

Interactive comment on “Comparison of direct and geodetic mass balances on an annual time scale” by A. Fischer et al.

Anonymous Referee #2

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Overview

This paper presents a very interesting dataset of multi-temporal DEMs acquired by high precision laser altimetry, generally separated by a time step equivalent to a mass balance year. The goals of the study are to compare elevation differences between these LIDAR DEMs to glacier mass balances measured on the ground using stakes and snow pits. These types of studies are popular at the moment, however, few locations around the world allow these types of direct comparisons. In addition, very few, if any, studies have attempted the annual comparison between elevation change and surface mass balance which enhances the importance of this research.

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While the dataset, alone, has incredible potential for the glaciological community, there are a number of theoretical and methodological problems with the study in its present form. The manuscript is difficult to read. The authors present a number of questions that are to be answered in the Introduction but the conclusions are not easily absorbed by the reader, and possibly all the questions are not even at all answered. I think this manuscript needs a full re-structuring, and possibly a re-submission. The authors should think about what the importance of their findings in light of all the previous research. In my opinion, if presented properly, with sound description and presentation of their methods, the relevance of this paper lies in the ability to provide a spatially distributed estimate of emergence/submergence on an annual time scale. This has never been shown before. Granted there will be errors from, for example, the "density assumption" for the elevation changes, the patterns provided in the lower maps of Figures 7 and 8 have possibly more potential than the authors realize, and will have a larger impact to the glaciological community if presented in a clear structured manner.

I hope that some of my suggestions below will help the authors in improving this research.

General Comments

1) Theoretical Considerations and Formulation:

The manuscript would benefit significantly with a proper theoretical basis for the comparison that is presented. My suggestion is to use (Cuffey and Paterson, 2010, pgs 330-337). This will help not only to clean up the terminology evoked, but also to make clearer what exactly is being compared, and what potentially can be the residual between this comparison. As far as I see it, you compare elevation changes to surface mass balance.

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I think there is a misconception of elevation differences at pixels and the geodetic balance which is an integration of all pixels over the glacier area. Equations [2,3,4] are provided to describe a "geodetic balance", and is presented in a manner that is difficult to follow. I suggest a significant revision of this section. For estimating the volume change, one can look at (Etzelmüller, 2000). A geodetic balance is the volume change of an entire glacier normalized by the glacier area, converted into mass or ice equivalent units (as the authors have described), generally provided as an annual rate. This balance is theoretically equal to the surface mass balance (as measured directly in the field using stakes and snow pits), only in cases of a non-calving glacier (which is the case for these two glaciers?) with negligible basal balance (?) AND only when the elevation changes are integrated over the entire glacier, such that the dynamical effects on individual elevation changes cancel due to mass conservation (see the equations in Cuffey and Paterson, 2010). There is much literature available that have not been referenced that describe this point and some that further make corrections to account for retreat in the non-equilibrium cases (see e.g. Krimmel, 1999; Elsberg et al., 2001; Cox and March, 2004).

In light of this point, I believe Figures 7 and 8 do not show geodetic balance (b_g), but rather show water equivalent elevation changes. An interesting comparison, which would significantly contribute to the results of this manuscript, is that the difference between the two, as presented in the bottom plots of Figures 7 and 8, is somewhat related to the emergence/submergence velocities. This point is discussed in the manuscript, but is not easily extractable by the reader. More focus should be given to this in a clear manner, possible as a section in the Results, or even inclusion as a Discussion. Also, how accurate could the estimate of the submergence/emergence estimates at each individual pixel be, as shown by the bottom maps of Figures 7 and 8.

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Furthermore, the terminology for ice submergence/emergence needs to be implemented along with an explanation of the difference between these velocities and the horizontal velocity. Following from the above comment, I also recommend a significant revision of section 5.4. The equation here is not completely wrong, though the derivation is difficult to follow, and Figure 2 is difficult to understand, and I am not exactly sure what the authors are calculating. If I understand this correctly, you are measuring the stake positions with dGPS and the mass balance from the change in *exposed stake length*, and then you combine this with the elevation change as measured from the DEMs? Or do you correct change in elevation of the top of the stake for the horizontal slope of the movement between the two measurements (where do you get this? The DEMs)? A nice summary of the equations for the latter approach can be found in Bamber and Payne (2004) (pg 12). This point needs to be clearer, as it probably is one of the most important results of the paper. By revising these points, the results presented in Table 7, 8 and 9 may become clearer. As these Tables are now, I find it difficult to understand what exactly is being presented by them.

