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Dear reviewers and Editor,

Please, find enclosed a revised version of our manuscript (MS) "Region-wide glacier mass balances over the Pamir - Karakoram - Himalaya during 1999-2011". Upon request, we can provide a track-change version of the revised MS.

We thank all reviewers for their constructive assessment of our study. You will find below a point-by-point response to all comments.

We hope that these corrections/clarifications make our paper now suitable for publication in The Cryosphere.

Yours sincerely,

Etienne Berthier, Julie Gardelle, Yves Arnaud and Andreas Kääb

0. Summary of our responses to the reviewer's comments and major changes in the revised MS

0.1. New estimate of the mass balance for the Hindu Kush

In the submitted MS, the mass balance of the Hindu Kush was simply computed as the average of the mass balances of the Karakoram and the Spiti Lahaul. Meanwhile, we acquired and processed a pair of SPOT5-HRG imagery from October 2008 to derive a new DEM. At -0.12 m w.e. yr^{-1}, the newly computed mass balance is close to one from the submitted MS (-0.14 m w.e. yr^{-1}).

0.2. Seasonal correction

Building upon the reviewers' recommendations, we modified our correction of seasonality whose aim is to account for the fact that the SRTM and the SPOT5 DEM are not acquired at the same time of year. The correction was kept unchanged for western sites (from the Pamir to the Spiti Lahaul) but now its magnitude (0.15 m w.e./winter month) is backed up by measurements on Abramov Glacier (WGMS, 2012) and Chhota Shigri Glacier (Azam et al., submitted). New, the correction is set to 0 for the eastern sites (from West Nepal to Hengduan Shan) to reflect the fact that those glaciers are accumulating mass mostly in summer. This was suggested by the reviewers and is confirmed by recent measurements on two Nepalese glaciers where no winter accumulation and even some possible winter ablation has been measured using the glaciological method since 2007 (Wagnon et al., submitted). Our error bar of ±0.15 m w.e. per month is left unchanged for both eastern and western sites and should account for this limited knowledge of winter accumulation throughout the PKH. We decided not to use ICESat data from (Kääb et al., 2012) to better constrain this correction (e.g., comparing autumn/winter laser periods to infer the seasonal mass balance) because this would have involved further assumptions and uncertainty about the density of the material gained/lost.

0.3. Improvements to our methodology

Following a detail visual inspection of the maps of elevation differences between SPOT5 and SRTM off glaciers (in particular in the North –so in China- of our "Bhutan" study site), we identified some important local artifacts: bull-eye regions where elevation differences can, erroneously, exceed 50 m. The same bull-eye elevation differences where observed when the two versions of the SPOT5 DEM (v1 and v2) are compared and thus, those artifacts are attributed to the SPOT5 DEMs. Thus, a strategy to exclude those outliers has been incorporated by excluding from further analysis all pixels for which the elevation difference between v1 and v2 exceeds 5 m. About 20% of the non-interpolated SPOT5 pixels are now excluded. This new processing step reduces slightly the standard deviation of the elevation difference off glaciers and on glaciers but has very little influence on the mass balance for each study site (typical less then 0.02 m w.e./yr), probably because of the efficiency of our outlier removal procedure using the 3-sigma Gaussian filter. A paragraph has been added in section 3.1 to describe this new step in our processing scheme. All mass balance estimates have been updated: for each study site and for the whole region.

0.4. Uncertainties

The uncertainties have been revised:

(i) the error on the correction of the SRTM penetration is treated as systematic and equal to 1.5 m. This value was obtained by comparing our SRTM penetration estimate (C-band – X-band) to those obtained independently by (Kääb et al., 2012). See revised text, section 3.5.

(ii) A sub-region specific error for the total ice-covered area is now used to account for the fact that RGI v2.0 is a heterogeneous product and has a varying accuracy depending on the source of the data.

(iii) An error has been added to represent the uncertain extrapolation to un-surveyed glaciers within each sub-region. This error (0.04 m w.e. yr^{-1}) has been estimated using ICESat data from Kääb et al. (2012) by comparing the ICESat mass balance for a 3°x3° cell centered on our study sites to the entire sub-region ICESat mass balance.

0.5. Hydrology (Kaser et al. 2010)

We kept the comparison between two complementary glacier contributions to river discharge: the one from decadal glacier mass loss and the seasonally-delayed one (due to seasonal storage/release from the glaciers). We do not see any fundamental reason why those two estimates cannot be compared, and rather believe this comparisons adds to the current discussion of glacier contribution to river runoff, particularly important in High Mountain Asia. We have clarified the Kaser et al. approach, especially the fact that their method assume a balanced mass budget. See also our more detail response to reviewer#2 below.

0.6. Updated reference list

Knowledge about mass balance in High Mountain Asia is increasing rapidly. As much as possible, we added to our reference list some recent papers relevant to our study and modified the MS accordingly. In particular, we have updated the text with findings from (Gardner et al., 2013). A paper dealing with debris-cover effect on the South-East Tibetan plateau is also referred to (Zhang et al., 2013b). A paper showing mass storage in the lakes of the Tibetan Plateau is also cited (Zhang et al., 2013a).

We have included references to some recently submitted papers in the revised MS (Azam et al., submitted; Wagnon et al., submitted). We keep a copy of those papers and can share them with the editor/reviewer upon request but we will need to ask first for the permission of their first authors. If citing submitted material is problematic, we can also cite some unpublished measurements but we think that citing submitted papers is more useful to the reader.

