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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 constructive response to our manuscript. Their attention to detail has been very valuable for addressing the key points outlined. The authors have taken time and care to respond to each of these points.

We have edited our manuscript accordingly, including the proposal of alterntive controls

on ice velocity, and revisions to Figure 5 and associated descriptions concerning the observed early-melt season speed-up event. Details of our responses to the individual comments are outlined below.

1. No attempt is made to investigate alternative controls on ice velocity apart from variations in subglacial hydrology. This is especially pertinent for the early season 'flushing event' which causes the up-glacier drainage of the supraglacial lakes.

The main focus of this manuscript is to examine subglacial hydrology at a tidewater glacier, and investigate its influence on glacier dynamics, including ice velocity. The authors felt that investigating alternative controls on ice velocity was beyond the scope of the study, and little data was collected to adequately examine other influences (i.e. calving dynamics and oceanic forcing). However, the reviewer rightfully emphasises throughout their comments that the exploration of other influences is important to presenting a rounded paper that is not weighted towards one set of influences. There is a risk that the reader could misinterpret that subglacial hydrology is the sole control on ice velocity.

For this reason, the manuscript has been extensively altered to better represent alternative controls on subglacial hydrology. A large effort has been made to better outline all alternative influences, especially in the interpretation and discussion sections where we begin to introduce explanations and ideas concerning the changes we see at Kronebreen over the 2014 melt season. We hope that this is reflected in the detailed responses to subsequent comments. To summarise here, the following alternative controls on ice velocity have now been included in the manuscript:

- Changes in calving activity
- Tidal influences

- Changes in fjord conditions (e.g. subsurface temperature)
- Basal frictional melting
- Ice thickness and shallowness

In particular, changes in calving activity have been more thoroughly explored as an explanation for the 'flushing event' that is observed at the beginning of the melt season.

These alternative controls have been outlined in the interpretation section (Section 6), explored further in the discussion section (Section 7) if needed, and acknowledged in the conclusion section (Section 8). Additional datasets (such as tidal data, calving activity, and fjord temperature) have not been included to examine these alternative controls within this study. The authors argue that too much focus on these aspects will detract away from the main focus of the paper which is to investigate subglacial hydrology and its influence on glacier dynamics. The authors wish to retain one of the main message of the study – that, in addition to glacier dynamics, subglacial hydrology plays a vital role in ice velocity at tidewater glaciers.

2. The borehole water pressure gradually decreases while ice velocity is increasing, which does not tie in with your explanations of ice motion being controlled ' by the location of efficient/inefficient drainage and the position of regions where water is stored and evacuated from' (pg. 1).

Reviewer 1 previously highlighted that the borehole water-pressure record may not have strong connectivity to the active drainage catchment of the glacier, based on similar observations to those made here. A gradual decrease in water-pressure at the borehole whilst ice velocity increases suggests that the borehole is effectively isolated from the main drainage system.

However, small, coinciding changes have been observed between water-pressure and

other observed signals for subglacial hydrology – for example, the observed pressure drop in the early-season 'flushing event' which coincides with the activation of the main plume and the drainage of the supraglacial lakes. Such changes indicate that the borehole is influenced by changes in pressure within the active drainage catchment. This is supported by the hydraulic potential modelling, which shows that the borehole is likely to be located within, or at least near to, an active channel network.

The manuscript has been changed to better convey these possible scenarios. Also, modifications have been made in Section 6, 7 and 8 (the Interpretation, Discussion and Conclusion sections) where arguments have been supported with evidence from the borehole water-pressure record. These have been made in an attempt to clarify that the borehole record may not be connected to the active drainage system.

3. The description of seasonal variations in ice flow (i.e. that the speedup is constrained to the southerly part of the near-terminus region) does not seem to be supported by the example velocity images shown. It would be useful to produce some plots showing relative changes in ice velocity, so that the reader can see the justification for the discussion.

During the 2014 melt season, sequential velocity maps from TerraSAR-X image pairs show an early-melt season speed-up which initiates at the terminus and propagates upglacier. The fastest velocities are seen in the southern/central region of the glacier tongue ($>$2.4 md^{-1}), whilst velocities in the north region generally are less ($<$2.4 md^{-1}). The high velocities subside by August. A second speed-up is observed in September, which propagates upglacier in a similar manner. The authors realise that the wording used to describe these events was misleading. High velocities are constrained to the southern/central region of the glacier tongue, not the speed-up itself. This has now been changed throughout the manuscript to better convey this, and hopefully this will make more sense in relation to the velocity maps presented in Figure 5.

It was challenging to convey all the information regarding the velocities in one figure, so three sequential velocity maps were chosen for Figure 5 that best represented the speed-up in the early-melt season. The reviewer's comment highlights that the images used in Figure 5, along with descriptions of the velocity event, did not effectively convey this. To rectify this, Figure 5 has been amended to include six sequential velocity maps to better show the nature of the early-melt season event. It is understood that plots showing relative changes in velocity would also convey this, however the authors feel that this is good opportunity to showcase more of the velocity maps produced from the CRIOS project.

4. Assertions made in the discussion should be backed up with data and results. For example the suggestion that tides influence the timing of plume pulses (but there are many other similar examples as detailed in the specific comments below).

