The Cryosphere Discuss., 7, C2975–C2979, 2014
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The Cryosphere Discussions

Interactive comment on "ESA's Ice Sheets CCI: validation and inter-comparison of surface elevation changes derived from laser and radar altimetry over Jakobshavn Isbræ, Greenland – Round Robin results" *by* J. F. Levinsen et al.

Anonymous Referee #1

Received and published: 10 January 2014

General comments:

Over the last few decades, satellite altimetry has become an important tool to monitor the state of the ice sheets. Several research groups have developed their own methods to process the data, and different sensors are being used. This makes that published results are not always directly comparable to one another. Although earlier studies have compared methods and sensors (e.g., Thomas 2008), a thorough inter-comparison as is the purpose of this manuscript is still missing (to my knowledge). This study therefore has the potential to make a relevant contribution to the field. For ex-

ample, the finding that radar altimetry is able to capture the surface elevation changes in the marginal regions of the Greenland ice sheet is quite important. However, I believe that in its present form, the manuscript shows some methodological (and other) shortcomings which should resolved before publishing the article.

Specific comments:

* As I mentioned above, the finding that radar altimetry is able to capture the surface elevation changes in the marginal regions of the Greenland ice sheet is novel and would imply that the ERS/Envisat observations have more potential than hitherto assumed. Unfortunately, no details about the processing and, in particular, how it differs to the processing of radar altimetry used so far are given. I assume that the authors will publish this in a separate article, but a short ($\sim$ one paragraph) description should be given, in my opinion.

* The manuscript focusses on the differences between different methods (crossovers vs. repeat track) and sensors (laser vs. radar) but a discussion of how the contributions of the different research groups given a method and sensor differ from one another (e.g. how do the ICESat repeat track (SEC-2 to 5) compare?) seems equally important to me, for example to give an idea of the spread and methodological uncertainties.

* I was very surprised to read that 'the method for finding the remaining [error] estimates [of the repeat track SECs] is unknown'. As you mention, 'the error estimates provide important information on the accuracy of the observations and methods and are therefore included', but without any background documentation on how they were computed, they are relatively meaningless. For example, SEC-2 and SEC-5 both are based on ICESat data and use a repeat-track method. Comparing their results in fig. 1, SEC-5 appears to be slightly more noisy than SEC-2. Yet, the SEC error estimates of SEC-5 are an order of magnitude lower than those of SEC-2, which is puzzling. Furthermore, on page 5441, the fact that the error estimates are at the sub-meter level is used to confirm the high accuracy of the results. Without a proper description of the

error estimates, it's hard for the reader to tell if such a claim is justified. According to the 'author contributions' section at the end of the manuscript, the Round Robin participants are co-author of this paper, so I would expect that it should be relatively easy to obtain this information and include this in the manuscript, which allows the reader the results correctly. Furthermore, I strongly recommend to ask the author of SEC-10 for his/her error estimates too. In science, every number should come with an error estimate to allow to draw valid conclusions. The fact that SEC-10 is being used in the RR inter-comparison in section 3.2 makes this even more important.

* In the inter-comparison section, the motivation for choosing the experiments to be compared is not always clear. Take, for example, the Laser repeat-track vs. cross-over comparison, where you compare SEC-3 (repeat-track) to SEC-7 (XO). Another option would have been to compare SEC-2 (which covers the full study domain, not only the drainage basin) to SEC-8 (which scores better than SEC-7 in the comparison with the LiDAR data), or SEC-4 to SEC-6, or ... This can have a substantial impact on the result of the inter-comparison and following discussion: figs 1 and 3 show that the SEC-7 dH/dt are all nearly-zero, so in essence, the mean and RMSE in table 3 are basically the mean and RMS of the SEC-3 dH/dt. It also explains the low slope of 0.01. Replacing SEC-7 by SEC-6 or SEC-8, which both show a much better (visual) agreement with SEC-3 in fig. 1, may lead to very different statistics and a different discussion. (The same applies for the other inter-comparisons, where other combinations are possible as well). The choice of experiments should be justified properly, and a more detailed discussion of differences in the results of experiments using the same method and sensor seems required (see my point above).

* The fact that SEC-3 only computes the dH/dt for the drainage basin may affect the statistics in table 4. You mention on page 5444, the differences between the LiDAR and altimetry observations are large north of the glacier basin. These points are not included in SEC-3, which likely leads to a lower mean and std of the LiDAR-altimetry differences. In order to get a fair comparison, the statistics should be computed for

identical domains.

*p. 5444, line 10: It might be worth to point out that SEC-7 uses a similar grid spacing as SEC-9, and has a similarly high mean(diff_lidar).

*p. 5445, line 3: Borsa et al. 2013 discuss the Gaussian vs. Centroid (G-C) offset, which explains only part of the inter-campaign bias. Even after correcting for the G-C offset, intercampaign biases will remain, they conclude.

technical comments: * I'm not a big fan of using abbreviations in the title. ESA is probably well known, but CCI should be written in full.

* p. 5435, line 4, "13 projects either affecting or affected by the concurrent changes": The projects are not affecting/affected by climate changes, but rather study parts of the Earth system affecting/affected by the changes.

* p. 5435, line 12: "most optimal method" -> "optimal method", there is only one optimal method

* p. 5435, line 17-18, "... due to the high accuracy of the former and the high spatial resolution of the latter": this implies that the Envisat measurements have the high accuracy and the ICESat measurements the high spatial resolution, but this probably should be the other way around (as you also mention on line 21).

* If necessary, the introduction could be shortened. At present, it discusses the ESA CCI in quite some detail, but this is not essential to understand the remainder of the article.

* p. 5436, line 17-18: Wouters et al., NatGeo, 2013 provided a detailed discussion and analysis of the increased mass loss of the ice sheets.

* p. 5436, line 23: What is the motivation for the dimensions of the grid (5x5km)?

* p. 5437, line 23: explain what CRYOLIST is, or provide url. Not every reader will be familiar with this mailing list.

* p. 5438, line 12-13: since only one of the submissions was rejected, it would make more sense (and be less ambiguous) to simply state which of the two potential rejection criteria applies (i.e., was the submission rejected because it is comparable or independent, or both).

* p. 5448, line 19: add the url where the Envisat SEC prototype can be found.

* p. 5455-5456: To interpret the results in tables 3 and 4 correctly, it would be very helpful to list the number of points used in the computation of the statistics.

Interactive comment on The Cryosphere Discuss., 7, 5433, 2013.