Reviewer#1, Graham Cogley

General Comments

This paper presents geodetic measurements of glacier mass balance for eight SPOT5 scenes spanning from the northern Pamir to southeastern Tibet and dating from 2008–2011, the measurements being derived by subtraction of SPOT5 elevations from those of the Shuttle Radar Topography Mission in 2000. The measurements build on the highly successful and reliable earlier work of the authors in other parts of south Asia. They confirm earlier patterns of spatial heterogeneity, and extend the region in which mass balance is zero or slightly positive northwards as far as the northern Pamir. In general the mass-balance rates for 2000–2011 rates are rather moderate, and when extrapolated from the SPOT5 scenes to the region as a whole are only slightly negative. Ancillary findings include further confirmation that debris-covered glacier tongues are not thinning at unusually low rates (although the measured rates are quite variable from scene to scene); new calculations of the contribution of glacier imbalance to the discharge of the major rivers draining the Himalaya and Karakoram; and new details about the prevalence of surging among glaciers of the Pamir and Karakoram, which is illustrated quite strikingly in the authors' detailed maps of glacier thinning and thickening rates.
I am surprised at how few substantive comments I ended up with, and at how minor they are. This is a valuable and highly competent study that should be published rapidly.

Substantive Comments

P979 (General)
It might be helpful to draw attention to the anomalously negative balances reported (to WGMS) for Hamtah Glacier. I have been unable to find any description of how those measurements were made, and they affect regional estimates noticeably.

> The introduction has now been considerably rewritten and the comparison to existing glaciological record reduced. A detail discussion of caveats of the Hamtah Glacier record has been recently published (Vincent et al., 2013) and the present PKH paper is probably not the right place to repeat those statements. A copy of the relevant paragraph in Vincent et al. (2013) is provided below.

> The paucity of MB observations available to compute the HK MB averages gives a large weight to individual MB measurements, some of them being questionable. Indeed, due to a difficult access to the accumulation areas, it seems that some glaciers are probably surveyed only in their lower part (which is not always clearly mentioned in sources), making the glacier-wide MB biased negatively. This may be the case of the Hamtah Glacier, for which the MBs are strongly negative (Fig. 7). The field MBs are not consistent with our space-borne measurements. For this glacier, we measured a geodetic MB of -0.45 ± 0.16 m w.e. yr^{-1} during 1999–2011 (Fig. 5), whereas the glaciological MB was -1.46 m w.e. yr^{-1} during 2000–2009 (Table 1). Consequently, some of the ground-based observational data and thus the HK MB averages, are probably biased toward negative MB.

P982

L5-6 There are very small glaciers (especially in the Hindu Kush) with up to 100% debris cover, but they are on the way to becoming rock glaciers. Perhaps there is no need to mention them.

Thanks for the information. However, we did not mention those glaciers in the revised text as the scope of our paper is not to enter into this level of details. We only focused into individual glacier mass balance for the Everest site (for the sake of comparison with earlier work) and for a few selected, generally large, emblematic glaciers.

P984

L22 What is the "along-track angle"? The azimuth, as in "the azimuth of each SPOT5 ground track"?

We used the more precise terminology suggested.

P987

L6-11 It was worthwhile to include these two very large glaciers.

L15 The density of 850±60 kg m–3 was introduceed by Sapiano, J.J., W.D. Harrison and K.A. Echelmeyer, 1998, Elevation, volume and terminus changes of nine glaciers in North America, *Journal of Glaciology*, **44**(146), 119-135.

Sapiano et al. is now cited together with Huss (2013)

P989

L9-11 These decorrelation distances can presumably be thought of as typical valley half-widths. Were they different enough between the scenes for it to be worth tabulating them.?

They are quite similar from one site to another so we do not think it is necessary to tabulate them. See table below where they are given in pixels (one DEM pixel being 40 m).

	Pamir	Karakoram	Spiti Lahaul	West Nepal	Everest	Bhutan	Hengduan shan
d (pixels)	11	11	13	15	11	10	14

L18-21 Avoid repetition; say just "Given the slender observational support for the seasonality correction (section 3.3 (v)), we assume its uncertainty to be ±100%.".

Text revised

L24-26 Repeats material at P987 L14-16. The two should be merged, in one place or the other.

Done

P994

L19ff. "of thick debris". The findings discussed in this section add to a growing body of evidence that debris cover does not retard ablation as much as might be expected. However the discussion does not mention, as it could, the possibility that one reason might be that much of the debris is thin (or discontinuous at a scale finer than that of a sensor pixel).

The text has been modified to include this possibility and a reference to Zhang et al. (in discussion, http://www.the-cryosphere-discuss.net/7/2413/2013/tcd-7-2413-2013-discussion.html) has been added as this new study nicely confirms the proposition of G. Cogley.

P997

L6-7 This sentence is weak and could be deleted, especially since the periods compared differ by only by two years out of 10–12.

Agreed

L16-21 Say more clearly why the standard error of 0.08 at L16 has become 0.14 by L21, and explain the "100%" (0.14 is not twice 0.08).

In the revised paper, the mass balance of the Hindu Kush study site is now calculated with the same method as the other study sites (see the revised "Data and methods" section). This paragraph has been deleted.

P1011

Table 1 Although this is not the place to discuss it, the RGI overestimate of 88% for the glacierized area of the Hengduan Shan scene is remarkable and deserves further investigation. RGI version 2.0 is basically the (first) Chinese Glacier Inventory (1970s–1980s) in this location.

The paragraph describing Table 1 has been improved to reflect the comment by the referee, to indicate the source of the RGI in China and to also highlight the accuracy of the RGI elsewhere.

"We note the remarkable accuracy of the RGI for all our study sites. The relative errors are generally of a few percents and up to 12% for West Nepal. The differences between our and existing inventories are probably due to the difficulty of delimitating debris-covered glacier parts (Frey et al., 2012, Paul et al., 2013) and accumulation areas. The Hengduan Shan study site is an exception. There, the RGI is based on the Chinese Glacier Inventory (Shi et al., 2009, Arendt et al., 2012) and overestimates the ice-covered area by 88% compared to our Landsat-based inventory."

