We thank the two reviews very much for their critical but encouraging suggestions. We found the referees' comments most helpful and took them seriously into account. The most important change of this manuscript is that we treated Inylchek Glacier as two individual glaciers Southern Inylchek Glacier (SIG) and Northern Inylchek Glacier (NIG), as they are indeed, separate glaciers. In addition, we also generated a KH-9 Hexagon DEM for the accumulation region of the two glaciers (SIG and NIG) and calculated the volume and mass changes for the period ~1975 - 1999. Hence, the title was changed to "Mass changes of Southern and Northern Inylchek Glacier, Central Tian Shan, Kyrgyzstan during 1974-2007 derived from remote sensing data". Furthermore we reassessed the uncertain high surface elevation gain in the accumulation of SIG region for the recent period. Following the suggestion of the reviews, the Figure 2 was moved to supplementary figure s3 and Table 2 was moved to supplementary table 1. We also added 4 supplementary figures and have redrawn all figures. Several sentences have been rephrased and finally we carefully proofread the English.

A detailed reply to all referee comments is presented below:

Referee 1

P2574L9: ... showed strong velocities ==> change to showed high velocities

Reply: "strong velocities" was changed to "high velocities".

P2574L10: I do not really understand this sentence – is it stagnant, then it does not flow or it flows at low speeds towards Lake Merzbacher

Reply: The sentence was changed to"Velocities of the SIG reached ~100 m/a in 2002/03 with a slight decrease in 2010/11. The main flow direction of SIG is towards Lake Merzbacher. The velocities at the end of the tongue after the lake, however, are likely very low."

P2574L11: Better rephrase: The northern branch of Inylchek Glacier advanced by … while the southern branch continuously retreated since 1974 (* km2).

Reply: The sentence was rephrased to: "The area of NIG increased by 2.0 ± 0.1 km^2 (~1.3%) in the 1974 – 2007 period. In contrast, the SIG shrank continuously in all investigated periods since 1974. "

P2574L16: ALOS PRISM

Reply: Changed to " ALOS PRISM"

P2574L21: A possible thickening … and a clear thickening" - reword to e.g. indication for a thickening have been revealed ...

Reply: The sentence was rephrased to "A continuous mass loss of both SIG and NIG was observed from 1974 to 2007. A slight mass loss was observed with -0.3± 0.4 m w.e.a^{-1} for NIG and -0.1± 0.4 m w.e.a^{-1} for SIG between 1999 and 2007. And the dominant mass loss

was observed with -0.3± 0.1 m w.e.a^{-1} for NIG and -0.5 ± 0.1 m w.e.a^{-1} for SIG for the period 1974-1999. ".

P2574L25: „... possibly due ...“ - in the following text this is not stated as possible, but attributed as surge
Reply: Yes. It was attributed as surge. Hence, we deleted the "possibly"

P2574L28: As it is, the sentence describes a relation between glacier velocity and debris coverage. The sentence is not clear.
Reply: The sentence was deleted. We think "Furthermore, our result indicated that the glacier thinning and glacier flow close to the Lake Merzbacher dam was influenced by Lake Merzbacher." is more useful.

P2575L8: ”... glaciers shrank also ...” – also to what?
Reply: You are right. It is our fault. It is not only... but also. Hence, the phrase was updated to "glaciers shrank not only in this central region "

P2575L13: “In addition, glaciers in Central Tian Shan are polythermal ...”, backing this statement by observations or a reference would be adequate.
Reply: Agreed; a reference (Aizen et al,1997) was added.

P2575L17: rephrase this sentence, very complicated formulation and difficult to understand
Reply: Agreed. In this sentence, we want to express that surface mass balance is directly linked to climate. Hence, we updated the sentence to: “only changes in ice thickness and mass balance can be directly linked to climate and runoff.“

P2575L19: “Glacier mass balance ...” - the authors most likely refer to surface mass balance and should consider the terminology of the UNESCO glossary throughout the manuscript.
Reply: The geodetic method does not only capture the surface accumulation and ablation. Internal or basal processes which can have also an influence on the surface elevation are, at least partly, also captured by DEM differencing. Hence, we consider the term mass balance to be more correct here.

P2575L23: Refer also to Kääb et al., 2012: Contrasting patterns of early twenty first-century glacier mass change in the Himalayas, Nature 488, 495-498.
Reply: Thanks. We refer now also to Kääb et al. (2012)

P2575L15: ALOS PRISM, SPOT-5 HRG
Reply: Improved as suggested.

P2576L1-5: Refer to Fig. 1. Locate mountains/glaciers referred to in the text in Fig. 1.
Reply: Agreed. We refer now to Fig. 1 and located the peaks in Figure 1.

P2576L1-14: Please clarify the relation between glaciers in the Ak-Shyrak Massifetc and the

IG.

Reply: The Ak-Shyrak Massif is the second largest glacierized massif in the Central Tian Shan. Hence, we added the information "the second largest glacierized massif in the Central Tian Shan"

P2576L4: mass descriptions should be expressed as m a^{-1} w.e.

Reply: Mass description can be expressed in m a^{-1} w.e. or m w.e.a^{-1}. We decided to use the latter which is commonly used in many other studies " a mass loss of 0.42 ± 0.23 m w.e.a-1 ".

P2576L26: ELA, please introduce any abbreviation first

Reply: Thanks. It was improved as suggested “equilibrium line altitude (ELA)”

P2577L5: The sentence is not clear. Do you mean 1500 to 2000 m ^{3}/s?

Reply: Yes, it means " 1,500 m^3/s to 2,000 m^3/s ". It was also changed to " 1,500 m^3/s to 2,000 m^3/s " in the manuscript.

P2577L7: Please provide a quantitative reference for the velocity measurements(e.g. Li et al; Hagg et al.)

Reply: Agreed. Two references (Li et al (2013) and Nobakht et al. (2014))are now cited.

P2577L8: It should be Merzbacher Lake not Merzbacher Lake

Reply: Yes. This typo was corrected.

P2577L10: Please mention Aksu River in Fig 1.

Reply: Agreed. Aksu River was added in Fig1

P2577L22: estimated or assumed?

Reply: It was estimated.

