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TCD

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Interactive
Comment

Interactive comment on “What’s in an elevation difference? Accuracy and corrections of satellite elevation data sets for quantification of glacier changes” by C. Nuth and A. Kääb

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Interactive Discussion

Discussion Paper



Author Reply to Reveiw by D. Quincey

Reviewer comments in bold

Author reply in normal text

1. Overall, it seems that there is so much in this paper it loses focus. The methodological sections (1-4) work really well, but the addition of the case studies in Sections 5 and 6 feels superfluous. The paper would benefit from integration of one of the case studies (probably New Zealand) into the methodological description so that there is some demonstration of the framework, without such verbose descriptions of essentially the same thing (but using different data) twice over. A more concise paper, that proposes the framework with reference to a single case study, would have much greater impact in my view.

Thanks for the suggestions! The manuscript has been shortened considerably with the removal of many insignificant details. In particular the "glacier elevation change" sections of both case studies were reduced and the methodology of estimating glacier volume changes and errors has been removed. Many other redundancies within the text are removed.

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We think that both case studies are required to demonstrate applications of statistical adjustments and corrections to elevation data sets in various settings. The New Zealand case study is important to demonstrate the types of biases that exist and the significance of the biases using the extensive availability of stable terrain on steep and gentle slopes. The Svalbard case study shows an application where the universal co-registration and bias corrections can be applied in cases where stable terrain elevation differences are limited, e.g. to less than 10% of the scene. In addition, the Svalbard case study exemplifies the use of ICESat for co-registration and proves the along-track bias correction of ASTER using repeat track ICESat elevation differences.

2. The section on the GDEM (5.2) sits a little oddly within the manuscript – essentially it is too long for simply stating that the GDEM is of insufficient quality (for calculating surface elevation changes). I think it could be reduced to a few lines without detriment to the paper (or even included in Section 3.1 as an important point to note).

Agreed. The section about the GDEM has been shortened. Some of the material is moved into section 3.1, as the reviewer suggests. The rest of the material has been reduced and moved to after the individual ASTER section for the NZ case study (now section 5.3) as suggested by reviewer T. Bolch.

3. The section on elevation changes in New Zealand (5.4) is confusing... having explained that there is significant potential error in each difference pixel, particularly over short timescales, you then present data where the trend is lower than the expected uncertainty (mainly on the east side of the divide, although the same can be said for both Franz and Fox on the short timescales).

I would suggest focussing on the sixyear data, where more confidence can be placed in the results, and not spend time explaining and interpreting effectively meaningless results just to illustrate a point. This modification could easily be implemented in line with comment 1 above.

Agreed. We have significantly shortened the "Glacier elevation change" section to one paragraph and present only results from the 6 year change data. Figures 8 and 9 are combined into one figure showing only the 6 year changes for all four glaciers of the New Zealand case study.

4. Also in Section 5.4 there is some interpretation of frontal dynamics on the Fox Glacier, where you jump from talking about ice thickness changes to frontal advance/ recession. First, some of the data being compared are from different seasons (e.g. April 2001 (at the end of summer) and February 2002 (at the height of summer)). So you should, as a minimum, make some reference to the impact this may have. Second, the thickness changes do not necessarily translate into frontal fluctuations in a given year. Third, I'm not sure a surge has ever been detected/suspected in New Zealand – I think you mean a small speed-up, which is very different to a surge. This section should be modified accordingly.

Agreed. The basis for the majority of this comment have been removed from the text. We no longer show any single year elevation changes and therefore must not indicate the impact of temporal sampling by the DEMs. Less interpretation is provided about the frontal thickening of Franz Joseph and Fox glaciers, but rather is referred to a recent publication by Herman et al. (2011) for interpretation.

Technical corrections

All technical corrections are accepted.

References

- F. Herman, B. Anderson, and S. Leprince. Mountain glacier velocity variation during a retreat/advance cycle quantified using sub-pixel analysis of aster images. *Journal of Glaciology*, 57:197–207, 2011.

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