In summary, I wonder with the data provided in this manuscript, if it is not possible to provide two independent estimates of ice submergence/emergence at the stakes? One from the comparison between surface mass balance and elevation change. And another by using the measurements of stake velocities as measured from the top of the stake, and using the equations in Bamber and Payne (2004) (pg 12). If so, this could make a wonderful discussion figure and possibly replace Table 9, in which the importance is difficult to extract.

2) Elevation Change methodological description and implementation:

In terms of Lidar DEM comparison, it is quite apparent that a lot of work was involved with measuring the ground control points (GCPs). It is, however, unclear what purpose they serve. Are they used to make a transformation between the LIDAR

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DEMs to the local coordinate system used by the GCPs? If so, how were the DEMs tied to the GCPs? Also, what projection are the GCPs and what projection are the DEMs?

It is clear, however, that the GCPs are being used to assess the quality of the DEMs. To this avail, the biases presented between each individual DEM and the GCPs in Table 2 represent the bias to the "GCP" system, but not to the biases in each DEM comparison. The difference in bias, for example between 2001 and 2002, makes up at least 30% of the difference between direct and geodetic balances presented in Table 5 (The bias between 2003 and 2004, Table 4, may actually explain up to 100% of the 2003/2004 difference, $b_d - b_g$ in Table 5 and Table 6). In a minimum, this point (for all DEM comparisons) should be analyzed, acknowledged and made clearer in the text.

Following this, I wonder why the authors do not simply use the DEM differences on stable terrain to assess the uncertainty of each individual geodetic balance (i.e. DEM 2001 vs. DEM 2002)? It is clear that GCPs are very accurate, but statistically, there are only 51 of them. Also, what is important is the coherency between each subsequent DEM to the previous one, rather than how they compare to the GCPs. One could also triangulation at least 3 DEMs which results in the overall uncertainty of the bias, and possibly make corrections if need be. At a minimum, one should analyze the mean difference over stable terrain to make corrections to the derived changes for this mean bias (Berthier et al., 2007; Schiefer et al., 2007; Nuth and Kääb, 2011). Additionally, the statistical strength of this comparison containing thousands, if not millions, of pixels, will provide a more robust estimate of the bias between the DEMs, and at a minimum, help explain some of the differences between the direct and geodetic balances provided in Tables 5 and 6.

3) Presentation quality:

There are 9 Tables and 9 Figures. I think that some of the tables can be reduced, and

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possibly replaced by figures. Also, Are figures 3, 4 and 6 necessary? Little additional information is provided by Figure 6, especially, and it leads the reader unsure on how you geometrically corrected those images to extract firn line locations? It would be interesting to see more of the elevation change and mass balance maps, and the difference between them.

4) Errors

Error estimates need to be provided for all numbers. For example as it stands now, it is not possible to assess whether the differences between the geodetic balance and the surface mass balance are significant.

5) Surface vs. Planimetric area

I have difficulty understanding the relevance of this in particular. Considering your stakes generally are drilled in vertically, and measurements are made vertical, then I do not see a problem with surface area, except on very large slopes. In addition, how does this affect your elevation changes? Lidar waveforms? Since your elevation changes are also measured in the vertical, what is the relevance of surface vs. planimetric areas?

6) Manuscript Structure

The manuscript requires restructuring in order to help the flow of the manuscript and the presentation of the results. One suggestion is to begin with mass continuity theory describing the individual parameters. Then the data and methods can be shown for each of the individual parameters. The results can then contain the comparison between the two. A discussion would be beneficial for errors associated with both measurements and the possibility that the residual between elevation change and surface mass balance may represent a dynamic term (submergence/emergence). I think the authors should determine what is exactly the significance of this work, and outline that as one story line. As it reads now, there are many story lines without any

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significant conclusions.

Technical Corrections

There are numerous technical corrections that are required, and all are not listed. Some of the major ones are listed below.

- General: Sentences should not start with numbers. (E.g. pg 569, line 24; pg 581, line 1; and many more...
- pg 569: Numerous statistics are provided, but the reader has difficulty understanding what the meaning of it all is. Come clearer to the point, possibly stating first, and then back up with the relevant statistics.
- pg 569, line 21-23: Two time periods are given, and only one annual precipitation number(661 mm). Is the average the same in both periods?
- pg 570: A very detailed description of the LIDAR is provided. Is it all necessary? I suggest removal and compacting some of the information as the paragraph is heavy and difficult to absorb.
- pg 572, line 15-16: "The distribution of snow ... is monitored by webcam". How is this monitored? Or more specifically, how do you geo-rectify the pictures to produce firn maps?

References

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