The Cryosphere Discuss.,
doi:10.5194/tc-2015-234-RC2, 2016

Interactive comment on "ICESat laser altimetry over small mountain glaciers" *by* D. Treichler and A. Kääb

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Received and published: 10 May 2016

The authors use ICESat satellite laser altimetry elevations as available from 2003 to 2008/2009 to estimate glacial elevation change of small mountain glaciers in Norway. The authors consider several angles to this problem. First, part of the paper could be read as a report on how to extract such glacial elevation changes from the relatively sparse available ICESat elevations over the Norwegian glaciers with the help of locally and globally available auxiliary Digital Elevation Model (DEM) data. An important second angle the authors consider is the influence of the required reference DEM and its possible misalignment on the quality of the results. A third angle, as also the title suggests, is an assessment of using ICESat elevations in general to estimate elevation changes of small mountain glaciers, as can be found all over the world. For this angle it is crucial to assess to what extend local and sparse glacial elevation changes are

representative for a glacial area as a whole.

The paper has valuable contents that are interesting for a larger audience. Small mountain glaciers are present at many different locations on Earth. Monitoring their elevation change by satellite laser altimetry data from ICESat-1 and maybe later ICESat-2 using an additional reference DEM is useful, if detected changes are indeed representative. My problem with the paper in its current state is its focus. If the paper is meant to guide how to extract glacial elevation changes for arbitrary mountain glaciers, at least an analysis on how ICESat is sampling glaciers as a function of latitude is missing: Norwegian latitudes are still relatively favorable, compared to e.g. many South American latitudes. The influence of DEM misalignment is clearly assessed in the manuscript, but how to identify and correct for such misalignment has already been discussed in existing articles. Therefore I suggest to focus the paper on the particular case the authors consider: detecting glacial elevation changes using ICESat and a reference DEM over small Norwegian mountain glaciers. Still, the discussion chapter could be used to generalize to other small mountain glaciers. In addition, the authors should address the following aspects: they don't distinguish between ICESat footprints sampling snow and ice. This should be discussed, and, the effect of this choice on the results should be assessed. The state of the glaciers during ICESat passes could be assessed using additional spectral data or by considering the raw ICESat full waveform signals. Similarly, there might be an effect of terrain roughness and slope on the results, which is not discussed. In addition, the authors confuse glacial elevation change with mass balance change, which are two different things. The authors should discuss why glacial elevation change can directly be linked to mass balance change, notably when one doesn't distinguish between ICESat footprints over snow and ice. Some more detailed remarks are given below.

Detailed Remarks:

1. As above: I would focus the paper on Norwegian glaciers, which should be reflected by the title.

2. p2r26: A more general question that is still open: “Is ICESat track density (in combination with average cloud cover) high enough for sparse glaciers at arbitrary latitudes?”

3. p3r20: “two to three month-long observation periods”, you mean “two to three observation periods each year of about one month each”

4. p3r21: “42 km” this may hold for Norway, but is in general latitude dependent.

5. p4r14: “ICESat tracks of more than one year”: funny English, please reformulate.

6. Section 2.3: what are the difference between: “vertical accuracy”, “mean error” and “standard error”, please define these notions.

7. p5r4: -> “The 2009 autumn campaign is excluded” (skip ‘usually‘ to avoid confusion)

8. p5r9: what is the influence of the 40m threshold for “ice border”? Apparently (Section 3.3) this threshold has a strong influence on the amount of ICESat elevations that are considered to fully cover glaciers (given the quotes of 2.5% on glacier points, and 0.9 % of border points)

9. p5: “snow heights”: (K”a”ab, 2012;2015) discusses Central-Asian glaciers. Why can conclusions on snow variations there be simply ported to a Norwegian setting? And would there be no big differences for valley glaciers compared to icefields, as this figure of Jostedalsbreen suggests: https://en.wikipedia.org/wiki/Jostedal_Glacier#/media/File:P1000290Jostedalsbreen.JPG Why is it not actively assessed if glaciers are covered by snow at the time of the ICESat passes? That could also assist in the issues on winter snow fall and December campaigns raised in Section 4.4.

10. p5, IWD, what parameter? I.e. what power?

11. p5: how did the outliers look like that were removed by the robust fitting? How did the spatial pattern of cloud affected ICESat elevations look like?

12. p6r1: can you quantify “larger number of outliers”?

13. p6: did you experience any issues in the LIDAR data due to not fully adjusted flight strips? (Remaining errors after strip-adjustment)

14. p6: what are possible reasons for the shifts in the Kartverkets DEMs?

15. Section 4.1: how do you know the dh are t-distributed?

16. Saturation may occur along track when ICESat hits bare ice after rock (as it takes the gain ∼5 shots to reset after hitting the more reflective ice). Did you consider the spatial distribution of the saturated waveforms? (compare Molijn RA, Lindenbergh RC, Gunter BC. ICESat laser full waveform analysis for the classification of land cover types over the cryosphere. International journal of remote sensing. 2011 Dec 10;32(23):8799-822)

17. You state: “However, these differences cancel out (Fig4)”. Could you help the reader seeing that in Figure 4.?

18. From the material just in this paper it is difficult to understand what you mean by p10r25-27: “This stresses...weight”. Could you explain this a bit more extensively?

19. p12r6: you say “terrain characteristics” are essential, but, as argued by me before you only consider these only in a very global way.

20. Section 5.3: do you believe that indeed the age of the DEM is crucial, or rather the way it was constructed (photogrammetry, radar, LIDAR)?

21. p15r13: saturation correction (and other flags). I would say this is an interesting topic for more study, to check how the rapid transitions between land-cover on small mountain glaciers influence the ICESat raw signal (and its corrections)

22. p15: quality of the reference DEM vs ICESat: should it not be only the quality, but also its spatial resolution compared to the ICESat footprints compared to the local relief variations?

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