

Interactive comment on “Elevation changes of Inylchek Glacier during 1974–2007, Central Tian Shan, Kyrgyzstan derived from remote sensing data” by D. Shangguan et al.

D. Shangguan et al.

dhguan@lzb.ac.cn

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Reply: We thank to to two referees very much for their critical but encouraging suggestions. We found the referees' comments most helpful and have revised the manuscript. We have taken referees' comments and suggestions seriously in account, answer or reply the comments and suggestions one by one, including the comments and suggestions in the attachment. Following the suggestion of the referees, all figures have been redrawn, some sentences have been rephrased, English have been updated one time. Please see the more revised details in the revised manuscript. Due to the time, we need much more time to analyse the new HK9 DEM derived from 1976. We hope get

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new results to compensate the incomplete elevation change.

referee 1

P2574L9: ... showed strong velocities ==> change to showed high velocities Answer & reply: The "strong velocities" will be changed as "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 Answer & reply: The sentence will be improved for clarification

P2574L11: Better rephrase: The northern branch of Inylcheck Glacier advanced by ... while the southern branch continuously retreated since 1974 (* km2). Answer & reply: Agreed. It will be changed as " The Northern Inylchek Glacier increased 2.0 ± 0.1 km2 ($\sim 1.3\%$) within the period of 1974 - 2007, the main reason was glacier surging between 1990 - 1999. The Southern Inylchek Glacier shrank in all study periods since 1974."

P2574L16: ALOS PRISM Answer & reply: It will be changed to " ALOS PRISM"

P2574L21: "A possible thickening ... and a clear thickening" - reword to e.g. indication for a thickening have been revealed ... Answer & reply: It will be rephased as " A thickening of the southern branch have been revealed as 0.5 ± 0.5 m a-1 occurring between 1999 and 2007 where a obvious thickening was measured in the accumulation area".

P2574L25: "... possibly due ..." - in the following text this is not stated as possible, but attributed as surge Answer & reply: Yes. It was attributed as surge, I will be deleted the "possibly"

P2574L28: As it is, the sentence describes a relation between glacier velocity and debris coverage. The sentence is not clear. Answer & reply: It will be updated as " The ablation region of the Southern Inylchek Glacier showed considerable lowering rates, especially in the distal part of the tongue covered by thick debris."

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8, C1593–C1622, 2014

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P2575L8: "... glaciers shrank also ..." – also to what? Answer & reply: You are right. It is my fault. It is "not only... but also. So it will be 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. Answer & reply: Agreed, a reference (Aizen et al, 1997) will be added.

P2575L17: rephrase this sentence, very complicated formulation and difficult to understand Answer & reply: Agreed. In this sentence, we want to express surface mass balance is directly linked to climate. So we delete "and allow,... to 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. Answer & reply: Sorry, DEM difference will measure mass balance. Thanks for your comment.

P2575L23: Refer also to KæžÛb et al., 2012: Contrasting patterns of early twentyfirst-century glacier mass change in the Himalayas, Nature 488, 495-498. Answer & reply: Thanks. it will be referred.

P2575L15: ALOS PRISM, SPOT-5 HRG Answer & reply: Yes, Thanks

P2576L1-5: Refer to Fig. 1. Locate mountains/glaciers referred to in the text in Fig. 1. Answer & reply: Agreed. It will be changed to Peak Pobeda/Tomür Feng. The same will be changed in L22

P2576L1-14: Please clarify the relation between glaciers in the Ak-Shyrak Massif etc and the IG. Answer & reply: The Ak-Shyrak Massif is the second largest glacierized massif in Tian Shan. So, I added the information "the second largest glacierized massif in Tian Shan"

P2576L4: mass descriptions should be expressed as m a-1 w.e. Answer & reply: Thanks. The reference of Pieczonka will be changed to " a mass loss of 0.42 ± 0.23

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m a -1 w.e. "

P2576L26: ELA, please introduce any abbreviation first Answer & reply: Thanks. equilibrium line altitude(ELA)

P2577L5: The sentence is not clear. Do you mean 1500 to 2000 m 3/s? Answer & reply: Yes, it means " 1,500 m³/s to 2,000 m³/s ". It will be also change to " 1,500 m³/s to 2,000 m³/s " in manuscript

P2577L7: Please provide a quantitative reference for the velocity measurements (e.g. Li et al; Hagg et al.) Answer & reply: Agreed. Thanks.two references (Li et al's and Nobakht's)will be provided

P2577L8: It should be Merzbacher Lake not Merzbacker Lake Answer & reply: Yes. It is mistake in writing.

P2577L10: Please mention Aksu River in Fig 1. Answer & reply:Agreed. It will be added in Fig1

P2577L22: estimated or assumed? Answer & 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

Answer & reply: agreed. It was observed in Tianshan Station. So, "The mean annual temperature is about" will be changed to " The mean annual temperature in Tianshan Station 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 number on accuracy and errors should be backed by references. Answer & reply: Agreed. The methods section will be abbreviated.(please look at the tracking version). The penetration depth

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along with the altitude will be evaluated in Supplementary Figure S2.

