

General comments

Rankl et al. used data from optical and SAR space-borne sensors in the Karakoram mountain range to (i) measure glacier length changes, (ii) provide the first complete surface velocity mosaic of all glaciers together with velocity variations for selected glaciers and (iii) document volume changes of a few glaciers. The quality and the diversity of the data presented in this paper is impressive and promising for the future monitoring of those glaciers on a regular basis. Unfortunately, the paper lacks a well-defined scope and the amount of new glaciological knowledge is low or, rather, these potential advances in our understanding of Karakoram glaciers are hidden in a long paper focused on technology/remote sensing. Right now, this paper is mostly “data”. Those data are needed but in the end, the reader is a bit disappointed. Some work is still needed to make this paper appropriate for The Cryosphere.

The authors need to clarify the scope of the paper. It is not clear if they are studying glacier advance/retreat in relation to climate change or if they are only interested in surge-type glaciers. The distinction between the two categories of glaciers (surging and non-surging) is unclear. The first sentence of the abstract is symptomatic of this ambiguity that persists throughout the paper. In this first sentence, glacier advance and surging behaviour are contrasted to the worldwide retreat of glaciers. There is no reason to contrast surge with worldwide retreat. The authors need to discriminate surging and non-surging glaciers. The same occurs a few lines later (L7-8): is there a difference between “surging glaciers L7” and “surging/advancing glaciers L8”? A glacier can advance without being in the active phase of the surge cycle. This ambiguity in terminology and in the classification of the glaciers persists throughout the text and it alters the proper understanding of the findings.

Some important references are missing. In particular the papers by (Scherler and Strecker, 2012; Scherler et al., 2011) are relevant for this study and their results need to be compared to the one of the present study. (Minora et al., 2013) is also relevant although not accepted yet (but the authors cited (Bhambri et al., 2012) which is also still in discussion).

Specific comments

ABSTRACT

P4066 L5. Not a single inventory but various are needed to study changes with time

P4066 L6. It would be good to also indicate the % of stable glaciers (within uncertainties) and the % of retreating glaciers. It was one shortcoming of the (Scherler et al., 2011) paper to have “stable and advancing” glaciers grouped. I recommend to have three categories (Advancing/Stable within uncertainties/Retreating). A non surging glacier whose length does not change over a decade or two does not have the same climatic significance than a retreating or advancing glacier, so there is no reason to place stable glaciers either in the advancing or retreating category.

P4066 L7. Surging behaviour is ambiguous. Using “the active phase of the surge cycle” would be clearer.

P4066 L16. "bi-static" not really necessary for the abstract

P4066 L17. Why only SAR? The present study used also other type of images (e.g., optical from the Landsat satellite).

INTRODUCTION

P4066 L22. (Church et al., 2011) could be replaced by (Gardner et al., 2013) because the latter paper has shown that the database of glaciological mass balance measurements (used in the Church et al. analysis) is biased toward glaciers with higher rate of mass loss. (Kaser et al., 2010) can also be cited regarding significance of glaciers for water resources.

P4066 L25. (Immerzeel et al., 2010) is of course a possible reference here but the authors will note that their 2013 paper (Immerzeel et al., 2013) is now drawing different conclusions than their 2010 Science paper... Rather than those papers, that speculate about future water resources in a region where model seriously lack observations on which they could be calibrated, I strongly recommend reading the brilliant introduction in the study by (Cook et al., 2013) on the question of water resources and stress in Pakistan. (Bookhagen and Burbank, 2010) is also a strong paper on the topic.

P4067 L5. (Gardelle et al., 2013) is more relevant than their 2012 paper for the Himalaya

P4067 L6. Not really controversial. There is rather now a good consensus that those glaciers are stable.

P4067 L9. "hosts" rather than "account for"

P4067 L25. "surging/advancing". Again this ambiguous mixing. For example, (Hewitt, 2005) have reported advance of glaciers that were not connected to the active phase of the surge cycle (and thus may be climate-driven).

