Response to Reviewer

Manuscript: Brief Communication: The Khurdopin glacier surge revisited – extreme flow velocities and formation of a dammed lake in 2017

Reviewer: V. Round

We greatly appreciate the concerns raised by the Reviewer and respond to each of them below. We have extended the discussion related to the processes and possible explanations and now provide greater detail in the discussion. We agree that we could expand even more on some details, but wanted to keep this study focused on this particular event and within the "Brief Communication" format.

Original comments by the reviewer are in bold, followed by our response. Note that the page and line number are always given twice, once for the document with markups which is provided at the end of the Response, and once to the revised manuscript without markups, which will be provided later.

P1, L12: 'during a glacier surge in of' remove 'in'

Thanks for pointing this out, it is now amended.

P1, L14: Does the 'fastest rates globally' refers to peak rates for surging glaciers or to glaciers in general? The use of 'm/a' in '5000m/a' could suggest that rate to be an average rate over a year. Using "m/d" may avoid this confusion.

This does indeed refer to surging glaciers, as there are other examples with similar or higher velocities such as the Lowell Glacier (Bevington and Copland, 2014) and the Variegated Glacier (Kamb *et al.*, 1985), however these extreme values (65 m d^{-1}) were measured just during a 2h rather than a daily interval.

We think that switching between units could be confusing and makes these values less easily comparable to other reported values in the region which are generally given in m a^{-1}. By referring to the month of May specifically it should be clear that this only refers to this period. However we refer to the importance of frequent image availability to derive such high velocities now on P6L7 / P4L39ff.

P1, L15-16: This sentence could do with some reworking. Firstly it isn't clear if the four year build up in velocity occurred over the whole glacier or just part of it. Also the term 'upper tongue' probably has little meaning to the reader at this stage.

We have reworded this part to increase clarity.

P1, L19: The 'however' at the beginning of the sentence implies contradiction of the hypothesis in the sentence before. Does the crevassing and disappearance of supra glacial ponds contradict the thermal switch mechanism, or reduce certainty in your hypothesis? I think these observations indicate a factor which amplifies the surge regardless of the initiation mechanism.

Thanks for pointing this out. We have amended this accordingly.

P1, L27: I suggest a slight rewording of the two general driving mechanisms, because the 'build-up of ice mass during the quiescence phase' applies to both mechanisms.

Thanks for pointing this out. We have adapted this accordingly to make it applicable to both.

P2, Section 2: There could be a few more details in the methods section here instead of only in the supplement. I would like to see at least an indication of the temporal resolution/number of images from Landsat and Planet (this is also missing from the supplement). Perhaps also spatial resolution (what is meant by 'high resolution') and/or indication of error margins.

We have now provided all details for the images used in the Supplementary as well as error ranges for the DEMs and how they were derived. Since the space for a Brief Communication paper is limited we do not want to go to great lengths in the methods section, as the approaches used are quite straight forward and well documented in the literature.

P2, L16: SRTM should be mentioned here too as it was also used for investigating mass changes.

The reviewer is right, this was an omission and we have added it in P2L26 / P2L19 of the revised manuscript.

P2, L23: Is this information about the source of the debris/medial moraine included because it is important to the glacier velocity?

The reviewer is right, this information – although interesting from a glacier flow point of view and the erosion potential of surge type glaciers – is not so relevant to this specific study. We have therefore omitted it in the revised manuscript.

P2-3, Section 3: I tended to get lost reading this section with its rather long chronological description of the three surges. One could present the results by describing the various phases of the three surges simultaneously. This could cut out some repetition and make similarities more apparent. Displaying this information about the temporal evolution of velocity as a figure would also allow the text here to be shortened and provide a very valuable summary and overview of the surges. Velocity over time could be shown for both the lower and upper parts of the tongue, as these show different behaviour, or better still for the whole length of the tongue.

We agree that this is a bit convoluted and have decided to specifically focus on the latest surge and leave the analysis of earlier surges to the already published work. We only refer to the similarities in behavior and we have included all velocity profiles of the surge in figure 1 and all velocity data in table S1 in the supplementary material, which should visualize the surge development in a more concise way.