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.

Answer & reply: agreed. However, Gorokhovich's paper evaluated the mountain ranges, it is not in Tian Shan. We also referred Surazakav et al (2006)' work which have also evaluated the difference 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 too 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 facies.

Answer & reply: Agreed. Here it was referred some result. Our results were shown in P2583L7. In addition, we will also provide the Supplementary Figure S2.

P2579L14: B/H. Please also explain for non-photogrammetrists Answer & reply: Agreed. It was changed as "Base to Height ratio(B/H)". It is a term.

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.

Answer & reply: You are right. "lines of demarcation" was changed as "lines"

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.

Answer & reply: We do not fully agree. One can identify the boundary also visually on a subpixel level. In addition, half a pixel is commonly used, eg(Bolch et al., 2010)

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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.

Answer & reply: agreed, "Finally" was deleted. There is misunderstanding about the uncertainty of area change due to the disorder sentences. The methods used to evaluate the uncertainty of area change is also buffer methods. and we just focus on the parts of absolute change.

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 overall 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.

Answer & reply: How was this accuracy assessment done? Reply: For accuracy assessment, a calculated RMSE value, which was determined by an analysis of significant displacements/ velocities, is used as parameter for erroneousess. Conciseness is derived by the ratio of RMSE and the resolution of the respective input data. Beside Inylchek glacier, some more neighbored glaciers were observed with the named input datasets. The calculation of the RMSE values considers all observations. Therefore the survey compass a huge amount of significant and non-significant velocity dates, which allow a solid assessment of accuracy.

What are the results? Reply: The results were a subpixel accuracy of the velocities of the named values.

How do the values translate into m/a-1 used as in the graphs. Reply: The given velocities show the average values of one year. Therefore one requirement of the input data was the time interval of acquisition of nearly one year. Here we have differences of 368 days in observation 2002/2003 and 352 days in 2010/2011. The measured displace-

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ment describes the pixel shifts between two images in NS- and EW-direction. With euclidean distance we get the horizontal displacement. By the ratio of these values and the time difference of the used input data you get the annual average velocity.

Removal of unsound values is not an accuracy assessment! Reply:"Unsound values" weren't removed! The RMSE values were used

One might suppose that the errors depend on the contrast and features in the image. Reply:Of course the errors depend on the used image quality. We wrote nothing against it! The written values concern 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. Reply:The tracking depends on software Cosi-Corr - therefore the method of phase correlation was used. For ASTER data measurements a previous done subpixel-coregistration were done as described in Leprince et al. (2007). The elevation source was a resampled version of the CGIAR SRTM3-DEM v4.1. Landsat data were assumed as quasi-coregistered, because of the same used registration method, GCPs and elevation source between the input imagery (considering the given meta data). Window sizes were determined referring to assumed annual average velocities, which were derived by previous works (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 predictable or the used input data is a bit noisy, it is recommended to improve the window size (Ayoub et al., 2009). Mayer et al. (2008) detect in 2003/2004 an annual average velocity of Inylchek up to 90 m/a. According to e.g. ASTER resolution (3N, 15 m/Px) , the theoretical search distance/ window size could be 12 Px. With due regard to the mentioned problems, the final window size was 32x32Px. The initial one amounts to 128x128Px. The step size is 4 for ASTER- and 2 for Landsat the same window size was used but a step size of 2, so both displacement maps have an resolution of 60 m. The SNR was calculated by COSI-Corr. A well-established value of 0.90 was used as threshold to remove obvious errors.

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

Answer & reply: The DTM has been generated with LPS 2013 using the frame camera model. The coordinates of the fiducials have been measured starting from the image center assuming that the image focal point coincides with the image center. For the DTM we measured ~40 GCPs with a final RMSE of ~1. GCPs coordinates and elevations were derived from a Landsat 7 ETM+ scene and the SRTM3 DTM. The post-processing of the DTM comprises the elimination of spatial trends and the co-registration (cf. Pieczonka et al. 2013). The overall quality of the DTM is promising; however, worse matching results are partly occurring in the accumulation regions and areas with cast shadows where less image contrast is prevailing and thus, influencing the quality on a local level. We think that, after filtering out unsound elevation values, one value is acceptable in order to describe the quality of the DTM.

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

Answer & reply: Agreed. We found and processed an additional Hexagon Scenes which cover the parts of accumulation region.

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

Answer & reply: This sentence is only used to explain why we should do the co-registration. So, It was revised as "Matching relatively well (including horizontal shifts or vertical offsets) to two different DEMs is important for the calculation of the glacier volume changes"

P2583L6: The difference between x-band and c-band is NOT the mean penetration!