Within the discussion section, events observed over the 2014 melt season are summarised and potential processes driving these events are proposed. Hydrological processes are largely discussed because subglacial hydrology is the main focus of the paper, and there is sufficient evidence from the data to suggest that these events are hydrologically driven to some extent. Other processes are also outlined, as recommended by the reviewer, such as oceanic influences and glacier dynamics (see the first comment in this response for more details).

The reviewer suggests that data (calving rate, tidal level, fjord temperature etc.) should be used to support assertions and explore these alternative processes in this study. The authors believe that this is beyond the scope of the study. The inclusion of other datasets would detract from the manuscript's primary focus on examining subglacial hydrology at a tidewater glacier. We intend to write a second paper at a later date which looks more closely at the dynamics of Kronebreen, specifically exploring calving dynamics in relation to oceanic forcing and glacial influences. We hope that the ideas

presented here are further explored in this future work.

We recognise that it is valuable to outline these additional influences though, as discussed in the first comment of this response. To address this, alterations have been made to the manuscript to emphasise where assertions have been made and where additional datasets are needed. In addition, each of these instances are stated as good ideas for potential work in the future. Specific changes are detailed in the subsequent comments.

Page 2, Line 3: Data are plural. 'data is' should be 'data are'.

Agreed. Change made.

Page 2, Line 16: This strikes me as a bit of a strange statement; what is a 'pressure environment'? Presumably you mean basal water pressure and effective pressure?

Here, 'pressure environment' is used as an encompassing term for basal water pressure and effective pressure. As this is not clear, the sentence has been changed for better clarification.

Page 2, Line 20: How rapid? Maybe better to specify several hours etc.

The rapid changes outlined here refer to processes such as abrupt channel collapse and extensive hydraulic overhauls in the subglacial zone. These changes commonly occur between 0 and 12 hours. This has now been specified in the manuscript.

Page 2, Line 23: Presumably the statement about 'inefficient evacuation of meltwater' relates to the fact that the basal hydraulic system at the terminus is not at atmospheric *pressure so the along-glacier hydraulic gradient is less steep? It would be worth clarifying this.*

The reviewer is correct, ‘inefficient evacuation of meltwater’ in the near-terminus area of a glacier is related to the area not being at atmospheric pressure. Therefore the along-glacier hydraulic gradient is small. To clarify this, we have included greater detail in the manuscript:

‘It has also been attributed to basal hydraulic systems which are not operating at atmospheric pressure, such as lake-terminating and tidewater glaciers, and there is an inefficient evacuation of meltwater because the hydraulic gradient is small (Sugiyama et al., 2011).’

Page 2, Line 26: In terms of land-terminating glaciers on ice sheets, this description is only really applicable to the marginal 20 km or so. Further up-glacier, maximum ice flow typically occurs later in the overall melt season (i.e. later than the onset of melting at the margin), once melt has commenced at higher elevations. It may be worth specifying that you are referring to land-terminating valley glaciers here?

The authors agree with the reviewer that the influence of basal water-pressures on glacier dynamics can be more accurately attributed to land-terminating valley glaciers. Change made.

Page 2, Line 28: What exactly do you mean by ‘subglacial drainage re-organisation’?

Subglacial drainage re-organisation refers to the changes in the network of channelised and distributed pathways that are typically observed at the beginning and end of the melt season. This has now been added to the text for clarification:

‘Ice velocities stabilise or fall later in the melt season in response to subglacial drainage

re-organisation (i.e. changes in the network of channelised and distributed drainage pathways at the beginning and end of the melt season) and the establishment of efficient channels that reduce water-pressure at the bed (Iken and Truffer, 1997; Hewitt, 2013).’

Page 2, Line 30: ‘Ice velocity records indicate similarities to land-terminating glaciers’. Do you have a reference for this statement? And do you mean the terminus region of tidewater glaciers of further inland?

This section has now been significantly changed to address one of the reviewer’s subsequent comments (second to last comment in this response) concerning the inclusion of the study by Doyle et al. (2015).

Page 3, Line 6: ‘The drainage of supraglacial lakes provides an additional meltwater input into the subglacial environment’. The use of ‘additional’ here is a little odd - additional to what? I assume you mean in addition to the drainage of surface meltwaters before they accumulate into lakes, but this is not obvious from the previous paragraphs.

The reviewer is right in assuming that by ‘additional’ we mean that the drainage of supraglacial lakes is an input of meltwater in addition to surface melt production that immediately enters the snowpack/englacial zone. A sentence has been added to the beginning of this paragraph to clarify this:

‘Meltwater typically enters the subglacial environment from the glacier surface via surface melt production. The drainage of supraglacial lakes provides an additional meltwater input into the subglacial environment.’

Page 3, Line 15: You need to make clear that this phenomenon has been observed in *the terminus region (last 20 km) of a single tidewater glacier in Greenland. As written, you risk suggesting that such drainage is as common or prevalent as the up-glacier progression of lake drainage with time, which is not the case.*

The authors agree that observations of lakes that drain in a down-glacier progression are uncommon in comparison to observations of upglacier-propagating lake drainage.

Changes have been made to the manuscript to clarify this:

'On the contrary, lakes have also been observed to drain in a down-glacier progression, albeit such instances are less common (e.g. Everett et al., 2016).'

