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The Cryosphere Discussions

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

TCD
4, C1685–C1692, 2011

Interactive Comment

Author Reply to Review by T. Bolch

Reviewer comments in bold
Author reply in normal text

A. General Remarks

...the authors are a bit too ambitious and aim to present lots of information in detail which leads to the fact that the paper got a bit lengthy and misses sometimes the focus. Hence, the reader gets a bit lost and the main results are sometimes a bit hard to identify. The manuscript can and should be shortened without significant loss of information.

The manuscript has been shortened considerably. Specifically, details about glacier elevation changes and insignificant elevation changes have been removed. Also, error methodology related to estimating glacier volume change is removed. Many redundancies between the introduction, data and methods section has been removed that resulted in a shorter manuscript. The focus of the paper should now be clearer and directed towards co-registration and bias corrections of elevation data sets, as the

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new title suggests.

... In addition, the paper would highly benefit if the effect of typical (especially horizontal) biases can be quantified for the presented glaciers or a representative subset thereof in a table. This would clearly show when adjustments are necessary.

The effect of a horizontal bias can be easily estimated using equation 3 and of course will be glacier specific depending upon the histograms of glacier aspect and slope. We now include a sentence about this at the end of the first paragraph in the ”Conclusions and perspectives" section.

Several figures showing the elevation differences are too small and the elevation differences are hard to identify.

Two figures are removed from New Zealand and supplemented with one glacier elevation change map of the four glaciers from 2000–2006.

B. Detailed Comments

Introduction
General: The authors jump a bit between background information, previous studies and the motivation of this study. I suggest to present the utilized data sets first, than present their limitations and previous studies and derive hereof

TCD
4, C1685–C1692, 2011

the motivation of this study. This also helps to avoid redundancies (e.g. P2015 L1-5 and P2016 L10-12) and makes the paper more concise and understandable. The "motivation" of the study (P 2015 L 21-25) may also be merged with the section 2 (Objectives and case study locations).

Good suggestion! The redundancies that are picked are now removed. The section is slightly restructured following the above advice and the motivations are combined into the objections and case study locations section.

Please clarify that the data from ICESat does not provide continuous elevation data in contrast to the SRTM and ASTER GDEM. Provide also here the info about the coverage of the SRTM (60N to 56S) which is one of the major limitations when you want to address glacier elevation changes in polar regions.

It is now mentioned that ICESat is not continuous as a DEM. Also information about the lack of SRTM arctic coverage is provided.

P2015 L15: "Many of these products". To which products do you refer to? You mention earlier only three global elevation data sets. Please clarify.

This is a general statement that has reference to the three datasets and products therein that are mentioned above. A statement was changed to, "In all these datasets"...

3 Data

3.1 Stereoscopic DEMs This section should be shortened. It is not necessary to provide basic knowledge about photogrammetric techniques, sensor geometries etc. I suggest to concentrate on the possible errors and uncertainties of

the stereoscopic DEMs and to refer to the literature for further reading.

This section is shortened. Some basics about parallax are still described to provide the theory behind stereoscopic DEMs.

5.3 Individual ASTER DEMs
The authors jump between methods and results and lack focus. I suggest restructuring and shortening. In addition, I suggest moving this section before the section 5.2 on the ASTER GDEM which is a merge of individual ASTER DEMs.

Again, nice suggestion! This section has been shorted and restructured. The ASTER GDEM results are placed after section 5.3.

5.4 Glacier elevation changes
Again, this section lacks of a clear focus and can be shortened without significant loss of information. In addition, this section mixes glaciological information with information about the DEM quality. I suggest clearly distinguishing between the findings related to the ASTER DEM and the glacier volume changes. In addition, the authors present uncertainties (P 2037 L. 22f) but do not include an uncertainty measure to the later presented results of volume change. Include the term of uncertainty to every presented number of the results. This would also clearly show if the results are significant or if they are within the uncertainty estimate. I would prefer the use the term "uncertainty" instead of "error" here (P 2037 L. 22) and later.

The glacier elevation change section (5.4) has been reduced significantly. Only significant (6 year, 2000–2006) elevation changes are shown and discussed rather than

TCD
4, C1685–C1692, 2011

the annual differences. Two figures are compacted into 1 figure to complement the changes. Errors are not calculated for volume changes because we no longer calculate volume changes. Methodology about estimating errors for volume changes is also removed. We now focus solely on glacier thickness change in the manuscript.

7 Conclusions and perspective

The suggested methodology (P2048 L5ff, Fig. 14) is one major result of the study and should be presented in the results section rather than in the discussion. This method should then be discussed more in detail and it can then be concluded that the method performs well and is easy to implement and is probably superior to other proposed methods.

The framework presented for calculating glacier elevation changes from multi-temporal DEMs (Figure 14, now Figure 12) is a result and a major conclusion of this study. We did not know this procedure of best practice before applying it in the two case studies and only after verifying that the bias adjustments significantly improved the results could we develop the framework. We feel that this conclusion does not fit in the individual case study results, and therefore it remains within the conclusions.

P2045 L22-24: I agree that this method is advantageous as opposed to the RSME minimization because it needs less iterations. However, most crucial is that the result is not worse (or even better?). I think it is the case but this needs to be stated as well.

The second paragraph in the Conclusions now addresses the comparison between the methods in greater detail.

P2045 L15-17: I agree that there will be a systematic vertical bias for steep

glaciers (or glacier parts). As mentioned in the general remarks the paper would highly benefit if the effect of this bias can be quantified. This would be not only helpful as a future advice but also to estimate the uncertainties for published results without adjustments.

See the second reply in **A. General Remarks**. These effects are glacier specific based upon the individual histograms of glacier slope and aspect. Therefore, no further calculations are made for this study.

P2045 L23f: In the respect of the penetration of the radar into snow it may also be stated that this is also an advantage taking into account that some glaciers tongues may have been covered by thick snow during the acquisition time of the Mission in February 2000

We do not want to go into detail about penetration of radar waves into snow and ice.

Figure 8, 9, 13: Please show the DEM differencing for the complete figure not only for selected glaciers. In this way the reader can better judge the quality of the coregistration and the significance of the elevation changes.

Figure 8 and 9: Agreed. Figure 13 (now Fig. 11), The land differences for the Svalbard scene are shown both in Figure 8 and 10. Including the land differences in figure 13 requires the inclusion of glacier masks. Because of glacier masks are lengthy around the nunataks, illustrations including the outlines masks out a large proportion of the changes within the figure, even after smoothing the masks. Due to this, we choose not to include the land areas in Figure 13 (now Fig. 11)

Figure 12: Please show the exactly the same area in figure a and b.

The authors do not feel this is required because the ICESat track in green provides a reference.

Other Technical comments and corrections.

All comments not addressed directly in this reply are accepted and modified in the text.

TCD
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