Point-to-Point response to reviews

We thank E. Berthier, M. Pelto, K. Scharrer, and an anonymous reviewer for their helpful comments. We largely agree with the points raised and considered many of them in the revised version of the manuscript. In the following, our changes are listed next to the points raised. The revised version of the manuscript was corrected by a native English speaker.

Reviewer #1 (anonymous reviewer)

Point 1: *The section describing the differential intereformetric phase approach needs to be expanded greatly. I am still unclear exactly how this is done and whether I would be able to reproduce this study. It could be useful to have a flow diagram here to describe it. Thus it is also not clear how the errors in the X-band DEM will propagate in the results. In particular, how is the later phase unwrapping (P1125, L1) to convert the differential phase into absolute change affected? Then, could the co-registration be affected by errors translated from the DEM into the simulated SAR image (P1125, L12). Also, do both DEMs use the same datum and has there been any co-registration here, or are they perfectly aligned? I am guessing this would have a significant effect on a simulated SAR image... In summary, there are many gaps to fill in the methods section*
Reply: We agree to this point and changed the method section significantly. We included a new Figure (Figure 2) showing the work-flow of the DInSAR approach. The whole method section is based on this new Figure and we hope that the surrounding text is clearer in the revised version of the manuscript.

Point 2: *A few comments about P1128, L4-12 and Figure 2. Satellite jitter and/or instrument shaking is easily visible on flat topography, as any slight resampling or mis-alignment between pixels will have little effect. However, in Figure 2, the differences that present patters are mainly visible in the steeper topographic areas. This lets me suggest that these variations have some sort of resampling issue since the steeper the terrain, the larger the magnitude of error. Figure 5 then shows a section of relatively flat topography which exhibits a similar frequency at a much smaller magnitude that may be considered as jitter. However, it could be expected that jitter effects would be hidden in the noise of comparisons over steeper topography, the opposite effect of what is shown here. Similar effects can be generated within re-projection step of the processing (is a reprojection performed?), especially depending upon the type of resampling (nearest neighbor, bilinear, bicubic etc.) that is chosen. Furthermore, there is some similarity here as in the Gardelle et al. (2012) paper about curvature related artifacts which suggests also a resampling artifact. One last point, is that the direction of pattern doesn't really seem to fit an along track or cross track direction, what could be the reason for this? I am not fully convinced yet that what is shown in Figure 2 is or is not related to the shaking of the arm.*
Reply: I am deeply impressed about this observation. Thank you very much! We re-processed both approaches using the original lat./lon. projection of the SRTM-X dataset as indeed a resampling was performed in the first version of this study. As you suggested the linear pattern in the difference maps vanished after the re-processing (see new Figure 5). This shows that the mentioned pattern were clearly related to the re-projection of the SRTM-X dataset.

Point 3: *How exactly was the mass balance calculated from the elevation changes? The use of the zero elevation change as a proxy for the ELA is not correct (think about the surged glacier that is shown). Also, on some parts of your glaciers, this transverse artifact seems rather large, how does this affect your total mass balance estimate? Since the artifacts are clearly visible, it should be interesting to discuss this point.*
Reply: We agree to this point and suspended the mass conversation based on the ELA estimate in the revised manuscript. The mass balance was calculated for the 12 single glaciers and for the entire ice cap. This is described in a new "Estimation of mass changes and error computation" section. Since we were able to remove the transverse artifacts due to your suggestions we think that the last two points can be dropped.

Figure 2: *Could be helpful to have the hillshade as a background for this map. Also, which year are your masks from, and are these the ones that are used for calculating volume change and geodetic mass balance?*
Reply: I am not sure about a hillshade. A hillshade from SRTM-X or TanDEM-X would be buried underneath the map of elevation changes and I think it would be misleading to introduce another DEM. In the new version of the manuscript the geometric union of the 2000 and 2012 glacier masks is shown which was also used for calculating the volume changes.

1120-18: *Spell out "Tibetan Plateau (TP)" for the first use in the introduction.*
Reply: Changed as suggested.

P1121-14: *change to "First,"*
Reply: Changed as suggested.

1121-16-18: *I am not sure "absolute" surface elevations is descriptive enough here. Maybe something more like "elevation changes were calculated by differencing the two (interferometrically or InSAR) derived DEMs*

Reply: We agree to this point and changed the sentence to: "In order to compare the DInSAR derived estimate with a more common method we constructed a DEM from the TSX/TDX acquisition and calculated the surface elevation differences between the two InSAR derived DEMs."

