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Interactive comment on "Surge dynamics and lake outbursts of Kyagar Glacier, Karakoram" *by* V. Round et al.

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We would like to thank Christoph Mayer for his review. It is particularly useful to have this input given his familiarity with the surge of the nearby North Gasherbrum Glacier. In this reply we provide responses to each of his points and indicate the changes we intend to make to the manuscript.

There are only a few minor points I want to raise in order to hopefully improve the paper. Specific remarks:

P. 2, l. 1-3: I do not agree that the nature of glacier surging in High Mountain Asia is unknown. The mechanisms are described for different glaciers across the Karakoram and the Pamir. The recent collapse of the glaciers in Southern Tibet, just reveals that there is more to investigate about accelerating glaciers

besides the known surge phenomena.

Perhaps the statement was a bit of an exaggeration as there have been a number of detailed studies on surging in the region, so we have adjusted the text accordingly. By including the reference to the Tibet glacier collapse example we want to point out that there can still be 'surprises' related to glacier instabilities in the region.

"While surging glaciers in North America and Svalbard have been investigated in considerable detail, the large concentration of surge-type glaciers existing in the central Asian mountains, including the Karakoram (Copland et al, 2011) are relatively under studied. Improved understanding of surge glacier dynamics in this region can assist anticipation of glacier behaviour and hazard development in the future. The recent unprecedented collapse of two surging glaciers in Tibet (GAPHAZ, 2016) highlights the potentially unexpected nature of glacier instabilities in the region."

P. 3, l. 4-11: This is a truly interesting relation between GLOF and surge timing. If you state that GLOFs are generally linked to the active surge phases, it might be worthwhile to mention Hoinkes (1969) who describes one of the very few other situations where the GLOF occurrence is clearly linked to surge activity: H.C. Hoinkes, 1969, Canadian Journal of Earth Sciences, 6(4), 853-861, doi:10.1139/e69-086

Thank you for the suggestion, this was an interesting read and we shall include a brief reference to it at p.3, l.11:

"Recurring GLOFs linked to periods of glacier surging have also been observed for other surging glaciers (e.g. Hoinkes, 1969)."

P. 3 l.32: It is preferably to use "North Gasherbrum Glacier" in order to distinguish from "South Gasherbrum Glacier" which flows into the Baltoro Glacier system.

Thank you for the clarification, we will use the name North Gasherbrum Glacier.

P.3: There should be a note in the Introduction that the glaciers of the upper Shaksgam valley seem to be prone for surging, because apart from Kyagar and North Gasherbrum Glacier also Urdok Glacier clearly shows signs of former surge activity (e.g. Kotlyakov, 1997; Copland et al., 2011).

This is a good point which we will include in the discussion (rather than the introduction) p.23, l.34:

"The fact that at least three of the five closest downstream neighbouring glaciers also experienced surging (Copland et al. 2011; Mayer et al. 2011; Quincey et al. 2015) also indicates possible locational influences on surging, for example due to local climatic characteristics (Sevestre and Benn, 2015)."

P. 5, l.9: Is the monitoring station 600 m upstream, or 500 m as noted in the caption of Fig. 3?

This will be changed to "about 500m upstream" in both instances. 500m is the best estimate of the distance of the station from the upstream ice margin of the terminus, although it is an approximate value because the position of the terminus is not constant.

P. 6, l.23: What is the reason for progressively updating the master scene for the TanDEM-X data?

A different co-registration algorithm was used for TanDEM-X data, one which updates the master scene as an average of all previously co-registered scenes, to reduce speckle and temporally average snowmelt and glacier movement. We shall mention this:

"(. . .) for TanDEM-X, another co-registration algorithm was used where the master was updated progressively as the average of all previously co-registered slave scenes in order to temporally smooth out moving features (e.g. crevasses on glaciers) or strongly changing patterns (e.g. snowmelt)."

P.7: Are you sure that the lake is only formed during surge phases?

Not at all. In fact we are quite sure that a lake can also form when the glacier is not actively surging (e.g. Fig. 2, 2009), but surging causes larger potential lake size and more probable lake formation. Periods of larger and more frequent outbursts seem to coincide with periods of suspected surging (p.3, l.7). We don't intend to give the impression that the lake exclusively forms during surge phase. Our rephrasing of the caption of Fig. 4 should avoid giving this impression:

"Images from the observation station upstream of Kyagar Glacier's terminus from (a) before and (b) during the surge. The glacier, flowing from left to right, blocks the flow of the river and causes lake formation. The dashed line in (b) indicates the ice dam height from 2012 (a), highlighting the dramatic thickening at the terminus."

