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The Cryosphere Discussions

# *Interactive comment on* "Region-wide glacier mass balances over the Pamir-Karakoram-Himalaya during 1999–2011" *by* J. Gardelle et al.

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This study provides for the first time a comprehensive estimate of mass balances for the Pamir-Karakoram-Himalaya region calculated based on DEMs from two different times. I fully agree with the first two reviewers (I could not read the third one as I am out of office for field work) that this is a very valuable and thoroughly conducted study. Nevertheless I would like to provide few additional comments and suggestions for further improvements.

The authors should be a bit more careful with their statement about the mass gain. Most numbers of mass gains are statistically not significant given the uncertainty es-



timates. The authors also state on page 997, line 14f that the results are, within the errors bars, in agreement with the study by Kääb et al. 2012. I fully agree with this statement. However, Kääb et al. (2012) report a slight mass loss. Hence, I suggest to write "possible mass gain" or similar (e.g. line 10 but also elsewhere) where the positive values are insignificant.

The authors use a constant value of 850 $\pm$ 60 kg/m$^3$ for the conversion of the volume change to mass change. A constant value is common and a reasonable assumption. However Huss (2013) shows the high variability of this conversion factor especially for shorter time periods. Another common approach is using a lower density for the accumulation area where assuming that the volume gain might not only be ice but mainly firn and snow. While this is problematic especially for the surge type glaciers as the ice flow is not considered it would be still valuable and interesting second scenario. This is especially true for the Karakoram and Pamir where the authors argue that the slight mass gain might also be due to increased precipitation and hence increased snow accumulation. In this case the mass gain in the accumulation area might be overestimated assuming a density of 850 kg /m$^3$ and the density of firn might be more appropriate. I would therefore suggest that the authors also provide an estimate of the mass budget of the non-surge type glaciers using a lower density in the accumulation regions and discuss the differences. The authors might be interesting to know that the mass balance measurements at Abramov Glacier were recently resumed within the CATCOS project. They might contact WGMS to obtain the results of the measurements. The balanced budget for this glacier during the last decade might be true, but nevertheless I suggest to double check the results. It might also be worth to mention in the text that the glacier is located in the northern most part of the Pamir.

The accuracy of the results of the study depends also on the accuracy of the glacier outlines. While the general procedure is well described and seems to be sound I am missing a more detailed analysis of the uncertainty. Especially the correct identification of debris-covered glaciers can be quite difficult in particular in regions where permafrost

might be present (e.g. Pamir). The correct identification of the accumulation areas for the common avalanche-fed glaciers in the Karakoram and elsewhere is also not straight forward and should be addressed more in detail. It would also be nice if the data from the GLIMS data base is cited correctly (how is mentioned in the downloaded file) because this data is of varying quality and from different analysts. P 984, L. 5: The ELA data presented by Yao et al. 2012 is based on the first Chinese Glacier Inventory and, hence, does not provide information about the recent ELA.

The Randolph Glacier Inventory has in this region indeed varying quality and was compiled based on different available data sets. While the glacier outlines in NW India and parts of the Karakoram are based on Bhambri et al. 2012 and Frey et al. 2012 and are of high quality, the quality is much lower in China and for other parts of the study area as mentioned in the RGI technical document. I suggest therefor to assign different uncertainty estimates for the different regions.

The figures showing the surface elevation changes are very interesting but partly hard to read. Maybe they can be enlarged slightly. In addition, it might be worth to try a slightly different colouring from with a more reddish colour for lowering and more bluish for elevation increase. I would also like to see figures (in the supplementary material) where the authors show the differencing results for the entire area (both glacier and non-glacier area) with the glacier outlines overlaid and also showing the data gaps. This will help the reader to better judge the results.

I appreciate the detailed comparison for the Everest area with the existing studies. As mentioned correctly my data (Bolch et al. 2011) have high uncertainties because I used an ASTER DEM with a lower accuracy for comparison. However, I would not state that the data do not agree. They do agree within the uncertainty and for some glaciers the values are even quite close. The authors should consider at least in this direct comparison the glacier ice which was replaced by water which I did in my study. As the authors did not so it is understandable that their estimate of the mass loss for Imja Glacier is much the much lower. I am also sceptical (though not impossible) about



the significant mass gain of the debris free Chukhung Glacier. I would also suggest to include the glacier size in Table A1. It would e.g. then be clear that the glacier with the highest deviation where I likely overestimated the mass loss, Duwo Glacier, is a quite small one.

I also request the authors to show a zoom of their results of the DEM differencing for the Everest area (showing the entire area like in my study). This would allow a much better direct comparison and maybe also help to understand the causes of the differences. A slight difference is also due to the fact that I used 900 and the authors 850 kg/m$^3$ for the volume to mass conversion. In addition, as mentioned by the anonymous reviewer but also the authors, the differences might also be explained by the slightly different time.

A minor point are the boundaries of the subdivisions in figure one. They should be a bit more precise in the figure and a better rational for this subdivision needs to be provided. At best they would consistent with Bolch et al. (2012) (where similar subdivisions are made, e.g. between the Himalaya and the Karakoram and West and Central as well as Central and Eastern Himalaya) so that the different studies can be better compared.

Referenes from above not cited in the manuscript: Frey, H., F. Paul, and T. Strozzi (2012), Compilation of a glacier inventory for the western Himalayas from satellite data: methods, challenges, and results, Remote Sens. Environ. 124, 832–843.

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