P2577L24: “The mean annual temperature is about -7.7°C …” - do these values refer to one or a mean of the weather stations? Please provide a clear location and elevation, otherwise it's a useless statement. Please indicate in Fig.1

Reply: agreed. It was observed at the Tian Shan Station located at an altitude of 3614 m asl. However, the weather station is slightly east of the region. Therefor we included the altitude, longitude and altitude in the text. "The mean annual temperature is about" was changed to " The mean annual temperature at Tian Shan station (3614 m asl., 78.2°N, 41.9°E) is about… ".

The methods section is very extensive and might be shortened particular in regard to data description. On the other hand important facts like penetration depth and coverage of the different DEMs are not addressed sufficiently. Many numbers on accuracy and errors should be backed by references.

Reply: Agreed. The methods section was shortened but the key information for our

research were kept. We also re-organized the sentences. Further improvements are: we added a supplementary figure S1 to show the SRTM extension and data voids, a supplementary figure s2 to show the hypsometry (including the coverage of the different DEMs). We also added reference for KH9 images (Surazakov et al., 2010), for radar penetration (Gardelle et al., 2012). We investigated that the penetration was found a variation from 1.7 to 6.0m with maximum 9.0m (std dv. 1.9m).
More details about the radar penetration is now included in section 3.5 (see below).

P2578L20: Perhaps it would be appropriate to state the official error by the processing teams and error values by other studies. Gorokhovich et al. (2006)might not be the most appropriate reference for such high mountain ranges as the Tian Shan.
Reply: We referred to Gorokhovich et al. (2006) as they evaluated the accuracy of the mountainous terrain with likely similar conditions to those in the Tian Shan. We refer now also to Surazakov et al (2006)' which have evaluated the difference between SRTM and Map-based topographic data in Tian Shan.

P2579L1: This statement is true in all cases. Surface conditions, firn structure and moisture content are important for the penetration depth of radar signals. Hence, this statement is to general and the authors need to address the penetration issue in more detail, in particular, as it may contribute differently to the error term in different elevations and glacier surfaces.
Reply: We agree and are aware that there could be also a small penetration of the x-band.However, there are no in-situ measurements of the penetration available. Estimating the penetration based on a comparison of the SRTM Band X and SRTM Band C is one of the most suitable ways for an estimate and can also be applied to other regions (e.g. Gardelle et al. 2012).The elevation difference between the x-band and c-band radar along with the altitude is shown in Supplementary Figure S4. In addition, the analysis of the snow extent from a Landsat ETM from 18 February 2000(which is close to the time of SRTM acquisition showed).We calculated the mean penetration in each altitude zone, and found a variation from 1.7 to 6.0m with maximum 9.0m (std dev. 1.9m). In this case, before evaluating the penetration, the mean penetration in each altitude zone was substracted.
Considering the difference between band x and band c in debris-covered regions less than 1.7 m which were shown to be discontinuous, and the penetration of band x in debris-covered regions are narrow.
The uncertainty of whole glacier (debris-covered region and debris free region) was evaluated according to the standard deviation (1.9m) and it was assumed that the possible slight penetration of the x-band radar beam is within this uncertainty range.

P2579L14: B/H. Please also explain for non-photogrammetrists
Reply: Agreed. It was changed to "Base to Height ratio(B/H)".

P2580L13: "... lines of demarcation ..." - this terminology sounds a bit strange and the reviewer never came across it for a glacier outline – sounds more like a line under political dispute or border line for a property.

Reply: You are right. "lines of demarcation" was changed to "delineation"

P2580L21: One might doubt that a glacier boundary can be identified on a subpixel level manually. Hence, at least an error of 30m should be assigned for Landsat TM/ETM+ multispectral imagery.
Reply: We do not fully agree. One can identify the boundary also visually on subpixel level. In addition, half a pixel is commonly used, see e.g.Bolch et al., (2010) or Granshaw and Fountain (2006).

P2580L27: “Finally, ...” - the study is not at its end yet. It is unclear how the authors come to their overall error. Please provide a basis for the computations from the uncertainties of the individual outlines to the overall error budgets.
Reply: Agreed, "Finally" was deleted. Because the glacier change usually occurred at the tongue of the glacier, and the uncertainty was derived from delineation of glacier change. Hence, the uncertainty of glacier area change is also evaluated considering the absolute change by using the buffer method. Hence, we adopted the propagation of uncertainty.

P2581L13: How was this accuracy assessment done? What are the results? How do the values translate into m/a^{-1} used as in the graphs? Removal of unsound values is not an accuracy assessment! The errors given are probably over all error. One might suppose that the errors depend on the contrast and features in the image. Please give more details also on the settings of the tracking, window and step size, signal-to-noise ratios, etc.
Reply:
How was this accuracy assessment done?
Reply: For accuracy assessment, we calculated RMSE value, which was determined by an analysis of significant displacements/ velocities, as parameter for erroneousness. Conciseness is derived by the ratio of RMSE and the resolution of the respective input data.
As well as Inylchek glacier, some nearby glaciers were observed with the named input datasets. The calculation of the RMSE values considers all observations. Therefore the survey compasses a huge amount of significant and non-significant velocity dates, which allows a solid accuracy assessment.

What are the results?
The results were at subpixel accuracy levels of the velocities of the named values.

How do the values translate into m/a-1 used as in the graphs.
The given velocities show the average for one year as the time lag between the used input data was ~1 year. The exact time interval for the 2002-2003 period was 368 days and 352 days for the 2010-2011 period.
The measured displacement vectors have been calculated by the pixel shift between two images in NS- and EW-direction and the final horizontal displacement was determined as the euclidean distance. By the ratio of these values and the time difference of the used input data we finally got the annual average velocity.

Removal of unsound values is not an accuracy assessment!
Of course, the removal of unsound values has nothing to do with a proper accuracy assessment; however, it is indispensable in order eliminate erroneous displacement vectors.
"Unsound values" weren´t removed! The RMSE were used

One might suppose that the errors depend on the contrast and features in the image.
Of course the errors depend on the quality of the used images. We wrote nothing against it! The written values are concerned with the illustrated input data and the used tracking method.