1121-25, 26: *What is the swath width of the C-Band? This to give context to the next sentence.*
Reply: We agree to this point and changed the sentence to: "As opposed to the C-band ScanSAR system with a swath width of 225 km, the X-band SAR system was operated with a swath width of 45 km leading to large swaths of no data in the X-band DEM (Rabus et al., 2003)."

1122-Section 2.2: *A deeper introduction to the terraSAR products would be useful for those readers that may not understand the potential quality and limitations of such data.*
Reply: We agree to this point and extended the section accordingly.

1123-1: *Does that mean the X-Band SRTM was used for orthorectification of the landsat?*
Reply: This point might have been a little bit unclear in the first version of the manuscript. We used the already orthorectified level T1 products provided by the USGS. SRTM-X was only used for the orthorectification of the SAR data. This sentence is more concentrated on the fact that the different datasets are horizontally well aligned. We changed the sentence to: "No horizontal shift was observed by visual comparison amongst the Landsat imagery, the co-registered TerraSAR-X coherence image and the SRTM-X DEM."

1123-15-19: *It is not how the SAR coherence images were used to delineate glaciers. Simple thresholding, band-ratio, classification as with the landsat scenes etc.? Also, if these were also used for glacier delineation, can you compare landsat and coherence based outlines, since you generated both.*
Reply: This point was also a little bit unclear since it was also raised by reviewer #2. We manually digitized certain obvious parts of the ice cap from the coherence image. These parts were used for an error estimate of the Landsat derived glacier outlines. We hope the entire procedure is described clearer in the revised version of the manuscript.

1124: *Include the footnote in the text.*
Reply: We excluded the reference in the revised version of the manuscript.

1124: *It is not clear how exactly you converted phase difference to absolute height change. Which unwrapping technique or? Either way, Full descriptions of your steps here (and in the rest of the methods) will aid the reading and understanding of your paper, and moreover provide a more convincing paper to the readers.*
Reply: We agree to this point and reorganized this entire paragraph in the new version of the manuscript.

1125-3: *Begin this sentence with, "before calculating phase differences. Otherwise, the reader must interpret when you do this step, and guesses that you did this before differencing the phases.*
Reply: We agree to this point and changed the sentence to: "Before calculating the difference interferogram and prior to the simulation of $\Delta\phi_{SRTM-X}$, precise horizontal offset registration and fitting between the TSX/TDX and the SRTM-X dataset is mandatory."

1125-18-20: *One sentence for describing the differencing DEMs is rather undescriptive. How did you resample the DEMs, which interpolation, is the datum the same or did this require a conversion, were they co-registered, is there any elevation dependent biases etc. etc. ???*
Reply: We extended the paragraph about the DEM differencing accordingly and hope that it is more convincing in the new version of the manuscript.

1126-5-7: *Was the mean elevation difference in off glacier terrain used as a correction or just added to the error? I wonder how significant your mean difference is, and how well you may be able to estimate it?*
Reply: Yes, we used the mean surface elevation difference in off-glacier regions as a correction, for a clarification of this point we added the following sentence to the manuscript: "In the next step, a constant vertical offset and a linear trend were removed from both difference maps. The latter was estimated by a two dimensional first order polynomial fit in off-glacier regions and is probably a residual not covered by the baseline refinement mentioned above. Finally, both datasets were translated to a metric cartographic coordinate system with a grid spacing of 25 m $\times$ 25 m employing bilinear interpolation." We also added a new section to the manuscript which describes the error computation in more detail.

1126-13: *The zero elevation change contour is not necessarily the equilibrium line altitude. Especially in the case of your surge! Therefore, this approach is flawed.*
Reply: We agree to this point and suspended this approach in the revised version of the manuscript.

1126-20: *The second sentence of this paragraph says the same thing as the first. Consider combining in some way.*
Reply: Changed as suggested.

1127: *This section could use a table to aid the reader.*
Reply: We agree to this point and added a table to the Results section.

1127-1: *How exactly did you calculate volume change? Was it using the curves (that are undescribed) in Fig 3 with the hypsometry? Or did you just take the mean elevation change? Which glacier outline did you use,*

older or newer? Or an average of the two? This is all important information as I am not sure how to compare your rates to estimates from other studies.
Reply: We agree to this point and described the calculation of volume changes in more detail in the revised version of the manuscript. For the changes in surface elevation we used the mean elevation changes of the single glaciers shown in Figure 1 and the mean elevation changes of the entire ice cap. For the glacier area we used the geometric union between 2000 and 2012 as suggested by Li et al. (2012).