P4068 L1. "Normal" is contrasted to "advancing". Not relevant. Better to have retreating and stable glaciers as other categories.

P4068 L2. "ice dynamics". No. A complete map of the surface velocity field.

P4068 L9. Not really a small glacier. Braldu is about 20 km long... Maybe provide the area of the three glaciers.

P4068 L22. "ranges" -> "is found"

P4069 L8. (Immerzeel et al., 2012) provide also useful information for the precipitation on Karakoram glaciers.

P4069 L10. A thorough analysis of climate change in the upper Indus Basin can be found in (Bocchiola and Diolaiuti, 2013)

P4069 L12. 'increase in winter elevation'. Rather 'increase in glacier surface elevation in winter'

P4069 L25. Kääb et al. did not find "slightly positive overall elevation changes in the region". They found a rate of elevation changes of -0.07 ± 0.04 m/yr when autumn data are used (their Table 1). So a slightly negative elevation change.

P4070 L12. Source of the SRTM DEM. Gap filled version of the DEM?

P4070 L20. Give the equivalent length. Rather than "1-2 pixels", cannot the authors select one of those two values (1 or 2 pixels) and classify glaciers as stable when their absolute length change is lower than this value.

P4070 L20-22. Clarify the unclear relationship between "focusing length measurements on glaciers larger than 3 km" and "the seasonal snow cover". Do the authors understate that larger glaciers reach lower elevations and generally show a snow free terminus?

P4070 L22. "In this study, we did not distinguish between surging and advancing glaciers": However it is really important for the significance of the present study to distinguish those two categories! Having them grouped alter the glaciological value of the paper.

P4070 L26. (Mayer et al., 2011) is also a relevant reference for this statement.

P4072 L14. I thought "speckle tracking" was different from "feature tracking". If I am right, can the authors clarify which one of the two techniques they used?

P4073 L19. Did the authors analyze mean offsets? Did they correct for any residual offset measured on the stable terrain? Is the error level determined by calculating the standard deviations of the offsets? Generally, clarify how the uncertainties are derived.

P4073 L27. Refer to Figure 1 for the location of those two glaciers.

P4074 L12. "despite very less" is unclear

P4074 L19. Not very clear how the authors managed to identify accurate GCPs from the 90-m resolution DEM on images at high resolution. Maybe clarify the procedure.

P4074 L23. Clarify at what resolution the comparison was made and how the DEMs were resampled. Comparing DEMs of different resolution is not straightforward (e.g., Gardelle et al., 2012a)

P4074 L28. "flat": did the authors use a threshold on the slope? And did the authors check for any mis-alignment of the DEMs before this comparison (e.g., Nuth and Kääb, 2011)

P4075 L2. Did the authors try to correct for the penetration of the C-Band signal? (Gardelle et al., 2012b) provide a curve of penetration with altitude in this area that the authors may use. The Gardelle et al. correction is probably not perfect but this is better than not

correcting at all and putting this in the error bar. If the authors do not correct for this penetration, then the elevation changes will be biased toward positive values.

P4075 L7. 900 not 0.9 kg/m³. And the authors also need to justify briefly their choice for the density (+ add uncertainties).

P4075 L11. How did the authors split the glacier complex in individual glaciers? Not explained in the Methods. Also they need to explain how they handled glaciers that are surging during various years. Counted once?

P4076 L2. This sentence was not clear to me.

P4076 L5. Figure 3 does not really illustrate such a marked increase. Can the authors compare the total number of surging glaciers during 1999-2005 and 2005-2011 to illustrate this large increase in surge activity?

P4076 L10. "Agree" rather than "correlate"

P4077 L2. "Strongly" rather than "slightly" because there are many more small glaciers than large glaciers. However, I can understand the reason for selecting the same minimum length to compare the two sets of glaciers. What is the size of each sample? 134 surging glaciers, right. How many non-surging glaciers larger than 3 km?

P4077 L4. If the median length is 9 km then it means that half of the surging glaciers are longer and the other half shorter... So "most of" is probably not appropriate. This part of the discussion is weak. The median size of non surging glaciers is smaller but the authors states that short glaciers are more likely to experience surges... the evocation of reaction time to climate is also not supported by anything. Suggest that surges are climate-driven which has to be demonstrated...

