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The Cryosphere Discussions
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Interactive comment on "LiDAR snow cover studies on glacier surface: significance of snow- and ice dynamical processes" *by* K. Helfricht et al.

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We appreciate the effort of anonymous referee #1 to review the manuscript thoroughly. The referee's constructive comments, suggestions for improvements and critical remarks will help to improve the paper. In the following we respond to each comment. The responses and corrections will be considered in a fully revised manuscript.

General comments

Comment (1): As outlined above, the manuscript does not contain enough information to be able to assess in any meaningful, quantitative way the significance of different snow and ice dynamical processes. We know from theory that they are important,

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and the authors outline this quite extensively. Yet, the authors present no information on time-varying rates of densification, ablation is alluded to in the results of another paper but is not mentioned much here, and ice flow likewise. Yes, all of these factors contribute to observed elevation changes, but there simply is not the data, nor the measurement accuracy in either laser altimetry or GPR data presented here, to be able to partition the effects of these individual processes on observed elevation changes. I get the sense that the authors have these fabulous radar and laser datasets and are now just reaching a little too far to be able to say something universal. It is not justified with these data, and references in the text should be either removed, or their language certainly toned down.

Response: Unfortunately, there was a misinterpretation of the word "significance". "Significance" was not meant to be a statistical term but simply influence. We agree that the word "significance" has to be replaced in the title and toned down in the text. We changed the title to "Lidar snow cover studies on Alpine glacier surfaces: comparison with snow depths calculated from GPR measurements" and avoided "significance" in the following text.

We have evaluated the difference between Δz_{ALS} and h_{GPR} and found that in firn areas it is negative and larger than uncertainties of applied measurement methods. In order to demonstrate this we have applied a moving window of 300 points to all profiles measured. The revised version will provide an additional figure where it is obvious that the difference of Δz_{ALS} minus h_{GPR} is generally negative in firn areas and close to zero on ice surfaces. Even though we do not have concurrent measurements of the individual processes we suggest a connection to densification of firn and submergent ice flow in the firn areas and will give orders of magnitude of both of these effects from earlier measurements.

The referee's view that there are not enough data to be able to partition the effects of these individual processes, is right. However, the aim of this study is to record the summation of all processes leading to deviations between Δz_{ALS} and h_{GPR}.

TCD
7, C1338–C1351, 2013

Comment (2): The organisation of the manuscript is generally fine up until section 4 'Results and Discussion'. Here, one large section of all the results and discussion points is difficult to read and follow. The text jumps around and it is difficult to keep up with which dataset is being discussed / compared, etc. I would prefer this section to be broken down into clearer, more organized sub-sections, examining in turn each set of results and each discussion point.

Response: We agree and will give subheadings of section 4 in a revised version.

Comment (3): There are numerous instances of ambiguous or confusing language in the text, which I would assume is due to the authors not having English as a first language. I point them out in the specific comments below.

Response: We want to thank the referee for the fair advice to revise the language. We carefully edited the manuscript, not only for the points mentioned in the specific comments.

Specific comments

Comment 1: Title, and throughout: Use of the capitalised acronym LiDAR. I don't like this. The editorial policy of The Cryosphere should decide on this matter ultimately, but convention for acronyms is that they are uppercase except for when the acronym takes on an identity as a regular word. Lidar has been around long enough for this and should be referred to in the lower case, in the same way as scuba or radar.

Response: We agree with the referee. Lidar shall be used throughout the text.

Comment 2: Lidar snow cover studies on glacier surfaces, surely? Plural, as you investigate more than one? However, this title should be revised, as the 'significance

TCD
7, C1338–C1351, 2013

of snow and ice dynamical processes' is not really justified in this work (see comments above).

Response: The title will be revised to "Lidar snow cover studies on Alpine glacier surfaces: comparison with snow depths calculated from GPR measurements"

Comment 3: Abstract, line 13: but you do not actually evaluate the magnitude of these processes, do you? And without their magnitude, you cannot say much about their significance. The motivation for this work must simply be to map the spatial distribution of snow accumulation. You can mention these dynamic processes, and say they are likely at work, here, but unless you can actually measure them (which you haven't directly, or even indirectly given the errors associated with your primary datasets), you cannot base the entire paper around this.

Response: The sentence in the abstract and several passages in the text will be changed accordingly.

Comment 4: Abstract, line 17: Submerging ice flow and densification are probably contributing to the discrepancy between ALS elevation change and GPR snow thickness – but you are assuming this, you have not measured it. You do not know if one is more important the other. You do not know if it's all submergence flow, or all densification. And in fact, how do you know its not measurement error of your GPR or elevation change signal (in this instance)?

Response: Lines 17 to 20 will be replaced by "In firn areas ALS surface elevation changes were mostly smaller than actual snow depths calculated from GPR measurements. Mean differences amount to -0.4 m with a standard deviation of 0.34 m."

Comment 5: Abstract, line 18 and throughout: Deviation is also a confusing word here, especially as it's in a sentence along with standard deviation. You mean the difference

TCD
7, C1338–C1351, 2013

between ALS elevation change signal, and observed (GPR or snow pit) snow depth. Is that a 'deviation'. Perhaps residual would be a better word? Here and throughout, e.g. 1792, line 17.