1127-1*: Are the number provided here really absolute magnitudes, or are they rates (divided by the number of years)? I find the error bars rather small and wonder whether they really include the uncertainty of the mean bias between the DEMs.*
Reply: Yes, these values were absolute magnitudes. We also found the errorbars relatively small and changed the strategy of our error computation. In the revised version of the manuscript we did not apply the standard error anymore as it probably underestimates the error for a large number of pixels. To account for the random part of the error we used the Normalized Median Absolute Deviation (NMAD) instead (Höhle and Höhle, 2009). The systematic error was calculated on the basis of the off-glacier trend in surface elevation changes. The error computation is described in a new section of the manuscript.

1127-7*: significant digits in the error estimate does not correspond to the area estimate?*
Reply: The area estimate was changed accordingly in the revised version of the manuscript.

1128-2*: This is the first time I see that the systematic error was much smaller for DEM differencing. Could be useful to have these numbers in a table, maybe a combined table with the glacier changes. Also, this point alone requires some discussion if you want to properly compare the methods.*
Reply: We agree to this point and included a new table to the results section which also includes our error estimates. Additionally we added the following sentence to the discussion: "The systematic error of the DInSAR approach is estimated to be slightly higher than of the DEM differencing. This is probably due to residual inaccuracies of the baseline estimation not covered by the additional polynomial fit."

1128-17*: change to "the datasets used".*
Reply: Changed as suggested.

1128-18*: "leading to unbiased results". Yes in principle, what if the snow pack characteristics and depths are different in 2000 and 2012?*
Reply: We agree to this point and added the following lines to the manuscript: "However, a certain bias introduced by the X-band penetration depth may have affected our results as the snow pack properties in 2000 and 2012 were probably not identical. Another bias can be expected from snow depth variations between 2000 and 2012 for which no measurements are available."

1129*: The conclusions are short and undescriptive. A more useful conclusion will aid the extraction of important information contained in this study.*
Reply: We agree to this point and changed the conclusions accordingly.

Reviewer #2 (K. Scharrer)

1120-11*: Really? Any proof?*
Reply: We agree to this point as we indeed can not be sure about the surface characteristics during the time of data acquisition and therefore excluded the sentence from the abstract.

1120-13*: extent*
Reply: changed as suggested.

1120-16*: is this really exceptionally fast? 11cm/day on average according to your data...*
Reply: We agree to this point and changed the sentence to: "Additionally, we detected one continuously advancing glacier tongue in the eastern part of the ice cap."

1121-14*: Which datasets do you use exactly? Name them.*
Reply: We agree to this point and changed the sentence to: "In this approach we subtracted a simulated SRTM-X interferogram from a single-pass TSX/TDX interferogram."

1121-16*: I'm not sure you can validate DInSAR (which is way more accurate) with DEM differencing. What you do is a comparison.*
Reply: We agree to this point and changed the sentences to: "In order to compare the DInSAR derived estimate with a more common method we constructed a DEM from the TSX/TDX acquisition and calculated the surface elevation differences between the two InSAR derived DEMs."

1122-2*: What about ASTER GDEM? Have you investigated? Comment.*
Reply: We considered the ASTER GDEM but in the end we did not use it, because (1) we do not know exactly in which year and at what time of the year the ASTER GDEM was acquired over the ice cap. (2) As we have two intereformetrically derived DEMs acquired at the same wavelength at almost exactly the same time of the

year, we have an ideal data situation and can assume similar methodical artifacts, which would not be the case for the Aster GDEM (especially in the accumulation area). (3) There are studies which attribute a better performance to the SRTM over glaciers (e.g. Frey and Paul, 2012).

1122-18: *You mention in your acknowledgements that you used Gamma Software. You should mention here as well...*
Reply: We agree to this point and added the following sentence to the manuscript: "For the interferometric processing of the CoSSC product we employed the GAMMA SAR and interferometric processing software (e.g. Werner et al., 2000)"

1122-21: *What did you do with these? What did you use them for?*
Reply: From the phases of these scenes we calculated a coherence image which was mainly used for an error estimate of the 2012 glacier outlines. We added the following sentence to this section: "From the phases of these scenes a coherence image was calculated which was employed to support and validate the Landsat derived glacier outlines shown in Figure 1."