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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 estimated? Answer & reply: There is not optical image to be referred. I am sorry not to answer the surface conditions in that time. In ice & snow free flat area, the difference is point which may cause by random uncertainty. This information may show that there is no snow covered in snow free flat area. In addition, we also survey the difference in debris covered region, the sign is also random which may cause by the crude surface. So, we think there is no penetration in debris covered region. The penetration was calculated according to each altitude zone. X-band SRTM also has a better resolution, was this considered? You are right. First of all, SRTM x band covered limited region in glacier research while SRTM C band covered almost all of central Asia region. It is why SRTM c band was used widely though it is a little low resolution. 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 bias of each altitude zone and evaluated 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 + xyz of the GPS points. It is a bit unclear for which DEM after coregistration the values refer.

Answer & reply: Agreed. The spot-GPS difference was kept in Tab.4. Others were deleted.

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

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where those are obvious)

Answer & reply: The DEM difference were evaluated by two aspects. One was evaluated by Tab.3, it was covered in glacier free region. Another was evaluated by GPS survey. We will put more emphasis on the uncertainty estimation and will improve the text accordingly.

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.

Answer & reply: First of all, I would explain why we used the two figures. Due to the velocity of Southern IG was measured by Li jia(2013), by Nobakht(2014) and others. However, all those works focused on the main stream up the Lake Merzbacher. There is no information about the tongue of southern IG. In this case, we have to do this work. Second, our work try to understand the relationship between the velocity and glacier area change, the velocity and glacier elevation change. However, It is only qualitative analysis. We try to get regular pattern.

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?

İAñ It would be clearer to present all values of elevation changes for each period in Tab. 6. Reply:Agreed. The Tab.6 will be updated. İAñ Please provide surface lowering/thinning data as negative values throughout the whole manuscript. Reply:all those lowering/thinning data were changes as negative values. In this section, all period were considered. but from 1999-2007, the elevation changes is positive. İAñ Why did you mostly consider the DEM differences from SPOT-KH9, which obviously covers the

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smallest area. Reply: Before, we considered Inylchek glacier as single glacier. The southern IG and Northern IG are two branches of Inylchek glacier. However, Now we have separated the southern IG and Northern IG in this section. And we will use new KH9 DEM to calculate the DEM difference. ĀŃ 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: Not, yet. From the Figure 5(KH9-SPOT5), we can see that the lake region is most in accumulation region. So we supposed how many the mean elevation change in those region was. And then calculated the whole elevation change. Now, We will use the KH9-DEM. And it will be educated guess.

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. Answer & reply: This sentence is not useful in manuscript because the Southern IG and Northern IG were analysed individually. The IG was not analysed. So, we will delete it.

P2585L17: Figure 5 is not referred to in the text. The Figures should be presented in a chronological order. Answer & reply: Yes, it is. We refer it in section 4.3. In addition Figure 5 was updated. And the chronological order was used.

P2585L21: Do you mean Table 6? The period 1974 to 1999 is not listed in Table 6! Could you please explain why you use the difference derived from SRTM-KH9 here and not SPOT-KH9 as above? It is hardly reproducible. Answer & reply: Yes. It is table 6, not table 5. It is 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? Answer & reply: Agreed. This sentence is not suitable here. So I moved it to L2586L27. Unfortunately, we cannot

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provide surface velocities due to lack of images in 1996/1997.

P2586L8: Do you mean Fig. 3? Answer & reply: Sorry. Here it is fig.4 . I gave a wrong order on figure 3 and figure 4.

P2586L9-11: The altitudes mentioned in the text are not clearly identifiable in Fig 6. 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). Answer & reply: I am so sorry. It is true. However, to keep the Fig 6 tidy, we only mark several characteristic points in Fig 6 which will help us identify where it was, Such as point a, b and c. The coverage of DEM is clear in figure 5. And we will compensate a DEM in 1976.

P2587L18: Which information? Answer & 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. Answer & reply: Nobakht was used the Landsat images acquired from Feb., 2002 to Mar. 2003 and from July, 2010 to September, 2011. Our work was used Aster images from Aug., 2002 to Aug. 2003 and the Landsat images from August, 2010 to August, 2011. The preprocessing technique is same (COSI-Corr method). The sentence was re-organized. .

P2587L23: A velocity rate would be velocity/time hence a de-/acceleration, but the units indicate velocities. Answer & reply: We provide velocity in formation per time (year). However, as the term rate is misleading here we just write velocity.

P2587L23: Unclear what is meant. Please verify logic of the sentence Answer & reply: Agreed. It was re-organized.

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. Answer & reply: It was

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reported that ice fall is one of the components of lake volume supply (Ng et al., 2007) which was due to and 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 Lake dam was pointed out.

P2588L20: an altitude higher than... Answer & 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. Answer & reply: Agreed. it is geodetic mass balance

P2589L18: "This result ...", the statement by Paterson & Cuffey (2010)? clarify Answer & reply: Yes, So it was changed as "His result"

P2590L2-6: reduced glacier flow, you did not mention any indication for a reduction of flow speeds nor did you measure this. The link to englacial conduits remains unclear. Answer & reply: It was mentioned in the last sentence in the section of 4.1 glacier flow. As you know, the glacier flow was surveyed by "Nobakht", li jia. However, those results are not involved with the velocity of glacier tongue. In this case, we compensated for this work. And we surveyed two years's velocity in glacier tongue. However, we do not have results of the englacial conduits. It is only referred. So, I think it is better to delete it.

