Summary of final author comments

Journal: TC
Title: Four decades of glacier variations at Muztag Ata (Eastern Pamir): a multi-sensor study including Hexagon KH-9 and Pléiades data
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Dear editors,

Please find below our author comments regarding the reviews of our publication in The Cryosphere. We thank the two anonymous reviewers for their helpful and valuable set of comments to improve the quality of our paper. We hereby provide a summary of point-to-point responses to the reviewers' comments and recommendations, as well as changes in the manuscript.

Point-to-Point response to reviews

Review #1

Summary:

Before its publication in TC, however, some structural aspects should be considered. As highlighted in the title, the paper has a tendency to focus a little bit more on technical aspects, which are greats, but thematic insights should remain dominant and be emphasized. The sections Introduction and Study site should be reconsidered. The section Data could be shorten. In the discussion part, the subsection 8.6 does not provide a lot of new informations and could be merged with 5.2.2. The linkage with climate drivers is probably the weaker part, due to the difficulty to have long-term in situ measurements and the lack of previous studies.

Reply: Thank you for your valuable reviewer comments that we addressed below. Our study has also a clear technical focus concerning the employed methods for measuring glacier variations from space, and we tried to bring technical aspects in line with the thematic outcome of our results. Both sections of Introduction and Study site were revised, and the data section was slightly shortened at those parts that you mentioned. We would keep subsection 5.2.2 and 8.6., however, separated and un-shortened, since appropriate corrections for the SRTM C-band penetration are still problematic and cannot be easily addressed, particularly at our remote study site. Inappropriate corrections can have significant impacts on the resulting volume change and geodetic mass balance outcome, and different assumptions need, hence, to be discussed. Please see our individual responses to your specific comments below. Thank you for reviewing our manuscript and for your constructive improvements.

Specific comments

Introduction:
P.1814. Please reconsider the structure of the introduction. The precise description of the Muztag Ata massif (L1-5) should be displaced to the study site. The introduction could open on what we know about the glaciers of the (East) High Mountain of Asia (your review, L10-20), highlighting the difficulties to know something (lack of glaciological data, lack of temporal depth, observed contrasted pattern, "anomalies" respect to temperature changes/other glaciers behavior...). By insisting a little bit more on the relative location of the (East) Pamir massif over Central Asia, the interest of studying the glaciers of Muztag Ata massif could be strengthen (water resources, climate proxy, temperature and precipitation variations...). Same remark for the (great) interest in having a dataset which allows a reconstruction back to 1973. It is also difficult to have an idea of the relative importance of the Muztag Ata glaciers compared to the East-Pamir or Pamir glacierized area (e.g., you can give a proportion by area).

Reply: We moved L1-L3 to the study site section and restructured the introduction as suggested. L4-L5 was moved to a later part of the introduction. By these changes we also slightly restructured and improved these sentences. The introduction now starts on what we know about the glaciers in (East) High Mountain Asia (L10-L20). By restructuration of the introduction we highlight now more explicitly the specific location of the site. We completely overworked the last third of the introduction according to the suggestions of the reviewer. This concerns in particular a better thematic transition the specific location of the site as well as to the datasets that could be used in this study for a long-term reconstruction.

L 20. Please change paragraph when you address the issue of the climatic drivers.
Reply: Done, new paragraph was inserted.

Study site:
P. 1815. Given the lack of in situ meteorological measurements, you could exploit here the equilibrium-line altitude data to improve the description of the study site. Apparently, this ELA data are not used in the result/discussion sections.
Reply: The equilibrium line altitude data (ELAs) are snow line measurements from the Chinese Glacier Inventory, published in Shi et al. (2008). This data was not collected by ourselves but is based on published results; therefore we did not use it in the results / discussion section. Exploiting the ELA data of Shi et al. (2008) was not part of this study and would go beyond its scope of this already extensive study, consequently we would like to avoid such investigations.

Section 8.4 L9-12 contains useful informations about mean annual precipitation at glacier site which should be cited here. Conversely, L26 to L29 or even to P1816 L7 could be reserved for section 8.4 ("climate change"). In Duan et al, 2007, some analyses performed on the extracted ice core (mentioned here) propose a snow accumulation reconstruction, which could be of some interest for that study (particularly in subsection 8.4).
Reply: Thanks for the hint, we did the suggested changes: We moved L9-12 from P 1834 (Section 8.4) to the end of the study site section. We also moved the other suggested part from P1815 L26 of the study site section to P1834, which is section 8.4 ("8.4 Glacier response to climate change). Thanks for pointing to Duan et al. (2007), we cited this reference and the reconstructed mass balance rates at this point: "*Reconstructed mass balances rates also show much higher wastage after 1990 (-0.42mw.e. a-1), compared to the mean at -0.12mw.e. a-1 for 1960 to 2003 (Duan et al., 2007)"*

Data:
P1816. I guess than this section could be slightly shorten (e.g. P1817. L5-10; L22-26).
Reply: We shortened the recommended data section of the Pléiades and ALOS-PRISM satellites by skipping some less important information and by revising both sections, particularly regarding the ALOS-PRISM sensor.

P1817. Pléiades Data. Please distinguish the accuracy between Pléiades 1A and 1B as mentioned in Berthier et al., 2015 :"Without ground control points (GCPs), the horizontal location accuracy of the images was estimated at 8.5m (CE90, Circular Error at a confidence level of 90 %) for Pléiades-1A and 4.5m for Pléiades-1B (Lebègue et al.,2013)."
Reply: We now distinguish the accuracy between Pléiades 1A and 1B as suggested. The results of Lebègue et al. (2013) are also mentioned by Berthier et al. (2014).

P1817 Line 15: this sentence is unclear to me. What zone does the image of 3 August 2013 cover ?
Reply: This image of 3 August 2013 only covers some cloud-covered areas in the image of 19 June 2013 in the south-west. We tried to make this clearer in the manuscript.

Data processing:
P1820. Could you precise the proportion of GCPs extracted from ICESat, and SRTM respectively, and the number of checkpoints for each source.
Reply: Measuring Check Points was initially not foreseen since it was already difficult to find suitable Ground Control Points in this remote and mountainous region. In case of Hexagon KH-9 we decided to use two initially as Ground Control Points measured coordinates as Check Points due to high residual offsets in bundle block adjustment. These Check Points might eventually not have been measured correctly enough (which explains the high residual offsets) and were not investigated further. We therefore decided to omit this information in the manuscript and eliminated the paragraph *"…but two of them showed high residual offsets and were subsequently set as check points*" (page 1820, line 15 to 16). We modified the sentence of the SRTM / ICESat proportion (line 10 to 11) as follows: "*The SRTM-3 DEM served as z-reference for one third of the GCPs, since no ICESat information was available*".

