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## ***Interactive comment on “Glacier changes in the Karakoram region mapped by multi-mission satellite imagery” by M. Rankl et al.***

**Anonymous Referee #2**

Received and published: 17 September 2013

**OVERVIEW** This paper provides a variety of data on the velocity patterns, length changes and volume changes of glaciers across the Karakoram Himalaya. The derived datasets are impressive, but unfortunately they are currently poorly presented due to two fundamental problems: 1. No separation is made between surge-type glaciers versus non surge-type glaciers. This means that it is unclear as to whether the observed changes are due to some kind of internal glaciological process (i.e., surging), or are due to external climate forcing. Without this separation, the meaning and causes of the changes cannot be properly understood. 2. It is unclear whether this study is a methodological one about the use of SAR data, or whether it is a glaciological study about ice dynamics and recent changes. At the moment the focus is ambiguous, which results in the paper not doing justice to either of these tasks. The underlying data appears to be solid, so choosing one of these goals would really help to strengthen it (or

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even splitting the paper into two separate studies).

Without a central goal, much of the paper currently reads as a description of somewhat random facts and figures about Karakoram glaciers. The findings aren't particularly well referenced to existing literature (many key papers are missed, some of which almost exactly duplicate what has been done here – see details below), and few meaningful conclusions can be drawn from the data. The paper requires a complete reworking to put it in a publishable state. The detailed comments below address some of the major concerns, but the next version of the manuscript will need to be in a completely different form to be acceptable for publication.

DETAILED COMMENTS P4066, L2-L4: the statement here that 'advancing terminus position or surging behaviour' is rare for glaciers outside of the Karakoram is incorrect. Glacier advances due to surging are currently found in many regions around the world (e.g., Yukon-Alaska, Canadian High Arctic, Svalbard, Greenland), even though these regions are experiencing long-term negative mass balances and overall retreat. This highlights the need for the paper to distinguish surging from non-surging glaciers.

A native English speaker needs to review the text for language. There are several places where wording is awkward or difficult to follow. For example, line 6 in the Abstract states that 'changes are mapped in addition', and line 10 states that 'data allows to investigate', both of which are linguistically incorrect. The Conclusions are also not very well worded.

P4067, L5-9: the statement that 'glaciers in the Karakoram are displaying controversial behavior' needs to be better worded and described. How exactly is their behaviour controversial? You need to better describe what individual studies have found about mass balances in this region (e.g., provide specific numbers), separate from a description of surging glacier activity.

P4067, L20-22: wording here needs to be clarified. It's not the rapid advance of a glacier tongue during a surge that causes a GLOF per se. Rather, an advance of a

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glacier tongue can block rivers, which in turn can cause the formation of lakes. GLOFs then occur when these lakes are released, typically after the surge has terminated.

P4067, L24-28: the wording here is confusing. In some places (e.g., start of L25), glaciers are classified together whether they are surging or advancing, but in other places (e.g., L28) ‘advancing termini’ and ‘surges’ appear to be classified separately. This leaves the reader with a confused understanding of what the paper is trying to measure

P4068, L3-5: the statement that high surface velocities close to the glacier snout can offer a way to identify surging/advancing glaciers is only true for surging glaciers. Non surge-type glaciers can advance with little to no change in their terminus velocity (e.g., due to a decrease in surface melt rate). This text would also be more appropriate in the methodology, rather than the introduction.

P4068, L26: I don’t agree that ‘heavily crevassed icefalls’ are particularly abundant in the Karakoram. For example, it’s possible to walk along the length of many of the large glaciers (e.g., Baltoro, Biafo) without encountering any significant icefalls.

P4070, L3-4: In this sentence do you mean that initial outlines of all glaciers in your study were determined from the Randolph Glacier Inventory? As far as I know, the RGI doesn’t distinguish between surging/advancing and non-surging/normal glaciers, so you need to clearly state how you distinguished between them in your study.

P4070, L22-24: This sentence describes the fundamental problem with this study. You need to distinguish between surging and advancing glaciers to make any meaningful conclusions about the causes of their changes. The argument that Landsat imagery is insufficient to identify surge-type glaciers conflicts with numerous other studies that have used it both in the Karakoram (e.g. Copland et al. 2011, Barrand and Murray 2006) and elsewhere (e.g., Grant et al., 2009, J Glac, 55, 960-972). There is also excellent imagery available in Google Earth, for example, that can help with their identification, as well as high resolution Declassified Intelligence Satellite Photography of

the Karakoram since the 1960s. The existing inventory of Copland et al. (2011) can also assist with the identification of surge-type glaciers in this study.

P4070, L11: ‘treating them’ should ‘treatment of them’

P4071, L3: A discussion and/or analysis of the accuracy of the SRTM DEM in the Karakoram would be useful as there is the potential for the DEM to be quite inaccurate in areas of high relief. This is already partly mentioned on P4074, L25, but needs to be expanded upon and the influence of these biases on your derived terrain variables should be discussed.

P4071, L15: This methodology sounds very similar to that of Kienholz et al. (2013, J Glac, 59, 925-937), so that paper should be referenced here

P4071, L28: ‘this statistics’ should be ‘these statistics’

P4072, L6-L8: the wording here makes it appear that identifiable surface features (e.g., crevasses) must be present for your intensity tracking process to work. However, if this were true it wouldn’t be possible to determine velocities in most snow-covered, featureless accumulation areas. I would suggest rewording this section to make it clear that only a unique speckle pattern is required for this technique to work, but that this doesn’t necessarily equate to distinctive surface features visible to the naked eye (as already partly addressed on P4073, L5).