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. Answer & reply: Agreed. we will be updated. detail information, please see the section "conclusion"

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 coverages. Locations and place

names are missing! Needs complete rework. Scale bar is cut to the edges. Answer & reply: According to two referees' suggestion, we have improved the Figure 1. Aksu rive and Aksu catchment were mentioned in Figure1. Different DEMs coverages were distinguish with color. Scale bar was cut to the edges.

Fig.2: Why only SRTM and SPOT? You also address ALOS PRISM, similar figures would be helpful at least in a supplementary file Answer & 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. Answer & reply: The graphic resolutions were broke down by the publisher. I think the page format is uncommon for publications... The figure will be updated. the graphics themselves could be bigger and become readable. As mentioned above, couple works did not focused on the velocity of glacier tongue of Southern IG. Our work will compensate the deficiency.

Fig.4: Again of bad quality in print. Lines cannot be identified well, place names (Merzbacher lake) missing. Scale bar needs improvement. Answer & reply: We have updated the glacier extent. And 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

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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. Answer & reply: Very small, impossible to read numbers/legend in a printed version. Reply: The figures will be magnification. The legend will be moved out figure 5a, 5b and 5c.

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: No agree. It is also covered by ice, but it is not in our research region. 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. Answer & reply: It will be updated. We have used the same elevation bands. The heading will be changed according to figure.

Fig. 7: Cannot be read. Needs magnification. Caption needs rewording (sentence should not start with “And...”). Answer & reply: Agree. It was magnified. The caption was also reworded.

Figure 6/7: The different periods in the figure can not be identified – which were equal which time period? Answer & reply: The caption was mentioned periods and sensors

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 advise 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

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hypsometry and typical AAR from ELA estimates or end-of-season snowlines. Answer & reply: I think this is a very good and helpful suggestion. I would definitely calculate the hypsometry. AAR might be a bit difficult but we could see if an ELA (and the AAR) can be estimated from the Late summer snow line. Dynamics depends if we can get information about the flow of Northern Inylchek. 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. Answer & reply: Thanks for your suggestion. We used a robust program which coded by Tino who is one of co-author. All those steps are referred Nuth and Kaab.

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. Answer & reply: I fully agree

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. Answer & reply: It is not "wild guesses", it was supposed according to the elevation changes in elevation zones. We have discussed the whole elevation changes in "Discussion section". Please note, due to the high uncertainty in unmeasured region, we give up calculating the mass balance.

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. Answer & reply: We will compensate a DEM. The northern branch is obviously influenced by a surge in the 1990s, but how about the

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2000s? Is the northern tongue thinning more than the southern one due to climatic conditions or quiescent dynamics? Answer & reply: Agree, I should be pointed out what happen after surging in text. Thanks for your mention. For detail information, please see the last paragraph in section 4.3 . 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]. Answer & reply: We will compensate a DEM. We have also browse the GLAS, however, we didnot find useful data close to 2008. Mentioned above, It is difficult to gain snowlines.

4. Surface velocities are extracted as yearly averages for 2002-03 and 2010-11 (Fig. 3, sorry I gave a wrong figure number. It is figure4). 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 the southeastern basin in 2002-03 which is not visible in 2010-11. Answer & reply: First, we considered that IG is one glacier, southern IG and northern IG are two branches of IG. So it is important to gain the velocity of tongue. . It is why we survey the velocity of southern branch. But now, we considered there is no IG. southern IG and northern IG are two glaciers We also want to find the images to survey velocity of the northern IG close to glacier surging event. But no image was found near 1996/1997.

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.

Answer & reply: Fhe velocity difference was in figure s3 of supplementary file. We fully agree it help to explain the reason of elevation change. The data in tongue region is useful to explain the relationship between elevation change and dynamic. We also

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hope to explain the relationship between elevation change and dynamic in southeastern basin.

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 300 mm/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)?

Answer & reply: Yes. it is true. We are also doubt it is true. However, other results also showed it is possible in accumulation (eg .Gardelle et al., 2013, The Cryosphere). Not yet, here according to Aizen's result (1997, Journal of glaciology, 43(145)), the precipitation at 6148 m asl. was 800 mm/y. We hope after we use new DEM, we can get more results.

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).

Answer & reply: This phenomenon occurred between 1999-2007(SRTM-SPOT). From figure 6, we know the maximum altitude is 4,600 m asl. in the northern IG. and 5400 m asl in the southern IG. Compared the altitude less than 4600 m asl, it looks like synchronous. We have updated the figure 5a, parts region covered by legend was disclosed. Point a is the stone between accumulation region and ablation region. From the color distribution, it looks like dynamics effect.

Finally, I have some minor comments and edits to specific parts of the manuscript. The

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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?
Answer & reply: Yes. southern IG is the largest glacier in Tian Shan

P2574, L8: delete multi-temporal (obvious) Answer & reply: Agreed. It was changed

P2574, L12: within 1974-2007 Answer & reply: Agreed. It was changed

P2574, L13: shrank in all study periods since 1974 Answer & 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) Answer & reply: Agreed. It was changed.

