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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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This is not a full review of this contribution. I rather would like to comment on what I think is its most problematic aspect. In my view, the authors do not pay sufficient attention to the representativeness of and potential biases in ICESat elevation measurements, which leaves it open if their results reflect actually geophysical signals or rather method noise and biases.

In summary, Fig 4 shows well that there may be something massively wrong with their method or results. If extrapolating ICESat 2003-2009 trends to the year 2000 (the SRTM acquisition), trends should roughly intersect at dh=0, except for SRTM pen-

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etration and the uncertainty of trend extrapolation (see Kaab et al. 2012 and their Supplement). However, in Fig 4, this exercise would result in dh of about +30 m for the southern glaciers, and -2 - -3 m for the northern glaciers. I.e. authors suggest that SRTM penetration was about 30 m on one side of the mountain and very small at the other side. This is very unrealistic and shows in my view rather that some massive biases contaminate (or even govern) the results.

In detail, problematic aspects of the study are among others:

(1) The authors use the CGIAR SRTM which is over the Himalayas shifted by 50-200 m against ICESat (see Supplement of Kaab et al. 2012). This is in fact theoretically able to introduce a north-south bias as the authors found.

(2) The GLIMS outlines over the Tibet plateau can be massively wrong (old) or shifted, as e.g. Fig. 6 shows. That means that a considerable number of off-glacier points could have been included in glacier trends. As a future improvement authors suggest use of satellite data instead. But this is exactly what Kaab et al, and Neckel et al did already, because of this problem. So, correct classification of the footprints using an up-to-date precise inventory or by overlying the footprint on satellite imagery from the 2000s is a mandatory step not a perspective.

(3) The density of ICESat points over a glacier (section 4.4) is not an appropriate measure of ICESat representativeness. One single ICESat dh point measurement could exactly equal the real mean dh over the entire glacier, or hundreds of ICESat points could give completely wrong (but statistically robust!) results if they, e.g., cover only the ablation or accumulation areas. Rather, elevation change on glaciers is, roughly, a function of elevation. The authors should have shown that the elevation distributions of their ICESat points used are for each glacier group sufficiently representative of the glacier hypsography of the same glaciers. (What if the northern and southern glaciers are simply at different elevations?). Similarly, there could, and in most cases is, a temporal trend of elevations sampled by ICESat (see Kaab et al. and their Supplement).

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This adds an apparent, but false dh-trend. The smaller sub-regions where ICESat data are gathered are, the more critical are those issues: namely at the scale of individual glaciers or glacier groups, as investigated here.

In other words, the crucial assumption page 2434, line 25: 'The average elevation difference dh is considered representative for the height of the glacial area above the SRTM base map at ICESat-sampling time ti.' is not proven, and in my view over-optimistic and in the most cases certainly wrong.

(4) It seems that all ICESat campaigns were used. The June campaigns (probably thick snow cover) cover only the first half of the full series (2004-2006), which may considerably biases trends. Also, authors mix winter (February-March) and autumn (October-November) data. This is, on the one hand, glaciologically wrong, especially for winter-accumulating glaciers. On the other hand it could introduce a massive bias, if e.g. winter dates prevail in the first part of a trend, or vice-versa, as snow thickness is naturally larger at this season than in autumn.

(5) How do authors decide on slope and roughness thresholds? The impact is massive as the authors show. This could actually be a sign of spatio-temporal elevation biases, or simply the lack of representativeness of the ICESat points.

(6) In most cases, the differences between northern and southern trends is statistically not significant as their error bars overlap considerably. So, what is actually the conclusion of the study? That there is on overall no significant dh difference between different aspects?

(7) Authors should also show off-glacier trends from areas close to the glaciers used (e.g., using a buffer length of 5 to 10 km) in order to put their on-glacier trends into perspective.

Best regards

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