

Interactive comment on “2001–2010 glacier changes in the Central Karakoram National Park: a contribution to evaluate the magnitude and rate of the “Karakoram anomaly”” by U. Minora et al.

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General:

The study presents a glacier inventory and glacier area changes including the changes of debris cover derived from Landsat imagery for large parts of central Karakoram for the last decade. In addition, snow cover changes based on preprocessed MODIS data for a similar period and time series of temperature and precipitation from few available weather stations were analysed. The topic is relevant and timely as the abnormal behaviour of the glaciers in the Karakoram is not well investigated and understood. However, the study has several shortcomings which need to be improved before it can be

C1756

accepted for publication in The Cryosphere. The major issues are: - The methods need to be better described as already mentioned by the other reviewer (see more details below). - Reliability of the snow cover data need a better proof (e.g. how large is the effect of data gaps due to clouds). A better and quantitative analysis of the uncertainty of the data is needed. - Some more general analysis of the glacier changes (e.g. glacier changes vs. size or vs. mean and minimum elevation) and a more specific analysis of the surging glaciers would be interesting and would help to better understand the causes of the Karakoram anomaly. - Are the analysed temperature and precipitation data homogenized or was a test for homogeneity performed? - The discussion section is weak. Discuss more in detail your results (relation climate- snow-glacier changes taking into account the surges and glacier response time) also and relate your results more in detail to the available studies for the Karakoram and the neighbouring regions. - The manuscript would benefit from explicit conclusions in an own section.

Specific comments:

Abstract

P. 2892, L. 1f: Glacier melt water is of high importance for the upper Indus Basin/Pakistan but it is not known if it is really the main water resource. Please revise.

L. 9: Please include information about the mapped glacier area.

1. Introduction

P. 2892, L. 22: Please use HKH as abbreviation; it is more common.

L. 26: The 60.000 km² quoted from Kääb et al. (2012) is likely to high for glacier coverage as they also include possible perennial snow fields. The number provided by Dyrurgerov and Maier 2005 based on older inventories for the this regions is ~55500 km², Bolch et al. (2012) have 40800 km² for the Himalaya and Karakoram (without the Hindu Kush) based on recent inventories; the ice cover from the RGI for the KHK

C1757

region is ~50000 km².

P. 2893, L. 2: The “Third Pole” is not clearly defined but usually the regions includes the Tibetan Plateau and the surrounding mountain ranges such as the HKH ranges but not solely the HKH region. Please revise or omit.

L. 4: Please write “likely more than 50% of the water in the Indus...”. The more than 50% are a result of a well know study but are uncertain.

L. 8-13: You may refer here to the comprehensive reviews and studies of Bolch et al. 2012, Kääb et al. 2012 and Yao et al. 2012, Gardelle et al. 2013 and Gardner et al. 2013. Ageta’s papers are valuable but do not include the last decade(s).

P. 2894, L. 5f: You may refer here to Gardner et al. (2013) who also investigated the biases between the in-situ mass balance data and the remote sensing derived data.

L. 14: Please provide more details about the “Pamir-Karakoram Anomaly” if you mention it here. You may also discuss this in the discussion section.

L14ff: Please include also Bhambri et al. (2012) who also found no significant area changes and several surge-type glaciers in NW-Karakoram close to your study area. You may also refer to Hewitt (2011).

2. Study Site

P. 2895, L. 26: Please mention the date of the establishment of the park. “newborn” will not be true anymore in few years.

P. 2896, L. 1ff: The information about the park might be of interest for the general reader but should be shortened for this journal. You may refer to a reference or website for further reading instead.

L. 28f: Please provide more details about the unpublished data. Is it different from Winiger et al. (2005)?

C1758

P. 2897, L. 10ff: The last part of this section should be moved to and merged with the Introduction.

3.1.1 The CKNP glacier inventory

General: The information about how the glaciers were separated into single entities is missing but essential. The scientific community would be very much appreciating if you would provide the outlines with the valuable information to GLIMS.

P. 2898, L. 11: What kind of ID do you use? The GLIMS id?

L. 11ff: I suggest to mention the main attributes in brackets in a row and not with bullet points This is a bit waste of space. An explanation of ID, coordinates etc. is not needed. Describe in the methods section how the glacier length etc. was derived.

P. 2899, L. 16: It is of course nice that you refer to Bolch et al. (2010), but Frey and Paul (2012) would be even more appropriate here.

L. 17f: Please provide more details. Are the utilised Landsat scenes L1G or 1T? Mention this information earlier. Did you perform a co-registration or just tested the accuracy of the geolocation?

L19ff: This section is a bit confusing. Please describe clearly how you mapped the glaciers and the debris-covered parts.