P. 9, 28/29: as the SAR system is a side-looking system, the baseline is perpendicular to the flight direction. Perpendicular to the line of sight might be misleading.

The total baseline, i.e. the distance between both satellites, can be composed into three perpendicular components: along track ($B_{||}$), parallel to line of sight (range offset), and perpendicular to the line of sight $B_{\perp}$). The two latter components form the across-track separation. We propose this slight reformulation:

"The phase gradient and hence the DEM accuracy depends on the perpendicular interferometric baseline $B_{\perp}$, which is the component of the distance between the two SAR satellites which is perpendicular to both the line-of-sight and the flight direction."

P. 10, 7-11. These two sentences are somehow describing the same thing. Maybe consolidate to one sentence.

There was some repetition which has been removed:

"The extremely rough glacier surface topography, with ice pinnacles up to 40 m high and 20-4 0m apart (estimated from shadow lengths and the observations from Haemmig et al. (2014)), caused strong decorrelation and phase wraps within the coherence

window of 15x15 m, meaning that DEMs could not be created over the glacier tongue with baselines $B_{\perp}$ < 200 m (HoAs below 20m)."

P. 10/11, l.31-35 and Fig. 5: A comparison of a sequence of dry to wet images during the onset of snow melt gives an indication of penetration depth.

That is correct. This was done to estimate the penetration depth at the onset of snowmelt 2015 as commented in the paper p.10, l.31-33.

A sequence of wet to dry conditions will not give the same results, because it is not possible to judge the snow height by remote sensing data independently. Why should a 2 m height difference between August and December indicate a 2 m penetration depth? Given that surface melt is terminated in August (no surface height change by melt and compaction afterwards), new snow on top of this surface will result in a higher surface elevation in subsequent TanDEM-X DEMs. The height difference in this case depends on the amount of snow and the snow humidity. Given that the entire snow column above the August level is dry in December, a 2 m elevation difference only indicates that there must be at least more than 2 m of snow.

We totally agree for the case when the elevation change is positive. We forgot to mention, that we observed a negative elevation change (see answer below).

Unless there is a dynamic effect during this period. If the penetration depth is actually 2 m, the snow depth needs to be 4 m in order to produce a 2 m elevation change in the DEMs, which is rather unlikely for the end of December.

An apparent 2 m height decrease between small baseline DEMs from Aug 2015 (Fig. 5a, wet snow, low backscatter) and December 2015 (Fig. 5b, dry snow, high backscatter) indicated a penetration depth of approximately 2 m into the refrozen summer snow ONLY IF elevation decrease from subsidence and compaction and elevation increase from snowfall are neglected for the four month period between the images. We have come to the conclusion that this is an unreasonable assumption and that it's true that we can't infer much about penetration depth from the image pair with such large temporal separation. We decided to only use the image pair with short temporal separation at the onset of snowmelt.

We therefore reformulate the whole paragraph from p.10 ,l.28 – p.11,l.3 as follows:

"Over the tongue of Kyagar Glacier, the backscatter intensity changed little between seasons (<5 dB), because infrequent snowfall means that the bare ice surface roughness dominates the backscatter signal from the tongue. Penetration is therefore expected to be negligible over the glacier tongue. In contrast, large seasonal changes in backscatter intensity indicate changing water content and thus varying penetration depths over the accumulation basin. Backscatter decreased by more than 10 dB at the onset of snowmelt in 2015 over the accumulation areas, and an apparent surface height increase of less than 2 m was calculated between two large baseline interferograms from before snowmelt (2015-06-02) and at the onset of snowmelt (2015-06-13). This indicates a TanDEM-X penetration depth of 2 m or less in dry snow conditions over the upper glacier. The relatively small penetration depths in the accumulation area might be a result of ice lenses formed by refreezing after strong melt events extending to over 6000 m a.s.l. in August, a phenomenon also observed by Dehecq at al. (2015). Figures showing the backscatter intensity changes are included in the supplementary material."

P. 16, Fig. 10: It might be a good idea to include the longitudinal profile again in the figure and indicate the distance along the flow line. This helps to relate the velocity profiles to the elevation changes.

We will add this to Figures 10 and 11.