P2-3, Section 3: Is the difference in peak velocities between the different surges, e.g 2000m/a in 1999 and 5000m/a in 2017, a real result or could it be an artefact of the temporal averaging period, where shorter periods are more likely to capture faster peak velocities?

Indeed it is quite likely that it's the more frequent availability of satellite images that makes it possible to only see these high peak velocities now. As the reviewer suggested above, this potential of Planet imagery should be further emphasized in the manuscript and we have discussed this in the Discussion at P6L7 / P4L39ff.

P3, L3: The advance of the 'surge front' is not clear to me. Quincey et al. (2011) show a very distinctive surge front at Kunyang Glacier but not for the 1999 surge of Khurdopin Glacier. Citing the surge front observed by Quincey et al. (2011) implies a similar acceleration pattern to the Kunyang surge. Perhaps the term 'surge front' is a bit subjective in this case. This is where a visual representation of the temporal changes, with more than three times steps, would be really useful.

Thanks for pointing this out. We agree that "front" may have been used too subjectively and we have rephrased it. As the reviewer suggested we have now added all velocity data from the surge to figure 1 which shows the development of the surge and also that the front advances slightly but that the peak of velocity actually remains nearly in the same position. The data therefore support earlier observations of Khurdopin without a clearly identifiable surge front (Quincey and Luckman, 2014).

P3, L9: The comment about not being able to discern length change is repeated in Section 4. I would expand upon it here and remove from section 4, or just remove it here.

Thank you for pointing this out, this is indeed redundant. As it fits better in section 4, we have removed it here.

P3, Section 4: DEM differences between 2000 and 2008 were calculated for Khurdopin glacier also by Gardelle et al. (2012), I think this paper is definitely worth consulting as they also focus on Khurdopin glacier for getting ablation rates. (Gardelle et al. 2012, Slight mass gain of Karakoram glaciers in the early twenty-first century, Nature Geoscience Letters, DOI: 10.1038/NGEO1450).

Thanks for pointing this out. We agree that this is important in this context and have added it in P4L10 / P3L22.

P3, L22: Was the mass change over the whole glacier assessed between 2000 and 2011, or just 2011 and 2016? Is there enough confidence in the results to give us a number for these periods?

The DEM differencing is for the period between 2011 and 2017, hence the total change in elevation is simply inferred. We have therefore adapted this and defined the possible range from dH = 50 m if we assume the tongue to have had a net mass change of zero in the build up phase to dH = 80 m if we assume that the net mass loss was equal to the quiescent phase.

P3, L26-27: This sentence makes is seem like there have been considerable damages in recent decades, but Iturrizaga (2005) shows most damages in the early 1900s. Is there another source showing more recent damages, or is it possible that the floods have become less severe or the settlements less vulnerable?

The reviewer is right that the more serious damages reported by (Iturrizaga, 2005; Hewitt and Liu, 2010) were reported before the 1960s. We have therefore removed this part and mentioned the level of damage in P4L35 / P3L39. It is difficult to say whether damages in recent decades have increased or decreased. While people may be less vulnerable today or better adapted to possible floods, infrastructure has also increased and people are more used to the fact that there is a road to the main Hunza valley. Indeed during the flood that occurred in 2017, one main bridge was destroyed and the road connecting the valley to the outside world blocked for a week (http://pamirtimes.net/2017/08/01/shimshal-river-flood-bridge-destroyed-road-damaged-cultivable-land-affected-at-several-places/). Unfortunately both discharge stations installed were destroyed during the flood, making peak flood measurements impossible.

P3, L29: The lake outbursts at Kyagar glacier discussed by Haemmig et al. (2014) were extremely rapid, jökulhlaup type events, not gradual as mentioned here.

Thanks and we have adapted the text accordingly in P4L37ff / P3L31ff. We have also added some comments on lake drainage, resulting from the actual drainage of the lake.

P3, Section 5: The potential lake volumes might have more meaning for hazard assessment than the surface area. I imagine this could be quite easily calculated given the DEM of the lake basin.

Thank you for the suggestion, we have now made volume estimates and discussed this in the paper.