Please give more details also on the settings of the tracking, window and step size, signal-to-noise ratios, etc.
For feature tracking we used the method of phase correlation implemented in the ENVI Add-on Cosi-Corr. The pro-processing of the used ASTER comprises a co-registration on a subpixel level following Leprince et al. (2007). As a vertical reference we used the CGIAR SRTM3-DEM v4.1 bilinearly resampled to 30 m.
Landsat data were considered to be quasi-coregistered, because of the same registration method, sets of GCPS and vertical references were used for orthorectification.
The respective window size was determined in dependence upon the annual average velocities given in the literature (e.g. Mayer et al., 2008 for Inylchek glacier). The approach is hierarchical, so for initial calculations larger sizes were used.
If strong displacements are expected or if the input datasets are noisy, the window size needs to be adjusted accordingly (Ayoub et al., 2009).
For the 2003-2004 period Mayer et al. (2008) detected an annual average velocity rate of up to 90 m/a for Southern Inylchek Glacier. According to e.g. the resolution of ASTER imagery (3N, 15 m/Px) , the theoretical search distance/ window size would be defined with 12 Px. However, with regard to the uncertainties/problems mentioned beforehand, the final window size was 32x32Px. The initial one amounts to 128x128Px.
The step size for ASTER was defined with 4 and for Landsat with 2. Thus, both displacement maps have the same resolution of 60 m.
The signal-to-noise ratio (SNR) was calculated by COSI-Corr. A well-established value of 0.90 was used as threshold to remove obvious errors.

To summarize: In the manuscript, we added how to perform the tracking for landsat and aster: "The tracking depends on software Cosi-Corr - therefore the method of phase cross correlation was used. For ASTER data measurements a previous subpixel-coregistration was done as described in Leprince et al. (2007). The elevation source was a resampled version of the CGIAR SRTM3-DEM v4.1. Landsat data sets were assumed as quasi-coregistered because the same registration methods were used, GCPs and elevation source between the input imagery (considering the given meta data). According to an annual average velocity of SIG (up to 90 m/a) observed in 2003/2004 (Mayer et al. , 2008) and images' resolution, the step size is 4px for ASTER and 2px for Landsat, so both

displacement maps have an resolution of 60 m.".

P2581L25: How was the Hexagon DEM generated? Give more details, what is theoverall quality, are there regional quality differences (s. figure)?

Reply: We improved the description of the DEM generation as follows: "
The study area was covered by two KH-9 stereopairs from the mid 1970s. For the stereo processing we measured 38 Ground Control Points (GCPs) for the DEM covering the lower part of Inylchek Glacier and 47 GCPs for the stereopair covering the accumulation region of Inylchek Glacier with a final RMSE of ~1 m. GCPs coordinates and elevations were derived from Landsat 7 ETM+ scenes and the SRTM3 DTM. For the processing the frame camera model in LPS 2013 was used and the final resolution of the KH-9 DEMs was 25 m." More detail was referred to by Piezconka et al.(2013). Mismatching partly occurs in the accumulation regions and areas with cast shadows where less image contrast is prevailing and thus, influencing the quality on a local level. More detailed information about KH-9 after outlier-filtering is shown in supplementary Figure S2.

P2582L6: The last sentences needs to be stressed much better in the abstract and conclusions. The coverage of the HEXAGON image strongly limits the entire analysis since elevation change values for the entire glacier are partly inter-/extrapolations. e.g. Tab 2

Reply: We agree. However, we found and processed an additional Hexagon scene which covers the accumulation region of both glaciers. This enables the calculation of the mass balance for both glaciers.

P2582L11: reformulate "... match relatively well to each other ...", be precise and give a mean and median error.

Reply: This sentence is only used to explain why a co-registration should be performed. The uncertainty measures are given later in the manuscript.

P2583L6: The difference between x-band and c-band is NOT the mean penetration! You measure the penetration difference, since also X-band has some penetration depending on surface conditions! X-band SRTM also has a better resolution, was this considered? How can no penetration be assumed in the debris-covered part? What were the DEM differences in ice&snow free flat areas? How was the uncertainty of radar penetration finally estimates?

Reply:

We are aware that both, C- as well as X-band, are penetrating into the ground depending upon the surface conditions (snow moisture etc.). Thus, it is likely that the comparison of C- and X-band will underestimate the penetration of the C-band. The calculated mean penetration depth can therefore be considered as a rough estimate.
We found a Landsat ETM on 18 February, 2000. It was used to infer the surface condition on 11 February 2000.We resampled both DEMs to 30m, then comparison between x-band and c-band. There are not large differences, and the difference between the two DEMs was considered as penetration following Gardelle et al. 2012)(see also supplementary Figure S4).For the uncertainty of radar penetration, first we calculated the penetration

according to the altitude zone(interval 100m); then we gained the mean penetration in each altitude zone; then we calculated the STD bias of each altitude zone and evaluated the uncertainty. The penetration of debris covered region in both glaciers is less than 1.7m which was less than 1.9m of the uncertainty.

P2583L22: Tab.4 does not follow a clear structure and needs more description in the text. Why not just showing differences, before after registration + yxz of the GPS points. It is a bit unclear for which DEM after coregistration the values refer.

Reply:Agreed. Now, Table 4 was changed to Table 3. Only the SPOT-GPS difference was kept in Tab.3. In the manuscript, the following text was added "In order to verify the accuracy of the DEMs, we also randomly collected six GPS points by using Uni-Strong GPS-RTK in situ surveys from 2010. Among the GPS points, three were in the debris covered region, two were in the glacier free region and one was in the glacier region (Table 3). The mean difference between GPS and SPOT DEM is -8.2 m with standard deviation 6.6m before co-registration. After co-registration, it is -0.4 m with a standard deviation of 5.7 m."