P4073, L20: more information about the error analysis would be useful, as this is crucial to provide confidence in the velocity results. For example, were errors consistent at different elevations? On slopes with different angles? In imagery acquired with different repeat cycles? A listing of the errors associated with each data source would be useful to add to Table 1, for example.

P4074, L12-L13: the wording ‘despite very less’ doesn’t make sense as written

P4075, L11: It would be useful to provide an assessment of whether any new surge-type glaciers have been identified in this study, compared to those identified in previous

studies such as Copland et al. (2011)

P4076, L10: References to many key papers concerning previous velocity mapping and glacier studies in the Karakoram are missing. For example, Jiang et al (2012) used ALOS PALSAR data to map the motion of many of the areas discussed in the present study: Jiang et al. 2012. Analyzing Yengisogat Glacier surface velocities with ALOS PALSAR data feature tracking, Karakoram, China. *Environmental Earth Sciences*, 67, 1033-1043.

...indeed, some of their figures almost exactly duplicate the ones shown in this study (e.g., Fig. 7a). Reference to Quincey et al. (2009) is also missing, which discusses connections between variations in mass balance short-term changes in velocity: Quincey, D.J., Copland, L., Mayer, C., Bishop, M., Luckman, A. and Belo, M. 2009. Ice velocity and climate variations for Baltoro Glacier, Pakistan. *Journal of Glaciology*, 55(194), 1061-1071

Reference to Scherler and Strecker (2012) should also be included: Scherler, D. and Strecker, M.R. 2012. Large surface velocity fluctuations of Biafo Glacier, central Karakoram, at high spatial and temporal resolution from optical satellite images. *Journal of Glaciology*, 58, 569-580.

Mayer et al. (2006) also discusses short-term variations in velocity: Mayer, C. et al. 2006. Glaciological characteristics of the ablation zone of Baltoro glacier, Karakoram, Pakistan. *Annals of Glaciology*, 43, 123-131.

And Ken Hewitt has recently published a book on Karakoram Glaciers that might provide a useful reference for background material: Hewitt, K. 2014. *Glaciers of the Karakoram Himalaya. Glacial Environments, Processes, Hazards and Resources*. Springer (see <http://link.springer.com/book/10.1007/978-94-007-6311-1//page/1>)

P4076, L20 to P4077, L23: The discussion of morphometric and environmental influences on glacier surging is unfortunately almost meaningless since surge-type glaciers

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have not been separated from non surge-type glaciers in this study. This also makes comparison with Barrand and Murray (2006) problematic, since they did properly separate surge-type from non surge-type glaciers in their study. Once surge-type glaciers have been separated, a more robust analysis of the controls on glacier surging could be undertaken using multivariate logit models such as those used by Jiskoot: Jiskoot, H. et al. 2000. Controls on the distribution of surge-type glaciers in Svalbard. *Journal of Glaciology*, 46, 412-422.

P4079, L9-L20: The velocities in this area have previously been mapped by Jiang et al. (2012), so that paper should be referenced here (as also mentioned above)

P4081, L12: Mayer et al. (2006) should also be referenced here

P4081, L15: It would be useful to compare these results to those of Quincey et al. (2009), who show somewhat similar long-term variations in motion for Baltoro Glacier. Climate reanalysis similar to that conducted by Quincey et al (2009) could also help to shed light on potential causes for the velocity variations (and other changes described in the paper)

P4081, L17: the discussion of volume changes here is entirely focused on one or two surge-type glaciers. The methodology suggests that volume changes over a larger area were calculated, so it would be useful to include this larger region in the discussion. In particular, this could provide a useful comparison with the findings of studies such as Gardelle et al (2012) and Kaab et al (2012), particularly if surge-type glaciers are distinguished from non surge-type glaciers.

P4082-P4084: the conclusions are very general, not very well worded, and completely lacking in references (even in places where other studies are mentioned – e.g., P4083, L18). Unfortunately most of the conclusions make little sense due to the failure to distinguish surge-type from non surge-type glaciers. Without this separation, the meaning and causes of any measured volume and length changes cannot be clearly discerned (i.e., changes in non surge-type glaciers are likely climate related, while changes on

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surge-type glaciers can be related to changes in both internal dynamics and external climate). The conclusions also suffer from the earlier criticism that it is unclear as to whether this paper is really a methodological study (2nd & 3rd paras), or a truly glaciological study (1st para).

Fig. 1: this figure is too small to determine exactly which glaciers are advancing. I would recommend including a table with the names and latitude/longitude of these glacier types so that their location is unambiguous. This table could also include their basic physical characteristics such as length, area, dates of advance, surge classification, etc.

Fig. 2: the red dots in this figure are too small

Fig. 3: the caption needs to be clarified to describe how surges were classified – i.e., was a surge only recorded for the year in which it started, or was it recorded for every year over which it occurred?

Fig. 5: This figure is too small to do the data justice. It would be better to break the map into separate regions and show those individually so that the velocity details for individual glaciers can be seen.

Fig. 7b: what do the numbers on these figures indicate? If they are glacier IDs, this information should be included in the table suggested in the comment for Fig. 1. This figure is also too small to clearly show the terminus variations over time

Fig. 8: it would be useful to separate the velocity profiles in this figure so that like is compared with like (e.g., summer vs. summer, winter vs. winter). At the moment everything is plotted together, so it's difficult to tell which changes might reflect long-term evolution versus normal seasonal variability.

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Interactive comment on The Cryosphere Discuss., 7, 4065, 2013.

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