To explain how the volume was derived we added a line in the Methods on P2L27 / P5L5 and we describe this in the supplementary material in section 3.

P3, L36: I assume the 15 meter height increase at the fringe represents the upper bound on potential lake depth. Is there any indication that this height will increase or decrease in the next couple of years and what factors might affect the likelihood of the lake reaching these various levels? Additionally, the 80 meter increase at the centre doesn't seem relevant for the lake.

We have improved the explanation and adapted the Text as well as the projected lake areas. 15 m are the approximate cliff height which presumably can act as the dam for potential future lakes. While water may pond beyond that on the tongue itself whether and how this could happen we do not know and we have therefore removed it. We now just use the former lake from 2000 as well as the ice wall as an indicator of possible future extents.

P3, L37: Do you mean the potentially large influx of subglacial sediments is into the potential lake basin? What effect would this have on the lake - decrease the potential volume of the lake?

The possible sources include the subglacial erosion of Khurdopin but also the sediment carried in from the Vijerab river. This would indeed decrease the potential volume of the lake. We have mentioned this now in P5L9 / P4L6.

P4, L2: Two surge periods is probably not sufficient to confirm 'a constant return period' especially over longer timescales, unless there are earlier indications of similar return period.

The earlier surges – if we can take the main floods from upper Shimshal as a proxy - happened in 1979, 1960, 1944, 1923, 1901 or 1904 and possibly 1882 which corresponds to return periods of 22 (19), 19 (22), 21, 16, 19, 20 and 18 years from the end of the 19th century until today (Hewitt and Liu, 2010). We have added this reference to support the claim made.

P4, L5: The observation of very different behaviour of the lower and upper parts of the tongue, separated by a steep part of bedrock at 12km, is interesting and has been observed on other surging glaciers (Quincey et al. 2014, Round et al. 2017). Possible questions to discuss here are whether there is something about the lower part of the tongue that leads it to experience such extreme changes in behaviour, or what the

significance of the steep section at 12km may be, or the significance of the avalanche mass deposits?

There is no data available on avalanche accumulation and we believe it would likely not be enough mass to argue for a considerable influence on a surge. However we now follow the discussion described in (Quincey and Luckman, 2014; Round *et al.*, 2017), which emphasizes the role of local topography into surge behavior.

P4, L12-13: Couldn't the increased pressure and 'tipping point' reached at the end of the quiescence also initiate the surge through collapse of the subglacial drainage system or failure of subglacial till? Is it possible to distinguish between these processes with the available data, or is there some other indication leading to the conclusion of a switch from cold to temperate basal conditions?

We have referred to both mechanisms, but with the data available we are not able to separate them. However, we have made an estimate of deformation contributing to the observed velocities, which shows that that during the surge it must be primarily the basal motion that dominates flow, exemplified by the low values for ice deformation (P5L27 / P4L22ff). During quiescence in the lower part the switch from cold to temperate could have a more sizeable contribution.

P4, L12-13: Do you mean this switch from cold to temperate based applies to the upper part of the tongue with the gradual acceleration, or lower part of the tongue with the sudden surge acceleration? Is it feasible that the velocities during the assumed cold based phase be purely due to ice deformation?

We have considerably revised this section and in particular have pointed out that the thermal switch hypothesis likely pertains to the steeper section and below only.

P4, L14: Quincey and Luckman (2014) suggested both the 'thermal switch' or 'subglacial drainage' as possible controls and didn't seem to have enough evidence to conclude one way or the other.

The reviewer is right that their findings were pointing not to one or the other specifically. We rephrased this and avoided the suggestions that the thermal switch is the dominating driver.

P4, L20: Did the velocity results show a parabolic velocity profile across the tongue during the quiescence? This wasn't mentioned in section 3 but would be interesting.

Indeed a parabolic profile was observed during quiescence and this is now mentioned in the text at P5L40 / P4L30.

P4, L21: The peak velocities of this surge are really incredibly high, as is the magnitude of the acceleration! A mention some of the feedback processes which could allow such extreme basal sliding velocities could be informative. Do you think subglacial till deformation plays much of a role?

