kamb et al., 1994; Sugiyama et al., 2011).'

Figure 1: it seems that you could change the aspect ratio of the figure to zoom in more on the study area. Areas to the north and south of Kronebreen aren't really necessary to include, other than to make room for your inset panels.

The authors attempted to: 1) change the aspect ratio; 2) move the inset panels; and 3) zoom into Kronebreen. We found that numerous problems occurred when trying to accomplish this. Primarily, the Landsat image becomes pixelated and coarse when we tried to zoom into our field site, which isn't as visually pleasing. It was also difficult to move the inset panels without covering the plume extents or camera positions, even when we had changed the aspect ratio. Equally we felt that it was valuable to include some of the fjord and neighbouring glaciers (Kongsvegen and Kongsbreen) as context for the reader.

For these reasons, the authors have decided not to change Figure 1.

P6, lines 9-10: it's not clear exactly what you mean here, or why this is necessary. Why is it necessary to smooth an initially high-resolution DEM, and what do you mean by 'homogeneous surface'?

The DEM used in this study was obtained from airborne photogrammetric surveying in 2008 by the Norwegian Geodetic Survey. This DEM is older than the time-lapse imagery, which was acquired in 2014. Even at an initially high-resolution, this DEM has topographic features that represent the surface at the time of acquisition. These features are not present in the actual 2014 glacier surface. It was therefore smoothed to create a homogenous surface (i.e. flattened, without abrupt changes/artefacts) to better represent the glacier surface in 2014.

This information has now been added to these lines for better clarification: 'This DEM was smoothed using a linear interpolation approach to reduce discrepancies between the glacier surface in 2008 and in 2014. Data could thus be projected onto a homogenous surface (i.e. flattened and without abrupt changes/artefacts).'

Melt modeling: what is the spatial resolution of the model? Is the model driven solely by the weather station data from Ny Ålesund? Are there any metrics of model validation/calibration that you can discuss?

The melt model used in this study is the same one used in the Van Pelt and Kohler (2015) study. The model calculated melt/runoff in 100 $\times$ 100 m grids across the Kronebreen/Kongsvegen/Holtedahlfonna catchment. The model is primarily driven by the weather station data from Ny Ålesund (i.e. air temperature, wind speed), but elements of the model were initially calibrated with in-situ measurements. For instance, the subsurface model was calibrated with snow density pits, and the energy balance model was calibrated with flux measurements of incoming and outgoing shortwave and longwave radiation from radiometer measurements in the field.

Details about the model validation and calibration are fully outlined in Van Pelt and Kohler (2015). It has been made clearer in the manuscript that full details about the model can be found in this citation:

'Further details about the model, including model validation and calibration, are outlined in detail in Van Pelt and Kohler (2015).' (Section 4.3 Melt modelling, paragraph 1, last sentence)

P8, line 31: It sounds here as if you're using a different surface DEM from the Norwegian Geodetic Survey DEM that you discussed above with respect to the photogrammetry. Is this the case? If so, how different are the two DEM's you used? Why not use the same DEM throughout the study? For example, how different would the hydraulic potential calculations be if you used the Norwegian Geodetic Survey DEM instead?

Two different surface DEMs were used for the photogrammetric measurements and the hydropotential modelling. The Norwegian Geodetic Survey DEM (from 2008) was used for the photogrammetric measurements whilst the radar surveying DEM was used for the hydropotential modelling. The DEMs for the hydropotential modelling were acquired simultaneously in 2009–2010 and 2014–2016 and will form part of a study soon to be submitted:

Lindbäck, K., Kohler, J., Pettersson, R., Myhre, P.I., Nuth, C., Langley, K., Brandt, O., Messerli, A. and Vallot, D., In Prep. Subglacial topography, geology and future bathymetry of Kongsfjorden, northwestern Svalbard.

The surface and bed DEMs are exclusive to this study. They were only used for hydropotential modelling because they represent the surface and bed topography at the same period in time. Differences between the Norwegian Geodetic Survey DEM and the radar surveying surface DEM are relatively small, and thus there would be little difference in the hydraulic potential calculations if they were interchanged.

Borehole GPS: it seems surprising (even if true) that the borehole GPS didn't add anything insightful. Why not show this and demonstrate that this is the case? It seems that you could overlay the GPS-derived velocity on Figure 2D to show this.