Stylistic Comments

Stylistic improvements suggested have all been included in the MS. We greatly acknowledge G. Cogley for making all those useful corrections. As non native English writers, we appreciated the time he took to clarify our language.

Reviewer#2

This paper presents an impressive and consistent new set of elevation change data for a significant amount of glaciers in the Himalaya-Karakoram-Pamir region based on a comparison of SRTM and SPOT digital elevation models. Thus, the authors provide a sound estimate of glacier mass balances (including their spatial variability) for a highly debated region. I fully agree with the first reviewer that this study is well performed, the methods and results are clearly described and that the article deserves to be rapidly published. Nevertheless, I have some more substantive comments that might require some additional discussion in a second version of the paper. These are not meant to criticize the presented results but might help the authors to refine some of their conclusions.

Substantive comments

- **Radar penetration correction**

The estimation of radar penetration depth by comparison of the different frequency bands is reasonable. However, it would be helpful to already provide the order of magnitude of this correction on page 985 (i.e. in the method description) to allow a judgment of its importance. This would be better than just referring to Gardelle et al. (2012b). Furthermore, I am not sure if it is given than the X-band (9.7 GHz) has no penetration depth at all. Obviously it is less than for the 5.7 GHz band, but the total effect is likely to be rather under- than overestimated with the correction. Wouldn't it be possible to dig deeper into this issue using GPR theory?

> Because Table 3 will be placed somewhere close to the paragraph, we do not think it is necessary to provide the order of magnitude of the penetration. We already stated in the text that it is of "several meters", probably sufficient.
>
> We agree with the reviewer that the non-penetration of the X-band has yet to be confirmed. It was clearly stated in (Gardelle et al., 2012): "*Clearly, this hypothesis requires further validation, especially by the German Aerospace Center (DLR) TanDEM-X mission*". In the revised MS, to make sure the reader is aware of this strong hypothesis, we write "*Since the X-band penetration is expected to be low compared to C-band penetration (an hypothesis that still needs to be confirmed; Ulaby et al, 1986), …*". The relevance of the SRTM penetration measured regionally for a specific glacier is also discussed now in the revised MS, section 5.1.
>
> We have now modify the calculation of the error estimate regarding this correction (see general responses)
>
> We do not think the present paper is the place to dig deeper into GPR theory. Part of the effort to dig into the GPR literature was made by (Gardelle et al., 2012) but was not really conclusive. Constraining the radar penetration using GPR theory may actually require some data (e.g., temperature, density profile, humidity content of the snowpack) that are hardly available at the time of the SRTM mission, especially in PKH.

- **Seasonality correction**

The quantification of the winter accumulation rates used for the seasonality correction is weak. One single value for the Karakoram is available from the 1980s. For all other glaciers the mean of surveyed glaciers in the Northern Hemisphere is used, i.e. winter accumulations in the Himalaya-Pamir region are quantified by including e.g. maritime glaciers in Norway... A better estimate could probably be achieved using almost every method (analysis of precipitation data, isolated data on accumulation rates from ice cores, etc). For glaciers with a summer-accumulation type (Himalaya), the winter accumulation rates are probably overestimated. The authors cover the large uncertainties in this correction with their error bars. Nevertheless, I suggest to try and get a more reasonable estimate that takes into account local characteristics.

See General response 0.2 where we built on the reviewer's comment to apply a different seasonality correction in the East and in the West.

We disagree with the statement that "better estimate could probably be achieved using almost every method". Precipitations are notoriously difficult to measure in the mountains and measurements in the valleys may severely underestimate (by a factor of 2-3, probably more when the stations are located in the dry valleys of the Karakoram) the accumulation on glaciers if not corrected for a realistic accumulation gradient with altitude that is glacier-specific (Immerzeel et al., 2012; Vincent, 2002). Furthermore, a single accumulation measurement (for example at an ice core drill site on a single glacier) even if available may not reflect the glacier-wide accumulation because of its high spatial variability (Azam et al., submitted; Machguth et al., 2006).

- **Off-glacier elevation changes**

Obviously, the off-glacier elevation changes between the SRTM and SPOT DEMs are an excellent mean to quantify the uncertainties. Numbers are provided by the authors (page 988, line 16) but are not further discussed. It would be highly beneficial to go into some more details here: Do the off-glacier elevation changes show an elevation dependence? Are there some significant differences between the study regions that might indicate regional biases? Are the off-glacier elevation changes equally distributed within one scene, i.e. are they consistent between the center and the edges of the scene? This discussion might be valuable to judge the spatial representativeness of the error bars.

In the revised MS, we have now:

(i) added a histogram showing the distribution of the elevation change off glaciers on each map of elevation change. By definition the mean difference is 0. The median and the standard deviation of the elevation difference are also given for each site.

(ii) shown the map of the elevation changes off glaciers in a supplement so that the readers can verify that there are very small spatial variations in the bias off glaciers (typically less then 1 m at length scale of a few kilometers). This is now briefly described at the start of sub-section 4.1.

Regarding a possible elevation dependence of the bias, this issue has been examined in detail in a previous study (Gardelle et al., 2012) where we proposed a specific correction which is used here and whose aim is to remove this possible bias.

- **Comparison of discharge to Kaser et al. (2010)**

The authors calculate runoff contributions due to glacier imbalance and compare these numbers to observed runoff in the main streams draining the study region. They acknowledge that only annual contributions can be quantified, and refer to Kaser et al. (2010) for seasonal contributions. I am troubled by this comparison and do not think that it is possible: Kaser et al. (2010) have based their analysis on global climate data sets but do neither include direct data on glacier mass balance nor runoff. The approach – and also the results – are thus inconsistent with the percentage contributions presented here. This probably explains the somewhat strange numbers given in Table 6: How can the seasonal glacier contribution (most probably the authors refer to the melt season here, although it is not stated) be smaller (!) than the annual mean contribution? This would require a better discussion of the results by Kaser et al. and more details on their approach, but I would just suggest to remove the comparison here as the methodologies are different and the numbers are rather worrying than helpful.