3.1.2: Glacier outline and error assessment

P. 2901, L. 22: 7.5 m is fine if you delineated the glaciers manually but this in not clear.

P. 2902, 1st paragraph: Here mainly methods are described but you should mention how these errors were considered. The percentage of cloud cover of the entire scene is not relevant. Mention if glaciers are hidden by clouds (similar for Table 1)

3.1.3 Supraglacial debris-coverage

This section should be presented before the error assessment. Maybe even merge

C1759

with the information about the glacier mapping in section. You may restructure in 3.1.1 Glacier mapping, 3.1.2 Glacier inventory, 3.1.3 Uncertainty Assessment.

I assume you already mapped the entire glacier including the debris-covered parts. Hence, the debris-covered parts may be easily obtained with an intersection of the mapped clean ice. It would be interesting in this regard, if your supervised classification is more precise. The accuracy of the debris-mapping should be lower than for clean ice (see e.g. Paul et al. 2013).

3.2 Snow cover data

The inclusion of the information about snow cover distribution and trend is interesting. However, a more detailed evaluation of the quality of the data and a quantitative analysis of uncertainty needs to be included (e.g. with few comparisons to snow derived from Landsat). The major problem might be the data gaps due to cloud cover. The best would be to fill the gaps considering the topography and neighbourhood analysis (see e.g. Gafurov and Baradosy 2009) but at least a more detailed analysis of the influence of data gaps and misclassifications need to be included.

3.3 Climate data analysis

General: This section is a bit long and can be shortened and more focussed without loss of relevant information. This is mainly true for the description of the Mann-Kendal test. In addition, information about the homogenisation of the data should be provided.

P. 2905, L. 6-19. This is background information about the study area and should be presented in the respective section and not in the methods section.

P. 2907, L. 10-12. Too many references. 3 to 4 are sufficient.

4.1 Glacier changes during 2001-2010

General: The section is a bit lengthy and could also be shortened and more focussed. The parts where the own results were more in detailed related to other studies should

C1760

be moved to the discussion (e.g. P. 2911, L. 7-12). The uncertainty term should be presented without brackets.

L. 24: The uncertainty seems to be a bit small ($\sim \pm 0.4\%$) especially when considering the extensive debris cover. The uncertainty is usually around 2-3%. But this might be due to the fact that the mapped glaciers are quite large.

P. 2909, L. 1: ICIMOD, 2012 is not in the reference list.

L. 17: One reference should be enough for the Italian Alps.

L. 19: Should be Bhambri et al. (2011)

P. 2910, L. 12f: This is interesting information and should be discussed; especially as the median elevation is often used as a proxy for the ELA (see e.g. Braithwaite and Raper, 2009).

L. 29: Please present more detailed information (e.g. increase in length/area) about the glaciers advances.

P. 2911, L. 7-11: You may include some more recent studies (e.g. Bhambri et al. 2012, Bolch et al. 2012, Yao et al. 2012, Gardelle et al. 2013).

L. 13-28: The information about the advancing and surging glaciers is quite interesting and should be extended. Are there more glaciers which could be of surge-type but were in the quiescence phase during 2001-2010? More information about the "diffuse glacier advance activity" needs to be provided. Do only the larger glaciers show an area gain or also smaller ones?

4.2 Debris-cover changes during 2001-2010

The uncertainty ranges are reasonable.

P. 2912, L. 8f: Please provide a reference and discuss shortly the most important sources in this region.

C1761

L. 10ff: I do not agree that the debris-coverage is “likely another cause of the stable conditions”. There is extensive debris coverage in many other regions which show significant mass loss. Several recent studies have shown that the glaciers in the Himalaya are losing mass despite thick debris coverage (e.g. Bolch et al. 2011, Gardelle et al. 2012, Kääb et al. 2012, Nuimura et al. 2012)

4.3 Snow cover variability

The performed analysis is a bit thin and should be extended at least a bit. You may e.g. shortly mention the general characteristics of the snow cover and perform analysis with respect to the aspect and more elevation bands. Is there any chance to obtain a late summer snow line which can be related to the ELA? In the discussion you may also refer to Gurung et al. (2011).

4.4 Climate trends

The section is a bit descriptive. Please highlight better the main message.

5. Discussion and conclusion

This section needs to be significantly improved. Many parts of the discussion present results and are not a discussion.

P. 2915, L. 23: “terribly”: Please find a more scientific expression.

Tables:

Table 2: The “Slope” is not well described in the Results section.

Table 3: Please only use 1 digit for the Temperature and non for the precipitation. The available data is not so precise. The seasonal distribution of T and P would be of high interest.

Table 4: The caption is not clear. Again (and Table 5): One digit only.

Figures

C1762

Figures 1 and 4: Political boundaries are sensitive in this region. I would try to avoid showing them or at least include the remark that the boundaries are tentative only.

Fig. 2: It would be interesting to include the hypsography of the debris-covered areas.

Fig. 4: You should not repeat information which is or can be easily shown in the legend (“red line is . . . , yellow outlines represent . . .”).

Fig. 7: Include scale

Additional References which are not considered in the manuscript

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C1763

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