P1822.L4 Which software or programming environment did you use to perform the analytical approach ?
Reply: We could take advantage of a program in Python programming language that was written by Tino Pieczonka (see acknowledgements). This program was successfully used for co-registration in Pieczonka et al. (2013). We make this now (more) explicitly clear in the manuscript.

Assessment of glacier variations:
p1823. L14. How many glaciers do represent "all glaciers" ? If you generated the inventories from the Pléiades data first (2013), I therefore suppose you that you did not notice any complete glaciers disappearance since 1973 ? Have you in mind to propose this inventory to the WGS, GLIMS or RGI databases ?

Reply: The word "*All*" (line 14) might be irritating and we deleted it. We did, by now, not investigate the number of glaciers at Muztag Ata and their disappearance. The definition of our study site to the East was depending on the coverage of our remote sensing datasets, and several additional glaciers to the East could therefore not been taken into account that eventually still might belong to Muztag Ata. We also think that area changes are more meaningful than an absolute number of mapped glaciers. To this regard, results might wrongly interpreted in case that a retreating glacier would split in two parts, resulting in an increase of the glacier count. Meanwhile the Chinese Glacier Inventory is published which includes this region. However, we will use our data for comparison with the other existing inventories as T. Bolch is actively involved in the activities in the international bodies. The results will also be reported to WGMS.

P1824. The sentences from L5 "All three [...]" to L11 should be move to subsection 4.2.2. You selected thirteen glaciers according to their orientation and size. Which is the representativity compared to the elevation range ? I guess that an extra figure showing area vs. hypsometry of the all glacierized area could help. Which proportion is windward or leeward, according to the north-south "natural" separation ?

Reply: We moved the sentence from line 6 to line 8, as suggested, to subsection "*4.2.2 DEM extraction*". The previous sentence from line 5 to line 6 was moved to section "*4.3.2 DEM co-registration*" since it is related to a processing step after co-registration. The sentence from line 8 to line 12 fits, in our opinion, best to this section, since it describes the problems of SRTM voids for geodetic glacier mass balance calculation and how we handled it. In section "*2 Study area*" we describe that the Muztag Ata Massif is divided "*into a western windward area with small valley glaciers and an eastern leeward part with higher gradients*" (page 1815, line 14 to 15). Since westward is windward, the proportion can be derived from the glacier orientation in e.g. Figure 1. The investigated glaciers are all situated at the same mountain massif (Muztag Ata), and the ELA in Table 3 can give a first impression regarding the elevation range. In consideration of the large number of figures in this manuscript we did by now not provide an extra figure showing area vs. hypsometry, but we can provide such a figure if this is desired.

P1824. L20 You could also have consider the mean of the glacier sizes for the two dates (Zemp et al., 2013).

Reply: This might have been one possibility, but we decided to use the maximal extend of the glacier sizes for the two dates by following e.g. Neckel et al. (2013) who employed the geometric union of both glacier extends.

Neckel, N.; Braun, A.; Kropáček, J. & Hochschild, V.: Recent mass balance of the Purogangri Ice Cap, central Tibetan Plateau, by means of differential X-band SAR interferometry, The Cryosphere, 7, 1623-1633, 2013.

P1825 L24 By ice, do you mean "summer surface" (see Cogley et al.,2011) ?

Reply: Yes, we meant in this context "summer surface", and replaced the word "ice".

P1826 L2 Could you check the sign of the offsets ? I am probably wrong on that, but I would have say the contrary

Reply: The offsets should be correct as presented, and result from the DEM differencing approach: "*Geodetic glacier mass-balances are based … differencing elevations of older dates … from more recent elevations* " (page 1824, line 1 to 3). The offset is positive if SRTM is representing glacier surfaces of older date (1999-2009/2013) because the "older" SRTM surface needs to be corrected for penetration. In case that SRTM is of newer date, the sign of the offset needs to be inversed, since the older dataset (here KH-9 Hexagon) is subtracted from SRTM.

Discussion:

P1831 L1 to 4: the end of the sentence is unclear to me. Why do you expect less glacier shrinkage at Muztag Ata ?

Reply: We changed the word "subject to" with "by reason of", and changed the sentence as follows to explain we would expect less glacier shrinkage: "In total, we would also expect less glacier shrinkage and retreat at Muztag Ata as in other areas of the Eastern Pamir study region of Yao et al. (2012) by reason of, on average, nearly balanced observed mass budgets in this study."

P1830. Surges are not linked with mass gain. Surges complicate the interpretation of glacier variations, but in my opinion it should not be directly "opposed" to the glacier shrinkage. Such ambiguously formulation is also present in the introduction (P1814 L15: "but"). Apparently, you did not observe surges in your glacier indicators variations ? If it is true, you could mention it.

Reply: We fully agree with the reviewer, that surges are not linked to mass gain and complicate the interpretation. The mass is redistributed from the accumulation area with elevation gain in the lower part but loss in the upper part of the glacier. However, a surge typically leads to a rapid advance followed by shrinkage. We observed that glaciers advanced or even fluctuated during the study time period at Muztag Ata**.** Regarding our data, we assume that Kuokuosele Glacier and possibly Kuosikulake Glacier might have surged (see also P1828 L25ff). We will investigate this further and will also reformulate the sentence *"This seems to be contrary to the observed high shrinkage in the Zulumart Ranges south of Pamir Alay…*" (P1830 L15ff) for clarification. We moreover changed the sentence on P1824 L15 to *"…average, while numerous glacier surges were observed at the same time*".

P1831: it should be interesting to say something about mass-balance variations along a vertical profile.

Reply: Are you addressing a specific glacier? We could provide an additional figure showing the vertical profile of elevation changes for e.g. Kekesayi Glacier and (surging?) Koukousele Glacier, if this is desired (by having in mind the numerous figures of this manuscript).

P1832 L3 to 11: this sounds very interesting. Being very careful, do you think we can make any assumptions about common (topo-)climatic (or meteorological, given the short period) drivers ("strengthening werterlies" ?) to make a link with section 8.4? Maybe some regional meteorological datasets could help (CRU/GPCP), and some references: a short review on that question (in French): (Berthier, 2015), about Global Precipitation Climatology Project (GPCP): (Adler et al., 2003), about the seasonality of the observed precipitation trends: (Fujita, 2008; Kapnick et al., 2014), and eventually about the elevation influence: (Hewitt, 2011).