In total the error budget / estimation needs improvement and clarification. Although the authors tried to give considerable detail – they miss to draw a clear picture nor they provide clear computations or a synthesis. Is there a difference made between the error assigned to the DEMs and an error resulting from the extrapolation to uncovered areas. How are spatially variable errors addressed(see figures with DEM differences where those are obvious)
Reply: The DEM differences were evaluated by two aspects. Firstly by evaluating elevation differences over glacier-free terrain (cf. Tab. 2). Secondly by comparing to GPS surveys (cf. Tab. 3). We put more emphasis on the uncertainty estimation and improved the text accordingly. We now write "The DEMs were evaluated from two aspects: One was evaluated by DEM difference with each other in glacier free region; another was evaluated by GPS survey. The different DEMs were subtracted from each other to calculate the elevation differences. Outliers identified by elevation difference values larger than 3σ are excluded in the calculation of elevation change (cf. Gardner et al., 2013; Gardelle et al., 2013) between 1999 and 2007.", For more detail, please see the section 3.5

P2584L15: It remains unclear why glacier flow was derived. What contribution to the overall analysis did it provide? Since the two flow fields look quite similar a difference map could have pronounced the changes better.
Reply: The main reason why we included the velocity information was to show the relationship between glacier dynamics and glacier changes. This was not done before and we found it suitable to include our own measurements in this article to directly compare both components (In addition, we put more emphasis on the parts below Lake Merzbacher which is not the case in Li et al (2013), Nobakht et al. (2014) .
The difference map of the velocities is now shown as supplementary figure S5.The velocity measurements help also to confirm a tributary glacier surge.

P2585L13-26: It would be clearer to present all values of elevation changes for each period in Tab. 6. Please provide surface lowering/thinning data as negative values throughout the whole manuscript. Why did you mostly consider the DEM differences from SPOT-KH9, which obviously covers the smallest area. Is there any synonym for 'significant' or at which level are the observed elevation changes significant? Did you test for this? How was the extrapolation on the entire KH9-SPOT5 area done?

Reply:

- It would be clearer to present all values of elevation changes for each period in Tab. 6.
  Reply:Agreed. We present now all values of elevation changes in one table (now Table 5)
- Please provide surface lowering/thinning data as negative values throughout the whole manuscript.
  Reply: Thanks. We kept elevation lowering/thickening being consistency with positive and negative

- Why did you mostly consider the DEM differences from SPOT-KH9, which obviously covers the smallest area.
  Reply: We used KH-9 DEM in1974 and 1976 to calculate the DEM difference/mass balance for the ~1975 - 2007 period.
- Is there any synonym for 'significant' or at which level are the observed elevation changes significant? Did you test for this? How was the extrapolation on the entire KH9-SPOT5 area done?
  Reply:Significance is a statistical term, however we did not perform a statistical analysis. We assume that the elevation change is significant when the value exceeds the uncertainty. Now, we used the 1976 KH-9 and recalculated the glaciers mass budget, though, there are still small data gaps in the accumulation regions.
  In section 3.6. The two sentences was added "For the lack of couple of altitude zones, we used the maximum, minimumand middle elevation change to make up those lack according to Figure 6. However, the weight of area of those regions are few (please see the supplementary figure 2), it is not sensitive for calculating mass balance using Area-average mass balance and could be neglected."

P2585L15: I wouldn't say 'overall thickening', because actually the whole glacier thinned between 1975 to 2007. It would be easier to follow the logic of this sentence when it would be divided into two.
Reply:We agree. However, this sentence was now omitted because the SIG and NIG were analysed individually.

P2585L17: Figure 5 is not referred to in the text. The Figures should be presented in a chronological order.
Reply: Figure 5 was changed as Per Figure 4. It was referred to in section 4.3. In addition Figure 5 was updated and now the figures are presented in a chronological order

P2585L21: Do you mean Table 6? The period 1974 to 1999 is not listed in Table6! Could you please explain why you use the difference derived from SRTM-KH9here and not SPOT-KH9 as above? It is hardly reproducible.

Reply: Now, Table 6 was changed to Table 5. We used the difference derived from SRTM-KH9 here because the surging event occurred in 1996. Hence, SRTM-KH9 is better to observe this event than SPOT-KH9.

P2585L22-23: Please refer to Tab. 5 and Fig. 4. The sentence does not clarify that your assumption of a surge event results from glacier thickening and advance. Could you also provide surface velocities for this period?

Reply: Agreed. This sentence is not suitable here. Unfortunately, we cannot provide surface velocities due to lack of suitable images in 1996/1997.

P2586L8: Do you mean Fig. 3?

Reply: Sorry. Here it is fig.2.

P2586L9-11: The altitudes mentioned in the text are not clearly identifiable in Fig6. This makes the text very difficult to follow and unclear. It also remains unclear how the areas not covered by a DEM at the respective data are considered/compensated (==> should have been addressed in the method section).

Reply: We are sorry. What you are writing is correct. However, to keep Fig 6 tidy, we only mark several characteristic points in Fig6 to help the identification.
The coverage of DEM is shown in figure 4.
We now include supplementary Figure S2 which also shows the coverage in each altitude zone. We used the mean value of the samples (covered area) in each altitude zone as the value of the whole samples in each altitude zone. In section 3.6, we mentioned " Thus, the mean volume of each zone was used to calculate the elevation change."

P2587L18: Which information?

Reply: It is "existing results"

P2587L20: What is the difference (dates, image, processing techniques) of Nobakht et al. (2011) and the results here? Both seem to have used Landsat and ASTER data. Splitting this sentence in parts would also be a good idea.

Reply: Nobakht et al. (2011) used Landsat images acquired in Feb.,2002 and Mar. 2003 and July, 2010 and September, 2011. In our study we used ASTER images from Aug., 2002 and Aug. 2003 and Landsat images from August, 2010 and August, 2011. The processing technique was the same for both datasets (using the ENVI Add-on COSI-Corr). The information was included and the sentence re-organized.

P2587L23: A velocity rate would be velocity/time hence a de-/acceleration, but the units indicate velocities.

Reply: We provide velocity in formation per time (year). However, as the term rate is

misleading here we just wrote "velocity".

P2587L23: Unclear what is meant. Please verify logic of the sentence
Reply: Agreed. It was re-organized to: “The velocity near the Lake Merzbacher in the period 2003/2004 (75 - 90 m a^{-1}) is also in agreement with in-situ measurements (80 - 90 m a^{-1}) by Mayer et al. (2008).”

P2588L1: Is there really calving observed and how can a velocity be assigned to calving? Reference to Fig.5 is unclear here as the resolution does not allow any identification of velocities near a lake. Indicate the location in the graph.
Reply: It was reported that calving is one of the components of lake volume supply (Ng et al., 2007) which was due to the surface velocity increased from the inner towards the outer part of the glacier blend observed in 2005 (Mayer et al., 2008) .
Figure 5 was updated and the location of the Lake dam was pointed out.
So, in manuscript, "Glacier calving could be observed for the SIG with mean velocities of up to 0.4 m day^{-1} between 2009 and 2010 (Nobakht et al., 2014). Furthermore, there was huge mass loss for the period 1974 - 1999, 1999 - 2007 near the lake dam." was mentioned.