> This section of our paper was not clear enough and, thus, perhaps misunderstood. Importantly, we failed to mention that the Kaser et al. (2010) analysis was based on the assumption that glacier mass budgets are in equilibrium. This is important to mention because it allows comparison of their values with ours. This assumption also explains why the seasonally-delayed contribution can be equal to 0 (end-member case of a summer-type accumulation glacier where, every month, the accumulated snow melts away) and still, the imbalance contribution be different from 0. Thus, Kaser et al. (2010) and our study are measuring two different, non-overlapping, components of the glacier contribution to river discharge. The methods to measure these two components are fundamentally different (by necessity) but it does not mean that the final numbers cannot be compared. We hope that, with our improved description of Kaser et al. (2010), this section will be better understood. We stress however that our paper is not the place to describe in full detail the methods of Kaser et al. (2010). Overall, we believe the comparison of seasonal contributions and imbalance contributions can add important insight to the current discussion of glacier contribution to river runoff by separating the seasonal contribution (not related to climate change) from the (climate-change related) imbalance contribution. Due to the difficulty of summarizing these details in just 1-2 sentences, the decadal glacier mass loss contribution to river discharge is not mention anymore in the abstract.

- **Thinning over debris-covered ice**

The authors convincingly show that the thinning of debris-covered ice is not smaller compared to clean ice. This would be expected from the well-documented melt reduction below supraglacial debris. The authors interpret this observation with differences in ice dynamics. I have the impression that the comparison of elevation change rates over debris-covered and clean ice surfaces might be biased (explanations see below). Based on my comments the authors might consider adding some more discussion on this important issue. Surface elevation change rates at given altitudes within individual regions are performed. The approach of comparing identical altitudes only is sound and removes a possible elevation bias. However, do the authors also consider *glacier size / elevation range* in their evaluation? In my opinion, a direct comparison of elevation changes over debris-covered and debris-free surfaces is only feasible for glaciers that exhibit the same elevation range, and thus comparable ice flow dynamics. I would speculate that clean ice surfaces at low elevations (e.g. below 4000 m a.s.l.) are just found on smaller glaciers, i.e. glaciers with relatively high accumulation rates and low ELAs, and that almost all glaciers with a large elevation range have debris-covered tongues. This might lead to completely different dynamic responses of the two glacier types to climatic changes that make an immediate comparison of the dH/dt impossible. Furthermore, the statistical representativeness would also need to be discussed: How many data points for clean ice are available at low elevation in comparison to debris-covered pixels?

First, in the submitted MS, we did not interpret the differential thinning rates only in term of ice dynamics (if it appeared so then our writing did not reflect our understanding). We did not discard the fact that surface ablation could actually be similar or higher on debris covered surface due to some features, such as ice cliffs or lakes, enhancing ablation even when the debris cover is thick. We tried to make this clearer in the revised MS and, building upon G. Cogley's comments and a recent paper (Zhang et al., 2013b), we now discuss that those thinning rates can be explained by a debris cover which is, on average, thinner than the thickness threshold between enhanced/reduced ablation.

Furthermore, in the ICESat study of Kaab et al. (2012) similar thinning rates over clean and debris-covered ice are found by comparing neighboring pixels (average distance between them is 1 km), most of them likely located on the same glacier and thus with probably similar dynamic, on average.

The Everest study site, where thinning is higher under debris (a confirmation of a previous study by (Nuimura et al., 2012) who had not performed a histogram adjustment though) would be a very interesting place to examine in more detail the response of individual glaciers and the relationship between thinning rate and the % of debris coverage, the altitude range, the size of the glaciers and the role of differential ice dynamics. But we believe that this glacier-by-glacier analysis is beyond the scope of our analysis that has a regional focus.

Regarding statistical representativeness, we show below the number of pixels in the lowest elevation bin. Note also that the number of pixels is rapidly increasing when elevation is increasing. The total differs but the fact that, generally, >100 pixels are present in this lowest elevation band give us some confidences in the differences observed.

	Pamir	Karak. west	Karak. east	Spiti	West Nepal	Everest	Bhutan	Hengduan Shan
Lowest elevation	2900-3000	3000-3100	3300-3400	4200-4300	4500-4600	4400-4500	4300-4400	3300-3400
Nb. debris	1361	1190	196	279	818	1975	2183	716
Nb clean	81	192	123	132	111	553	142	125

- **Consideration of year-to-year mass balance variability:**

The study provides an extensive validation of calculated mass changes against previous studies. However, I miss a comparison to direct glaciological time series. I am aware that very little is available for the region and that the uncertainties are high. Nevertheless, annual mass balance time series (such as from Chhota Shigri Glacier, Azam et al., 2012) might provide some valuable information about year-to-year variability. Strictly speaking the validation of the period mean mass balances with other studies (covering slightly different periods) is only possible after removing artefacts coming from year-to-year variability. It is impossible to provide a sound correction based on the available in-situ mass balance data sets but it would be interesting to see a short discussion about the mass balance variability within the considered 11-year period and whether this variability might explain some of the disagreement with other studies. The present results mostly give smaller mass losses (Fig. 5, Table A1). Could this observation simply be explained by above average mass balance in the last years (i.e. after about 2008) that are covered by this, but not by the other studies (Bolch et al., 2011; Nuimura et al., 2012; Kaaeb et al., 2012; Berthier et al., 2007)?