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. Answer & reply: Agreed. The same question with first reviewer. It was described. More detail please see the abstract.

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

P2575, L15: turn-over Answer & 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]. Answer & reply: Agreed. I 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, a zemp's reference was also added.

P2575, L24-27: Is this true? I think that globally the most common studies have com-

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pared SRTM or satellite DEMs with historic maps from aerial photogrammetry. Are there any older maps available for Inulcheck? Even if they are not of sufficient quality, it's worth to mention somewhere that you have looked into this. Answer & reply: recent digital model is not clear. SRTM DEM is definitely important and useful in glacier elevation change. So I changed it as "compare satellite DEMs, historic maps from aerial photogrammetry with the SRTM DEM from February 2000". We have digitized the topographic maps at a scale of 1:50k in 1981. However, there is an apparent mistake in contours compared with the three DEMs. I cannot tell the uncertainty for the topographic DEM. In addition, "while declassified and ...are only rarely used" was changed as "including declassified and low cost stereo data (such as Hexagon 9)..."

P2576, L24: This is the third mention of "largest glacier". One is enough. Answer & 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. Answer & reply: There is Tian Shan station . and also a correlation between precipitation and annual accumulation in 6,148 m asl. was referred.

P2577, L25: Where was this observed? Altitude? Answer & reply: It is in Tian Shan Station.

P2580, L6: Any suitable ASTER for the northern branch? Answer & reply: It would be checked.

P2580, L16: Fig. 4b Answer & reply: Thanks for your mention. Here it is fig.3b. However, there is a mistake on the order of the fig3 and fig4.

P2581, L5: I assume you first generated orthophotos using a DEM. Not mentioned anywhere as far as I can see. Answer & reply: Thanks for your mention. the SPOT and KH9 are orthorectified. So, In section 3.2, we mentioned the SPOT and KH9

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orthorectified.

P2581, L14: What is this “sound accuracy assessment” about? Answer & reply: For accuracy assessment, a calculated RMSE value, which was determined by an analysis of significant displacements/ velocities, is used as parameter for erroneousess. Conciseness is derived 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 compass a huge amount of significant and non-significant velocity dates, which allow a solid assessment of of accuracy.

P2581, L24: as 10 m Answer & reply: Agreed. It was changed

P2582, L1: /% coverage Answer & reply:Agreed.

P2583, L9: A reference is appropriate here, e.g. Gardelle et al. [2012]. Answer & reply:Agreed. Gardelle'paper was referred and also added in reference

P2583, L10: Was this applied as a correction? Zonal or gradual transition? Answer & reply:We have given a supplementary file. Please see the supplementary figure 2. It was performed according to altitude zones.

P2583, L14: Explain what NMAD is. Answer & reply: Normalized median absolute deviation= $1.4826 \text{ MED}(|x_i - \text{medj}(x)|)$

P2584, L18: Does this imply that the lower tongue is a relict feature, e.g. from previous glacier surges? Answer & reply:There is ice to supply the lower tongue through flow. However, the main stream direction is toward Lake Merzbacher. So, I think it is a relict feature from previous 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. Answer & reply:Thanks for your useful suggestion. Treat them as two individual glaciers. Detail information, please see the manuscript.

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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. Answer & reply:Agreed. keep all the negitve signs if it is thinning.

P2588, L18: Are you talking about the northern branch here? P2589, L18: This is essentially the definition of a surge, so that is obvious. Answer & reply: It is not clear. we are talking about Pieczonka's results compared with our results. So, we combined two sentences.

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. Answer & reply:Agreed. Two reviewers have mentioned this question.The conclusion was re-organized. 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. Answer & reply:Mentioned above, The conclusio was re-organized.

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. Answer & reply: Agreed. It 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. Answer & reply:Agreed. The two glaciers were separated to analyse.

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. Answer & reply:Agreed. It was updated.

Fig. 2: Ok, but not really needed. Answer & reply:Yes. It may show in supplementary

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file.

Fig. 3. Use a and b instead of above and below. A difference image would also be interesting to see potential acceleration/deceleration. Answer & reply:Agreed. A difference image was provided in Supplementary Figure S3

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. Answer & reply:The legends were moved out from Figure 5. Actually, the rates of elevation change was shown in Figure 6. If Rates of elevation change was shown in Table 6, it may repeat.

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). Answer & reply:Agreed.

Fig. 7: The ALOS section extends to point c, not a – right? Please refer to Fig. 5 for locations of the longitudinal profiles. Answer & reply: Yes, you are right. In fig5 there are also marked in SPOT- KH9 and SPOT-SRTM in left side.

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/8/C1593/2014/tcd-8-C1593-2014-supplement.pdf>

Interactive comment on The Cryosphere Discuss., 8, 2573, 2014.

