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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.

Penelope How et al.

p.how@ed.ac.uk

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We would like to thank the reviewer (Shin Sugiyama) for their comments, and their positive response to our manuscript. Sugiyama's enthusiasm and curiosity for the subject is evident in his feedback, which is very refreshing to read. We have edited our manuscript accordingly, including edits to Figure 2, the inclusion of glacier dynamics as an explanation for the cause of the lake drainage at the beginning of the 2014 melt sea-

son, and the inclusion of a scenario where the borehole pressure does not represent basal conditions in the region.

Details of our response to the reviewer's three key comments (numbered) and minor comments are outlined below. All typos, grammatical corrections and minor sentence changes that were suggested by the reviewer have also been agreed to and changed within the manuscript. These smaller changes are not outlined here in order to keep this response as brief as possible.

1. I understand the borehole pressure was recorded from September 2013. Why not showing all the data from the beginning of the observation? Water pressure over one year period provides insights into basal conditions as well as the connectivity of the borehole to the subglacial hydrological system. At least, overview of the pressure record over the entire period should be described in the text.

The borehole pressure record covers a 14-month period from September 2013 to December 2014. We understand that this is a very valuable dataset that should be shared with the scientific community as soon as possible. However, it was decided to only focus on the 2014 melt season because of two main reasons:

- We believe that the inclusion of the whole record is beyond the scope of the paper. The inclusion of the whole record may detract from the key aim in this paper, which is to build a detailed theoretical model of the hydrology at the glacier terminus of a tidewater glacier during a single melt season. We believe that the entire dataset is not needed to fulfil this aim.
- The beginning of the record (September 2013–March 2014) is strikingly different from the rest of the record. For instance, basal water-pressure appears to exhibit strong, consistent diurnal variability (roughly between 10–50 kPa) from September 2013–March 2014, whilst the rest of the record does not indicate any

diurnal variability. This may be because sensor took a while to settle and give consistent readings, or basal pressure drastically changed over the monitoring period, or the sensor may have been located on a different part of the bed and was subject to a different pressure/hydrological environment. This in itself is an interesting observation and we are still attempting to understand this. Once we have gained a better understanding (and potentially integrated it with subglacial hydrology modelling), it is intended to publish the borehole dataset in its entirety at a later date in a CRIOS project publication.

For these reasons, the entire borehole record will not be included here. Also, an overview will not be included in the text because we believe that the significant difference in the record from September 2013 to March 2014 does not reflect the subglacial conditions in the 2014 melt season.

2. I wonder if glacier dynamics can be the cause of the lake drainage. When the glacier accelerates near the front, a longitudinally stretching flow regime is enhanced. This causes crevasse opening and increases chance of lake drainage. Assuming that such acceleration initiates near the glacier front and propagates upglacier, the observed lake drainage can be explained by this process.

Section 7.2 (Upward-propagating supraglacial lake drainage) outlines the dynamics of the three lake clusters monitored in this study and compares their dynamics to other observations from the literature. The lakes in Cluster 1 are focused on in particular because of the coincident timing of their drainage in relation to changes in velocity, runoff and plume activity. The nature of their drainage is discussed in relation to hydrology and it is hypothesised that their drainage is related to their connectivity to efficient drainage in the subglacial environment. Glacier dynamics were not discussed here to avoid repetition with Section 7.5 (Implications for subglacial dynamics).

However, the reviewer rightfully points out that glacier dynamics may be the cause of

the lake drainage and the reader may gain the impression that the drainage of the lakes in Cluster 1 is exclusively linked to hydraulic connectivity. Glacier dynamics may also play a key role in their drainage. Longitudinal stretching is likely to be enhanced at the beginning of the season when the glacier begins to accelerate and this could, in turn, promote the likelihood of lake drainage. As suggested by the reviewer, this hypothesis has now been included in section 7.2 to provide a more detailed explanation for the drainage of these lakes. It is suggested that their drainage may be related to glacier dynamics as well as glacier hydrology:

'The lakes in Cluster 1 are of particular interest because of the coincident timing of their drainage in relation to changes in surface velocities, runoff, and activation of the plume at the beginning of the melt season. This suggests that these lakes are linked to a common channelised system when they drain. The upward-propagating nature of their drainage indicates that channels develop in an upglacier progression as reflected in the timing of their connection to thr subglacial environment. The hydraulic potential modelling supports this as it indicates that Cluster 1 may be situated close to a large channel/flow accumulation pathway. Glacier dynamics may also play a key role in the cause of this lake drainage. Longitudinal stretching occurs as the glacier accelerates at the beginning of the season, which facilitates the opening of crevasses and increases the chance of lake drainage. The upward-propagating nature of the drainage may be a result of this early-season acceleration, assuming that it initiates at the glacier front and propgates upglacier.'