Page 3, Line 18: 'terminus' should be 'near-terminus'.

Agreed. Change made.

Page 3, Line 16: What exactly are 'Subglacial transient pressure waves'? How do these control up-glacier progression of lake drainage? This requires more explanation and a reference.

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 fill/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.

Reviewer 1 and reviewer 2 have both 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.

Page 3, Line 21: What is 'the bed system'? Better to avoid these vague terms and simply describe what you actually mean, which you do in the next-but-one sentence. Also, I'm not quite sure what the point of this sentence is 'This has largely been studied in inland and near-terminus settings'. Are there other 'settings' from which such observations are missing?

The bed system refers to the subglacial basal dynamics in a large area of the glacier, in this case being at glacier margins. Bed system has been changed to 'bed' to be more direct. The sentence that states 'This has largely been studied in inland and near-terminus settings' has been removed from the manuscript.

Page 3, Line 23–24: 'long periods of time'. Be more specific - days, weeks, months, years, decades? I assume decades based on the Tedstone reference but this would not be obvious to someone unfamiliar with the literature.

In this context, 'long periods of time refers to decadal time frames, as correctly pointed out by the referee. The sentence has been changed to clarify this.

Page 3, Line 26: How do these 'long residence times' tie in with observed reductions in ice surface uplift and velocity about 24 h after a lake drainage?

The 'long residence times' are in reference to the water pockets that have been observed and modelled (e.g. Dow et al., 2015), whilst the ice surface uplift is in reference to how rapid lake drainage can cause ice surface uplift over much shorter timescales (seen by Stevens et al., 2013). The sentence has been re-written to better distinguish these two aspects:

'This has been observed to cause ice to uplift from the bed over short (0–48 hours) time periods (e.g. Stevens et al., 2013), and is likely to form subglacial water pockets or "blisters" with relatively high residence times over longer (days to weeks) periods (e.g. Dow et al., 2015).'

Page 3, Line 27: Would not injection of water into a distributed system or a small channel also be capable of causing uplift?

It is unclear what the reviewer is particularly questioning here. Yes, the injection of water into a distributed system or a small channel would indeed be capable of causing uplift. This idea has been conveyed with regard to below thick ice in the interior of an ice sheet (see previous amendment, Page 3, Line 26). It is assumed that the reviewer would also like this to be clarified at glacier outlets of ice sheets and near-terminus settings. Uplift has also been observed in these settings and this has now been added to the beginning of the paragraph where these settings are discussed (Page 3, Line 23):

'For instance, a rapid input of meltwater has been observed to cause localised uplift of the ice surface, and has also been observed to make a channelized system become more efficient...'

Page 3, Line 28: I think there are too many 'systems' mentioned in these introductory sections. It gets a bit confusing after a while.

The authors understand that this may confuse the reader at times. Where possible, the word 'system' has been replaced with an appropriate alternative in the introduction and background sections. There are still one or two 'systems' in relation to the drainage

system of a glacier, which is unavoidable, but the majority have been changed.

Page 4, Line 5: I think you need to note here that Slater et al (2017) used photographs from a time-lapse camera rather than satellite images. Also, time-lapse is also temporally intermittent - it is the frequency of that intermittency that is important!

Slater et al. (2017) used time-lapse imagery to distinguish plume activity, as did Schild et al. (2016) who used this in conjunction with MODIS satellite imagery. To better distinguish this, references to plume detection from time-lapse imagery have now been distinguished from those referring to plume detection from satellite imagery.

Time-lapse imagery provides high-temporal resolution data, but it is agreed that this is dependent on equipment that does not malfunction and, in some cases, re-visiting the field to download data. This means that, like satellite imagery, time-lapse imagery is also intermittent. This has now been acknowledged in the manuscript.

Page 5, Line 5: This sentence is convoluted: 'Kronebreen retreated 1 km between 2011 and 2016' would say the same more succinctly.

The sentence has been changed to convey that Kronebreen has recently undergone rapid retreat:

'Kronebreen is currently in a period of rapid retreat, having retreated 1 km between 2011 and 2016.'

Page 6, Line 7: The GPS position errors seem quite large. Why not post-process them using for example TRACK software? Also, it is not mentioned anywhere how these positional errors affect your measurements, of, for example, supraglacial lake area.

The camera sites were occupied for a minimum of 15 minutes, and post-processed using Trimble Business Centre software. The position errors are larger because of the lack of satellite passes in Svalbard (this is a problem we have each time that we survey our camera sites). Errors would likely be just as large even if they were post-processed using another piece of software. We have rectified this in future work by occupying camera sites for longer, to ensure that we gain adequate satellite coverage.

These positional errors introduce uncertainty into the measurements of supraglacial lake area and plume area. A sensitivity test was carried out on a subset of the supraglacial dataset to examine the uncertainty range. This was undertaken by simulating changes in the camera position, and examining changes in the lake areas based on georectification using these altered parameters. Changes in the vertical position of the camera ($\pm$1.92 m) introduce the greatest uncertainty as expected, with an uncertainty range of $\pm 5\%$. Changes in the horizontal position of the camera ($\pm$1.15 m) introduce an uncertainty range of $\pm 0.6\%$.