P2588L20: an altitude higher than…
Reply: Agreed. It was changed to "higher".

P2588L26: Geodetic mass balance is meant ==> again, keep terminology clear. Please give some reasoning for the use of a comparably high sensitivity.
Reply: Agreed. It is geodetic mass balance.
This sentence was changed to" This tendency is in line with our results for both SIG and NIG where glacial thinning was found during 1974 - 1999 followed by non-significant mass changes between 1999 and 2007 with strong thickening between 4,400 and 5,400 m asl. (Fig. 6)". The reasoning was probably due to the climate change (higher summer temperature and decreased precipitation during 1974 and 1999; and lower summer temperature and slight decrease precipitation)(in section 5 last paragraph).

P2589L18: “This result ...”, the statement by Paterson &Cuffey (2010)? clarify
Reply: Yes, So it was changed to "their results"

P2590L2-6: reduced glacier flow, you did not mention any indication for are duction of flow speeds nor did you measure this. The link to englacial conduits remains unclear.
Reply: It was mentioned in the last sentence in the section of 4.1 glacier flow. However, we do not have results of the englacial conduits. It was only a guess. Hence, we deleted it and the sentence was changed to" Therefore, the significant mass loss can be explained by the influence of backwasting at ice cliffs and melting at supraglacial ponds (Fujita & Sakai, 2009; Han et al., 2010; Juen et al. 2014) but likely also due to reduced glacier flow from the accumulation region (Quincey et al. 2009; Schomacker, 2008; Benn et al., 2012)."

P2590L16: The conclusions read more like an abstract – please provide real conclusions and impacts of this study. The last sentences are not clear at all.
Reply: Agreed. We updated the conclusions and improved the last sentence to " Thus, glacier thinning and glacier flow close to the dam was influenced by Lake Merzbacher and more detailed investigations are needed to understand the influence of this lake to the glacier's mass balance besides debris cover and climate change."

The quality of the figures needs to be considerably improved. Often the legend cannot be read or even the entire figure.

Fig.1: It is difficult to differentiate the different DEM coverage. Locations and place names are missing! Needs complete rework. Scale bar is cut to the edges.
Reply: According to two reviews' suggestion we have improved the Figure 1. Aksu River are labelled in Figure1. Different DEMs coverage are shown with were distinguish with colour. Scale bar was improved.

Fig.2: Why only SRTM and SPOT? You also address ALOS PRISM, similar figures would be helpful at least in a supplementary file
Reply: Agreed. According to two referees' suggestion, we have added SRTM-ALOS PRISM co-registration in supplementary file.

Fig.3: Legend and dates cannot be read. Arrows for flow direction cannot be identified. Figure is kind of useless as is. Since ice dynamics are only a marginal objective and do not reveal substantial new information in regard to other papers, consider revising the figure for velocity change or better integrate the velocity information in the paper.
Reply: Figure 3 was updated.
We mentioned the marked points, and two individual figures were marked 3a and 3b.the graphics themselves could be bigger and become readable.As mentioned above, earlier works did not focus on the velocity of distal part of the glacier tongue of SIG. Our work compensated for the deficiency.

Fig.4: Again of bad quality in print. Lines cannot be identified well, place names(Merzbacher lake) missing. Scale bar needs improvement.
Reply: We have updated the glacier extent. Lake Merzbacher was mentioned in figure1. Scale bar was improved.

Fig.5: Very small, impossible to read numbers/legend in a printed version. There are sometimes obvious offsets on slopes also on ice free areas (same magnitude as changes) that are not discussed in the text (a, b). In particular in panel (b) the large elevation increase in the northern ice free area north of the glacier is striking – same magnitude as the elevation change on the glacier. Please explain in the Text. It gives the impression that the quality of one of the DEMs is very heterogeneous and should not be described by one single error value. The

entire glacier is not covered, but hidden by the legend in all panels. Place names are missing.

Very small, impossible to read numbers/legend in a printed version.
Reply: Now, it has been changed as per figure 4.The figures were magnified. The legend was moved for both glaciers in 4a, 4b and4c.

There are sometimes obvious offsets on slopes also on ice free areas (same magnitude as changes) that are not discussed in the text (a, b). In particular in panel (b) the large elevation increase in the northern ice free area north of the glacier is striking – same magnitude as the elevation change on the glacier. Please explain in the Text
Reply: We further improved the co-registration and this could improve the results. Parts of the areas with surface elevation decreases belong to other glaciers which we did not investigate.

Fig.6: Too small to read numbers well. Scale to same axes. Use the same elevation bands/intervals in order to allow a comparison. Headings are strange.
Reply: It was updated. We have used the same elevation bands. The heading was changed using format "year-year".

Fig.7: Cannot be read. Needs magnification. Caption needs rewording (sentence should not start with “And...”).
Reply: Agreed. It was changed as per Fig.5. It was enlarged. The caption was also reworded. In order to read clearly, the elevation changes for the period 1974 -1999 and for the period 1999 - 2007 were shown. We removed the elevation change for the period 1974 -2007.

Figure 6/7: The different periods in the figure cannot be identifies - which were equals which time period?
Reply: The periods and sensors were mentioned in the captions
.

Referee 2

1. The south and north branches are separated by a major mountain ridge and only share boundary along a short alpine divide according to the Randolph Glacier Inventory. I expect that the two units have no real influence on each other except from their interactions with the glacier-fed lake. I therefore advice you to treat them as two separate ice bodies and rather focus on the contrast between them in terms of glacier morphology, hypsometry, AAR, dynamics and surface mass balance. Even if you are unable(?) to obtain multi-temporal DEMs for mass balance in the accumulation area, you should still be able to derive some more basic glaciological parameters such as hypsometry and typical AAR from ELA estimates or end-of-season snowlines.

Reply:

We think this is a very good and helpful suggestion. We treat now the two branches of Inylchek Glacier separately.

We used the hypsometry to gain the area-elevation distribution; shown in supplementary Figure 2. The images used were too snow covered to be useful to obtain a suitable AAR or ELAs.