1122-25: *You should list the band(s) you used in this table as well.*
Reply: We employed all available bands of the corresponding sensors in order to create layer stacks. These 30 m layer stacks were pan-sharpened with the 15 m band 8 using a resolution merge with a principal component method. The following sentence was changed in the manuscript: "We used all bands of the orthorectified level T1 products provided by the United States Geological Survey (USGS)."

1123-1: *Have you compared with TSX/TDX amplitude images as well? If not, why?*
Reply: In order to assure consistency regarding the method, both glacier outlines (2000 and 2012) have mainly been detected from the processed Landsat images. Therefore we did not use the TSX/TDX amplitude images for the semi-automatic creation of glacier outlines. The TerraSAR-X coherence image which was mainly used for the validation of glacier outlines was co-registered to the SRTM-X DEM and no horizontal shift could be observed between both datasets by visual comparison. The corresponding sentence in the manuscript was changed to: "No horizontal shift was observed by visual comparison amongst the Landsat imagery, the co-registered TerraSAR-X coherence image and the SRTM-X DEM"

1123-5: *Did you use DNs or converted to e.g. radiances? Thermal band for shadow?*
Reply: The creation of layer stacks was performed on the DN images. The thermal band has been incorporated in the following pan-sharpening. This was undertaken using a principal component method, which slightly favors spectral features compared to other methods (Alparone et al., 2007). Thus the spectral features of the thermal band had also an influence on the later classification of the ice body.

1123-10: *which band(s)? NDSI, band ratio, thresholding?*
Reply: Due to the high spectral contrast between glacier area and non-glacier area in our study region we conducted an unsupervised 2-class classification for the delineation of the ice cap including all spectral bands. We also tested methods based on band ratios and thresholding as suggested by Paul et al. (2004) and Bolch et al. (2010b) but found the results very similar. The following sentence was added to the manuscript: "Due to the high spectral contrast between glacier area and non-glacier area in our study region we conducted an unsupervised 2-class classification to delineate the ice body for the years 2000 and 2012. For the classification we employed all bands of the 2000 and 2012 pan-sharpened Landsat ETM+ scenes. We also considered a method based on band ratios and the use of a specific threshold as suggested by Paul et al. (2004) and Bolch et al. (2010b). However, for our study region the results of both approaches were almost identical."

1123-19: *and? what was the result? You couldn't produce a DEM but you used it for the delineation?*
Reply: The TerraSAR-X coherence image was mainly used for an accuracy assessment of the mentioned semi-automatic classification of the ice cap. As there was an 11 day repeat-pass of TerraSAR-X in August 2011, the coherence image revealed high values outside the glacier area, whereas the values are distinguishably low on the ice cap. This circumstance has been used to delineate several parts of the ice cap manually. Subsequently, it has been compared to the outlines created by the 15 m Landsat classification in order to estimate a mean error for the semi-automatic classification. The manuscript was changed accordingly and we hope that this point gets clearer in the revised version.

1123-23: *and? what results did you get?*
Reply: The estimation of the classification error is based on Granshaw and Fountain (2006) and Bolch et al. (2010a). They compared their glacier outlines to independently generated outlines based on high resolution aerial imagery at random locations. In our case, this higher resolution image was the TerraSAR-X coherence image. We calculated a mean overestimation of 3.2% and a mean underestimation of 1.5% resulting in an overall mean error of about ±2.3%. This number was added to the manuscript.

1123-24: *extent*
Reply: changed as suggested.

1123: *This entire paragraph needs reworking and clarification, it leaves me with many unanswered questions. A figure and/or a table would be good. How good was the classification result? You could e.g. add a table listing the number of glacier pixels... Have you used outlines from the Randolph glacier inventory?*
Reply: Thank you for your suggestions concerning this section, most of the uncertainties should be clearer by now. We had a look at the outlines of the GLIMS Randolph Glacier Inventory within our study area. However, we found large spatial inaccuracies between the glacier outlines and the Landsat data. A corresponding sentence was added to the manuscript.

1124-3: *Ionosphere?*
Reply: As the ionospheric contribution should also be zero, we did not include it for reasons of simplicity.

1124-14: *It is not clear to me how you produced the phase differences. I suggest you explicitly list the dates of the scenes that you used.*
Reply: We reorganized the whole section and explicitly listed the data takes in the new version of the manuscript. We hope that the whole paragraph is clearer in the revised version of the manuscript.