This point was also noted by Reviewer 1. As stated in their response, the GPS data was not included in this study for three main reasons:

- The GPS velocity record is incomplete. The GPS was offline at the beginning of September 2014, whilst the rest of the dataset record carries on till the end of September 2014. The record duration is therefore mismatched.
- The higher temporal resolution of the GPS velocities does not appear to add anything new to the study. There were difficulties in processing the GPS data and short-term variations cannot be distinguished from the daily positions that we extracted. The dataset generally appears noisy. To resolve this and provide an alternative, velocities were derived from the TerraSAR-X imagery and then a spot velocity was extracted from the borehole site. These appear much less noisy and fit well with the rest of the 2014 record.
- The key findings from the velocity data focus on the spatial variability in velocity over the glacier tongue, rather than changes in velocity over time. These are better addressed with the TerraSAR-X velocities rather than the GPS velocities. The inclusion of the TerraSAR-X velocities from the borehole site are also consistent with the velocities derived from the other ROI's (i.e. from the centreline and the supraglacial lakes).

To better show the noise in the dataset, we have included Figure 2 as supplementary material which includes the GPS velocities in panel F (shown as the green plotted line) along with the TerraSAR-X derived velocities. It is clear from this that the GPS velocities do not add any additional information to the study, and any changes seen in velocities are difficult to associate with the other datasets.

For these reasons, the GPS data will not be included in this paper. The difficulties with integrating the GPS velocities has been clarified in the methods section of the manuscript (Section 4.4, page 8, line 11).

Figure 4 caption: panels are not numbered clockwise as indicated.

Agreed. The caption has now been changed accordingly:

'Meltwater plume scenarios from time-lapse imagery at Kronebreen. Top-left to bottom-right: 1) Surfacing meltwater plume from the main source on the north side of the glacier terminus, N1; 2) Sources from Plume N1 and Plume N2; 3) Sources from Plume N1 and Plume N3; 4) Plume N1 and Plume S1, the main source on the south side of the glacier terminus.'

Section 5.6: it's not clear how you arrived at a value of k=0.6 as a sort of threshold for routing of meltwater between the northern and southern sections of the glacier. You state that "results suggest" this, but don't specifically describe why. "Several scenario were considered" (line 15), but what do you mean by this? How do you arrive at the conclusion that flow routing changes between a value of k=0.5 and 0.6 (line 19)? This seems different than what you describe in line 18 about threshold routing above and below a value of k=0.6? I guess I'm just a bit confused about this section, perhaps it's just a matter of describing more specifically what you've done here.

Subglacial hydraulic potential was calculated primarily based on ice thickness and bed elevation. The crostatic pressure factor (k) is the ratio of water-pressure to ice overburden pressure. Variations in the value of k reflect the degree to which subglacial drainage is pressurised with k=0 reflecting open channel flow at atmospheric pressure, and k=1 reflecting pressurised flow.

We calculated subglacial hydraulic potential over several iterations, changing the value of k each time. In total, we ran 11 simulations with the value of k between 0.0–1.0 (i.e. hydraulic potential was calculated each time with a k value of 0.0, 0.1, 0.2, 0.3, 0.4, 0.5, 0.6, 0.7, 0.8, 0.9, and 1.0). These are what are referred to in Section 5.6 as the 'several scenarios' that we considered.

We found that there is little change in the configuration of the channel network when k is 0.0–0.5. In all these instances, the calculations suggest that a major channel connects Holtedahlfonna to the south region of the glacier tongue. There are significant changes in the channel configuration when k is 0.6 and above (i.e. k=0.6–1.0). The major channel diverts to the north region of the glacier tongue in these scenarios. Therefore there is a significant difference when we consider hydraulic potential with a k value of 0.5 and below, and 0.6 and above. This is what we refer to in the manuscript as the 'threshold' as it is apparent that this difference occurs between a k value of 0.5 and 0.6.

The authors appreciate that some of the terms used in this section are not accurate and more appropriate, detailed wording could be used instead. We have changed the section accordingly to make this clearer:

'Several scenarios were considered in calculating the hydraulic potential at the bed of Kronebreen based on the k value, which represents cryostatic pressure ratio (i.e. the extent to which meltwater routing is dictated by ice-pressure gradients). Subglacial hydraulic potential was calculated over several iterations, changing the value of k each time. In total, we ran 11 simulations with the value of k between 0.0–1.0 (i.e. hydraulic potential was calculated each time with a k value of 0.0, 0.1, 0.2, 0.3, 0.4, 0.5, 0.6, 0.7, 0.8, 0.9, and 1.0).' (Section 5.6, paragraph 1)

Borehole pressure: the pressure variations you record indeed seem to suggest that you are not actually located to a connecting channel. I would expect more pressure

variations if you were. You seem to suggest that you might not be located at a channel, but argue that you are 'near' a connecting channel if not connected to a channel that is consistently full of meltwater (in which case I would still expect to see more pressure variations).

The reviewer is correct in stating that we would expect to see more changes in the water-pressure record if the borehole sensor was located in a connecting channel. This is now clearly stated in Section 7.4 (Subglacial drainage of Kronebreen) following similar comments from Reviewer 1.