Reply: By being very careful with this statement, we suppose that there is a positive anomaly when comparing our findings with the results of these cited publications. Such an anomaly was already postulated by Yao et al. (2012), as mentioned in chapter 8.4 (P1834, L17ff). Your approach sound very interesting, but we think that any further assumptions beyond our careful statement would go beyond the scope of this study. Such assumptions would be rather speculative without further investigation and profound analysis, that would, for sure, be truly interesting.

P1832 : maybe you can cite Zhou et al., 2013 to complete the comparison on glacier surface dynamics ?

Reply: Thank you for the hint. We now consider Zhou et. al (2013) for surface dynamics comparison.

P1833 L27 to P1834 L3: due to the very different time periods considered, I think that it is out of the scope of this study. You can move it to the introduction section or simply remove it.

Reply: The studies of Seong et al. (2009a, b) are one of the few detailed investigations that have been employed at Muztag Ata, and we would therefore like to keep it in the manuscript. However, as also remarked by the second reviewer, we moved this paragraph to the revised introduction, and also shortened it.

P1834 L20: Is it possible to better characterize this "cooling period" in temperature and precipitation changes ?

Reply: We cited this information from Shangguan et al. (2006), who unfortunately do not provide further data, expect of an additional climate diagram from Taxkorgan meteorological station. We could not find further more information to better characterize these "cooling periods", and had to rely on the following information of Shangguan et al. (2006): "*This time-span included three cold periods: 1961–68, 1973–77 and 1985–93*.", and "*However, some glacier advances might be a response to the three periods of cooling and the increase of annual precipitation…*"

Conclusion:
The conclusion should tell something about the possibility of a wide-regional "positive anomaly" (from section 8.2 and 8.4).

Reply: This is true, and we changed the sentence "*Slightly positive observed budgets after 1999 are, however, more likely a response to strengthening westerlies with increasing snow accumulation*" as follows: "*Slightly positive observed budgets after 1999, however, could possibly reflect a regional-wide positive anomaly with increasing snow accumulation from strengthening westerlies*. "

Table 1: please precise which images are from Pléiades 1A or 1B.

Reply: We now precise in Table 1 which images are from Pléiades 1A and 1B

Table 4: you should consider a more classical chronological way: 1973-1999; 1973-2009 and so on.

Reply: We revised this table in a more classical chronological way, as also suggested by the second reviewer.

Figure 1: this figure is a little bit dense. Glacier extents variations are difficult to read (particularly 2009 outlines, in blue). You should propose a new figure, highlighting the location of Muztag Massif in a "regional" context (with Taxkorgan meteorological station location and hydrological network for example). Extracted ice core location could be also mentioned.

Reply: We will modify this figure accordingly with several changes and improvements according to the suggestions.

Figure 2 is constituted by two type of images: please split it into two figures or choose between one type of data. Distortion vectors image (KH-9) is maybe less common.

Reply: OK, we agree to split both figures if desired. Both (sub-) figures show important information, on the one side the high quality of the 1m-resolution Pléiades DTM (with the clearly visible steep and advancing glacier tongue of Kuokuosele Glacier), on the other side the effects of film distortions. In this context are the distortion vectors that visualize the film distortion from unprocessed KH-9 imagery of particular importance, as mentioned in chapter "4.1 KH-9 image pre-processing"

Figure 3: maybe the title should be remove; Muztag Ata is also a glacier, so it is a lit bit confusing.

Reply: *We agree and removed the title from the figure.*

Figure 4 to 6: you should consider a more classical chronological way, starting from 1973 (see also table 4).

Reply: We changed the chronological way of the figures, also accordingly to the changes in Table 4.

Technical corrections

P1813 Line 10: you should precise the year (2011) for the TerraSAR-X amplitude tracking.
Reply: The year 2011 was added, as suggested.

P1813 Line 11: you should precise: "[...] temporal glacier variations [...]".
Reply: The word "*glacier*" was added at this position, as suggested.

P1815 Line 18: when introducing Kekeyasi Glacier for first time, please precise that the number into parenthesis is a GLIMS Id. The word "Glacier" is missing after Kekeyasi.
Reply: We precise that the number into parenthesis is a GLIMS ID at the beginning of chapter "7 Results", page 1828 line 2 to 3. Before that, the GLIMS ID is only mentioned twice and in the context of well known Kekesayi Glacier. Since this is the only specific glacier that is mentioned before, we therefore eliminated the GLIMS ID from Kekesayi Glacier at page 1815, line 18 (and added the missing word "Glacier") as well as page 1826 line 7.

P1816 Line 13: please precise that the number into parenthesis refer to the mission Id.
Reply: We added the word "mission numbers:" into the first parenthesis and moved them to the end of the sentence.

P1818 Line 5 : it is maybe clearer if you give the date information first : "The data was acquired on 10 September 2009, and provided with RPC".
Reply: We changed the sentence as suggested (without comma).

P1827 Line 12: is the verb "be" correctly located ?
Reply: We do not see a verb "be" at this position… do you mean at another line? (Sentence in this line: *"…multi-temporal DEMs (cf. Höhle and Höhle, 2009). Similar to DEM co-registration, is …"*)

P1832 Line 3: please change paragraph.
Reply: OK, paragraph changed.

P1836 Line 23:"eventually"should be replace by "possible" or an equivalent adjective (this confusion seems to appear in some other parts of the text).
Reply: We would not change "eventually" to "possibly" since we believe that our penetration depth correction is correct. However, there might be some underestimation, but this is rather "potentially" as "possibly" the case. Hence, we used the word "potentially", and also replaced "eventually" at page 1820 line 22 with "potentially" and at page 1836 line 2 with "possibly".

P1837 Line 13 to 15: is the verb "present" correctly located ?
Reply: We changed the order of the words to correct for the location of the verb "present": "*This study presents, in combination with the recently recorded high-resolution Pléiades imagery, the longest time series…*"

Review #2

Summary:

Generally, following changes could help to improve the presentation of data, methods and results as well as the discussion: The climate data is now part of chapter 2, Study area. This should be part of the data section, and the location of the climate measurements should be evident from Figure 1. At least seasonal mean(s) winter precipitation, summer temperatures) should be shown in a graph, as these are discussed later. Where in the introduction only one station is located above 3000m, later on high elevation climate changes are discussed. It would help to have more clarity on the data base. The methods, results and discussion parts are mixed up somehow. The term mass balance is used for geodetic mass balances as well as direct mass balances in the same paragraphs, which is confusing. If mean annual change rates are derived from geodetic balances, it should be clearly distinguished in the phrasing from measured annual balances, as the difference could be high. The presentation of the periods is also confusing, I would recommend to present the total period 1973-2013 and the subperiods (1973-1999, 1999-2009, 2009-2013). I miss a general discussion if the accuracy of the DEMs does allow this high temporal resolution, when large parts of the glaciers show low altitude changes. The amount of snow covered or oversaturated area should be indicated in the remote sensing images, which would be nice to see in the article. The impact of the method for calculating dh on the volume change and mass balance should be more explicitly discussed. Some of the following detailed comments might just be a hint on a lack of clarity in the description, but should help to find out where changes in the text could help to avoid such misunderstandings.

