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Interactive comment on "Orientation dependent glacial changes at the Tibetan Plateau derived from 2003–2009 ICESat laser altimetry" *by* V. H. Phan et al.

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Overview

The manuscript by Phan and others generates elevation changes on the Tibetan Plateau between ICESat laser altimetry and the SRTM global digital elevation model (DEM), and subsequently estimates glacier elevation change trends from the collection of differences for the period 2003-2009. They propose to analyze the effect of terrain characteristics, slope and roughness, in order to generate a filter for removing spurious glacier elevation changes. They further conclude that glacier elevation changes are generally dependent upon the general glacier orientation.

Overall, there is a major lack of basic glaciological concepts missing in this manuscript. I strongly suggest the authors to read Cuffey and Patterson (2010). Glacier elevation changes at specific points on the glacier are the result of surface mass balance processes (accumulation and ablation) and dynamics (submergence and emergence of ice). For a single land terminating glacier, if glacier elevation changes are measured over the entire glacier, then they can be integrated over the largest glacier area to produce volume changes, and as such, a geodetic mass balance. This uses mass conservation theory which states that the dynamic effects will cancel. This procedure over a single glacier is relatively straight-forward, though becomes complicated if the entire surface is not sampled. In terms of ICESat, the inherent sampling procedure is dictated by satellite's orbit, which at the latitudes of the Tibetan plateau, do not even come close enough to estimate volume changes of individual glaciers or glacier basins, as done in this study. This is one of the major weaknesses of the entire methodology and thus affects all results in this manuscript, which in turn make the entire study unreliable. It may not be impossible to use ICESat on the Tibetan Plateau (i.e. Kääb et. al., 2012), though a much deeper investigation into data aggregation with relation to the glaciological assumptions is required in order to arrive at significant elevation change trends. In fact, the majority of elevation change trends provided in Table 3 and 4 are below the significance levels provided. . .

Moreover, in the present form, the manuscript is missing many important aspects in terms of methodology that clearly affect the results and conclusions of the study. Among others, assessment of ICESat and SRTM horizontal mis-alignments, which is known to exist in the region, can easily produce the conclusions made in this study. As an example, horizontal mis-alignments will produce larger elevation differences for greater slopes, as shown in Fig. 9. In addition, elevation difference trends on stable terrain (i.e. non-glacier) are not analyzed anywhere in this manuscript. As an example, Figure 4 shows the trends for elevation differences, but if extrapolated back to the year 2000 (i.e. SRTM acquisition), the intercept should be 0 (assuming no SRTM surface radar penetration), or at least somewhere close to 0, but there is clearly a difference

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Interactive Comment

for both lines here, with one of the lines intercepting at somewhere around 30 meters. The explanation lies in the methodology.

In summary, I do not think this manuscript provides reliable results for estimating glacier elevation difference trends between ICESat and SRTM, and the conclusions from this analysis is strongly biased upon the aggregation of data and lack of preprocessing. Therefore, it is hard to foresee acceptable publication without generating an entirely new study. The rest of the comments below should portray the weaknesses in the study and may help the authors re-consider their approaches.

Major Remarks

- Use of glims outlines? Are they really from 2002 (P2430L10)? The outlines seem to be extremely outdated and I wonder if the authors also considered looking at the RGI? Since the outlines look outdated, many ICESat footprints classified as glacier may actually not be on glacier which biases the results.

- Section 2.2.1 (P2431-2): Slope and Roughness are elementary parameters derived from a DEM, and are standard functions provided in any GIS or mathematical software package. This section describes, in too much detail, how these parameters were calculated. In my opinion, it is not necessary to include these basic details which can be found in the help of any GIS, or from a standard terrain analysis textbook, such as Burrough and McDonnel (1998) or Wilson and Gallant (2000). Moreover, Figure 2 is also not necessary and is a waste of space to show a visualization of a standard 3x3 processing window, or kernel.

- ICESat spatial sampling is very sparse at these latitudes. Figure 3 shows aggregation of data for two glacier basins and Figure 4 shows the resultant trends. It is clear from this distribution that each time track samples a different part of the tongue (Glacier A for example) which lies at a different elevation range which will then produce a different average elevation change per track, as done in this study. Therefore, at the spatial scale of data aggregation, the results are strictly dependent upon the locations of the

ICESat tracks through time, and clearly will not reliably estimate total glacier elevation changes for this basin. Moreover, the glacier sampling distribution produced by each of these tracks clearly does not satisfy assumptions of mass conservation and cancelling of ice dynamical effects in the individual glacier elevation change estimates.

- Glacier elevation differences: Co-registration between SRTM and ICESat needs to be investigated. These mis-alignments can produce the results you have concluded upon. Also, was there any bias detection correction performed, for example using the collection of differences not located on glaciers? Finally, Saturation bias corrections and the flags are based upon the waveforms and are suggested to be only applicable for the GLAH06 products, not GLAH14, and in particular, only for low sloping surfaces.

- P2435 L13-19: Was this applied for each glacier coverage, as shown in Figure 3? I think, in general, there is a major problem with the approach, mainly based upon glaciology. Glacier elevations change largely at the tongue, and less so at upper elevations. It is not certain the effect of using a threshold to remove points iteratively, in your case a standard deviation of 10 m, will actually remove valid glacier elevation changes. Since each of your samples is relatively small, the size of the sample will have a huge effect on the standard deviation, and also the sampling distribution of each ICESat profile from time, t, will effect this routine greatly. If 2 points fall on the tongue, and 15 points in the upper accumulation areas, then your routine will most likely remove those two points on the tongue.

- P2436 L1-25: What assumptions are made here with this approach? This needs to be considered in great detail, as I assume that at least two of the standard assumptions fail for this regression. As a hint, 1) Randomness, 2) normality.

- P2439 L7-23: It is impossible to judge the significance of this result. The ICESat track distribution needs to be shown on Figure 8. Also, without co-registering ICESat and SRTM, this orientation effect may directly be related to horizontal mis-alignments between the datasets (Nuth and Kääb, 2011). Also, the spatial sampling of ICESat

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for each of your basins remains questionable, and if there is not a good distribution of points, then your sampling will strongly bias the results.

Specific Comments

P2427 L24: reference to this statement? P2428 L8: This manuscript was rejected... I do not think it is appropriate to reference it. P2430 L7: ICESat elevations are provided relative to the TOPEX/Poseidon ellipsoid. Was the difference to WGS84 applied? P2430 L17: Is SRTM high resolution? Not by today's standards... P2430 L18-20: The DEM is resolution is stated twice here, once will suffice. P2434 L1-2: The Saturation Flag is acceptable on low-sloping surfaces, but once slope becomes greater than 10-15 degrees, this flag may not be representative since the width of each ICESat footprint increases with slope and roughness. Therefore, using this parameter to remove footprints probably biases your entire slope analysis since a good proportion of points on steeper slopes will not be included in your analysis. Also, the saturation elevation correction is only valid for the GLAH06 products which uses only 2 Gaussian fits. P2434 L6-8: What is your basis for this threshold? Are you sure there are not surging glaciers which experience drastic elevation changes? P2436 L5-10: the use of the word 'velocity' here is very confusing. This is actually the annual elevation change rate estimate. Glacier velocity is a very different thing, and I think this needs to be change throughout the manuscript (i.e. also in Table 2 and all other places.)

Fig. 2: This figure is not needed. It is easy to describe with works that you used a 3x3 window or kernel for estimating slope and roughness.

Fig 6,7,8: Where are the icesat track distributions? This needs to be shown here!

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