TCD

8, C1593–C1622, 2014

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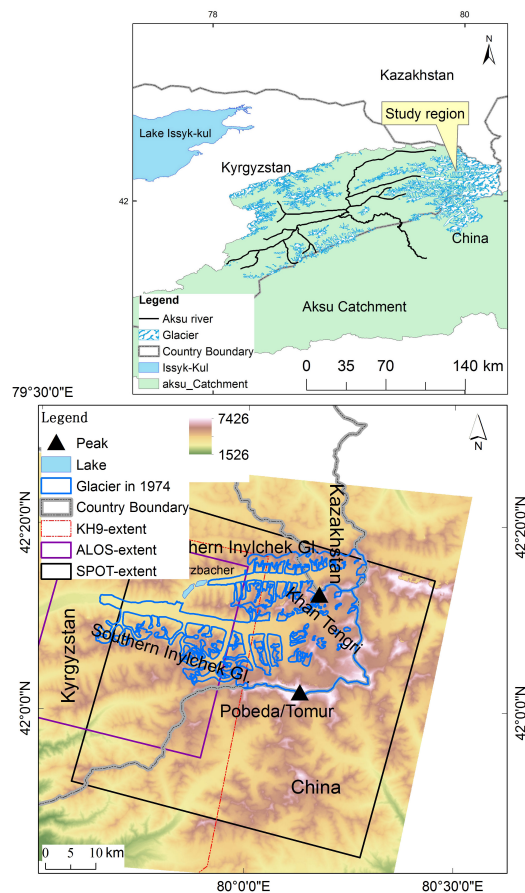
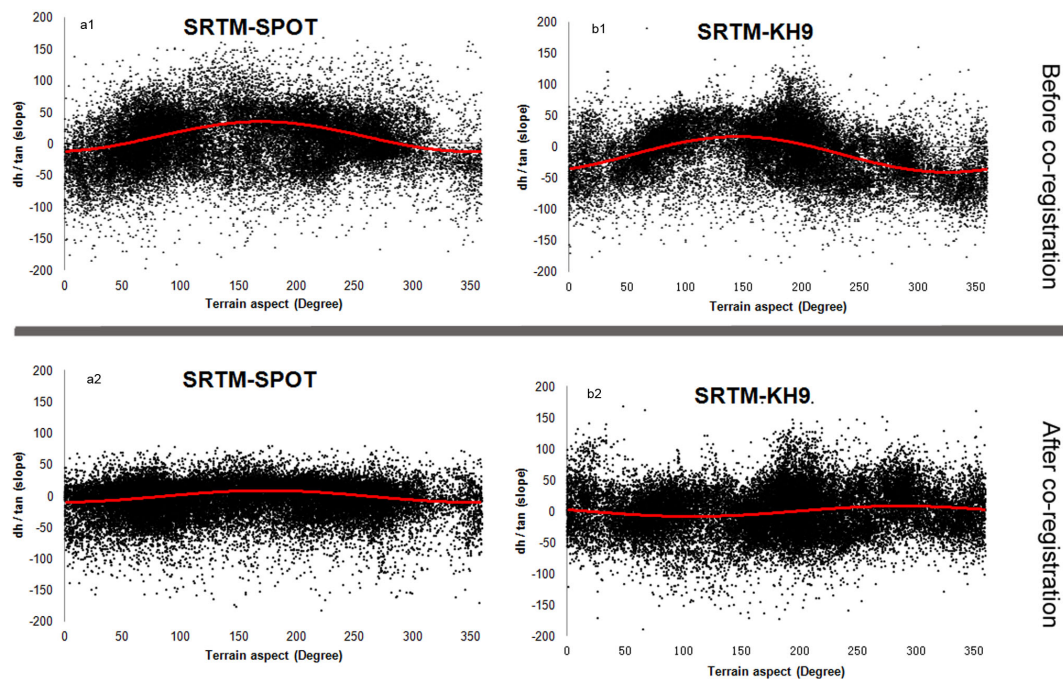
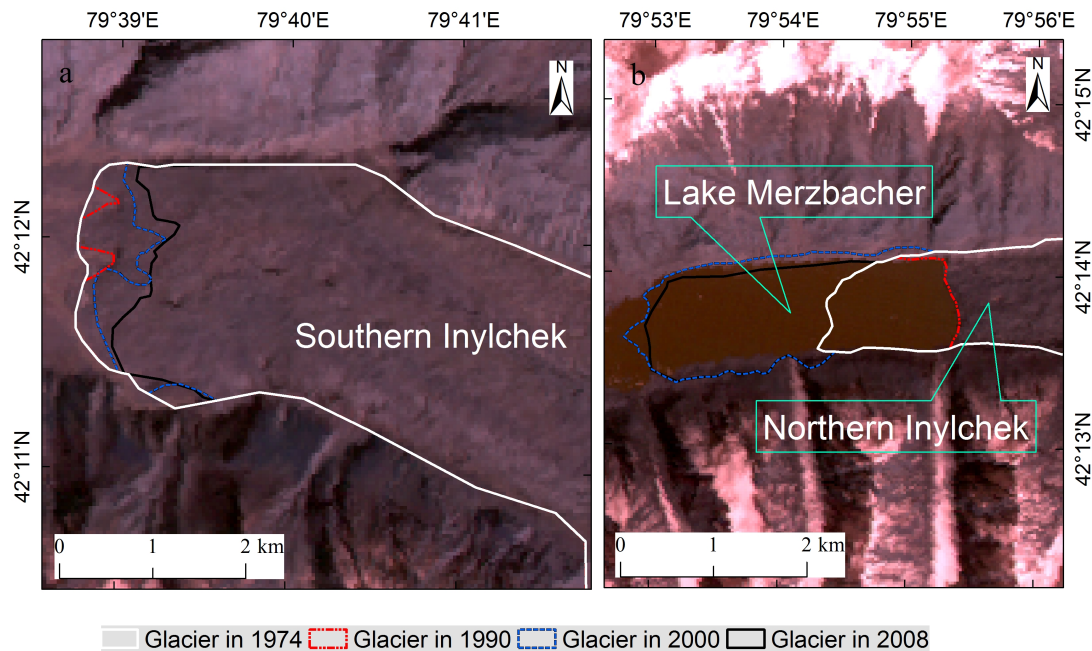