Reply: Thank you for your comments, and please have a look to our detailed individual responses below. Our study does focus on the investigation of glacier variations at Muztag Ata from space, and an essential part of our research concerns the derived (geodetic) mass balance results. We did not intend to provide a detailed background of the local climate and think that such additional investigations would go beyond the scope of this already extensive study. By addressing this point, we did not employ any climate measurements at Muztag Ata as part of our study, all climate data mentioned in this manuscript was cited from already published results. We tried to make this now clearer at several points in the manuscript. Maybe there has been some misunderstanding by the reviewer, which is reflected in some parts of our author responses. In our understanding we clearly separated methods, results and discussion chapters. We agree that it should be clearly separated in between geodetic and in-situ derived mass balances, and we tried to make this clearer according to your specific comments. We also agree that the presentation of the periods in Table 4 is confusing, the order will be improved. A temporal resolution of ten years or more is generally considered as long enough for geodetic mass balance assumptions as it is the case in this study, and the problem of the short time period of only four years from Pléiades to ALOS-PRISM is clearly addressed in our manuscript (P1829 L24ff). Low altitude changes give in this context no conclusion regarding the accuracy of the DEMs and the outcome. We employed extensive uncertainty estimation, and in case that the uncertainties are higher as the observed glacier variations, this would just indicate that the variations are not significant. Please see our individual responses to your specific comments below, and thank you again for reviewing our manuscript.

Specific comments

Abstract: 1:
Does this first sentence refer to results of this study, or to direct measurements? Is there a research question to ask here to explain the aim of the study, e.g. to find if these measurements represent singularities or largescale mass balance trends?
Reply: The first sentence refers to recent results of previous studies. To make this clearer we replaced "*recent*" by "*previous*" in this sentence. To point out our research question and motivation more clearly, we added "*contrary to the global trend*", to demonstrate that the previously observed results are contradictious to what is observed elsewhere. We focus on singularities and largescale trends later in the abstract.

12: What is meant by fluctuated or advanced? Aren't fluctuations advances and retreats?
Reply: True. We observed that some glaciers show only an advance on the available images , but some fluctuated, thus showed an advance and retreated during the period of the study, or vice versa. . We changed the sentence to "*Some south-west exposed glaciers advanced or even fluctuated…*"

13: Did you really observe continuous shrinkage, or just in the resolution of your data (maybe missing some short annual or seasonal advance?)
Reply: Continuous shrinkage to this regard means that we observed significant (in terms of uncertainty calculation) and subsequent glacier shrinkage in all of the employed remote sensing data. We can, however, not preclude that intermediate short annual or seasonal advance occurred.

14: What is a visual advance, do you mean that as synonym of meaasureable, or as contrast to any other (radar?) method?
Reply: This means that no change was obvious in the imagery by visual investigation, and to this regard it means "measurable". We changed "visible" by "measurable" to make this clearer.

21: The choice of presenting overlapping periods is somehow obscure. If the accuracy of the DEMs and the amount of seasonal snow does allow a presentation of the single periods, I would prefer that. If not maybe just present 1999-2013?
Reply: The fact of having overlapping periods results from the scarce availability of stereo satellite imagery in the remote region. A part that only very few stereo satellite imageries are available that cover the site, several of them are covered too much by clouds and / or snow, or are not useful due to other limitations (e.g. acquisition season). We oppose the statement that overlapping periods would be obscure, we rather think that this approach proves the quality of the derived results. By having overlapping periods we could show that our results, derived from different sensors, are in line with each other.

1814 16-20: For which periods have these mass balances been measured?
Reply: From line 12 to 14 we mention that the subsequent studies (as Gardelle et al. (2013) from line 16 to 20) refer to the "*last decade*". We added "Since 1999" to the study of Gardelle et al. (2013) due to varying time periods from 1999 to 2008/2010/2011, depending on the study site. We now mention "*from 2003 to 2008/09"* for the study of Gardner et al. (2013) from 2003-2009 and Kääb et al. (2015) from 2003 to 2008.

23: Is this really gridded data, and what is the variability? Or do these numbers refer to a specific station (in this case we would like to know which station, coordinate, altitude : : :). What is the reference period of the presented anomaly? Are the 7.4 mm /decade significant? Please also give the annual mean and precipitation of the reference period.
Reply: The presented data is not part of this study, but was already published before by Zhang et al. (2012). We already cited this publication in the subsequent sentence, but moved the citation now directly to the referenced data to make

things more clear: *"…+0.3 °C and +7.4mm (Zhang et al., 2012)*." We think it is beyond the scope of this manuscript to further analyze the presented climate values in the publication of Zhang et al. (2012).

24 ff: What do you mean by warming? A seasonal mean would be better than a annual mean. How many stations and where, in which elevations, show changes in which climate parameter? Does that mean that close to these glacier tongues stations are located? Maybe shift this paragraph to the data section and describe the measurements more extensively.
Reply: Similar as in case of the previous comment, this was cited from previous results. Climate data was cited from Zhang et al. (2012) (see previous author comment), the subsequent statement from Yao et al. (2012). We, again, think it would be beyond the scope of our foreseen publication to analyze the results of Yao et al. (2012) more in detail, since this study should be focused on glacier variations at Muztag Ata and not on climate variations. We did not collect any climate data as part of this study. All data used in this study is already presented in the data section.

1815 20: Here comes another piece of climate, please shift that to the data section. Why do you present annual values and not seasonal ones? 27: Is summer June July August or May to September? I do not get the message: Did you compare periods (which) or calculate a trend (how) to end up with a warming of 0.7C. Is 1957 the start of your period and 2000 the end? But what did you compare that to? Please explain that more explicitly preferable in the data section.
Reply: Climate data mentioned at P1815 L20ff is cited from other publications. We think that we clearly and properly cited references from the already published climate data. Since it is not our data, it makes, hence, to our understanding no sense to move such information to the data section and to explain it more explicitly. Please also note that reliable climate data at our remote study site is really rare, and we therefore have to rely on the sparsely published data that is available to the scientific community. Our study is based on remote sensing data from space. We did, hence, NOT have compared or calculated any trends related to such or previously mentioned climate data, but we have cited the information that was available, here from Taxkorgan meteorological station that started operation in 1957 (cf. Shangguan et al., 2006). Unfortunately, we could not find published seasonal values, and we think that this would go beyond the scope of shortly presenting the study site. We could, though, find information that the mean summer temperature from 1957 to 2010 at Taxkorgan was measured at 15.1°C (cf. Yan et al., 2013b; Yang et al., 2013). This information was added to the manuscript. Summer temperature means here from June to August (cf. Shangguan et al., 2006), this information was also added to the manuscript. Please also remark that we moved L9-12 from P 1834 (Section 8.4) to the end of the study site section, and that we moved the part from P1815 L26 to P1816 L7 of the study site section to P1834 (section 8.4), as suggested by the other reviewer.