The authors believe that these errors are relatively small. The main aim of the plume and supraglacial lake areas is to show relative changes in two key signals of subglacial hydrology over the 2014 melt season. As shown in this manuscript, key findings are drawn from the timing of relative changes rather than absolute values. Therefore absolute values are not essential, and the authors argue that the uncertainty ranges calculated here do not need to be added to this manuscript. If the reviewer has good reason as to why they need to be included then the authors are happy to further discuss this.

Page 6, Line 10: ‘data was’ should be ‘data were’.

Agreed. Change made.

Page 6, Line 16–17: How did you avoid falsely identifying shadows and sediment-rich ice as lakes (both also have a high contrast with bare ice)?

The imagery went through a number of enhancements and processing steps to reduce the chance of misidentification:

- The images were masked to the specific area where lakes are visible. This reduced the chance of classifying other artefacts as lakes and also quickened processing time.
- The images are enhanced to better distinguish blue colours and ensure that ‘lake-like’ objects are distinguished. This filters out a large majority of noise. However, this also does mean that re-frozen lakes are often undistinguished (as clarified in the manuscript).
- These ‘lake-like’ objects are manually verified in PyTrx to filter out falsely detected lakes, such as shadows and sediment-rich ice, and only retain real lakes.

This information has been added to the paragraph to better inform the reader about PyTrx and the process of detecting lake from time-lapse imagery. Also, as outlined in the manuscript, sequential image sets with changes in illumination and shadowing are unavoidable. To counteract this, we detected lake surface areas in images from every half-hour and then took a moving-average of these measurements. This effectively “flattened” any noise introduced by these factors.

The manuscript has now been changed accordingly:

‘These lakes were automatically detected from images based on the high contrast in pixel intensity between the ice and water surface. This process involves multiple steps to reduce the change of misidentification: (i) the images were masked to reduce the chance of misidentifying lakes; (ii) the images were enhanced to better distinguish blue colours and ensure that 'lake-like' objects were distinguished; and (iii) these objects were manually verified in PyTrx to filter out falsely detected lakes such as shadows.'

Page 8, Line 32: Seems like a word is missing before 'which'. Could it be 'campaigns'?

The reviewer is correct, there is a word missing from the sentence. The word 'surveys' has been added.

Page 9, Line 8: 'but more central and nearer to the glacier's central flow line' should be 'but nearer to the glacier's central flow line'

Agreed. Change made.

Page 9, Line 21–23: Both sentences are pretty much repeated from the first paragraph of this section.

The authors agree that there is repetition in this paragraph. Repeated sections have been merged together into the first paragraph.

Figure 2: It would be useful to also put the dates along the top x axis.

Agreed. Change made.

Page 11, Line 6–7: Or more accurately, fill enough that water is visible in the TL imagery...

The authors agree that this is a more accurate phrasing and has thus been changed in

the manuscript accordingly.

Page 11, Line 22–23: 'Modelled melt production has a diurnal pattern with a maximum in the day and minimum at night' – I think this is well enough known that it is not necessary to report here.

Agreed. Sentence has been deleted from the manuscript.

Page 11, Line 27: The bracket after the units shouldn't be superscript.

Agreed. Change made.

Page 11, Line 29–31: Some repetition from previous paragraph re diurnal variations etc.

Whilst the authors agree with the reviewer that there is repetition, a broad description is needed to illustrate the dataset to the reader. For this reason, obvious notes about the diurnal pattern have been removed from the manuscript. This has been replaced with a brief description of the diurnal range that is visible:

'...coinciding with the drainage of Lake Cluster 1 and the activation of the meltwater plumes. From this point, melt and runoff regularly reaches 20–26 m^3 s^{-1} in the day and between 0–3 m^3 s^{-1} at night. Towards the end of August...'

Page 12, Line 6: It would be informative to outline these on the velocity plots in Figure 5.

The locations of two of the ROI's (the supraglacial lakes and the borehole site) are already included on both Figure 1 (the site map) and Figure 6 (the hydraulic potential modelling). The inclusion of these locations on the velocity plots in Figure 5 would merely be repeating information already relayed elsewhere.

In addition, Figure 5 already has a lot of information on it and the inclusion of the ROI locations would detract and/or obscure the information presented.

For these reasons, the authors have decided not to include the additional information in Figure 5.

Page 12, Line 16: Based on Fig. 2, it seems like the melt season lasts through until midway through September. I therefore do not think it is correct that the velocities were consistent for this period.

Agreed. This has been corrected to '...till the end of August...'

Page 13, Line 3: They definitely coincide (inasmuch as the temporal frequency of the TSX data allows), so do you mean 'possibly caused by'?

Agreed. The sentence has been changed to the wording suggested by the reviewer.

Page 13, Line 3–4: This is a bit vague. Ok, at the near-terminus centreline, velocities are higher in September than in the pre-melt season period, but at the other two ROIs they are broadly the same as before the melt season.

The reviewer is correct. Velocities remain high at the near-terminus centreline, but velocities from the other two ROI's remain constant. This has now been clarified in the manuscript in order to provide better detail:

'Whilst velocities remain constant at the lake and borehole ROI's through the rest of

September, high velocities persist at the centreline ROI and they do not return to pre-melt season conditions.'