Fig. 1.

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**Fig. 3.**[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

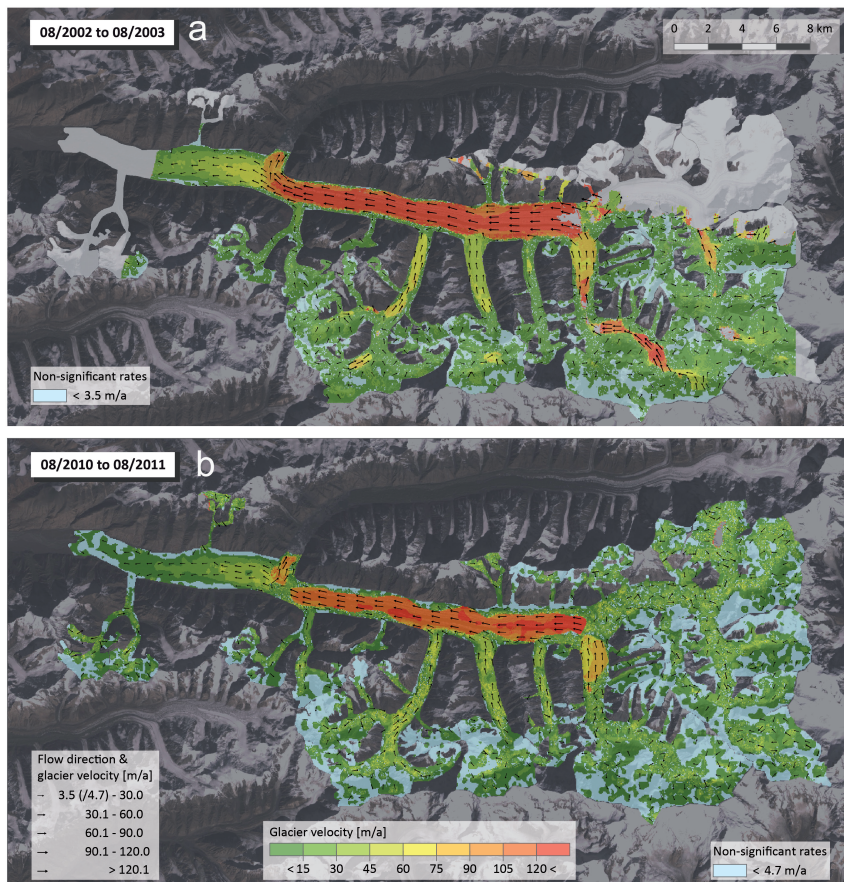


Fig. 4.

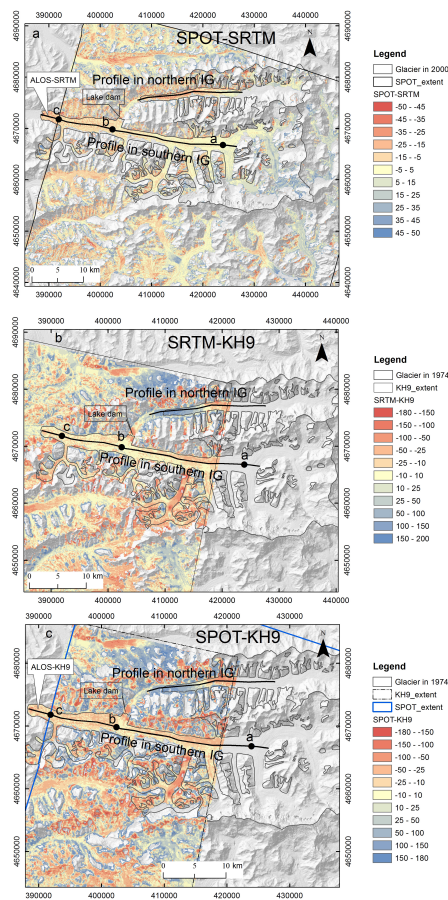


Fig. 5.