3. Throughout the paper, the authors assume the borehole pressure represents the subglacial water pressure over the region. Nevertheless, the lack of short-term pressure variations gives me an impression that the borehole is not well connected to active subglacial drainage system. The pressure drops in September, but it is only 15 m out of 280 water depth. I agree that the authors' interpretation is one of likely scenarios, but it is worth mentioning that there is a possibility that the borehole pressure does not *represent basal conditions in the region.*

Hydraulic potential modelling suggests that the borehole is located close to/within the catchment of an efficient channel system, and thus the record reflects basal water-pressure in a well connected region of the glacier bed. However, the borehole record shows few short-term variations over the entire study period that this manuscript covers (May–September 2014), which suggests that the borehole is isolated from the active subglacial drainage system.

The reviewer is right to point out that there is a possibility that the borehole may not be located in an efficient drainage catchment based on the lack of short-term pressure variations. A paragraph (page 22, line 16 – 21) has been added to Section 7.4 (Subglacial drainage of Kronebreen) to address this point:

‘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.’

Page 1, title: I think ‘Rapidly changing subglacial hydrology pathways’ is not supported by evidence and does not fit the presented results. For example, "a stable efficient drainage system effectively transported this water through the north region... (page 1, line 6–7)’ contradicts to ‘rapidly changing pathways’. What about something like ‘Subglacial hydrology at a tidewater glacier as revealed through ...’?

The title ‘Rapidly changing subglacial hydrology pathways’ is in reference to the unstable, changing drainage system beneath the south region of the glacier terminus, which is suggested to facilitate the upward-propagating nature of the speed-up observed at the beginning of the 2014 melt season. This is what we believe is the key take-home message of the paper, hence why the paper is titled accordingly. The authors wanted a title that made the paper stand apart from other subglacial hydrology studies. The title suggested by the reviewer, ‘Subglacial hydrology at a tidewater glacier as revealed through. . .’, is over-used in our opinion and does not grab the readers’ attention. The title we have chosen reflects the uniqueness of the study and makes it stand apart from others. For these reasons, the title remains unchanged.

Page 3, line 16: ‘Subglacial transient pressure waves’. Not clear what are these waves. Please provide citations if this term is defined and used in previous studies.

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.

Both reviewer 1 and reviewer 2 have stated 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 was largely used to describe the events at the beginning of the melt season, which has now been replaced with better details concerning the glacier-wide drawdown of meltwater in the near-terminus area.

Page 5, line 12: Why 'Calving activity persists throughout the year'? Because of absence of sea ice?

Calving activity is persistent throughout the year because of the warm, saline Atlantic water that can freely enter the fjord throughout the year (Luckman et al., 2015). This sentence has now been changed to clarify that this:

'Calving activity persists throughout the year due to the presence of warm sub-surface ocean water, even in the winter season…'

Page 5, line 31: 'real-world'. Is this a common expression? 'velocities, areas and distances in real space'?

The term 'Real-world' measurements is often used in photogrammetry to distinguish absolute measurements (e.g. metres, md^{-1}, m^3) from relative, pixel measurements that are made from images. It is important to distinguish between these two types of measurements. The text has been left unchanged.

Page 7, line 29: 'This was undertaken in order to isolate the hydrology of the glacier tongue (isolate from what?) and better observe direct hydrological influence (influence of what?) in the region of interest'.

It was decided to reduce the melt/runoff model catchment size to isolate the hydrology of the glacier tongue from hydrological influence in the upper catchment (i.e. Holtedahlfonna), and better observe direct, immediate hydrological effects in the region of interest. This sentence has now been changed accordingly:

'This was undertaken in order to isolate the hydrology of the glacier tongue from hydrological influence in the upper catchment area (i.e. Holtedahlfonna), and better observe direct hydrological effects in the region of interest.'

Page 8, line 2: Can you explain more about the wireless pressure sensors (specification of the sensor and communication system)? Any citation for the borehole instrument used in this study?