We suggest that the borehole is possibly located near to an active drainage system based on instances where changes in pressure have coincided with other changes related to subglacial hydrology (e.g. the early-melt season 'flushing' event, and the significant pressure drop in September). This is also supported by the hydraulic potential modelling which indicates that the location of the borehole intersects with one of the main channels in the catchment. We propose that the borehole is located within the catchment of an active drainage system based on these arguments. Absolute changes in the water-pressure record suggest differently as noted by the reviewer.

Therefore we have two lines of evidence, with one suggesting that the water-pressure is indicative of an active drainage catchment, and the other suggesting that the record reflects an isolated, consistently pressurised region of the bed.

A paragraph has been added to better outline these ideas in Section 7.4:

'Few short-term pressure variations are observed in the water-pressure record from May–September 2014, apart from the significant drop in pressure at the end of the melt season. It is possible that the borehole is located on an area of the bed that is not well connected to an active, efficient drainage system. However, changes in water-pressure have been observed to coincide with other features in the hydrological system (i.e. plume activity and supraglacial lake drainage), which suggests that the borehole is hydraulically connected to some degree. This is also supported by the modelled hydraulic potential, which indicates that the borehole is located close to, or possibly within, an efficient drainage catchment.'

Section 6.2: you describe here a cyclic pattern of the plume surface area, and suggest that it may be related to internal cycles of storage and release within the glacier. You describe the pulsing as having a 'duration' of 4-5 days, what do you mean by 'duration'? Is there a particular period of the cycling? It seems that there could be sources of cyclicity in fjord circulation or tides which could also play a role in the patterns you see.

From measuring the surface area of the plume expressions, we found that the plume surface expression fluctuates in size on a regular basis. This behaviour is repeated throughout August (08–28 August), and each fluctuation phase (i.e. a period of expansion followed by a reduction in surface area) has a duration of 4–5 days. This is what we refer to in the manuscript as a cyclic pattern, and we associate this with hydraulic pulsing.

Changes in surface melt and runoff appear to have little influence on this pulsing. The reviewer rightfully points out that the source of this cyclicity could be associated with fjord circulation and tides. From looking at the time-lapse images, the authors believe that wind direction may also play a significant role as the plume size can be affected by sea ice and icebergs that have been pushed towards the terminus.

These further details have now been added to Section 6.2:

'Plume activity at the north side of the terminus is persistent throughout August (Fig. 2B). The main plume (N1) is visible throughout, the secondary plume (N3) is present for most of the month (01–20 August), and the third (N2) is briefly active on 29 August. The total surface area/expression of these plumes fluctuates in size on a regular basis. This behaviour is repeated throughout August (08–28 August), and each fluctuation phase (i.e. a period of expansion followed by a reduction in surface area) has a duration of

4–5 days (Fig. 2C). Changes in surface melt and runoff appear to have little influence on this pulsing. This implies that there are additional controls on subglacial outflow. The source of this cyclicity could be associated with marine influences such as fjord circulation, tidal cycles, and wind direction. However, it is difficult to examine these influence here due to the limited datasets. Cycles of internal storage and release in the subglacial environment could also be a influence on subglacial outflow, which is possibly confined to the terminus zone because the signal is not evident higher up the glacier tongue in the water-pressure record from the borehole.' (Section 6.2, paragraph 2)

P19, line 16: here (and in other places) you describe 'storage' of water at the base of the glacier. I think you should define what you mean by storage. Does this mean that you think the water is held stationary beneath the glacier, or rather that it is just inefficiently drained? You actually provide a bit of a definition on p20 lines 3-4, but it might be worth defining this sooner.

'Storage' is used when describing inefficient drainage at the south side of the glacier tongue. In these cases, the storage of water is used as an encompassing term to outline that water is being inefficiently drained and, as a result, is likely to also be held stationary beneath the glacier.

Changes have no been made throughout the manuscript in instances where the 'storage' of water is described in relation to inefficient drainage at the south side of the glacier tongue. For each case, we have now added that meltwater at the bed is slow-moving and/or being stored. For example:

'It is likely that this meltwater is slow-moving and/or being stored, which would enhance basal lubrication and is a likely reason for high surface velocities in this region at this late stage in the melt season' (Section 6.3, paragraph 2, last sentence)

P21, lines 18-19: you say here that runoff has a diurnal signal, but then state that plume pulsing is independent of meltwater inputs. Related to comments above, I think you should describe the cyclicity of the plume pulses in a bit more detail to support the claim that this pulsing is not related to diurnal meltwater inputs (unless I've missed something here).