1816 3: is there a reference to cite, at least any indication where this information comes from?
Reply: The reference was cited in the subsequent sentence. We now also mentioned the similar reference at this sentence to make things clearer. Please note that we moved the section from P1815 L26 to P1816 L7 to P1834 (section 8.4), as suggested by the other reviewer.

5: Where was a warming observed – in the core? Or was it an isotope variation, which is for sure not related to a shift in the precipitation regime? What means the 'from2,0 C to 2.4 C – a range for different stations, an error bar, different periods, an altitudinal effect? Does it make sense to compare a station at 3000 m with a station (or whatever) at 7000 m? And why?
Reply: Similar as for the previous climatological measures at Taxkorgan, this is not our data, and we cited here results from the study of Tian et al. (2006). The ice core at 7000m a.s.l. was drilled by Tian et al. (2006), and the data at 7010m a.s.l. was coming from this ice core. "*The detailed annual δ18O in ice core record allowed us to compare it with the local meteorological station air temperature data. The annual variation of δ 18O in this ice core is consistent with the local air temperature record from the Taxkorgen meteorological station."* Tian et al. (2006). The comparison with the Taxkorgan station data makes sense since the temperature variations are in good agreement. "*From2,0 C to 2.4 C*" means that a "*warming trend of +2.0 to +2.4°C per decade*" was observed by analyzing the ice core: "*The regression result shows that the decadal warming trend is around 2.0~2.4°C per decade from the decadally averaged temperature at Muztagata, while only 0.18°C per decade for Taxkorgen meteorological station*" (Tian et al., 2006). We believe that we adequately presented the relevant results of Tian et al. (2006) in the manuscript which allows extracting the necessary information properly. Please note that we moved the section from P1815 L26 to P1816 L7 to P1834 (section 8.4), as suggested by the other reviewer

1818 12: Are the images free of seasonal snow?
Reply: We did not mention explicitly that imagery was acquired under the premise of having a minimum of cloud and snow cover. This is not only important for the Landsat dataset as commented here, but particularly for the DEM extraction process of stereo imagery which were acquired in summer (see Table 1). The employed Landsat dataset of 11 September 2000 is shown in Figure 1. It can be seen that the Muztag Ata study site is virtually free of seasonal snow in this image, but that the mountain range east of the site is locally affected by some snow coverage. This was the best image of all available Landsat datasets for about the year 2000 in terms of cloud and snow cover. We will integrate short information about that in the manuscript.

15ff: What about the steep parts – was the geometry of SRTM sufficient to map all the areas? Which parts were hole filled?
Reply: The geometry was sufficient as reference for co-registration of all extracted DEMs as well as for mass-balance calculations. For high resolution mapping purposes we could take advantage of our extracted Pléiades DEM at 1m resolution (see section "5.1 Glacier area and length changes"). We excluded steep parts in our data processing, as

mentioned on page 1822 L22-L24: "*The vertical accuracy of SRTM-3 decreases in case of steep terrain, and we thus only considered flat areas until a slope angle of 10° (Falorni et al., 2005)*". Hole-filled parts could be identified by a mask provided by the Consultative Group for International Agricultural Research (CGIAR) (see page 1818, L19-L21). "*SRTM voids are particularly observed at steep slopes and mountain ridges, while most of the glacier areas consist of non-interpolated data. We restricted mass-balance calculations to the original SRTM-3 surfaces and excluded gap-filled voids because of high inaccuracies* " (see page 1824, L8-11).

1819:1: What about the snow conditions then?

Reply: We are not sure if this comment is related to page 1819 L1 as indicated (section "3.6 TerraSAR-X": "…10 August and 1 September 2011 during the descending pass of the satellite."). Since imagery was recorded in summer, low seasonal snow can be expected in the ablation area. Good results were also achieved in some parts of the accumulation area, as can be seen in Figure 3. Information about that will be added in the manuscript. Phase-based methods such as DInSAR failed due to low coherence (page 1826, line 12), therefore we had to use amplitude tracking with known limitations in low contrast areas. If this comment is related to snow conditions in the SRTM dataset, please refer to section "5.2.2 SRTM-3 C-band radar penetration" and "8.6 SRTM C-band penetration depth correction" where we explain how we took snow conditions into account.

6: I did not really get if you excluded the moving ice and snow areas for coregistration. Did you?

Reply: Page 1819 Line 6 is pointing to the title of section "4 Data processing". We therefore assume that the comment is referring to "4.3.2 DEM co-registration" on page 1822 line 3. To this regard, we mentioned on line 4 to 6 that "for each DEM we calculated its difference image relative to SRTM-3 by excluding non-stable terrain such as (rock) glaciers, ice-cored moraines and lakes.". All non-stable (and moving) areas were, hence, excluded for co-registration that is based on this difference image. We might be wrong, but we cannot actually see a relationship of this comment to subsequent section "4.1 KH-9 image pre-processing" on line 7ff.

1820: 10 What about the ICE SAT footprint – which accuracy has the elevation of this data in the view of the rough terrain?

Reply: GCPs were only situated at stable and plain terrain, in general at a slope at less than 10°. Since this is was by now not clearly explained, we changed the sentence on page 1820 line 7 to 9 as follows: "*GCPs were situated at stable and plain terrain, ideally close to laser altimetry measurements of the Ice Cloud and Elevation Satellite (ICESat)…*". Accuracy of elevation datasets such as ICEsat and SRTM decrease with steeper terrain. In case of SRTM this was mentioned (in another context) on page 1822 Line 22 to 24: "*The vertical accuracy of SRTM-3 decreases in case of steep terrain, and we thus only considered flat areas until a slope angle of 10° (Falorni et al., 2005).".* ICEsat provides a much higher vertical accuracy as SRTM, and elevation inaccuracies at GCP positions can therefore be considered as marginal for ICESat as compared to SRTM. In case of SRTM is the accuracy *"stated to be ±6m relative and ±16m absolute (Rabus et al., 2003)."* (Page 1835 line 4 to 5). An ICESat-spot is 65-70m in diameter at 175m separating distance of each spot, with a horizontal accuracy of 10.6±4.5m of spot geo-location, and a vertical accuracy of up to ±34cm (±6.7 cm under best conditions) according to Magruder et al. (2007). Kääb et al (2012) successfully used ICESat in rough terrain for glacier thickness and mass change estimates over the Hindu Kush–Karakoram–Himalaya region. We did not provide accuracy information of ICESat in the manuscript due to the high accuracy of ICESat compared to SRTM, which has a much higher impact to the error budget as ICESat.