2. The co-registration of DEMs is an important step that you have carefully described and shown in Table 3-4 and Fig. 2. Since you have multiple DEMs, you can also triangulate their co-registration (in Table 3) to check for remaining misalignments and potential impacts on the elevation changes. Nuth and Kääb [2011] provide several examples of that, so I suggest you follow their approach and include the results in the existing tables.

Reply: Thanks for your suggestion. We followed Nuth and Kääb (2011) and show the results in supplementary figure S3.

3. The major weakness of the paper is that the derived elevation changes are spatially incomplete and temporally inconsistent. Hence, there is not much that can be said about glacier mass balance or climate change. The authors try to compensate for this by making some crude assumptions about the unmeasured accumulation area. Instead of such "wild guesses", I rather want to see a more thorough analysis of the elevation change data itself and potential other sources of information for missing areas.

Reply: We generated an additional KH9 DEM which covers the accumulation regions and hence, we could calculate now the mass balance for the period ~1975 - 1999, 1999 - 2007 and ~1975 -2007 with higher confidence.

Firstly, to get any meaningful temporal information from Table 6, I would also calculate area-averaged elevation changes for the common areas in all time spans, even if it's only 10-15% of the total. Secondly, you need to investigate the sources of elevation change for the different periods.

Reply: We used area-averaged elevation changes in each zone (see section 3.6) and also calculated the mass changes for the period ~1975 - 1999, 1999 - 2007 and ~1975 -2007.

The northern branch is obviously influenced by a surge in the 1990s, but how about the 2000s? Is the northern tongue thinning more than the southern one due to climatic conditions or quiescent dynamics?

Reply: We agreed. It was pointed out that the stronger thinning at the tongue in comparison to SIG could be due to the quiescent phase after the surge.

For detail information, please see the last paragraph in section 4.3. "In the 1999 - 2007 period, however, NIG experienced higher mass loss between e 3,300 - 3,600 m a.s.l. (-2.0 ± 0.5 m a^{-1}) than SIG (-0.9 ± 0.5 m a^{-1}). Hence, the stronger thinning at the tongue in comparison to SIG could be due to the quiescent phase after the surge. "

.

Regarding the unmeasured areas at high altitude, there could be useful data from nearby glaciers (in situ or DEM differences), satellite altimetry [e.g. Gardner et al., 2013] or satellite imagery where snowlines can be tracked at the end of the ablation period [e.g. Shea et al., 2013].

Reply: We have compensated a DEM for the ~1975 - 1999 period. We found a slight surface elevation gain at the area where the ICESat points measured while Gardner et al. (2013) found a slight lowering. We discuss now our result in comparison to Gardner et al. (2013). However, in Gardner's paper there are two profiles(left and middle) of the three profiles(left, middle right) that passed through the Inylchek. The elevation change in the left profile that looks to be losing less mass than the middle profile. It meant that the mass loss in lower altitude of a glacier is less than in higher altitudes(red is more negative). Comparison with our result, it is adverse.

As mentioned above, it is difficult to find enough suitable Landsat or other suitable imagery to track snowlines. MODIS is rather uncertain for this purpose in our region according to our experience.

4. Surface velocities are extracted as yearly averages for 2002-03 and 2010-11 (Fig.3). Why do the results only cover the southern branch? The northern branch could have been even more interesting considering its surge activity. The two velocity fields for the southern branch look more different than expected. For example, there appears to be a fast-flowing unit in the southeastern basin in 2002-03 which is not visible in 2010-11.

Reply: Sorry we gave a wrong figure number. It is figure4. We agreed that it would also be interesting to investigate Northern Inylcheck Glacier. However, the data available to us only covers the Southern Inylchek Glacier. In addition, no image was found for the years near 1996/97 where the surge likely occurred. For the souther eastern basin, we described in manuscript as "And a significant surface elevation increase of a southern tributary (region 2 in Fig. 4a) in the period 1999 - 2007 provides evidence for a tributary surge. This finding is corroborated by clearly lower velocities in 2011/12 than before.
"

How can that be? And if correct – how does that influence the observed elevation changes in the SPOT-SRTM period? Moreover, you should try to difference the velocity maps in Fig. 3 to get an impression of acceleration/deceleration and potential errors. This will in turn help to

interpret the climatic/dynamic components of the elevation changes.

Reply: The velocity difference is shown in supplementary figure S3. The velocity was faster close to the lake dam where the elevation change was also larger. Hence, we agree it is useful to explain the reason of elevation change and included some information in the discussion. In our work, the velocity effecting the elevation change was clearly shown close to lake dam, so in discussion section, we discussed on “High velocities transports mass from upstream and offset the mass loss due to ice melt. Furthermore, the lake enhances melt and causes calving. The water likely also lubricates the glacier base bed. Hence, the lakes likely causes the high velocity until the lake margin and influences the ice dynamics (cf. Mayer et al. 2008) and the mass change of a glacier.”

5. Is the strong thickening of the southern branch in 1999-2007 realistic? We are here talking about a thickening of up to 20 m over a period of only 8 years (Fig. 6)in a semi-arid region where the annual precipitation is expected to be around 300mm/y, though probably somewhat higher in the alpine. These anomalous changes need to be discussed in more detail. Could there be effects from glacier dynamics (e.g. starting surge)?

Reply: We were also doubtful that it is true. Therefor we reinvestigated the elevation change in the accumulation region.
According to Aizen's result (1997, Journal of glaciology, 43(145)), the precipitation at 6148 m asl. was 800 mm/yr and the thickness of annual snow-firn layers was less than 275 mm/year from 1969 to 1989 (Aizen et al., 1997). In addition, the seasonal snow depth was calculated with a maximum of 9.0 m by comparison with the SRTM C-band and SRTM X-band. Based on these findings threshold of 20 m was now introduced used in the accumulation region. This led to more realistic values (moderate elevation changes above 4,000 from 1999 to 2007 (cf. Figure 6).
.

Why is the strong thickening not seen in the northern branch? You need to show the spatial field of this thickening in Fig. 5 (extends only to point a) or elsewhere. The consistency of the thickening in different tributary basins will give a good indication of whether it is caused by surface mass balance, dynamics or DEM errors. Note that Gardner et al. [2011] derived glacier thinning across the firn area of both these branches in 2003-2009 (see the middle ICESat profiles in Fig. S1c of their supplementary material).