By definition, the geodetic method does not provide the mass balance variability during the study period, but only the cumulative mass balance. As stated by the reviewer, it would be illusive to attempt a region-wide correction of the mass balances measured over different periods to make them exactly comparable. Following the reviewer's advice we have now included a full paragraph to make clear that mass balances should be compared with care when they do not span the same time period. To our knowledge, Chhota Shigri Glacier is the only peer-reviewed (Azam et al., 2012; Vincent et al., 2013) annual mass balance record that can be used to estimate this year-to-year variability for most of the first decade of the 21st century. The inter-annual variability from this record is now quoted in the text, together with the one from Abramov Glacier (WGMS, 2012). We also justify in the revised MS why we do not compare with more field mass balance records.

Specific comments

• page 976, line 15-17: This sentence is difficult to understand in the abstract. Following my substantive comment above, I recommend omitting it or replacing it with another important conclusion.

We omitted the sentence and now concentrated the last two sentence of the abstract on a comparison to global glacier mass balance.

• page 977, line 15: A short definition of the glacier imbalance in the present context would be helpful.

Reworded to avoid "imbalance", "decacal mass loss" is used instead.

• page 984, line 3: The ELA digitized from Landsat images corresponds to this one given year and might show a considerable variability. This might need to be acknowledged in a sentence and/or some references could be provided to back up the assumption of a constant ELA.

We agree that this is a strong assumption and that ideally regional ELA should be measured throughout the study period. This is now better reflected in the text (see revised section 3.2). As suggested by T. Nuimura, we added the standard deviation of those ELAs.

• page 986, line 13-15: The seasonality correction would only be lower by 1-2 orders of magnitude than the cumulative signal if the mass balances are significantly different from zero. With the balanced conditions in the Karakoram and the Pamir the uncertainty in this correction might well make the difference between a positive and a negative mass budget.

True, our statement was only valid for the study sites where the mass balance is clearly negative. The statement has been removed.

• page 987, line 16: The density assumption might require some more discussion as it linearly influences the final results. Will the density of volume change be the same for all regions although they exhibit strongly different mass balances?

> We now describe two alternative density scenarios (i) 900 kg/m^3 everywhere (Sorge's law) and (ii) 600 kg/m^3 in the accumulation area and 900 kg/m^3 in the ablation area. The same two density scenarios where used in Kaab et al. (2012) and in earlier papers. We have estimated the maximum difference between our preferred scenario (850±60 kg/m3 everywhere) and those two others scenarios. For the eastern sites with negative mass balances, the maximum difference is small, at 0.03 m w.e./yr. For the western sites, the maximum difference is higher, at 0.06 m w.e./yr. Those uncertainties, due to the choice of a given density scenario, remain low compared to other sources of errors. Those alternative density scenarios are now included in the revised text (section 3.5)

• page 990, line 6: Elevation changes averaged over the ablation area might be mistaken as mass balances / melt rates. I see the benefit of discussing these data here but I would suggest to clearly state the meaning of ablation area elevation changes and their limitations.

> This section is now shorter and discusses rates of elevation changes in the two zones to avoid that the reader interprets them as melt rates. We have also tried to better highlight some of the important pattern revealed by these maps (e.g., no elevation changes in the accumulation zone of all the eastern study sites).

• page 991, line 25: Are there any explanations for these strong differences in the mass balance of neighbouring glaciers? Whereas the authors discuss mass balance differences between the regions in detail (in connection with climatic patterns) the glacier-to-glacier variation in mass balance (which can obviously be significant) is not addressed.

> First, we stress that we did not compute the mass balance for each individual glaciers on each study site but simply singularized out some emblematic glaciers (Siachen, Fedtchenko, Baltoro, Rongbuk), some glaciers that are followed in the field or some glaciers previously observed using the geodetic method (e.g., in the Everest area). Examining systematically glacier-to-glacier mass balance variability would require an additional, non trivial, step which consist in splitting the inventory in individual glaciers. A multiple regression between glacier mass balance and topographic parameters/debris coverage such as perform by Huss (2012) would certainly be an interesting next study. But we think that this is beyond the scope of the present paper that is dedicated to regional assessment.

• page 993, line 22: Interesting. Can these numbers be put into physical context? i.e. do they correspond to a penetration into the winter snow coverage only, or also into the uppermost firn layers?

> Penetration can reach up to 7-8 m in the upper reaches of Karakoram for example, so the radar signal probably go deeper that the annual snow layer. However, given that there is a complete lack of knowledge of the winter accumulation for most (if not all) these study sites we do not think it is possible to really put these numbers into a physical context. To do so, it would actually require some measurements (snow pits and if possible more than one given the 90 m SRTM pixel size) in mid-February 2000 at the time of the SRTM mission.

• page 997, line 8: Here and elsewhere: unit: m yr−1 w.e. Wouldn't it be more logical to write it as m w.e. yr−1 ?

Changed everywhere. Indeed more logical.

• page 999, line 8-22: Although interesting I was not quite sure if this paragraph is actually necessary for the results / conclusions of this paper. The topic is only loosely related and it could be removed.

Agreed and deleted (the same suggestion was made by T. Nuimura).

• page 1001, line 4: I think, most importantly high-elevation precipitation measurements would be needed. And weather stations in these environments probably have troubles in accurately determining precipitation. So, a sentence might be added that direct measurements of accumulation on High Mountain Asia glaciers would (also) be required to understand to ongoing processes.

We fully agree with the referee. The statement was changed and a reference to (Azam et al., submitted) is added as an example of the sort of studies that are needed to better estimate annual accumulation and understand interannual/decadal trend in both summer and winter mass balance.

Reviewer#3, Alex S. Gardner

Summary

Reviewer 1 and 2 have already provided very thoughtful evaluations of this work so I will try to keep my comments as brief as possible. Firstly, I think this is excellent work that adds greatly to our knowledge of recent glacier changes in the PKH, a subject that is somewhat controversial. The authors are very knowledgeable in both the methods and the study region. The work directly builds on, and greatly compliments, their previous work and is of good quality and of significant interest to The Cryosphere readership.