Magruder, L. A.; Webb, C. E.; Urban, T. J.; Silverberg, E. C. & Schutz, B. E.: ICESat altimetry data product verification at White Sands Space Harbor, IEEE Transactions on Geoscience and Remote Sensing, 45, 147-155, 2007.

1821 25: Would be nice to indicate erroneous parts in the map and find them in the discussion of the uncertainty of the geodetic mass balance.

Reply: Erroneous parts were excluded from the DEMs and are subsequently not affecting the elevation difference images to DEMs of other dates. Erroneous elevation values at poor quality are, hence, not affecting the geodetic mass balance and its uncertainty directly, since they were set no-data in the DEMs and, thus, in the difference images . Regarding (resulting) gap-filling and also further outlier processing for geodetic mass balance calculation, please refer to section "5.2.1 Outlier detection and gap-filling" on page 1824. By considering also the noise of poor quality elevation areas, and for clarity reasons, we would not recommend to map such parts in the difference images.

1822: 3: Did you exlude moving parts (glaciers) from coregistration?

Reply: We think that this comment is referring to the previous author comment "*1819 6: I did not really get if you excluded the moving ice and snow areas for coregistration. Did you?"* Yes, moving (glacier) parts were excluded for coregistration, based on the calculated difference images: "*For each DEM we calculated its difference image relative to SRTM-3 by excluding non-stable terrain such as (rock) glaciers, ice-cored moraines and lakes*." (page 1822 line 6 to 8).

1823: any decorrelation ?

Reply: This is probably refereeing to section "4.4 SAR image co-registration". We are not sure if we correctly understood the comment. We employed amplitude tracking instead of phase based methods, since "*it was not possible to retain the interferometric phase due to temporal decorrelation*" (page 1826 line 9). The imprecise matching of the glacier surface features was estimated over non-moving terrain, as mentioned in section "*6 Uncertainties of glacier variations*". A SNR

(signal-to-noise ratio) of 4.0 is used to select the correlated windows which are 94 % of the windows in our dataset, undergone the amplitude correlation. The offsets determined for these correlated windows are further employed to estimate the bilinear offset polynomial. Hence the amount of decorrelation is less than 6 %

11: I would see here rather a section on results with subchapters: : :

Reply: The previous chapter "*4 Data processing*" presents necessary pre-processing as well as more general data processing steps that were needed for later glacier assessment (chapter 5). This chapter now presents in three sections our approach how we assessed glacier variations with our data: "*5.1 Glacier area and length changes*", "*5.2 Glacier mass-balance*" and "*5.3 Glacier surface velocities*". Following chapter 7, after explaining the uncertainties of glacier variations, is then presenting the results.

1824: 1: So this is rather a chapter on geodetic balances, which I would like to read in the title. What about seasonal snow, the accuracy of the DEMs and the resulting maximum temporal resolution? This could be stated in a section on methods, together with the density assumption. As the geodetic balance can only be calculated for the full glacier area, especially in case of surging glacier, how did you proceed with data gaps? What was the threshold for example to skip a glacier in case a part of the area was not mapped? Why did you choose these glaciers?

Reply: We changed this title and the title of chapter 7.2 to "*Geodetic glacier mass-balance*". This is still a section on methods, as we present the steps that we employed to assess glacier variations based on our data. We hereby assume an ice density of 850±60 kg m^{-3} (Huss, 2013) (page 1824, line 18). We calculated the geodetic balance on the full glacier area. Gap "*…filling of remaining δh voids in glacier areas were employed separately for each glacier accumulation and ablation zone*"(page 1824, line 24-25). Our approach to proceed with data gaps are explained in subsequent section "*5.2.1 Outlier detection and gap-filling*". Particularly regarding glaciers of different size due to temporal changes (also regarding surging glaciers), was their size "*defined by the largest extent of the correspondent mass-balance investigation period*" (page 1824, line 20 to 21). The accuracy of the DEMs is estimated by the Normalized Median Absolute Deviation (NMAD), summarized in Table 2 and described in chapter "*6 Uncertainties of glacier variations*" (page 1827 line 10). The satellite images were acquired in summer and were virtually free of seasonal snow (see also previous author comment "*1818 12: Are the images free of seasonal snow?*"). Do you mean with "maximal temporal resolution" the minimum time difference in between acquisitions regarding DEM differencing and its uncertainties? Apart from DEM differencing of ALOS-PRISM to Pléiades is our minimum time difference 10 years (ALOS-PRISM to SRTM) which should be long enough for geodetic mass balance estimates . The *"… time period between the ALOS-PRISM and Pléiades data takes is only four years and should be considered as too short for reliable results.*" (page 1829, line 24 to 26). We mapped all glaciers as described in section "*5.1 Glacier area and length changes", and we* "*selected thirteen larger glaciers of different orientations*" (page 1824, line 16) for individual geodetic mass balance calculation. These were chosen regarding their size, their aspect, and the quality of the difference images within the glacier.

26: It is not clear how these ELAs have been derived, and how you can cross check it with satellite images, especially if there is a potential offset between ELA determination time and acquisition of the satellite data? Later is seems that you presume that this ELA has something to do with accumulation and ablations zones on the glaciers in your data, if I understand correctly. Why?

Reply: ELAs are based on snow line measurements that were obtained, among others, from the Chinese Glacier Inventory (cf. Shi et al., 2008). These ELAs were provided as elevation value per glacier. You are right, there might be a potential offset between the snowline measurements of Shi et al. (2008) and the acquisition time of the satellite data, particularly regarding the temporal baseline of our datasets. However, ELAs were not used for investigations on glacier variations, such as assessing snowline variations. We only used the (in case modified) ELA values of Shi et al. (2008) and others to separate individual glacier accumulation and ablation zone for later statistical gap-filling and outlier detection, described in the subsequent paragraph. This might not have been clearly enough described in the manuscript by now. We changed the paragraph from page 1824 line 25 to page 1825 line 1 as follows, referring to the separation of ablation and accumulation zone: "*These were separated by Equilibrium line altitudes (ELAs), based on snow line measurements from the Chinese Glacier Inventory (cf. Shi et al., 2008). ELAs were cross-checked in ALOS-PRISM and Pléiades satellite images and adapted if necessary (see Table 3).*" Moreover, we added the following note to Table 3: "*ELAs adapted from the Chinese Glacier Inventory (cf. Shi et al., 2008)*"

17ff: If I do understand correctly you set the elevation change in the accumulation area to zero? Why? Especially in case of surging glacier one would expect to miss an important part of mass balance when doing so, even on every other glacier one could not calculate mass balances without including the accumulation area.