Reply: This phenomenon occurred between 1999-2007(SRTM-SPOT).In Figure 6, comparing the altitude zones of 5,800 - 6,000 m asl., the NIG also showed surface elevation increase, but less than in southern Inylchek Glacier.
We also mentioned the difference between our results and Gardner's in the discussion section. I don't know what the reason is. However, there are two profiles(left and middle) of the three profiles(left, middle right) that passed through the Inylchek in Gardner's paper. The elevation change in the left profile that looks to be losing less mass than the middle

profile. it meant that the mass loss in lower altitude of a glacier is less than in higher altitudes(red is more negative). However, in our results, it is adverse in this region.

Finally, I have some minor comments and edits to specific parts of the manuscript. The language will eventually need a more careful editing and proofreading, so I have only provided a few obvious corrections here.

P2574, L3: Is it also largest if the south and north branches are treated separately?
Reply: Yes. SIG is the largest glacier in Tian Shan.

P2574, L8: delete multi-temporal (obvious)
Reply: Agreed. It was deleted.

P2574, L12: within 1974-2007
Reply: Agreed. It was changed

P2574, L13: shrank in all study periods since 1974
Reply: Agreed. It was changed

P2574, L17: average elevation difference of the lower part of: : : (since you didn't measure the whole glacier and should avoid confusion with mass balance)
Reply: We Agreed. Now，We have measured the whole glacier

P2574, L19: This can be misleading since a lot of elevation changes occurred. A mean value for the whole period over a random section of the tongue does not have much value. Describe the mass redistribution through the surge instead.
Reply:Agreed. The same question was asked by the first reviewer and was described.

P2574, L21: overall negative values are -> the dominant thinning is (since your values are actually positive!)
Reply: Agreed. It was changed.

P2575, L15: turn-over
Reply: Yes. Thanks.

P2575, L22: It has now passes the stage of being a "promising" technique, it's even used to calibrate time series of in situ mass balance [e.g. Zemp et al., 2013].
Reply: Agreed. We have added " it's even used to calibrate time series of in situ mass balance (e.g. Zemp et al., 2013)" at the end of sentence. Also, zemp's reference was also added.

P2575, L24-27: Is this true? I think that globally the most common studies have compared SRTM or satellite DEMs with historic maps from aerial photogrammetry. Are there any older

maps available for Inylcheck? Even if they are not of sufficient quality, it's worth to mention somewhere that you have looked into this.

Reply: Yes, we also used four aerial photogrammetries at a scale of 1:50k in 1981. We have digitized the topographic maps at a scale of 1:50k in 1981. However, we found a obvious mistake in contour lines. We cannot reliably interpret the data because of this mistake and therefore, we did not use the map derived data.

SRTM DEM is definitely important and useful in glacier elevation change. Hence, we changed it to " Several studies have shown that remote-sensing derived geodetic mass balance estimates are suitable to extend in-situ measurements in space and time (e.g. Berthier et al. 2010, Gardelle et al. 2013, Paul and Haeberli, 2008), and it's even used to calibrate time series of in-situ glaciological records (e.g. Zemp et al., 2013)."

P2576, L24: This is the third mention of "largest glacier". One is enough.

Reply: Agreed. " is the largest glacier of the Tian Shan and" was deleted.

P2577, L21: How about nearby glaciers? Are there any measurements of the altitudinal accumulation gradient from stake profiles? This is interesting in relation to the observed thickening at higher elevations in 1999-2007.

Reply: Yes, there are glacier No.1 and Tuyuksu glacier. Now, we used a threshold 20 to calculate the elevation change. The results are more comparable with the two glaciers. we discussed about the mass balance of three glaciers.

Karabatkak: 1974-1990 -766mm/year (Unger-Shayesteh et al., 2013; Cao,)

Tuyuksu -586mm/year (Unger-Shayesteh et al., 2013; Cao,)

Tian shan: -300mm/year(Wang, 2012)

P2577, L25: Where was this observed? Altitude?

Reply: At Tian Shan Station(altitude: 3,614 masl.). This information was included.

P2580,L6: Any suitable ASTER for the northern branch?

Reply: Sorry, we did not find suitable ASTER for NIG.

P2580, L16: Fig. 4b

Reply: Thanks for your careful read. It was corrected to fig.2b.

P2581, L5: I assume you first generated orthophotos using a DEM. Not mentioned anywhere as far as I can see.

Reply: Yes, you are correct. SPOT and KH9 images were orthorectified. So, In section 3.2, we mentioned that SPOT and KH-9 data are orthorectified after generating DEM.

P2581, L14: What is this "sound accuracy assessment" about?

Reply:

For accuracy assessment, a calculated RMSE value, which was determined by an analysis of significant displacements/velocities, is used as parameter for uncertainty. We now write in the text " As a final step a sound accuracy assessment was performed by the

ratio of RMSE and the resolution of the respective input data. Beside Inylchek glacier, some more neighboured glaciers were observed with the named input datasets. The calculation of the RMSE values considers all observations. Therefore the survey compasses a huge amount of significant and non-significant velocity dates, which allows a solid assessment of accuracy".

P2581, L24: as 10 m
Reply: Agreed. It was changed

P2582, L1: /% coverage
Reply:Agreed.

P2583, L9: A reference is appropriate here, e.g. Gardelle et al. [2012].
Reply: Agreed. Gardelle et al. (2012) is now referred to.

P2583, L10: Was this applied as a correction? Zonal or gradual transition?
Reply: We have now provided a supplementary figure for clarification (figure S2). It was performed according to altitude zones.

P2583, L14: Explain what NMAD is.
Reply: In the manuscript, (which was expressed by $1.4826 * \mathrm{MED}(|\tilde{x} - x_i|)$, x_i: elevation difference; $\tilde{x}$:Median) was added)

P2584, L18: Does this imply that the lower tongue is a relict feature, e.g. from previous glacier surges?
Reply: The velocity show two parts: one is higher towards lake, another is relative lower towards the distal tongue. In manuscript, we described it as "We noticed high velocities with an average flow of about 120 m/a for the SIG towards Lake Merzbacher while the remaining part of the debris-covered tongue below the lake has significantly lower velocities with decreasing rates and is even partly stagnant (Fig. 3)". So we think it may be not directly related to glacier surges.