The wireless pressure sensor is a WiSe (Wireless Sensor system) developed at the Institute for Marine and Atmospheric Research, Utrecht, first used by Smeets et al. (2012) at Russell Glacier (and at the drill site of the North Greenland and Eemian Ice Drilling project – NEEM –.for testing purposes). These probes are custom-made for glacial applications. It measures in-situ pressure, temperature and tilt every two hours, and these readings can be transmitted to a receiver through $<$ 2500 m thick ice. Smeets et al. (2012) fully document the design of the WiSe systems, therefore it was decided to include this as a citation in the manuscript:

'More details about the specifications of these wireless sensors is presented in Smeets et al. (2012).'

Page 8, line 3–4: Can you give uncertainties to the bed elevation and the ice thickness?

Spot heights (for the bed and ice surface) from the borehole sites were derived from the bed and surface DEMs outlined in section 4.5 (Methods: hydraulic potential modelling). Ice thickness was calculated from these spot heights. The maximum vertical root mean-squared uncertainty in the interpolated surface and subglacial DEMs is approximately $\pm$15 m. This information has now been added to both section 4.4 and section 4.5.

Page 8, line 9–10: "High temporal resolution of the GPS data did not add any further insights to this study." Even if you did not find short-term variations, it gives very important information to this study. Please clarify what you measured by the GPS.

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).

For these reasons, the GPS data will not be included in this paper. The difficulties with integrating the GPS velocities has been clarified at the end of the paragraph (page 8, line 11) stating:

'It was decided to use the surface velocities derived from TerraSAR-X images rather than the GPS because the GPS velocity record was incomplete and the higher temporal resolution of the GPS data did not add any further insights to this study. The GPS data appeared noisy due to difficulties in processing the positions.'

Page 8, line 31–32: Any citation for the radar measurement?

The paper that includes this work is still in preparation for submission, albeit the paper will be submitted imminently:

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 citation will be added if it is submitted before all corrections are compiled and re-submitted for this paper.

Page 9, line 9–18: Please refer to each of the photographs in Figure 3. Please also refer Fig. 2E to explain the lake evolution.

The paragraph has been changed to include references to each of the photographs in Figure 3 and to the surface areas from the composite graph (Figure 2):

'While the lake clusters appear to act independently, the lakes within Cluster 1 fill and drain almost simultaneously, indicating that they are hydrologically linked. A timeline of changes in lake surface area at Cluster 1 is shown in Figure 3. Cluster 1 fills and drains first, beginning to fill from 01/06/2014 07:00 (Fig. 3A–D) and initially draining on 27/06/2014 03:00 over 59 hours (Fig. 3E–F), decreasing from a total surface area of 41,374 m^2 to 2477 m^2 (see Lake 1 group surface area in Fig. 2A). The lakes gradually drain after this, leaving them empty by 21/07/2014 14:00 (Fig. 3G–J).'

Figure 2: The order of the subplots is not consistent with that in the text. Why not listing the plots starting from the lake measurements, then melt modeling, velocity, and borehole pressure?

The authors agree with the reviewer that the plots should be listed in the same order as introduced in the text. This will make the manuscript easier for the reader to follow. The order of the plots in Figure 2 have been changed to:

A) lake areas; B) Plume presence; C) Plume surface area; D) Melt and precipitation; E) Runoff; F) Velocity; G) Borehole pressure

Figure references in the manuscript have also been changed to correspond with this new ordering.

Figure 2A: MPa is more common (MKS unit system) as a unit of pressure.

Pascal is the SI unit of pressure, and it is agreed that these units would be more appropriate than bar units. It was decided to use kilopascal (kPa) references rather than megapascal (MPa) references as changes in water-pressure in this study are relatively small.
Kilopascal now replaces bar as the used unit of pressure. This change has been made to Figure 2 and also any reference to bar pressure measurements have been changed to kilopascals.

Figure 2B: Unit of rainfall should be mm/time (mm/d?). To avoid the overlapping of the line (melt) and bar (rainfall), I suggest to plot rainfall upside down, i.e. bar extending downward from the top axis.

This was merely a typo mistake and the units of rainfall have now been changed to mm per day. The plot has now been changed so that rainfall bars are extending downward from the top axis, making it easier to distinguish.

Figure 2D: Can you provide uncertainty range in the plot and describe in the text?

The uncertainty range for the velocities derived from TerraSAR-X image pairs is <0.4 m/day (Luckman et al., 2015). The figure has been changed to include this uncertainty range in the velocity plot. The figure caption has also been changed to clarify this.

Figure 2E: The variations of Lake 2 and 3 are very difficult to read. What about plot them for a more suitable scale taken on the right axis?

The authors agree that variations in Lakes 2 and 3 are difficult to read. Therefore the plot has been changed and they have been plotted on a seconary axis with a more appropriate scale.