Reply: Following the post-processing that we previously described and for individual glacier mass balance calculations, we only set missing elevation difference values as well as outliers in the accumulation zone to zero, but we kept valid values of elevation change in the accumulation areas (Page 1825 line 11 to 15). Similar statistical gap-filling and outlier detection was, however, not possible for the entire glacierized area at Muztag Ata, which is based on individual calculations in the accumulation and ablation zone, separated by the ELA. Diverging elevation changes at similar altitudes at the glacierized area at Muztag Ata hampered such an approach that we employed for individual glaciers (page 1825 line 15 to 17). Observed glacier elevation changes were in most cases comparatively low at Muztag Ata, particularly when considering the long temporal baseline of four decades. By also this taking into account, and "*since no plausible statistical replacement values could be derived, we set missing δh pixels to zero by assuming only minor elevation changes for these areas*

(cf.Schwitter and Raymond, 1993)" (line 11 to 13). We subsequently defined the elevation values within the accumulation zone of the remaining glacierized area (i.e. that was not previously calculated for individual glaciers separately) to zero since we could not derive plausible replacement values for outliers and no-data pixels. For individually investigated glaciers, however, we kept valid values of elevation difference in the accumulation area and set only outliers and missing values to zero, by considering the entire glacier accumulation area.

20: In case this is a section on results, I clearly see the penetration depth in a methods or data section.
Reply: Since this is still a section on methods about how we handled SRTM penetration depths, we would keep it at this place.

1826: 6: If this is a section on result, basic explanations of how to measure velocities should be part of a methods section.
Reply: Similar as in case of the previous author comment is this still a section on methods on how we measured velocities.

16 ff either present first the eq. 1 and explain the symbols or eq 2 and the symbols, but do not mix it.
Reply: Normally we would not mix equations, but in this case are the same two symbols (d_range and d_azimuth) part of both equations, so it would be difficult to explain it separately. Moreover, both equations together present the surface dynamics, in term of magnitude and direction. Practically, the offsets in range (d_range) and azimuth (d_azimuth) are saved as complex numbers (offset = d_range + i d_azimuth). Hence it's more understandable if these equations are written in such a way.

21: Section 6 is important, but parts of it are spread all over the text. The discussion of the uncertainties should be point by point, and the results of every step on the geodetic mass balance should be summarized at the end. In the current version, important sources of uncertainty are not discussed, and partly the quantification is missing.
Reply: We are not sure if we correctly understood this comment. This is a chapter about methods on how we estimate uncertainties in our results, which is based on well established approaches (see citations). We do not see that the uncertainties of our results are spread all over the text (do you mean the entire manuscript or specific sections?). We also think that this section is well structured, by presenting in three paragraphs the uncertainties of area change, then of geodetic mass balance, and finally of glacier surface velocities. Moreover, we think that we address in this chapter all significant uncertainties that are affected with our datasets. In case, could you please precise which important sources of uncertainties are not mentioned, and what could you be improved in this chapter? We agree that the discussion of uncertainties should be more quantitative to some extent. This discussion is presented in chapter "8.5 Uncertainties of geodetic mass-balances from optical data" (see also later comment), and we will consider some modifications to be more quantitative concerning the uncertainties.

1827: 21: The term mass balance rate is not very clear. The discussion of the effect of hole filling and skipping accumulation areas is missing.
Reply: We added "annual" to this term to address for the annual rate. We did not skip accumulation areas in our mass balance calculation, but we set no-data pixels and outliers in accumulation areas to zero, since no (statistical) replacement values could be estimated (see previous comments). We hereby assume only minor elevation changes in the accumulation area (cf. Schwitter and Raymond, 1993), and discussing the effect of varying elevation changes would be rather speculative. Please refer to section "5.2.1 Outlier detection and gap-filling" regarding our hole filling approach. On page 1824, line 27, to page 1825, line 1, we also mention well that "*Gap-filling by zero in glacier accumulation zones is a consequence of lacking statistical alternatives, but might induce biased estimates in volume change*".

1828: I do not completely understand the difference between the glacier variation chapter and the results chapter.
Reply: Chapter "*5 Assessment of glacier variations*" is a chapter on methods for specific glacier calculations, following the more general methodological chapter "*4 Data processing*" for previous data pre-processing. This methodological part as well as the chapter about uncertainties follows the chapter on results ("*7 Results*").

5 ff: This collection of numbers is nearly unreadable; the table does its job. I would rather see here some text.
Reply: We agree and we will modify the text to make it more readable. Moreover, we will provide an additional table to present the selected values in the text.

25: What do you mean by steep tongue? Possible not an average slope or something like that?
Reply: It can be seen in our Pléiades data that the front of these glacier tongues is quite steep, which is proven by the calculated slope of its derived DEM. This is particularly visible in the hillshade of Figure 2a for Kuokuosele Glacier.

1829: 1: This should be GEODETIC mass balance
Reply: We changed the title of this chapter to "*7.2 Geodetic glacier mass-balance*".

26: Seem to fit is not very precise, especially as it is not clear to what.
Reply: We changed "seem to fit" on line 27 to "is well in line"

1830: 3: We all know that this assumption is not valid, so what is the sense of the extrapolation? Would be nice to have the velocity map.

Reply: We already provided a velocity map in Figure 3, and mentioned a reference at this position. You are right, this assumption might be irritation, and we therefore deleted the sentence, by only keeping *"… corresponding to ~70m per year (Fig 3)."*

1831: 5: Geodetic mass balances

Reply: We would – in this case – keep the title as is ("*8.2 Glacier mass-balances*"), since we here discuss our geodetic mass balance results with other non-geodetic mass balances. Moreover, we discuss until now unpublished in-situ mass balance data and compare it with our results.

10 ff: I find the wording a bit confusing, and think it could help to add either direct or geodetic to the mass balance results. Is there any possibility to present the various results, periods, methods, authors and regions in a table?

Reply: P1831 L10ff refers to direct measurements in the field published by Yao et al. (2012), with some extended measurements that were by now not published. We mention well that these are direct measurements from measuring stakes, and in the last sentence of the paragraph we conclude that "*the in-situ data is on average slightly lower but in tendency in good agreement with our geodetic estimations*". In the subsequent paragraph we also think that we correctly address the measurement base. If desired, we could summarize the various results in either a table or even a figure, but we need to think about on how we will do that.