1124-21: *Is this true? Don't you have different dates here, therefore different scattering, atmosphere, ionosphere...*
Reply: Yes, the dates of the intereformetric data takes were different. However, both data takes were single-pass so we assume same atmospheric/ionospheric conditions and scattering per data take leaving only $\Delta\phi_{topo}$ and $\Delta\phi_{orbit}$. We hope that this point gets clearer in the reworked manuscript.

1125: *I think you should make a clear error budget for the methods you use.*
Reply: We agree to this point and added a new subsection titled "Estimation of mass changes and error computation" to the manuscript.

1125-4: *for what? which method? DEM differencing of DEM elimination InSAR?*
Reply: The co-registration of the datasets is mandatory for both methods. We hope this gets clearer in the reworked manuscript.

1125-8: *So? Did you do that or not?*
Reply: Yes, we could minimize the linear phase ramp by a baseline refinement. In the final map of surface differences we still found something linear and its origin is not 100% clear. However, we could remove it with a first order two dimensional polynomial which we estimated from the elevation differences in off-glacier regions. I know that the team of GAMMA remote sensing is working on this at the moment.

1127-2: *How does this all correspond to the errors of your methods? Please elaborate.*
Reply: We hope this gets clearer from the new "Estimation of mass changes and error computation" section.

1127-14: *What happens if you exclude this unusually behaving tongue from your analysis?*
Reply: On demand of E. Berthier we included a new table to the Results section which lists the mass balance estimates for the single glaciers of the ice cap. This gives also an idea what is the effect of this single glacier.

1128-10: *So did you actually correct for them? It's unlikely that there is jitter only over bedrock.*
Reply: Yes, the jitter-like pattern could be removed over the whole scene in the revised manuscript.

1128-25: *Can you name them?*
Reply: We agree to this point and changed the next sentence to: "A possible mechanism could be a compensation of the temperature driven melt-off due to an increase of precipitation in high altitudes."

1129-5: *interpreted?*
Reply: changed as suggested.

1129-8: *In the abstract you call it "an exceptional fast advance"? So what do you it is think then? Now you talk about glacier velocities, how were they derived, which periods, etc?*
Reply: We agree to this point and changed the section to: "Overall we found negative elevation changes in glacier tongue regions except for one glacier in the eastern part of the ice cap. This glacier shows thickening at the terminus while negative values are found further up the glacier (Figure 6). These areas could be interpreted as reservoir and receiving areas of a surging glacier (Paterson, 1994). However, it is questionable if the observed continuous advance can be termed "surging"."

1129-19: *How did you get that? One outlet advances by about 500 m, the others clearly retreat (see e.g. Fig 1). Did you end up with similar glacier areas? Did the gain in the interior make up for the marginal losses? Elaborate!*
Reply: In the revised version of the manuscript we deleted this sentence and included the following in the conclusions: "In the same time period, the ice cap retreated at a relatively slow rate of -0.15±0.01 km^2 a^{-1}."

Fig. 3.: *Additionally, I think it would be a good idea to plot those errors in relation to the across-track distance. That might give you a number for jitter correction.*
Reply: Thank you very much for this suggestion. As we plotted the elevation differences along x and y we found a slight linear trend which is probably a residual of the phase ramp mentioned in the text. We estimated a two

dimensional first order polynomial in the off-glacier regions of the scene and detrended the data accordingly. However, as the jitter-like effects were related to a resampling issue and could be removed in this version of the manuscript we did not include an additional figure to the manuscript.

Fig. 4.: *Terminus.*
Reply: Changed as suggested.

Short Comment #1 (M. Pelto)

1120-13: *Here and in other locations change, "extend" to "extent".*
Reply: changed as suggested.

1120-19: *Reference for most extensive non polar ice? Alaska is generally listed as having more ice than the Tibetan Plateau.*
Reply: I took it from the Nature news section: http://www.nature.com/news/2008/080723/full/454393a.html but you are right, see also the comment of Evgeny Podolskiy underneath the article. We changed the sentence to: "The Tibetan Plateau (TP), also known as the third pole, is characterized by many glaciers and ice caps."