Figure 4: Plume 1–4 in the plots should be Plume N1, N2, N3 and S1.

The authors agree with the reviewer and the plumes in this figure are now labelled accordingly.

Page 14, line 4: 297 m is a little higher than I expect as the floatation level of the 320 m thick ice. What kind of ice density did you use?

Floatation (m w.e.) is calculated given the ice thickness (320 m at the borehole site) multiplied by ice density. For ice density with no snow or firn layer, a density between 910 and 917 kg m^{-3} is commonly used. Therefore the floatation level here is between 291–293 m, so the reviewer rightly points out that the value given in the paper (297 m) is a little higher than expected. This is simply a mathematical error that was not spotted by the authors. However, there are a lot of unknowns and the local bed topography around the borehole will change and could easily vary by 5–10 m. Therefore, 297 m is still a realistic floatation value.

The floatation value has been changed to 291–293 m, based on an ice density between

910 and 917 kg m^{-3}. This has been clarified in the manuscript.

Figure 6: The lakes and plumes on the map are difficult to find. Please indicate the texts (Supraglacial lake, Plume N1, etc.) directly on the map. Please indicate numbers on the bed contour lines on the map and provide the contour interval in the caption.

All recommendations that the reviewer outlines here have been agreed upon and changed. The plume names and lakes are now labelled on the map, and bed contours have been annotated with the contour interval (50 m) provided in the caption.

Page 20, line 3: What is "This water"?

Sentence omitted. Not needed.

Page 20, line 7: 'Water is not being stored in the snowpack and firn layer.' It is not likely that meltwater penetrates through snowpack and drains without storage in snow. Is there a possibility that snow cover was not accurately modelled, and in reality bare ice was already exposed in June?

The authors agree that it is unlikely that meltwater penetrates through the snowpack and drains without storage in the snow. This is what the authors were attempting to convey in this sentence, but obviously better clarification is needed. It is likely that either water is transported to the bed via crevasses or there is bare ice already exposed in the early part of the melt season.

The wording of the first sentence has been changed to better clarify that water is potentially bypassing storage and the paragraph has been changed to better encompass different scenarios for why this is occurring:

'This implies that water is bypassing storage in the snowpack and firn layer. The lower area of the glacier tongue is a heavily crevassed surface, providing abundant meltwater pathways to the glacier bed. It is likely that early-season melt production is directly routed to the bed in the lower region of the glacier tongue via these pathways. Equally, there is a possibility that snow cover is absent in June, and bare ice is already exposed in the early part of the melt season. Van Pelt and Kohler (2015) clarify that the model does not account for small-scale variability in precipitation and snow cover. For this reason, it is possible that water is being delivered to the bed earlier than the model anticipates.'

Page 20, line 15–16: 'sufficient pressure has accumulated to force a channel, or multiple channels, to open.' I wonder if high pressure can open subglacial channels. Enlargement/closure of a channel is the result of melting of the conduit wall and ice deformation due to isostatic pressure. This expression 'pressure accumulates and force a channel open' appears again and again (page 21, line 21 and 33; page 24, line 23). Please make sure if this sentence accurately explains processes in your mind. Is it pressure or meltwater which accumulates at the bed?

It is also possible that channel melting is also a key process at Kronebreen. However, it is difficult to distinguish in this study whether it is pressure or meltwater which accumulates at the bed. For this reason, a scenario where meltwater accumulation causes channel melt-back has been added to instances where channel opening is hypothesised.

Page 21, line 21: Sentence changed – 'It is likely that it is released either when sufficient pressure has accumulated to force a channel to open, or when subglacial water has sufficiently melted the cavity/conduit wall.'

Page 21, line 33: Sentence changed in a similar manner to previous.

Page 21, line 33: Sentence removed to avoid repetition.

Page 24, line 23: Sentence changed – 'Meltwater is released when a sufficient amount of pressure has accumulated to force a channel open and/or when subglacial meltwater has sufficiently melted the cavity/conduit wall.'

Page 22, line 11: What kind of glaciers are you referring to by "other tidewater glaciers"? This question is because water depth is usually far below sea level at the fronts of tidewater glaciers. Kronebreen is not a special case, I think.

The sentence was meant to highlight the key difference between subglacial water-pressure at tidewater glaciers (including Kronebreen) in comparison to land-terminating glaciers. It is clear that the sentence was not appropriately worded to convey this. This idea is also explained in page 22 line 27 – page 23 line 2. The sentence has been removed to avoid repetition.