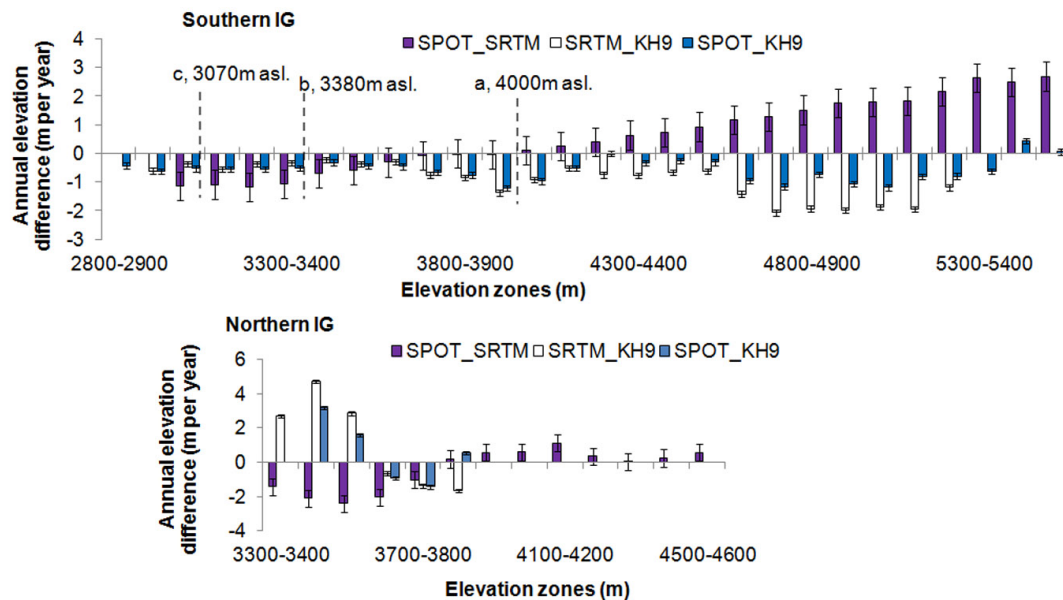
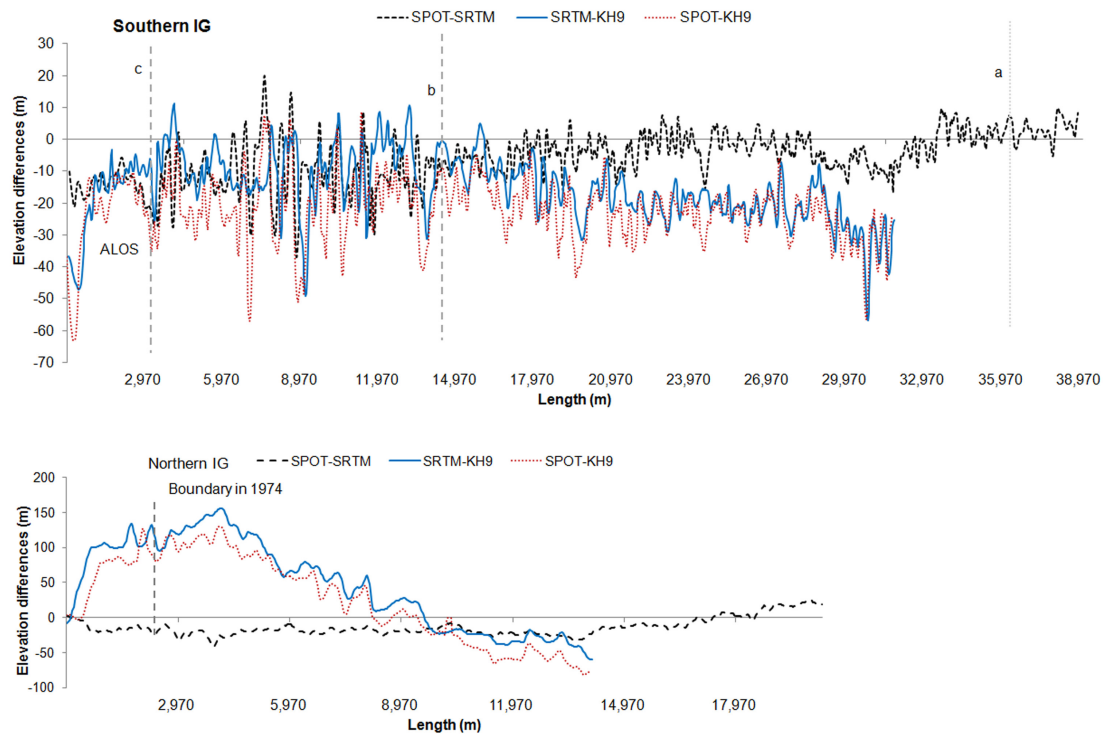


Fig. 6.

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