1126-15: *Why assume such a low accumulation area density, there is an ice core from this ice cap that indicates the depth at which the snow transitions to ice. This can be used to determine a mean density for the entire ice. It is reasonable to expect that thickness changes over such a short period are mostly from gains in firn versus ice, but this will still have a density somewhat above 600 kg m^{-3}. Better justification of both density choices is needed. Reference to firn core densities would be useful in this.*
Reply: In the revised version of the manuscript we dropped our second density scenario. We contacted M. Davis from the Byrd Polar Research Center who provided us with the drill site locations and the information that no firn phase was found at the Purogangri Ice Cap. Therefore we applied an ice density of 900 kg m^{-3} for the volume to mass conversation in the revised version of the manuscript.

1126-24: *One of the potentially most important findings of this paper is that the icecap is close to equilibrium overall, despite rapid thinning at the terminus and modest thickening in the accumulation zone. This can only be accomplished by having a steeper balance gradient. Steeper balance gradients are found in warmer and wetter climate zones. Raper and Braithwaite (2009) or Braithwaite and Raper (2007) and Rasmussen (2004).*
Reply: Thank you very much for sharing this observation with us. We very much agree to this point and changed our manuscript accordingly. We added several sentences to the abstract, the results, the discussion and the conclusions concerning this issue.

1127-5: *The two different density approaches yield different results, the more negative mass balance results is discarded without due justification and is not mentioned in the abstract or conclusion. Why is the lower density approach is considered less valid? If it is because of better agreement to area changes, that is not a robust validation. Glacier extents do not respond in the same decade to most climate change. Since, more glaciers are retreating than advancing, extent change does not strongly support the notion of an equilibrium balance.*
Reply: In the revised version of the manuscript we only applied an ice density of 900 kg m^{-3} and estimated a slightly more negative mass budget for the DInSAR approach and the DEM differencing after reprocessing the data.

1127-12: *The authors correctly point out the significant ice advance of one outlet, but ignore three glacier retreats that are of greater magnitude according to their Figure 1 and five other retreating fronts that are evident. I have attached an annotated Figure 1 indicating these eight. The point is that though the area change is not great, terminus response has been generally one of retreat, and the authors do not address this. The extent of thinning is extraordinary on several of these and is worth noting.*
Reply: We agree to this point and added the following sentence to the results section: "For the observed time period we estimated an annual change rate of -0.15$\pm$0.01 km^2 a^{-1} for the entire glacier area, suggesting a general but slow retreat of the ice cap in the last decade."

1129-19: *"did not retreat" is incorrect as several glaciers did retreat significantly. That the ice cap did not lose significant area is more accurate.*
Reply: We agree to this point and changed the sentence to: "In the same time period, the ice cap retreated at a relatively slow rate of -0.15$\pm$0.01 km^2 a^{-1}."

Short Comment #2 (E. Berthier)

Point 1: *Nothing is said about the generation of the 2012 Tandem-X DEM. Built by the authors? With what tool/software? Or did they receive it from DLR?*
Reply: We constructed the TSX/TDX DEM from SLC data employing the GAMMA SAR and interferometric processing software (e.g. Werner et al., 2000). A corresponding section was added to the manuscript.

Point 2: *Can the authors confirm that they provided (P112-7) the cumulative mass budgets over 12 years and not annual mass budgets (I was a bit unsure)? I think it would be best to provide annual mass budget all along the paper because this is a more common practice and the values can readily be compared to other estimates on the Tibetan Plateau (e.g., Yao et al., 2012) or elsewhere in High Mountain Asia. If the authors gave the cumulative mass budgets then the differences between the two methods and two density scenarios are rather small (if not, they are large).*

Reply: Yes, we provided the cumulative mass budget over 12 years in the first version of the manuscript. We agree to this point and included a table in the revised version of the manuscript where we also listed the annual mass budget.

Point 3: *Do the authors really trust more the DEM difference method than the INSAR approach as suggested by its smaller uncertainty? Need to be discussed.*

Reply: It seems that the DInSAR result shows lesser data noise than the DEM differencing. Therefore we think the DInSAR result might be a little bit more trustworthy which is also shown by the new error estimate.

Point 4: *Given that the ice cap has already been split in to individual glaciers (according to Figure 1), the analysis could be strengthened/deepened by examining the variability of the mass budget among the glaciers.*

Reply: We agree to this point and included a new table in the results section where all glaciers are listed.

Point 5: *It is not clear whether the authors corrected for a vertical offset between the DEMs off glaciers. If they indeed corrected a vertical offset, then what value did they use in eq. (4) for the mean of non-glacier elevation differences?*

Reply: In the revised version of the manuscript we applied a vertical offset and a linear fit to the data. We calculated the systematic error component employing an off-glacier elevation trend. Our whole error computation is described in a new section now.