P2585, L13-26: As mentioned in the general points: Treat the two branches separately and only infer temporal variations if the sampling areas have been homogenized.
Reply: Thanks for your useful suggestion. We treated them now as two individual glaciers. For detailed information, please see the manuscript. In the sampling areas, it is not homogenized(see supplementary figure s2). In higher accumulation region and lower ablation region, the weight of area is small.

P2585, L9: Confusing numbers. Keep it simple, e.g. 0.4-0.6 m a-1. More in general, you sometimes talk about elevation change and sometimes lowering/thickening, which makes it easy to confuse positive and negative signs. Be consistent throughout.
Reply: Agreed. We kept elevation lowering/thickening being consistency with positive

and negative.

P2588, L18: Are you talking about the northern branch here? P2589, L18: This is essentially the definition of a surge, so that is obvious.
Reply: This sentence was changed to " a significant surface elevation increase of a southern tributary (region 2 in Fig. 4a) in the period 1999 - 2007 provides evidence for a tributary surge. This finding is corroborated by clearly lower velocities in 2011/12 than before."

.

P2591, L2: Considering the inconsistent coverage, the tendency is not "clear". This is also evident from the three numbers you state – they do not sum up to each other.
Reply: Agreed. Both reviewers have mentioned this question. After we updated the DEMs, the mass balances were calculated. The conclusion was improved. Please see the manuscript.

P2591, L7-11: These general statements are not really a part of your results. The conclusion should focus on your own findings.
Reply: We focus the conclusions on our own findings which were mass balance of both glaciers from ~1975 to 1999, from 1999 to 2007 and from ~1975 to 2007; velocity near lake dam effected the mass loss.

Table 5: It would be sufficient to only state the area for 1974 (or 2007) since the other years are implicit from the area changes in each period. Totals are not needed.
Reply: Agreed. "Total" record has been deleted.

Table 6: Also state the relevant years for each row and possibly the area-averaged elevation change for homogenized areas so that the numbers become comparable. Total numbers for IG are not needed.
Reply: Agreed. Certainly we used area-averaged elevation change to get the results, however, there are still couple of altitude zones lacking. The total record has been deleted..

Fig. 1: The glacier outlines are somewhat difficult to see due to the thin lines and similar color as the thicker line with country boundary. A color bar for altitude is missing.
Reply: Agreed. It was updated.

Fig. 2: Ok, but not really needed.
Reply: Yes. It was moved to supplementary file (See supplementary figure s3).

Fig. 3. Use a and b instead of above and below. A difference image would also be interesting to see potential acceleration/deceleration.

Reply: Agreed. Now, Figure 3 was marked" a" and "b". A difference image is now provided in the supplementary material (Figure S5)

Fig. 5: Nice, but would also like to see the full extent of the DEM differences between SPOT and SRTM. Rates of elevation change, instead of total change, would make the panels more comparable and in line with Table 6.

Reply: The figure is now figure 4.The legends were removed from both glaciers in order to show the full extend The calculated mass balance is shown in Table 5.Figure 4, Figure 5 arenow shown by using rates of elevation change.

Fig. 6: Mention the interval of the elevation bins and the connection between sensors and periods in the caption, e.g. 1974-1999 (SRTM-KH9).

Reply: Agreed. In the caption we used the format "year-year" such as 1974-1999 (not SRTM-KH9).

Fig. 7: The ALOS section extends to point c, not a – right? Please refer to Fig. 5 for locations of the longitudinal profiles.

Reply: Yes, you are right. In fig5 there are also marked in the format "year-year" in left side.

reference list

Aizen, V. B.,Aizen,E., Dozier, J., and Melack, J. M.: Glacier regime of the highest Tien Shan mountain, Pobeda-Khan Tengry Massif, Journal of Glaciology, 43, 503-512, 1997.

Kaab, A., Berthier, E., Nuth, C., Gardelle, J., and Arnaud, Y.: Contrasting patterns of early twenty-first-century glacier mass change in the Himalayas, Nature, 488, 495-498, 2012.

LiJia, L. Z.-W., WANG Chang-Cheng, ZHU Jian-Jun, and DING Xiao-Li: Using SAR offset-tracking approach to estimate surface motion of the South Inylchek Glacier in Tianshan, Chinese Journal Geophysics, 56, 1226-1236, 2013.

Nobakht, M., Motagh, M., Wetzel, H.-U., Roessner, S., and Kaufmann, H.: The Inylchek Glacier in Kyrgyzstan, Central Asia: Insight on Surface Kinematics from Optical Remote Sensing Imagery, Remote Sensing, 6, 841-856, 2014.

Surazakov, A. B., and Aizen, V. B.: Estimating volume change of mountain glaciers using SRTM and Map-Based Topographic data, IEEE Transactions on Geoscience and Remote sensing, 44, 2991-2994, 2006.

Surazakov, A., and Aizen, V.: Positional Accuracy Evaluation of declassified Hexagon KH-9 Mapping Camera imagery, Photogrammetric Enginneering& Remote Sensing, 76, 1-6, 2010.

Gardelle, J., E. Berthier, and Y. Arnaud (2012), Impact of resolution and radar penetration on glacier elevation changes computed from DEM differencing, J. Glaciol., 58(208), 419-422, doi:10.3189/2012JoG11J175.

Gardner, A. S., et al. (2013), A Reconciled Estimate of Glacier Contributions to Sea Level Rise: 2003 to 2009, Science, 340(6134), 852-857, doi:10.1126/science.1234532.

Nuth, C., and A. Kääb (2011), Co-registration and bias corrections of satellite elevation data sets for quantifying glacier thickness change, Cryosphere, 5, 271-290,

doi:10.5194/tcd-4-2013-2010.
Zemp, M., et al. (2013), Reanalysing glacier mass balance measurement series, Cryosphere, 7, 1227-1245, doi:10.5194/tc-7-1227-2013.
Pieczonka, T., Bolch, T., and Buchroithner, M.: Generation and evaluation of multitemporal digital terrain models of the Mt. Everest area from different optical sensors, Isprs Journal of Photogrammetry and Remote Sensing, 66, 927-940, http://dx.doi.org/10.1016/j.isprsjprs.2011.07.003, 2011.