I do, however, see some room for improvement. In particular I agree with many of the more substantive comments of Reviewer 2. Having skimmed the article a month ago before finally finding time to complete my review, I identified most of the same points of concern as identified by Reviewer 2. I also found the quality of the writing a bit lacking, which I attribute to the native language of the author not being English. I would recommend that the first author work closely with his co-authors to improve the writing, particularly the abstract and introduction.

> Although they are more experienced at writing papers, co-authors are not necessarily better than the first author for writing and, in fact, all authors already worked closely on the MS before submission. We have now included all stylistic comments from G. Cogley and we also tried to improve the abstract and the introduction. If the editor or one of the reviewers thinks that the writing is still too weak and that it alters the readability/understanding of our MS, we will make sure it is proof-read by a native speaker. Also, all papers accepted for TC are subject to professional copyediting before publication.

General

See general comments by Reviewer 2.

I would only add that the calculation of uncertainties should be revisited.

> See General answer 0.4 to see how the calculation of uncertainties was modified.

Specific

Units: my personal preference if for SI units of kg m^{-2} yr^{-1} over m yr^{-1} w.e. but it ultimately boils down to personal choice.

> m w.e. yr^{-1} is used everywhere.

P976 – 15-17: Provide more context for this sentence.

> The sentence about contribution to water resource is now deleted from the abstract.

P976 - 4-7: The wording of this sentence could be improved
P976 – 20-23: I bit difficult to follow with all the directional references
P976 - 10-12: The wording of this sentence could be improved

P976-16 to P979-1: Three numbered lists in a row. Try to rework some of the lists into full paragraphs.

The introduction is now much more focused and considerably revised.

P977 –20: add "SLE" and provide area covered by study Done
P977 – 26 "shrinking rates" -> "rates of retreat " Done
P978 – 10: delete "obvious" Done
P978 – 14: replace "point-wise elevation" and with altimetry Done
P978 – (ii) provide a reason why the geodetic method is a good alternative.. It's obvious to me but may not be for other readers. Section reworked
P978 – i to iv: could better describe each method and their respective strengths and weaknesses This section has been reworked and we hope that it now clarifies the advantage/disadvantage of each method.
P978 – What about repeat gravimetry (GRACE)? Gravimetric method now added
P978 – include assessment of interannual variability, probably the more valuable measure that you can get from the in situ records. Inter-annual variability from glaciological record is now provided at the start of the discussion.
P980 – 6: How do you "extrapolate"? Linear interpolation? We assign to the unmeasured glacier area of a subregion the mass balance measured over the corresponding study site.
P980 – 23: "melt water" -> "meltwater".. can change throughout Done
P980 – 23: what about the basins on the northern sides of the mountain ranges? The PKH, as defined in this study, is not hydrologically connected to the basins of the Tibetan Plateau except for the northern part of the Karakoram which flows into the Tarim basin. This is illustrated in fig.1, which displays the outline of the major basins. Tarim is not added to the list of the basins.
P982 – 12: In general, the writing for the intro could use some improving. Deeply rewritten and hopefully, improved.
P982 – 17: "comes along with" -> "is provided with" Done.
P983 – 1: what method was used to resample the SRTM? Bilinear, now specified.
P983 – 5: delete "over the whole study site (Hengduan Shan, Everest and West Nepal), the"
P983 – 13: Can you make these outlines publically available either through GLIMS or RGI? We are already in contact with Bruce Raup and Anthony Arendt to share those outlines. They are also available upon request to the corresponding author.
P983 – "(before the Scan Line Corrector failure in 2003, which used to compensate for the forward motion of the Landsat 7 satellite, and results in a _ 20% data loss within a scene after 2003) -> "(prior to the failure of the Scan Line Corrector of the ETM+ sensor onboard Landsat 7 that resulted in image striping)" Done
P984 – (ii) does this cause error in the "relative elevation" as the image geometry is incorrect when doing the bundle adjustment? Not clear to us what the reviewer means here by "relative elevation".
P985 – 11: I would not use the word "value" as the "value" does change… maybe "does not impact the utility of validity of the correction. We just kept "validity"
P985 – (iv): For reasons already articulated by Reviewer 2 this section could use some more work. See response to Rev #2, our general responses and revised text, section 3.5.
P985 – (v): Why not use Kaab's estimates of seasonal elevation change? I agree with Reviewer 2, this correction could be improved or maybe just add it to the uncertainty. See general response 0.2. We already allowed for a conservative uncertainty for this correction.

P986 -20-22: Why would they bias your results? You are binning by elevation. It would (very slightly because they cover a very small percentage of the total glacier area in each site) bias the final mass balance of the study site if only their ablation/accumulation areas were sampled. It is now explained in the revised MS.

P986 – I don't know about the separation of surge type glaciers, as long as you have regionally proportional sampling then it should all average out. Do you anticipate surge-type glaciers to exhibit a regionally averaged dh/dt that differs from non-surge-type glaciers? When binning by elevation and 3-sigma filtering of outlier, we assume dh/dt to be homogeneous within each elevation bin. This assumption no longer holds when surge type glaciers are included in the binning process because they exhibit different dh/dt than other glaciers. If we did not separate them, their real dh/dt would be excluding when applying our 3-sigma filtering technique (Figure R1).

