Reviewer#4, T. Bolch

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 estimates. 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.

> We agree and we now systematically replaced "slight mass gain" by "slight mass gain or equilibrate mass budget" everywhere.

The authors use a constant value of 850±60 kg/m3 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/m3 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.

> We also calculated all mass balances using two alternative density scenarios (900 kg m^{-3} everywhere and 600/900 kg m^{-3} see our response to reviewer#2) and now mention in the revised MS that the differences are below other sources of errors. We do not see any reason to exclude the surge-type glacier when testing the sensitivity of the mass balance to the density. We now mentioned that Abramov is in the northernmost part of the Pamir study site and we added a sentence in section 5.1 to state the SRTM penetration, calculated for the whole Pamir study site, may not be appropriate for a single glacier. No data are available yet for this glacier on the WGMS new web interface.

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.

We agree that glacier delineation is hampered by the presence of debris or avalanche-fed accumulation basins especially when working at the scale of the PKH. This is now clarify in the paper and two recent papers are cited (Frey et al., 2012; Paul et al., 2013). Following another comment by T. Bolch, we have now included a different uncertainty for the ice-covered area in each site that should better account for those difficulties. When the Hengduan Shan study site is excluded, we note a remarkable consistency between our ice-covered areas and the one from RGI 2.0. We have now added in the Supplementary Material all references from the GLIMS database.

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.

Ok, reference deleted

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 therefore to assign different uncertainty estimates for the different regions.

We now assign different uncertainties to the RGI for each study site: the difference between our inventory and the one from the RGI (given in Tab.1) is taken as the uncertainty of the RGI for the sub-region extrapolation.

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.

Figures will be provided in high resolution to the publisher and we hope he will print them in large size.
Coloring has been slightly modified.
Dh maps off glaciers: now included in the Supplement.

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.

> We have now explicitly written that ours and Bolch et al. values are not statistically different when error bars are considered.
> Indeed, we did not take into account the glacier ice that has been replaced by the lake, as this was stated in the submitted MS (Section 4.3). We now provide a new mass balance of Imja Glacier (-0.70 ± 0.52 m/a w.e.) that takes into account the volume difference of the Imja lake between 2000 and 2011.
> Note that the mass balance of Changri Shar/Nup Glacier has changed: the value in Tab. A1 of the submitted MS was only concerning the Changri Nup Glacier whereas the new value also includes the Changri Shar Glacier.

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.

> The glacier areas have been now included in Table A1. We agree that previous and also ours estimates are more uncertain for small glaciers. The case of Chukhung Glacier is now discussed in the MS.

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/m3 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.

> We included a zoom of the elevation change as Figure S1. The revised text indicates now that part of the differences between us and previous studies could be due to the use of different outlines and the difference in time periods.

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.

> The boundaries have been refined and should facilitate comparison to earlier works (see Fig. 1).

Referenes from above not cited in the MS: Frey, H., F. Paul, and T. Strozzi (2012), , Remote Sens. Environ. 124, 832–843.