Page 13, Line 7 : But despite this, there is actually very little variation in borehole water pressure (as shown in fig. 2). Perhaps the borehole is actually not that well connected to the regional basal hydrological system?

This was also remarked by reviewer 1. The authors believe that the introduction of this hypothesis early on in the results section would be inappropriate. The results section should be reserved for presenting data with broad descriptions to convey key observations. However, a paragraph has been added to the discussion section (7.4 Subglacial drainage of Kronebreen) to discuss this idea that the borehole may not be well connected to an active, efficient drainage system.

Page 14, Line 26: 'supraglacial lakes' should be 'supraglacial lake area'

Agreed. Change made.

Page 16, Line 11 (and throughout): 'upward-propagating' should be 'upglacier-propagating' (otherwise suggests vertical propagation).

The authors agree that the term 'upward-propagating' is vague and could infer vertical propagation rather than upglacier propagation which is the intended description. All references to 'upward-propagating' processes have now been changed to 'upglacier-propagating', including the title for Section 7.2 (Upglacier-propagating supraglacial lake drainage) and the several instances that the term is used in the interpretation and discussion sections.

Page 16, Line 18–19: 'This implies that meltwater is present at the bed and is enhancing basal lubrication '. Why is this necessarily the case? You should refer to another dataset - e.g. basal water pressure etc. to support this statement. What is to say that the acceleration is not due to a reduction in buttressing at the calving front?

The surface velocity of the glacier begins to gradually increase from 10 June, based on the velocities from the ROI's (the centreline, the region of the supraglacial lakes, and the borehole site). This is likely to be associated with the presence of meltwater at the bed, enhancing basal lubrication, based on observations of the on-set melt production and the filling of the supraglacial lakes. Assuming these lakes are connected to the bed at this point in time, these lakes are an indication of a build-up of meltwater in the subglacial zone.

Other causes for the speed-up are not explored because the main focus of the paper is to examine subglacial hydrology and its influences on dynamics at a tidewater glacier. Interpretation is limited to the datasets that we obtained in this study. It is unlikely that the acceleration is associated with a reduction in buttressing at the calving front because substantial sea ice rarely forms in Kongsfjorden given the year-round input of warm, saline Atlantic water. Marine influences (such as submarine melting and the seasonal increase in fjord temperature) could play a role in this speed-up, but it is beyond the scope of this study to fully explore this idea with additional datasets.

The authors believe that meltwater presence at the bed is a key influence in the speed-up at the beginning of the season based on the evidence we present in this paper. We understand though that different, alternative hypotheses should also be outlined here to convey to the reader that there are many potential influences on glacier speed-up events. Other influences (including marine influences) have now been outlined in this section as possible additional contributing factors. It has also been noted that it is difficult to examine these influences in this manuscript with the data available:

'There are several likely reasons for this. Marine influences could play a key role, such

as submarine melting and the seasonal increase in fjord temperature, which could be causing changes at the terminus that reduce back-stress further upglacier and enable glacier flow. It is difficult to examine these influences here with the data available, but could be better examined in future work. Another influence is the presence of meltwater at the bed, which enhances basal lubrication and enables sliding...'

Page 16, Line 21–22 : And all other glacier catchments assuming a normal lapse rate...

The authors agree that meltwater could have originated from other glacier catchments, in addition to higher elevation on Holtedahlfonna. This has been added to the sentence:

'This meltwater may have originated from higher elevations and/or other glacier catchments, but it is unlikely given that early-season melt production is understood to first originate from the lower elevations of this glacier catchment (Van Pelt and Kohler, 2015).'

Page 16, Line 23: So do you think that basal frictional melt is an important factor for accounting for the remaining meltwater at the bed?

Basal frictional melting could account for the remaining meltwater at the bed, given the little surface meltwater inputs predicted by the runoff model. Modelling attempts suggest that changes in basal friction is evident in the near-terminus area of Kronebreen, but this is not as strong an influence on surface velocities in comparison to changes in surface runoff. This idea comes from a paper currently in review, which models conditions over the same time period to our study:

Vallot, D., Pettersson, R., Luckman, A., Benn, D.I., Zwinger, T., van Pelt, W.J.J., Kohler, J., Sch¨afer, M., Claremar, B. and Hulton, N.R.J.: Surface changes influence on spatio-temporal variations of basal properties for Kronebreen, Svalbard, Geophys. Res. Lett., In review.

The reviewer has made a key point that basal frictional melt should be considered as an alternative hypothesis for why there is meltwater present at the bed in the early part of the 2014 melt season. The authors feel that it is appropriate to present this hypothesis in the discussion section (Section 7.1: Early melt season meltwater storage) rather than in the interpretation as this is where hypotheses are first introduced to explain this. A sentence has been added to provide the alternative hypothesis that basal frictional melting could also be a contributing factor to the presence of meltwater (Section 7.2, second paragraph, first line):

'This implies that water is either being generated at the bed, or it is bypassing storage in the snowpack and firn layer. Basal frictional melting could play a role in the generation of meltwater at the bed, but modelling of the Kronebreen's basal properties suggest that surface runoff is more likely to be the key influencing factor (Vallot et al., In review.) The lower area of the glacier tongue is a heavily crevassed surface...'