P. 19, l. 15/16: Is there a reason for such large ELA changes over a short distance?

A possible reason for some of the difference in ELA between the three glacier branches could be differences in snow redistribution, wind drift, or precipitation, as temperature and radiation are likely to be very similar for all branches. However, the manual estimation of ELA from optical and radar images is rather subjective and the margins of error which we supplied in the manuscript were too low to reflect this uncertainty. After looking again at the ELA estimations is seems that an error margin of ±80m is more appropriate. We rephrase p.19, l.14-16:

“The equilibrium line altitude (ELA) estimated from the location of the snow line at the end of the ablation period observed from Landsat and TanDEM-X images, was 5350 ±80, 5400 ±80 and 5510 ±80 m a.s.l. over the western, middle and eastern branches, respectively.”

P. 22, l. 20-24: How does this relate to the fact that the summer of 2013 probably has seen the most intensive melt amounts, according to the PDD calculations? After such an ablation season, I would expect the drainage system to be very effective.

2013 did have higher melt potential in summer and autumn than the three years which followed, so one would indeed expect that an efficient drainage system would be more likely to form in 2013. Perhaps the gradual increase in basal sliding which took place over the two or so years before the surge hindered the formation of an efficient enough drainage system, despite possibly larger meltwater input in the summer before the surge started.

P. 23, l.4: A survey of existing photographs of Kyagar glacier back to the 1920s reveals that the surface of the glacier constantly is extremely rough and broken. This indicates that drainage of surface melt water into the glacier is rather effective.

Yes, the deeply pinnacled, crevassed surface is very important feature of the glacier and we also think this may assist vertical drainage. We have however noticed on the high-resolution Sentinel-2 optical images from 2016 (one of which we will add as a supplementary figure and is included at the end of this document), that some meltwater ponds form on the glacier surface between the pinnacles during summer, indicating that some meltwater at least is not well connected to vertical drainage channels (and does not percolate through the cold ice). However, we still think that there must be sufficient vertical drainage cracks/channels to allow meltwater to reach the glacier base. We will insert the following text on p.23, l.3:

"The seemingly extremely rapid response of surface velocity to the onset of surface melting indicates an efficient transfer of surface water to the glacier base which was in a critical state before the melt season started. The heavily crevassed surface, as observed during past expeditions (Mason 1928; Haemmig et al., 2014) and seen on remotely sensed images, may significantly contribute to the efficiency of vertical drainage. We note, however, that on some images supraglacial lakes are present on the glacier surface (Fig. 3 in supplementary material). This observation might indicate that surface water is not always connected with the subglacial drainage system despite of extensive crevassing. Based on the available evidence, we can however also not rule out the possibility that the supraglacial lakes are an expression of high englacial water pressures during the surge."

P. 23, l 24ff: this is also seen at other glaciers in the Karakoram. E.g. at North Gasherbrum Glacier also only the flat part below the ice fall is affected by the surge.

We add the following sentence at p.23, l.25:

"Surging confined mainly to the flatter, lower part of the glacier has been observed for a number other Karakoram surges (Mayer et al, 2011; Quincey et al., 2015)."

P. 24, l.14: as you already have calculated the PDD sums, this relates to a realistic degree day factor of about 9 mm/day.

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This is a nice comparison and a confirmation of the melt estimate, thanks for bringing it to our attention. We will briefly mention it when we present the PDD results, p.19, l.22:

"The melt rate at the terminus is estimated to be around 5 m a^{-1}*, according to the terminus surface elevation decrease during quiescence (Fig. 12) and the melt rate of icebergs left in the empty lake basin after lake drainage in 2009 (Haemmig et al. 2014). Combining this melt rate and an average of 552 PDDs annually gives a realistic degree-day factor of about 9 mm w.e.* $^{\circ}C^{-1}$ d^{-1}*."*

P. 24, l. 20ff: There is an interesting discussion about discharge amount and discharge seasonality in Ng et al., 2007. Climatic control on the peak discharge of glacier outburst floods, GRL, doi: 10.1029/2007GL031426

Thanks for pointing out another relevant reference. Ng et al. (2007) show that higher temperatures during GLOF events cause higher peak discharges through increasing meltwater supply rate and lake water temperature. This effect may also impact the peak discharge during GLOFs from Kyagar Glacier, but we think the most significant factor for GLOFs from Kyagar Glacier are the glacier surge dynamics and the properties of the ice dam (height, existence of drainage channels beneath the terminus etc.). We plan to mention this reference briefly as follows at p.24, l.25:

"Meteorological factors such as air temperature during the GLOF may also influence the peak flood discharge (Ng et al. 2007)."

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Fig. 1. Optical Sentinel-2 image from 27.05.2016 showing the presence of supraglacial lakes.

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