We have now added a rough quantification of ice deformation following (Round *et al.*, 2017), which shows that while it may episodically important, basal flow is likely to be the main driver. These feedback processes are indeed an interesting topic to be investigated in regard to this extreme acceleration, but so far we have no access to any kind of data (or modelling like (Damsgaard *et al.*, 2015) to support such claims. We have however added a suggestion for future investigation in this regard in P5L3 / P4L34.

P4, L26: I'm not sure how the increased resolution and overpass frequency of the Planet satellite data have led to better understanding of the surge. Is it the ability to resolve the peak velocity over shorter time frame or observation of more temporal fluctuations or spatial patterns (e.g. transverse variation) in velocity? If so then this should be discussed somewhere.

The main advantage of these satellite images is the high overpass frequency which improves the temporal resolution. This we have now emphasized by showing all velocity profiles for the surge itself in Figure 1 and have additionally discussed this advantage in Section 4.

Figures 1 and 2: The right hand panels show the inferred glacier bed elevation, however it would make sense to also show the observed glacier surface elevation. Showing the surface elevation from the 2011 and 2017 DEMs would provide an additional visualization of the mass redistribution, and if shading or dashed lines are used the readability of the plot shouldn't be affected.

We have made this change accordingly.

Figures 1 and 2: The maps should be in some way georeferenced.
We have tried to add a grid to the maps, but since we use a rotated north inclusion of the grid makes things unclear. We believe that the glacier coordinates we now provide in the text and the use of the RGI outline provide sufficient georeferencing for the reader.

Figure 3: Very nice to have some photos from the ground, but maybe indicate the date (month)

Thank you for the suggestion, we have added the date.

Figure 3, L3: The traced 'centreline' would more appropriately be referred to as 'former centreline' or 'former medial moraine'

Thank you for the suggestion, we have changed it accordingly.

Figure 3, L8: I would say the tongue below the dashed green line "showed no change during the surge" rather than "remained stable".

Thank you for the suggestion, we have changed it accordingly.

Supplement Table S1: How many images were used from each Satellite? It would be interesting to have this information about the potential temporal resolution of the data.

We have now provided this information in Table S1 in the Supplementary Material.

Supplement Table S1 (DEM data): This table should be labelled Table S2.

Thanks for pointing this out, we have changed it.

Supplement Table S2: The SRTM from 2000 should also be shown here as it was also used for the surface elevation analysis.

Thanks, we have added this.

References

Bevington, A. and Copland, L. (2014) 'Characteristics of the last five surges of Lowell Glacier, Yukon, Canada, since 1948', *Journal of Glaciology*, 60(219), pp. 113–123. doi: 10.3189/2014JoG13J134.

Damsgaard, A. *et al.* (2015) 'A new methodology to simulate subglacial deformation of water-saturated granular material', *Cryosphere*, 9(6), pp. 2183–2200. doi: 10.5194/tc-9-2183-2015.

Hewitt, K. and Liu, J. (2010) 'Ice-Dammed Lakes and Outburst Floods, Karakoram Himalaya: Historical Perspectives on Emerging Threats', *Physical Geography*, 31(6), pp. 528–551. doi: 10.2747/0272-3646.31.6.528.

Iturrizaga, L. (2005) 'New observations on present and prehistorical glacier-dammed lakes in the Shimshal valley (Karakoram Mountains)', *Journal of Asian Earth Sciences*, 25(4), pp. 545–555. doi: 10.1016/j.jseaes.2004.04.011.

Kamb, B. *et al.* (1985) 'Glacier surge mechanism: 1982-1983 surge of Variegated glacier, Alaska', *Science*, 227(4686), pp. 469–479. doi: 10.1126/science.227.4686.469.

Quincey, D. J. and Luckman, A. (2014) 'Brief communication: On the magnitude and frequency of Khurdopin glacier surge events', *The Cryosphere*, 8(2), pp. 571–574. doi: 10.5194/tc-8-571-2014.

Round, V. *et al.* (2017) 'Surge dynamics and lake outbursts of Kyagar Glacier, Karakoram', *The Cryosphere*, 11, pp. 723–739. doi: 10.5194/tc-11-723-2017.