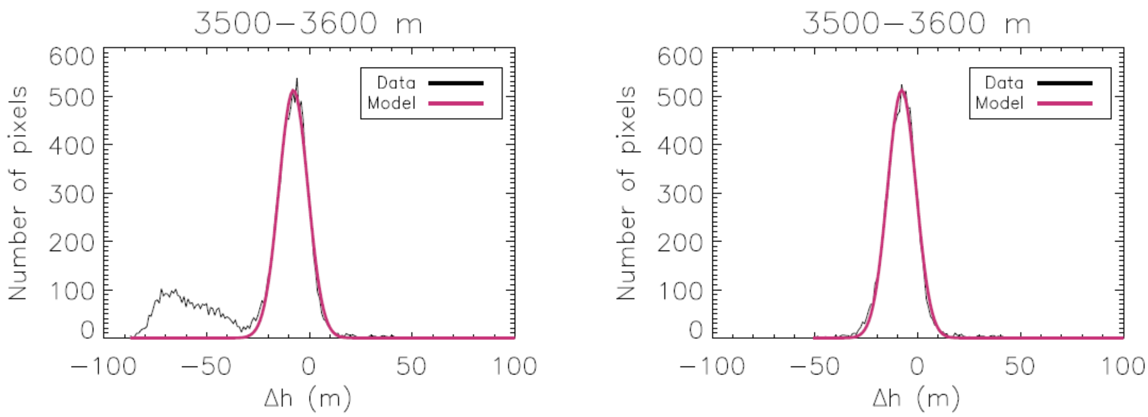

Figure R1 : distribution of elevation changes for the 3500-3600 m a.s.l. elevation band in the Karakoram West study site. On the left panel, surge-type glaciers are included, on the right panel, they were excluded. The "model" curve correspond to a Gaussian fit to the data.

P987 – 8: Flip figure order. The sentence was removed

P988 – are all stated errors for a 90% confidence interval? Maybe I missed this. Error levels correspond to one standard error. This is now specified in the revised MS.

P989 – 13: Gardner et al. 2013 used a correlation length of 50 km. Here we decided not to refer to previous estimate of the correlation length and thus removed reference to Berthier et al. and Bolch et al. The correlation length is proper to each dataset and thus comparison between the values from different studies is only relevant if the same data is used.

P989 – 14-17: This is a correlated bias… so should not be reduced with increasing study area. I believe this will substantially increase uncertainty bounds. SRTM penetration correction is now treated as a systematic error. It is now the main source of error for our mass balances. In fact, this was already the case in the submitted paper because the standard deviation of the elevation difference between SRTM C-band and X-band was high and led thus to large errors.

P989 18-21: Again, maybe this can be better constrained using the Kaab et al dataset. See General response 0.2

P989 – 22-23: I can't quite follow what you've done here. We have now improved the text, and hopefully it is now clear how the errors were computed.

P990 – 15% error seems way too large when I look at Table 1. Did you average % error or did you sum all regional areas then determine the % difference? I think the later is probably the best approach. In the submitted MS, we averaged % error, but we agree that an area-average would be more appropriate (see also Nuimura comment). But as suggested by T. Bolch, we assigned different uncertainties for each sub-region, given that the RGI have varying quality.

P990 – what about the largest source of uncertainty, the uncertainty introduce from extrapolation of mass changes to regions without measurements? By extrapolating mass changes to unmeasured areas for each sub-region, we assumed homogeneous changes throughout that sub-region. This is partly supported by (i) the very similar mass balance for the two Karakoram study site and (ii) the fact that the new mass balance for the Hindu Kush is rather close to the one previously assumed by computing the mean of the two nearest sites. This is also supported by the relatively smooth pattern of mass balance changes throughout the region as mapped by Kääb et al. (2012, their figure 1). New, we compute now the ICESat trends for the entire regions, based on the data and methods of Kääb et al. (2012) and compared these results to the equivalent results of 3°x3° cells around the study cites only. The according spatial variations turn out to be small and are now taken into account in our revised error estimate (see MS section 3.5).

P990 – would you be able to show figure 2 with and without ice-free ground masked out? It would be valuable to see how much noise there is in the dh/dt over non-ice surfaces. Map of elevation changes off glaciers now shown in a supplementary file and also a histogram showing the elevation difference off glaciers is added as inset in each figure of the main article.

P991 – 21: The uncertainty in the SRTM penetration is at best 1.1 m or 0.14m/yr so a total mass budget uncertainty 0.11 m/yr is much too small. I would revisit the estimation and propagation of errors. We now provide the full equation used to compute our errors bars. The error on our estimate of SRTM penetration is now assumed to systematic and equals to 1.5 m. It is actually the main source of error and explains why the error bars are generally just slightly higher than 1.5 m *0.85 / ~10 years = ~0.13 m w.e. yr^{-1}

P992 – 12: mass change to discharge equivalent? Are these not the same. ?? Indeed, this is simply a unit conversion

P992 – If you mention proglacial lakes then you should also mention evaporation, ground water storage, and lake expansion, all of which make the glacier mass balance a maximum estimate. This paragraph has been moved in the method section because it was not related to the hydrology.

P992-993 – I find the analysis of surge glaciers not all the helpful.. maybe just group all glaciers together. You'll get the same result. We think it is interesting to see that the surges (displacing large amount of ice toward low elevation where ablation is high and fracturing the glacier) do not have influence the glacier mass balance. It may have been expected by the reviewer but, still our dataset provides an opportunity to verify this. See also above why, for the sake of mass balance calculation, surge-type glaciers have to be processed separately, because their presence does not guaranty a proper Gaussian distribution of elevation changes within elevation bins.

P993 – 5.1: should this be in results? Also see Reviewer 2's general comments. We did not want to create a very short sub-section in the Results section of our paper and thus we kept this part in the discussion. The comparison to previous estimates of the penetration is clearly relevant for the discussion.

P996 – 7: Your results are not significantly different. Correct, also noticed by T. Bolch in his Short Comment. Sentence modified.

P996 – 16 "dynamically little active" -> " slow flowing"? Corrected
P996 – 22: "took" -> "take" Done
P997 – 4-6: maybe ref Nuth et al., 2010 and Gardner et al., 2013 Ref added.
P997 – 9: This provides no evidence for a gradual speed up.. We are just pointing out that the equilibrate or slightly positive mass balance of Baltoro Glacier is compatible with the gradual speed up mentioned by Quincey et al. (2009).
P997 - 1-14: all much budgets are not significantly different from zero.. This supports that glaciers are near equilibrium not that they are gaining mass. Ok, reword to "equilibrate mass balance". Text has been modified.