Page 16, Line 25–26: No, it only indicates that theoretically this is the expected route of subglacial water, not the configuration of the drainage system.

The presence of a surfacing plume has previously been associated with active channels, based on modelling and observations which suggest that plumes fed by distributed drainage pathways are less likely to surface in a fjord (Slater et al. 2017). Here, we also suggest that the presence of a surfacing plume infers that there is at least one active channel is active below the north region of the near-terminus area of Kronebreen.

The authors appreciate that the wording of the sentence may be misleading and the reader may gain the impression that a larger-scale drainage system is being inferred by the presence of surfacing plume activity. The sentence has been changed to better

describe that plume observations can be linked to the presence of at least one active basal channel:

'The continuous presence of a plume at the north side of the terminus (N1) suggests that a channel is established here from 25 June (Fig. 2C).'

Page 16, Line 30–31: Can you not easily quantify this from your hydropotential analysis? This would be a useful addition.

The reviewer is questioning whether the proportion and/or likelihood of meltwater routed through the north region of the glacier tongue can be quantified. The authors are unsure in how this could be usefully quantified. It is evident in Figure 6 that the channels on the north side of the terminus cover a significant portion of the Kronebreen/Holtedahlfonna catchment, and therefore it is logical to assume that they drain a large proportion of the glacier catchment. This section has been left unchanged for now, but we are happy to explore this further if the reviewer can specify how we could go about quantifying this.

Figure 6: Based on the size of the subglacial catchments (N bigger than S), why is the southern plume expression so much more extensive? What date is the plume extent from?

Plume activity was observed from one of the time-lapse cameras positioned on Collethøgda (camera site 1 in Figure 1). The plume/plumes on the north side of the terminus (N1, N2 and N3) were clearly visible from this camera position (as seen in Figure 5). However, the plume on the south side of the terminus (S1) was much further away which made it challenging to distinguish its shape accurately. Adequate surface areas could not be derived from the plume on the south side for this reason, as stated in the methodology section (4.1.2 Visible meltwater plume extent). Plume S1 activity could still be observed though, hence it was included in the plume activity timeline shown in Figure 2.

For the purpose of creating Figure 6, shapes representing the surface expressions of each of the plumes had to be derived from different days because there was never an instance where all plumes were active simultaneously. Listed below are the dates and times for each of the plume shapes shown in Figure 6:

- Shape for Plume N1 taken from 11/07/2014 08:00
- Shape for Plume N2 taken from 11/07/2014 08:00
- Shape for Plume N3 taken from 20/07/2014 10:00
- Shape for Plume S1 taken from 16/09/2014 04:00

An effort was made to acquire shapes for Plume N1, N2 and N3 from approximately the same period of time for use in this figure. Shapes representing the surface expression of Plume S1 were taken from the day that the plume expression was most apparent to ensure that we were exhibiting an accurate surface expression. This happened to be on a day where Plume S1 was significantly larger than the surface expressions for Plumes N1, N2 and N3.

The authors understand that this could mislead the reader to believe that Plume S1 is more extensive than the other three plumes. Information has been added to the caption of Figure 6 detailing the date-time of each plume expression, and to explain that the expression of Plume S1 is not representative of its expression over the entire melt season:

'Potential subglacial water pathways at Kronebreen, as calculated from a scenario where hydraulic potential is governed by ice-pressure gradients (i.e. the cryostatic pressure ratio, K, is above 0.6). The surface expressions for Plume N1 and N2 are

taken from 11 July 08:00, Plume N3 is taken from 20 July 2014 10:00, and Plume S1 is taken from 16 September 04:00. The expression of Plume S1 is, on average, smaller than the expression shown here. This expression was chosen because is is the most accurate shape of the surface expression that could be acquired during the monitoring period. The base map is a Landsat image (taken on 11 June 2014) overlaid with bed elevation and corresponding contours at 50 m intervals.'

Page 17, Line 3–4: Might this indicate that the drainage is linked to a perturbation at the calving front? For example, an acceleration and consequent longitudinal stretching related to a calving event or break-up of seasonal cover adjacent to the glacier?

This comment is related to ideas concerning whether calving is predominantly the cause or result of changes in glacier dynamics (i.e. acceleration/deceleration). Although we understand a considerable amount about calving dynamics, there remain uncertainties in this master-slave theory that need to be addressed.

In specifically investigating subglacial hydrology and glacier dynamics, the authors appreciate that the ideas presented in this section promote a scenario where glacier dynamics are the predominant cause of changes at the glacier front. The reviewer is right to point out that the change in glacier velocity (and the coinciding lake drainage) could also be caused by changes at the terminus (such as an increase in calving). By presenting this alternative scenario, we present a balanced outlook of the master-slave theory concerning calving, and also convey to the reader that there are many different scenarios that could explain the events observed in the early-melt season at Kronebreen (the lake drainage, the activation of the main plume, the drop in basal water-pressure, and the gradual increase in surface velocity).

A paragraph has been added to this section to better outline other scenarios concerning the events that happen at the beginning of the melt season, including a scenario where these events are linked to increased calving activity and consequent longitudinal stretching

Page 17, Line 4: 'upglacier-propagating'

Agreed. Change made.

Page 18, Line 8: What exactly does 'spatially discrete' mean in this context?