Title: *I think "ice cap" should be capitalized given that this is here associated to a geographic name.*

Reply: changed as suggested.

Abstract: *The range of possible annual mass budgets using different methods and density assumptions should be quoted in the abstract.*

Reply: We agree to this point and added the following lines to the abstract: "The first method is based on differential synthetic radar interferometry while the second method uses common DEM differencing. Both approaches revealed a slightly negative mass budget of -44±15 mm w.eq. a^{-1} and -47±23 mm w.eq. a^{-1} respectively. A slightly negative trend of -0.15±0.01 km^2 a^{-1} in glacier extent was found for the same time period employing a time series of Landsat data."

1120-L15: *"exceptional fast advance", somewhat in contradiction with the "long time period of constant glacier advance" quoted P1129 L7..."*

Reply: We agree to this point and changed the sentence to: "Additionally, we detected one continuously advancing glacier tongue in the eastern part of the ice cap."

1122-1: *Can the authors provide the % of the glacier surface covered by the data?*

Reply: We found 90% of the glacier surface covered by the data. This number was added to the manuscript.

1122-3: *Is not the original SRTM Band-X DEM provided in Lat/Lon with a grid spacing of 1 arc second? This is different from a 25 m by 25 m on a cartographic grid. Did the authors reproject the DEM? If yes, how (resampling filter) and using what projection system?*

Reply: Yes, you are absolutely right about this point. We changed the sentence to: "The DEM is sampled to a grid posting of one arc second and is referenced to the WGS84 ellipsoid." The resampling issue is addressed later on.

1122-4: *Can the authors provide the mean bias? The number of points? Any outliers excluded to reach this impressively low standard deviation? Which ICESat data did the authors use? From all campaigns? Did the authors exclude cloudy footprints if any? Can the authors also make it clear in the text that there are no ICESat footprints on the ice cap itself (if this is really the case as suggested by figure 1)? See also my comment below about Figure 5: those very low standard deviations against ICESat suggest that the ICESat sample may not be representative of the rest of the terrain.*

Reply: We agree to this point and added the following lines to the manuscript: "For an accuracy assessment of the SRTM-X DEM we utilized data from the Geoscience Laser Altimeter System (GLAS) carried on-board the Ice Cloud and Elevation Satellite (ICESat). We employed the GLA 14 data product from all ICESat campaigns provided by the National Snow and Ice Data Center (NSIDC). SRTM-X surface elevations were extracted by bilinear interpolation at each ICESat footprint location. ICESat measurements were excluded from the analysis if the difference between GLA 14 and SRTM-X elevation exceeded 150 m, which can be attributed to cloud cover during the time of data acquisition. Compared to the ICESat data, we found a mean and standard deviation of -3.93±2.07 m for the SRTM-X DEM. These values are in agreement with a standard deviation of 2.67 m estimated by Hoffmann and Walter (2006) who compared the DEM with Global Positioning System (GPS) measurements in Germany. However, it should be noted that ICESat measurements are only available in a relatively flat off-glacier region (Figure 1) making the ICESat sample distribution not fully representative for our study region."

1122-19: *Same as comment just above. Sample size? Mean bias?*
Reply: We added a link to the previous section in the revised version of the manuscript.

1123-1: *It is not straightforward to estimate horizontal shift between a DEM and an image. How what it done? Visually?*
Reply: Yes, this was done visually. The sentence was changed to: "No horizontal shift was observed by visual comparison amongst the Landsat imagery, the co-registered TerraSAR-X coherence image and the SRTM-X DEM."

1123-22: *Authors could note here that their error estimate will also include one year of glacier change so is rather conservative.*
Reply: We agree to this point and added the following sentence to the manuscript: "It should be noted that this error estimate is rather conservative as it also includes one year of glacier change."

1124-11: *Why a footnote here? Papers could be cited directly in the main text. The more recent review paper on the topic by Rott (2009) may also be cited.*
Reply: The footnote was removed in the revised version of the manuscript.

1124-23: *Are GCPs distributed in the whole scene? Or close to the glaciers? A bit more details would be welcome.*
Reply: The GCPs are randomly distributed over the whole scene. We changed the manuscript accordingly.