P997 – 20: It would be better to assign an absolute error, unless you expect your error to scale with the measured mass budget.
The mass balance in the Hindu Kush is now calculated using a DEM derived from a pair of stereoscopic images acquired by SPOT5-HRG in 2008. The error on the mass balance in the Hindu Kush is thus now computed in the same way as for the other study sites.

P997 – 23 "yr" -> "years" Done

P989 – 5: "However, this" -> "The" Done

P998 5-19: This discussion is a bit weak. We think it is important to discuss these differences with the previous study by (Heid and Kääb, 2012). This part of the discussion is thus retained.

P998 – 25: estimates are not significantly different.. This will become even more apparent if the calculation of uncertainties is revisited. Our error bars are now modified and, as foreseen by the reviewer, are now larger. Estimate overlap within their error bars.

P998 27 to P999 -2: This is speculation. Both gravimetry and altimetry have the strengths and weaknesses. Jacob et al. 2012 provide adequate error bounds to account for the limitations of their methods. Reworded and speculation removed.

P999 18: "On the opposite" -> "On the contrary". Section removed.

P999 – 24: "is negative" -> "is slightly negative" could also say "nearly in balance". Changed

P101 5.5 See reviewer 2's comment.. I fully agree with his/her assessment. We disagree. See General response 0.5

Figure 1: Maybe change Study site outlines to red of green? I found it a little difficult to distinguish between all of the dark lines. Changed to brighter red

Figure 2-3, A1-6: Would be helpful if yo include a hill shade or Landsat base image to provide spatial context for the glacier changes. Could also include drainage divides and lake. Can't see glacier outlines or surge/quiescent markers (maybe increase size and change color to green or magenta?). Suggested changes have been performed.

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.

Reviewer#5, T. Nuimura

General comments

In this paper, authors presents to evaluate mass balance in extent area over the PKH region. The DEM differentiation method used in this study is well established robust method. Therefore this is valuable and important result as validation against recent extensive mass balance evaluation by advanced/developed procedure (e.g. Jacob et al., 2012; Kääb et al., 2012). Appropriate pre-processings, developed by previous studies including authors themselves, are comprehensively well performed before DEM differentiation. The evaluated heterogeneous mass balance are basicaly consistent with previous studies. I also fully agree with comments by other reviewers. Therefore, I consider this paper has quality for publishing after revision about comments from reviewers.
I am also interested in off-glacier elevation change same with anonymous referee 2, Gardner, and Bolch. Showing off-glacier elevation change is helpful for reader to evaluate quality of calculation.

They are now shown in the supplement and a histogram showing the distribution of the elevation difference off glaciers is shown on each map of elevation changes (Fig. 2-10).

Specific comments

P980/L6–7 : How do you extrapolate the result to extent area? Did you consider altitudinal distribution and geographical proximity? Further explained in Section 3.4.

We did not consider altitudinal distribution, but only assumed that the mass balance of the whole sub-region equals the mass balance of the study site. This strategy is confirmed by an analysis of ICESat data from Kääb et al., 2012 and, new, is included in the uncertainties for the overall PKH mass budget (see General response 0.4).

P984/L1–3 : Could you show standard deviation of digitized ELA in Table 2? Ok, done.

P986/L17–19 : Is the screening threshold for unexpected elevation change from average or median?

The selection of the threshold for discarding spurious elevation changes is neither based on average nor median but results from a visual inspection of the elevation change map for each study site (80 m for site without surging glacier, +/- 150 m for sites with surging glaciers). This is now explained in section 3.4.

P987/L9–11 : Isn't there surge type glacier with small truncated part.

We are not sure we understood the point made by T. Nuimura. The maps of elevation changes include all glaciers, even the truncated ones (Fig. 2-10). However, the latter are excluded from mass balance calculation, except for Fedtchenko and Siachen glaciers, which are not known to be surge-type glaciers.

P987/L27–29 : Including explanation about that why you did not use all three adjacent study sites to average calculation might be helpful for reader.

The need to extrapolate the mass balance from neighboring study regions to estimate the one for the Hindu Kush was a clear weakness of the submitted paper. In the revised

MS, we now compute the mass balance of the Hindu Kush by differencing a SPOT5-HRG DEM from 2008 with the SRTM DEM and obtain a value of -0.12 ± 0.16 m w.e. yr^{-1} during 1999-2008 (see more details in the revised MS).

P990/L1–4 : Did you calculate the 15% by simply averaging all error between user-defined and RGI glacier area in Table 1? As Cogley pointed out, RGI in Hengduan Shan needs further investigation. It could be omit for calculating average. And I also agree with Gardner that area-weighted average should be used.

We agree that an area weighted average would be more appropriate. Since the accuracy of the RGI is variable, we chose to assign different uncertainties depending on the sub-region. We also fully agree that errors in the RGI need to be further investigated in Hengduan Shan (among other regions).

P991/L8 : Can you show the standard deviation of elevation change in each altitude bin as error bar in Fig.4?

For the sake of clarity, the standard deviations of the elevation differences are not shown but the legend was modified to indicate that they are on average of ±7m.

P995/L21-23 : Showing numbers are helpful for reader.

This comment was not 100% clear. Numbers are shown in Table 5. We hope this is sufficient?

P999/L8-22 : I agree with anonymous referee #2's comment that it is only loosely related and could be removed. Especially, the suggestion about that "supragracial lakes are not appropriate indicators" is arisen suddenly here.

Agreed and so the paragraph was removed.

Stylistic comments

P995/L2,P996/L5 : Table A2 does not exist.

Sorry for the mistake. It was in fact Table A1. Now changed to Table 5 (included in the MS and not as an appendix).

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