The sequential velocity maps show that this speed-up is confined to a specific area of the near-terminus zone. The phrasing 'spatially discrete' here refers to the confined nature of the speed-up to the south region of the glacier tongue. This phrasing has now been changed in the manuscript to better describe this:

'The sequential velocity maps show that this speed-up propagates $\sim$4 km upglacier between 31 May – 16 August. Surface velocities appear to be highest to the central/southern region of the glacier tongue, with some of the north region only reaching velocities between $\sim$1.6–2 md^{-1} (Fig 6).'

Page 18, Line 23: But if the plume is periodically visible, does this not suggest that the basal water is also purged periodically? Do you see a difference in velocity between times when the southern plume is and is not visible?

The reviewer is correct in suggesting that periodic visibility of plume activity on the south side of the terminus may be linked to periodic release of basal water. This is discussed in greater detail later on in the discussion section (7.3 Controls on meltwater plume activity). The reviewer is suggesting that we compare the plume activity on the south side of the terminus with the velocities to examine links between the two datasets. Links could suggest that short-term variations in glacier velocity may be linked to this hydraulic pulsing.

Adequate comparison between velocity and plume activity is beyond the scope of this study because of the temporal resolution of the datasets. As outlined in the manuscript, the plume on the south side of the terminus surfaces for short phases every 5 days on average. The velocities were acquired from TerraSAR-X imagery, with daily velocities averaged from feature-tracking between image pairs that were 11 days apart. Therefore we cannot examine short-term changes in velocity and relate these changes to plume activity.

Although we cannot investigate this idea within this study, a remark has been added to the discussion section (7.3 Controls on meltwater plume activity) to illustrate the potential for further work (Section 7.3, paragraph 3, sentence 5 and 6):

'This pulsing could be the cause of short-term changes in glacier dynamics in the near-terminus area, such as basal sliding and velocity. Although this idea cannot be further explored here, the examination of glacier dynamics in relation to plume presence could be a promising area for future studies.'

Page 20, Line 14–15: Could it not be that the volume of meltwater is just insufficient for the plume to either reach or be visible at the fjord surface?

The reviewer is correct in suggests an alternative explanation for the surfacing of the plume on the north side of the glacier at the beginning of the season. This has now been added as a possible explanation to the section:

'The activation of the main plume on the north side of the terminus (N1) suggests that either a sufficient volume of meltwater is being discharged to surface in the fjord, or an efficient system is established to evacuate meltwater on 25 June. If the second of these instances is true then meltwater is stored at the bed for $\sim$15 days before it is evacuated, based on the timing of the onset of the speed-up and the activation of Plume N1.'

Page 20, Line 18: 'upglacier-propagating'

Agreed. Change made.

Page 21, Line 15: 'unexpectedly absent during periods of high runoff'. Suggesting more distributed outflow of meltwater at the grounding line?

The authors agree with the suggestion that activity of the plume on the south side of the terminus is intermittent and unexpectedly absent during period of high runoff possibly because a distributed drainage network presides. This information has now been added to the manuscript:

'The activity of this plume is intermittent and it is unexpectedly absent during periods of high runoff, suggesting that the outflow of meltwater is not channelised and is instead more distributed at the grounding line. '

Page 21, Line 18: 'varies over only a small range' (it does vary).

Agreed. Change made.

Page 21, Line 22–23: 'The precise timing of each outflow is possibly controlled by marine dynamics such as tidal level.' Do you see any evidence for cyclicity on the frequency of tides? Otherwise, what evidence is this statement based upon?

Further exploration of the influence of tides on outflow is not presented here because it is beyond the scope of the study (see major comment 4 for more details on this). It has been emphasised that this passage is proposing mechanisms for hydraulic pulsing at tidewater glaciers and this could be an interesting focus for future work. A sentence has been added to paragraph to convey this:

'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.'

Page 21, Line 25–26: 'The trigger for the release of this water could be related to this hydraulic pulsing'. This seems a little too speculative.

As stated in the previous comment and in the major comments section, further exploration of the causes of hydraulic pulsing is beyond the scope of the study. Ideas are merely proposed here and it has previously been emphasised that these ideas provide motivation for future work. Two sentences have been added to the end of the paragraph to emphasise this:

'The trigger for the release of this water could be related to this hydraulic pulsing. This pulsing could be the cause of short-term changes in glacier dynamics in the near-terminus area, such as basal sliding and localised speed-up events. Although this idea cannot be further explored here, the examination of glacier dynamics in relation to plume presence could be a promising area for future studies.'

Page 22, Line 11: 'The key difference at Kronebreen, and other tidewater glaciers, is the high hydraulic base-level' should be 'The key difference at Kronebreen, and other tidewater glaciers, compared to land-terminating glaciers, is the high hydraulic base-level'

This sentence was drastically modified based on the recommendation of referee 1. No further changes have been made.

Page 22, Line 15–16: You should say why 'a stable drainage system cannot exist in *this region' presumably because the high velocities preclude the formation of persistent channels?*

Agreed. Sentence changed to:

'This permits fast flow, which precludes the formation of persistent channels in this region.'

Page 22, Line 24: What do you think causes the 'glacier-wide transient low-pressure wave that is initiated near the terminus', and what evidence do you have to support this assertion?