More detail has been added to Section 6.2 has stated in the previous related comment about cyclicity in the plume surface expression. This adequately conveys that the hydraulic pulsing observed in this record is not related to diurnal meltwater inputs. The sentence referred to in this comment is convoluted due to the mention of diurnal patterns in runoff. It has now been made clearer that this pulsing is independent of dirunal changes in runoff:

'It is proposed here that this plume activity is a signal for subglacial hydraulic pulsing. As the water level at the borehole site varies over only a small range (298–300 m), it is suggested that this pulsing is independent of meltwater inputs and is the result of processes confined to the near-terminus region (i.e. not glacier-wide).' (Section 7.3, paragraph 2, last sentence)

P21, line 22 (and elsewhere): you describe in numerous places that pressure forces a channel to open. However, the prevailing theories for meltwater channels is that they represent a balance between creep closure due to ice overburden pressure and melting caused by water flowing against the channel walls. So are you really claiming that you have something different than this at Kronebreen, some kind of elastic deformation at the base of the glacier from water pressure forcing channels to open?

Meltwater channels open and close due to two main processes: 1) when subglacial water has sufficiently melted the channel wall; and 2) when the overlying ice causes the channel to close. Channels remain open when these two forces are balanced, and open/close when one prevails over the other. These two processes have largely been

studied on land-terminating and alpine glaciers.

There is limited understanding about channel formation at tidewater glaciers due to the difficulty in obtaining direct observations. There are additional controls at tidewater glaciers, such as marine influences, which affect the pressure environment at the bed. This could promote elastic deformation at the base of the glacier which could 'force' a channel to open. Although these processes cannot be thoroughly examined in this study, the authors felt that it was necessary to differentiate the processes for channel opening and closure from those at land-terminating and alpine glaciers.

However, it is understood that channel meltback could also be an active process at Kronebreen and therefore should be outlined as a possible mechanism for channel opening. We have added this as a possible mechanism in each instance where we describe that pressure forces a channel to open:

'Hydraulic pulsing represents a periodic flushing of meltwater in the local vicinity, which occurs when sufficient pressure has accumulated to force a channel open and/or when subglacial water has sufficiently melted the cavity/conduit wall.' (Section 7.3, paragraph 3, first sentence)

This was previously recommended by Reviewer 2, in which all instances were changed. No further changes have been made.

P21, lines 21-26: you imply here that marine dynamics such as tides may play a role in the periodic flushing of meltwater. However, this claim is not supported by subsequent sentences that describe supraglacial lakes and velocity signals. So what is the evidence for your claim that marine dynamics plays a role?

The main aim of this manuscript is to better understand subglacial hydrology at a tidewater glacier from direct and indirect observations. The examination of marine influences, such as tidal level, in relation to the periodic flushing of meltwater is beyond the scope of the study. Therefore it is difficult to support this idea with evidence, but the authors felt that it needed to be mentioned in order to represent alternative influences on subglacial hydrology and glacier dynamics. As recommended by Reviewer 2, the manuscript was changed to better outline that the examination of these alternative influences will not be further explored in this study, but could be a promising focus for future work. No further changes have been made following on from the changes recommended by Reviewer 2:

'Hydraulic pulsing represents a periodic flushing of meltwater in the local vicinity, which occurs when sufficient pressure has accumulated to force a channel open and/or when subglacial water has sufficiently melted the cavity/conduit wall. The precise timing of each outflow is possibly controlled by marine dynamics such as tidal level. Although it cannot be further explored here, this could be an interesting focus for future work.' (Section 7.3, paragraph 3, first line)

P22, line 24: can you describe what you mean physically by a "transient low-pressure wave"? What would be the source of such a wave, and how would it originate at the terminus?

The term has commonly been used to describe events where high-pressures propagate through the subglacial zone of a glacier due to high pressure gradients. They have been associated with surges (Kamb et al., 1985) and have been used to propose an alternative explanation to hydrofracturing for the filling/draining of supraglacial lakes (Everett et al., 2016). However, the term can also lend itself to instances where low-pressures propagate through the subglacial zone of a glacier.

All the reviewers have mentioned that the use of the term 'subglacial transient pressure wave' is convoluted and it appears that this may be misinterpreted by the reader. For this reason, the term has been omitted from this paper. The term is largely used to describe the events at the beginning of the melt season. It has now been replaced with

better details concerning the glacier-wide drawdown of meltwater in the near-terminus area.

P 24, line 3: here you list melt and runoff under "measurements of hydrologic components" but these are actually model outputs.

Agreed. The sentence has now been changed to:

'Subglacial hydrology has been examined at a tidewater glacier in Svalbard using direct measurements of the basal pressure environment in conjunction with measurements of hydrological components (supraglacial lake drainage, meltwater plume presence, and plume surface area), modelled components (melt, runoff, and hydraulic potential), and surface velocities derived from TerraSAR-X imagery.'