1832 7: Fedchenko 20: could also be the case that a surge type glaciers stores mass in the accumulation are, despite mass loss at a tongue. So basically what happens at one single part of the glacier never can give an indication on total mass balance.

Reply: We fully agree. However, the volume loss mentioned for Fedchenko considers all parts of this glacier (see Lambrecht et al. 2014).

23: terminus position I suppose

Reply: Yes, terminus position, we added "terminus" at this text position.

1833 9: What is the toe? IS this tongue?

Reply: Toe was here used in the context of tongue. We replaced toe by "terminus", since the word "tongue" was already used quite often.

27: Why opening here the field of Holocene oscillations? Maybe better in the introduction?

Reply: As also suggested by the first reviewer, we moved this paragraph to the introduction.

1834

5ff: I do not understand the sentence with ablation in summer and why we find it here. The next sentences on the climate at 5910 m is a clear contradiction to the introduction, with only one station above 3000 m located close to the study site. This climate data would rather fit into the climate section before – why is it placed here? Lines 5 to 19 are either rather speculative or fit into the climate section.

Reply: We well cited the references of Seong et al. (2009a, b) regarding this and the subsequent sentence, these are not our results, but part of literature discussion. We agree that this is a speculation and therefore also write "*this might be one of the reasons…*" But we think it is valuable to discuss here the possible reasons for the balanced budget based on existing findings and the literature. These paragraphs fit, hence, well in the discussion section "glaciers response to climate".

19-21: I presume the colder years are too few to cluster in a period. It is unclear which normal period you refer to when classifying these years as 'cooler'. Cooler than what? And how much? And how large have the precipitation changes been?

Reply: This information was cited from Shangguan et al. (2006), please see also the relevant response to a similar comment of the first reviewer: Shangguan et al. (2006) do unfortunately not provide further data, expect of an additional climate diagram from Taxkorgan meteorological station. We could not find further information to better characterize these "cooling periods", and had to rely on the following information of Shangguan et al. (2006): "*This time-span included three cold periods: 1961–68, 1973–77 and 1985–93*.", and "*However, some glacier advances might be a response to the three periods of cooling and the increase of annual precipitation…*"

21 ff: We have just gone through a chapter on uncertainties, so that we do not want to go back to this once more. In any case, the impact on mass balance is not discussed!

Reply: The previous chapter on uncertainties ("*6 Uncertainties of glacier variations*") was a methodological explanation on how we estimated uncertainties in our results. This chapter "*8.5 Uncertainties of geodetic mass-balances from optical data*" now discusses uncertainties that might have an impact on our geodetic mass balance results, and demonstrates that our results are coherent and in line with what we would expect. We think this discussion is well placed here, and that the most import impacts are discussed. We, however, agree that this section could be more quantitative to some extent (see also previous comment), and we will, hence, consider some modifications to this regard at this section ("8.5 Uncertainties of geodetic mass-balances from optical data").

1835: 13: This would fit in a metheod section, or in the chapter on penetration depth.

Reply: We already presented how we considered for SRTM penetration depth and how we corrected it in the methods chapter "*5.2.2 SRTM-3 C-band radar penetration*". This is now a discussion chapter regarding penetration of the SRMT C-band beam, previous approaches of its correction, and how it affects our results.

1836: 24: Please give also the second period.

Reply: This is probably referring to the previous sentence *" …are slightly but insignificantly negative before 1999 (…) and positive afterwards (…)*."? If we understand right, we address both periods in the sentence of P1836 L24: "*This might still result from an eventually underestimated SRTM-3 C-band penetration into snow and ice*". Additionally, we address a possibly wide-regional "positive anomaly" for the first period, which we would not confirm for the second period: "*Slightly positive observed budgets after 1999, however, could possibly reflect a regional-wide positive anomaly with increasing snow accumulation from strengthening westerlies*. ".

Table 3: please organize the last column similar to the previous one, the -+ in one line. How is the ELA calculated?

Reply: The last column is currently organized so that the last +- sign in the parentheses is in one line. We did not succeed to manipulate the Latex document in a way that the values before the parentheses are also in one line, maybe this could be considered for final typesetting before publication. The ELA origin is presented in section "*5.2.1 Outlier detection and gap-filling*", please see also previous comments related to the same ELA data. To avoid confusion and to address for similar comments of the first reviewer, we now mention in a remark of this table its origin "*ELAs adapted from the Chinese Glacier Inventory (cf. Shi et al., 2008)".*

Table 4: See main remark on periods (Main remark: The presentation of the periods is also confusing, I would recommend to present the total period 1973-2013 and the subperiods (1973-1999, 1999-2009, 2009-2013).. Annual mass balance: Should be mean annual geodetic mass balance.

Reply: An inappropriate organization of the periods was also remarked by the first reviewer, and we will improve the periods accordingly in the table. In the title, we now mention "*geodetic mass balance rates*". We would not add "annual" since this is already implied by "rates".

Figure 1 : Stations lacking (Main remark: … and the location of the climate measurements should be evident from Figure 1)

Reply: We will improve this figure, as also commented by the first reviewer.

Figure 3: Below T3 some stripes are visible – is that an artifact?

Reply: As TSX is very high resolution data, it details the precise displacement offset results. However, to ensure the possible artifact, especially in featureless accumulation zone, we used SNR threshold of 4.0 and discard the possible decorrelated offsets (see chapter "4.4 SAR image co-registration").

Figure 4: The ELA is a calculated value, and could not be indicated in an image as line as done here. Is this a snow line, or a contour line of elevation? What is the black area?

Reply: We simply used the ELA to separate accumulation from ablation area needed for statistical gap-filling and outlier handling, as presented in section "5.2.1 Outlier detection and gap-filling" (please see also previous comments related to similar questions). The origin of the ELA is also described in this chapter. If desired, we could include in the figure caption that the ELA was estimated based on Shi et al. (2008), as in case of Table 3. To which black area are you refereeing to in Figure 4? Do you mean the shaded area from the hillshade in case of steep south-east exposed slopes?

Figure 5: Where does the volume loss outside the glaciers come from?

Reply: The visible elevation change stems from the uncertainty of the utilized DEMs. It is particularly observed when differencing with the KH-9 Hexagon DEM and reflected in higher NMAD values (see Table 2). We already mentioned this in the discussion at P1835 L1ff: "*KH-9 Hexagon shows high noise at low-contrast terrain in its DEM, but much better results at debris-covered and crevassed glacier surfaces.*". To this sentence, we added *"(reflected in higher NMAD values)*" after "*in its DEM…*".