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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.

Anonymous Referee #2

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This paper investigates glacier elevation changes over two periods (1974-2000 and 2000-2008) for the lower part of the Inulcheck Glacier in Tien Shan. With incomplete spatial coverage, they are not able to derive reliable mass balance estimates, but they detect a surge in the northern branch and show considerable along-flow variability in elevation change for the southern branch. Their findings are backed up by area-change estimates, and they also derive one-year surface velocities for 2002-03 and 2010-11 although not used for anything in particular. All in all, they provide interesting historic data on a major glacier system that has so far not been studied much, but they are unfortunately not able to reach convincing conclusions on the causes of the observed changes.

I think the paper can become publishable with some additional analyses and considerations. I have summarized my concerns and suggestions in five main points:

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.

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.

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. 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. 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? 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].

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 the southeastern basin in 2002-03 which is not visible in 2010-11. 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.

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)? 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).

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?

P2574, L8: delete multi-temporal (obvious)

P2574, L12: within 1974-2007

P2574, L13: shrank in all study periods since 1974

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)

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.

P2574, L21: overall negative values are -> the dominant thinning is (since your values are actually positive!)

P2575, L15: turn-over

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].

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

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

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.

P2577, L25: Where was this observed? Altitude?

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

P2580, L16: Fig. 4b

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

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

P2581, L24: as 10 m

P2582, L1: /% coverage

P2583, L9: A reference is appropriate here, e.g. Gardelle et al. [2012].

P2583, L10: Was this applied as a correction? Zonal or gradual transition?

P2583, L14: Explain what NMAD is.

P2584, L18: Does this imply that the lower tongue is a relict feature, e.g. 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.

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.

P2588, L18: Are you talking about the northern branch here? P2589, L18: This is essentially the definition of a surge, so that is obvious.

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.

P2591, L7-11: These general statements are not really a part of your results. The

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conclusion should focus on your own findings.

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.

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.

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.

Fig. 2: Ok, but not really needed.

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

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.

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).

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

References:

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.

Shea, J. M., B. Menounos, R. D. Moore, and C. Tennant (2013), An approach to derive regional snow lines and glacier mass change from MODIS imagery, western North America, Cryosphere, 7(2), 667-680, doi:10.5194/tc-7-667-2013.

Zemp, M., et al. (2013), Reanalysing glacier mass balance measurement series, Cryosphere, 7, 1227-1245, doi:10.5194/tc-7-1227-2013.

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