The chain of events recorded at the beginning of the 2014 melt season (the plume activation, upglacier-propagating drainage lakes, drop in basal water-pressure, and gradual increase in surface velocities) have been described in this section to set-up accessible comparison to other studies which examine subglacial hydrology at tidewater glaciers. Potential causes for these individual events have been proposed in earlier sections (i.e. Section 7.1: Early melt season meltwater storage, and Section 7.2 Upglacier-propagating supraglacial lake drainage), including alternative scenarios as recommended by the reviewer.

The authors feel that exploring causes of the chain of events here would merely be repeating what has previously been outlined. It would also detract from the main focus of this section of the discussion which is to compare the hydrological observations at Kronebreen with other studies, with particular interest in comparisons to other borehole records and comparisons to model simulations of subglacial hydrology. For these reasons, no changes have been made in regard to this comment.

Page 22, Line 29: What does 'This' refer to?

The structure of this paragraph is a little muddled. The opening two sentences of the paragraph have been changed to give better context for the reader.

'The observations from the borehole water-pressure record are strikingly different from borehole records in Alpine settings. These usually exhibit a diurnal signal, which reflects changes in delivery of meltwater to the bed and creates transverse hydraulic gradients that make meltwater pathways highly changeable (Meier et al., 1994; Hubbard et al., 1995).'

Page 23, Line 8: 'due to the difference in ice thickness'. Be more specific, it is not just the difference, but the fact that the ice is shallower that is key here. Also, you should state that the thinner ice leads to slower creep closure rates, meaning that channels are easier to open and maintain.

Agreed. Detail has been added to better specify why the subglacial drainage network can reconfigure on the north side of the glacier tongue:

'It is likely that the subglacial network can reconfigure because the ice is shallower and thinner, compared to large ice sheet outlets. This also means that channels to be open for longer because the thinner ice promotes slower creep closure rates.'

Page 23, Line 10–11: You should be clear that you are referring here to the mean melt rate for the entire submerged ice front rather than the localised melt rate (which is likely to be greater for more spatially-focused discharge).

Agreed. Change made.

Page 23, Line 13: I'm not sure I agree with this statement. From Fig. 5 it looks like there is also a speedup at the northern part of the tongue. Could you provide a relative *change in velocity map to evidence your assertion?*

Changes have been made to Figure 5 and related descriptions to better convey the nature of this speed-up event. For more details, see major comment 3.

For this instance, the sentence has bee modified to better describe the speed-up event:

'The seasonal speed-up observed at the beginning of the 2014 melt season shows that the highest velocities exhibited are within the central/southern region of the tongue of Kronebreen, as presented in the velocity maps in Figure 5.'

Page 23, Line 20: What exactly is mean by 'consistent' here? Spatially consistent (if so this is different to the results presented in this paper), temporally consistent?

The word 'consistent' refers to the temporal consistency in speed-up events year-on-year at Kronebreen. The word has been deleted from the sentence to avoid confusion.

Page 23, Line 27–29: This sentence seems contradictory - please clarify...

The authors agree that the first two sentences of this paragraph are convoluted. They have now been changed to better convey the idea that the 2014 melt season was abnormal for the dynamics of Kronebreen:

'We also argue here that 2014 is actually an abnormal year for the dynamics of Kronebreen, based on the observations of a speed-up at the end of the melt season (Luckman et al., 2015; Vallot et al., In review). It is likely that the speed-up at the end of the 2014 melt season was caused by an unprecedented high rainfall event that overwhelmed a subglacial drainage system in a late-season phase with low efficiency.'

Page 24, Line 9: It might be worth also referencing Doyle et al. (2015), Nature Geo-

science who saw a similar effect at a land-terminating glacier in west Greenland.

The authors agree that the study by Doyle et al. (2015) is a key comparable study, especially the similarities in late-season speed-ups which are likely to be linked to the timing of high-rainfall events. We believe that it would be appropriate to introduce this study first in Section 2 (Background) of the manuscript, and then discuss it in greater detail in Section 7.5 (Discussion: Implications for subglacial dynamics).

A sentence has been added to Section 2 (Background): 'Precipitation can disrupt drainage due to the high influx of water over a short period of time, in some cases causing speed-ups due to the timing of high-rainfall events in relation to a melt season (e.g. Doyle et al., 2015).' (Section 2, paragraph 2, sentence 4)

Two sentences have been added to Section 7.5 (Discussion: Implications for subglacial dynamics): '...here we propose that this speed-up was largely caused by an unprecedented high rainfall event. Doyle et al. (2015) observed a similar event near to the end of the 2011 melt season at Russell Glacier. They suggest that such speed-ups are amplified due to their late-season timing.' (Section 7.5, paragraph 3, sentence 3 and 4)

The citation has also been added to Section 8 (Conclusion) in reference to observed high-rainfall events and speed-ups in the late part of a melt season.

Page 24, Line 27: Or indeed that it is something other than meltwater that triggers this initial speed-up - e.g. a calving event, break up of sea ice etc.

Other alternative scenarios have now been discussed in the interpretation and discussion sections, as recommended by the reviewer. The authors believe that there is no need to re-iterate them in the conclusions, as this would be merely repeating ideas outlined earlier in the manuscript. No change made.