1125-3: *"achieve"?*
Reply: The sentence was changed to: "Before calculating the difference interferogram and prior to the simulation of $\Delta\phi_{SRTM-X}$, precise horizontal offset registration and fitting between the TSX/TDX and the SRTM-X dataset is mandatory." in the revised version of the manuscript.

1125, Equation 3: *The number of non-glacier grid cells (n) can be very large if a very long strip of SAR data and all non-glacier terrain (even far away from the ice cap) are used. What value of "n" was used? Should not "n" be restricted to a reasonable number of grid cells close to the glacier?*
Reply: In the revised version of the manuscript we did not apply the standard error anymore as it probably underestimates the error for a large number of pixels. To account for the random part of the error we used the Normalized Median Absolute Deviation (NMAD) instead (Höhle and Höhle, 2009). In order to calculate the $NMAD$ for a reasonable number of grid cells close to the glacier we only employed grid cells located in a 1 km buffer around the ice cap. The manuscript was changed accordingly.

1127-25: *A bit counter-intuitive to have 1-sigma value for INSAR twice smaller than for DEM differencing and then mass budget uncertainties 5-10 times smaller for DEM differencing. It is explained in term of systematic error component but no value is given in the text for the latter error (I think). The reader is left a bit confused (see also my general comment #3)*
Reply: We agree to this point and included a new section to the manuscript describing the error computation. Errors are mentioned also in the text now.

1128-3: *"pattern" of what?*
Reply: The sentence was changed accordingly to: "...similar pattern of surface elevation changes..."

Figure 2: *I suggest a larger histogram to improve readability by enlarging the insets to their lower right and increasing the font size. It is not a problem if the labels of the geographic coordinates are masked. Authors should explain in the legend what is the dark solid line crossing 89°15' and 34°N (= refer to figure 5).*
Reply: The histograms were enlarged to the lower right. The dark solid line was excluded from the Figure in the revised manuscript.

Figure 3: *What is the solid line through the upper panel? A polynomial fit? If yes, at what order? Also explain in the legend what is the dot (=altitude of 0 elevation change, although I expect this dot will be removed in the revised version because the 0 elevation change is generally not the ELA as pointed out by rev#1). Can the authors indicate the slope of the line fitted through the non glacier elevation change? To illustrate numerically that "elevation dependent bias" is not significant. Can they also add the glacier hypsometry as an additional central panel to see how the glacier area is distributed with altitude?*
Reply: Yes, the solid line represents a 3rd order polynomial. We changed the caption accordingly and added the polynomial term to the figure. As suggested we removed the black dot in this version of the manuscript. Additionally, we added the slope of the linear fit in off-glacier regions to the figure and added the glacier hypsometry above the two subplots.

Figure 4: *Legend. "in glacier tongue" is vague. Is not there a name for the glacier? Or maybe his code in the Chinese glacier inventory? Or in the GLIMS database? The color scale should include the value for the central tick to confirm that the color scale is linear and, also, not centered on 0 (and thus different from Figure 2).*
Reply: We changed the caption of this Figure to: "Positive surface elevation changes in glacier tongue region of

glacier 5Z213E0012 (World Glacier Monitoring Service id). DInSAR derived surface elevation changes are color-coded. In the background is the TSX/TDX DEM. Glacier terminus positions are based on Landsat imagery. Location is shown in Figure 5a." An additional value was added to the central tick of the colorbar.

Figure 5: *Provide the mean and standard deviation (SD) of the elevation difference for those two profiles so that the two methods can be compared numerically and not only visually (on the plot or in the legend). On this profile, it seems to me that the SD of the "DEM diff. " is higher than the SD of the individual DEM evaluated against ICESat (SRTM Band-X, SD = 2.67 on P1122, L8 ; Tandem-X, SD = 1.0 m on P1122, L19) summed in quadrature: square($2.7^2+1.0^2$)=2.9 m. Can the authors check that their comparison with ICESat does not sample a flat/smooth terrain where the DEM will have a higher accuracy than on the rougher terrain close to the glacier? This presumption seems to be confirmed P1127 L25 where you quote an off-glacier SD of the elevation difference of 7.3 m, nearly three time larger than the ICESat-derived SD.*

Reply: We removed this Figure from the revised manuscript as it was just to illustrate the "jitter-like" artifacts which could be removed. As suggested earlier in this interactive comment is it questionable if the ICESat footprint distribution is representative in our study region. This is stated in the Data section in the revised version of the manuscript.

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