P4077. L22. "Thus": the causal relationship between the two sentences is not clear...

L4077. L25. No need to repeat the name of the SAR sensor, I think.

L4078. L12. Maybe refer to a textbook (Cuffey and Paterson?) to back up this statement.

P4079. L3. The figure does not really show a propagation of the surge front. It seems rather stable. Provide the uncertainties with the velocity so that the significance of the velocity change can be assessed.

P4078-79-80. The detailed description of the flow behaviour of individual glaciers make the paper very long. I am uncertain if it should be retained. At least probably reduced? Or kept for a further study at the individual glacier level to combine all the observations the authors have on a given glacier target? An example of such a detailed glacier analysis is in (Scherler and Strecker, 2012). Right now it is not clear what the authors want to show with those data. In the case of Batura Glacier, the authors have a nice time series of velocity fields but do not use it much. Again the main problem of the paper is the lack of scope. Those are great

measurements but the authors need to reach some glaciological conclusions from them. For example, one implication of this velocity time series is that comparing two snapshots of the velocities a few years apart (for example from 2006 and 2011 for Batura Glacier) may not reflect a “long”-term evolution of the velocity. A good temporal resolution is needed.

P4081. L17. Weak statement as is. Delete or explain why.

P4081. L25. Do the authors understate here that debris-covered parts experienced a high ablation rate? Was the tongue stagnant during the whole study period 2000-2012? If this is the case, the authors can estimate a rough ablation rate (or rather a lower bound value) as equal to the elevation change rate because emergence velocity can be neglected. That would be an interesting glaciological contribution in a context where it is not understood why debris covered glaciers are thinning at a similar rate or sometimes faster rates as debris free ones (Gardelle et al., 2013; Kääh et al., 2012; Nuimura et al., 2012; Zhang et al., 2013).

P4082. L1. Clarify why a “marked flow pattern” (what do the authors mean?) would be indicative of different melt rates between debris cover/free parts? Seems an interesting observation that need to be explained and demonstrated.

P4082. L10. I do not think the elevation gain in the highest areas are related to a surge. They could be due to uncorrected SRTM penetration or to the increased in accumulation. It has been described that surges often do not affect the whole glacier length but often initiate in the central part of the glacier (e.g., Quincey et al., 2011).

P4082. L12. Are those glacier-wide mass balances? **There are unrealistically positive!** (they would correspond to an glacier-average thickening of 40 m). Or are they average values only for the parts of the glaciers that are thickening? How did the authors compute the error bars? Need more details. Need to include uncertainty for the volume to mass conversion (not 0.9 but 900 kg/m³)... see (Huss, 2013) on this topic

P4082. L20. What about the possibility that the surge did not lead to an advance of the glacier front? Do the authors have some repeat velocity measurement of this glacier to analyze into details? This glacier, like Batura, could be the topic of a dedicated study. Right now it is a bit frustrating because all the data the authors potentially have are not merged to provide a synthetic view of the surge.

P4083. L7. “proved” rather than “proofed”, I think.

P4083. L19. Not convincingly demonstrated in the paper.

P4083. L22. Repetition of what was said above. I suggest to separate the methodological and glaciological conclusions of the study.

P4084. L7. The authors did not demonstrate the accuracy of the mass change retrieval (and they actually state it a few lines later in the conclusion). But I fully agree that the lack of bias and the ~3 m standard deviation on the stable region are very encouraging. The penetration will remain an issue though.

Table 2. I thought SRTM was flown during 10-20 Feb. 2000. Can the authors double check their date?

Figure 1. What is the source of the background image?

Figure 6. If possible, it would be good to have panel b and c side by side to facilitate the visual correspondence between the image and the velocity field.

Figure 7. In the text, I think the authors should mention that useful velocity data are obtained during all seasons; this is an interesting technical results. Any evidence from variation related to the season (spring speed up event)?

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