

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

T. Bolch (Referee)

tobias.bolch@geo.uzh.ch

Received and published: 10 December 2010

A. General Remarks

The topic of the paper is very important as there is a great demand on an improved knowledge on glacier volume changes. These can be e.g. calculated based on multi-temporal digital elevation models (DEMs) and then converted to glacier mass balance. Therefore it is necessary that the DEMs are as accurate as possible relative to each other. Otherwise significant errors can be introduced. The manuscript describes the different biases which can occur with different data sets with the focus on the (nearly) globally available DEMs. The authors present a very valuable and innovative method

C1331

to adjust two DEMs relative to each other in order to minimize the effect of elevation biases. This method is rather simple and comparatively easy to realize. In addition, volume changes of some glaciers in New Zealand and Svalbard are presented. The relevant references are cited, the presented results are reliable and the authors come to reasonable conclusions. The method to adjust the DEMs is described in an understandable way.

However, 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. Some suggestions for that are given below.

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

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, then present their limitations and previous studies and derive hereof 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).

Please clarify that the data from ICESat does not provide continuous elevation data in

C1332

contrast to the SRTM and ASTER GDEM. Provide also here the info about the coverage of the SRTM (~60°N to 56°S) which is one of the major limitations when you want to address glacier elevation changes in polar regions.

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

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.

3.2 Interferometric DEMs This short section refers only to the SRTM DEM. Hence, either this subheading should be renamed (e.g. to "Interferometric SRTM DEM") or other DEMs generated by InSAR (e.g. TanDEM-X) or SAR data which can be used for DEM generation (ERS . . .) should be mentioned at least shortly.

P 2020 L 13: You may clearly state the earth was mapped in February 2000.

3.3 Lidar profiles

The same (name of subheading) heading applies for this section as only the GLAS data is tackled. Include information about the global coverage and density of the footprints.

3.4 Post-processing

Avoid redundancies with P 2015 L 26ff.

4 Methods

General: This section contains some redundancies which can be avoided.

e.g. P2022 L 10-17: I suggest moving to the Introduction (or vice-versa) and merging with P2016 L17ff where the same topic (adjustments of DTMs) is addressed and mostly

C1333

the same references are cited. or P2025 L 11ff vs. P2017 L3ff. and others

5 Case Study 1: New Zealand

Redundancies should also be avoided in this section. I suggest presenting some background information about the study area and its glaciers in the beginning of this section and not later in section 5.4

5.1 Global data sets

Omit 1st sentence; repetition.

P 2031 L7-9 I suggest merging this general statement about the DTM with section 3.4 Post-processing.

5.2 The ASTER GDEM

I suggest presenting the general characteristics of the ASTER GDEM in the section 3.1 about stereoscopic DEMs and focus here on the special characteristics in New Zealand.

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.

P2036 L24ff This paragraph about the penetration of the radar waves into snow and ice is misplaced here. Include this (important) background information in section 3.2

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

C1334

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.

6 Case Study 2: Svalbard

P2041 L 6ff: Omit the statement about the objective. This was already stated earlier.

6.1 Universal co-registration correction

The authors also jump also in this section between methods, results and discussion and lack focus. I suggest restructuring and shortening so that the most important results are presented more prominent.

6.2 Glacier elevation changes

See my comments on section 5.4.

7 Conclusions and perspective

The conclusion section should not contain results and discussion and should be more focused on the major conclusions of this study.

E.g. the comparison with the results of this study with Leprince et al. (2007) (P2047 L1ff and L 13f.) should be included in a discussion section rather than in the conclusions.

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

C1335

methods.

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.

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.

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

References:

A general comment to the formatting of the references (which is probably not subject to the authors): The publication year of the references is a bit hidden before the indication of the page(s) where the reference is (are) cited. This is especially because the page numbers start with 20 like most of the references.

Figures

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 co-registration and the significance of the elevation changes.

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

Interactive comment on The Cryosphere Discuss., 4, 2013, 2010.

C1336