Response: We agree, that deviation, especially in combination with standard deviation, may cause confusion. So we will use the term "difference" (Δh_{abs}) rather than "residual", which would be appropriate for the remainder of a balance, or so. To keep the replies comprehensible, "deviation" was maintained in the response, but will be replaced by "difference" in the revised manuscript.

Comment 6: Abstract, line 21: How do you know that this is emergence flow? How do you know its not greater densification as its lower at the terminus and some melting has occurred? You mention another paper has some measurements of vertical velocity. If you showed that data and could actually demonstrate emergence flow, much then this would be a more convincing result.

Response:

We agree. This sentence will be changed to "Distinct overestimation of snow depths by ALS surface elevation changes were found at the front of the glacier tongues only". For the revised version of the article ice velocity measurements performed on the investigated glaciers are collected and included in the discussion.

Comment 7: page 1789, line 24: what sort of 'empirical, process-based way'? More detail required.

Response: The introduction on page 1789/90 will be rewritten in the revised version.

Comment 8: page 1790, line 2: these gauges may underestimate the total precipitation volume. You're saying here that they always underestimate, up to 50%.

TCD
7, C1338–C1351, 2013

Response: Corrected to "may underestimate"

Comment 9: page 1790, line 11: what ‘additional information’?

Response: Corrected to "...additional information like elevation, the curvature of the surface, wind sheltering, and exposure to solar radiation..."

Comment 10: page 1790, line 19: Lidar has been around since the 1990s, so I don’t think it can still be referred to as an ‘upcoming’ technique.

Response: ’upcoming’ was deleted

Comment 11: page 1790, line 23: I see little use throughout this entire manuscript of the ‘example refer- ence’ (e.g. Author x et. al.). Here you are stating that only Hopkinson et al and Kraus have calculated surface elevation changes from multi temporal DEMs, which clearly is not the case – many others have too, and long before these two references. If your cited cases are examples, then cite (e.g. Author x..).

Response: (e.g. Author x..) was applied were reasonable.

Comment 12: page 1791, line 7: I see the authors have previously published SWE distribution and accumulation gradients from ALS data. I do not have access to this particular publication, so I am unable to check whether or not these are the same data. If they are, then the editors of The Cryosphere should check whether there is duplication in the results presented here.

Response: http://www.adv-geosci.net/32/31/2012/adgeo-32-31-2012.pdf. Data used for this article are from previous ALS acquisitions.

Comment 13: page 1791, lines 11 onwards: This is all fine, but following my comments above, how relevent is this information now? It’s textbook stuff.

TCD
7, C1338–C1351, 2013

Response: The authors decided to leave these information in the introduction in a somewhat condensed form. It may help the reader to locate the processes leading to Δz_{ALS}. As required below, Eqs. 1-3 and Fig. 1 were revised in terminology.

Comment 14: page 1791, line 23, equation 1: You are defining snow accumulation as the difference in elevation between the snow surface at times t2 and t3 (here, z2 and z3). In fact, this is simply a surface height change. Accumulation is a mass balance term, measured in water equivalent units, and calculated taking account of both snow depth and density. Equation 1 is thus invalid (which makes equation 3 invalid), unless you change ACC to Δ h.

Response: The term ACC was replaced by Δz_{HS}, as was densification (Δz_{DSF}) and ablation (Δz_{ABL}). So these terms may clearly indicate a surface elevation change independently of the storage or release of SWE.

Comment 15: page 1791, lines 26-27. Careful here. Densification of snow and firn layers only leads to an underestimation of actual accumulation on a static ice body if you define 'accumulation' as you have done here (that is, a surface elevation change, and therefore, incorrectly). A traditional surface mass balance measurement takes account of the density of snow and firn. Even on a static ice body (neglecting an elevation change due to ice flow),surface at time 2, minus surface at time 1 does not equal accumulation. 'Accumulation' is measured by stake and snow pit with measurements of density. Surface elevation change at a point is only surface elevation change – not 'accumulation'.

Response: Corrected to "The densification of snow and firn layers existing at t_1 leads to an underestimation of Δz_{HS} in the case of snow and firn layers on a static ice body at t_2"

Comment 16: page Page 1791, line 3: this section needs to be re-thought

and framed in the correct glacier mass balance terminology. I suggest Cogley et al's reference guide to see the difference between elevation change at a point and surface mass balance (Cogley et al., 2011, Glossary of Glacier Mass Balance and Related Terms, 86, IHP-VII Technical Documents in Hydrology - http://unesdoc.unesco.org/images/0019/001925/192525E.pdf).

Response: The authors agree and terminology will be changed (see Comment 14). Capital letters for indexing were maintained, because they account for processes and not for mass equivalents. Glacier mass balance terminology would be misleading here, because, as the referee explained correctly, information on density are required to transfer elevation changes into SWE.

Comment 17: page 1792, line 6: Again, you do not measure accumulation here, you measure snow depth. One is not the same as the other. Essentially what this study is doing is to measure snow depth from repeat lidar measurements of surface elevation change between intervals throughout the accumulation season, and to validate (to an extent) those spatially distributed snow depths with ground penetrating radar measurements. A major confusion appears to arise from mistaking snow depth for accumulation.

Response: Corrected to "We compared actual snow depths..."

Comment 18: page 1792, lines 10-14: You can measure (i). You can speculate (ii) based on theory, but you cannot measure these processes unless you have lots and lots of snow pits. (iii) again, you can speculate, but you need to know about the spatial distribution of densification and ice flow, neither of which you present here.

Response: This section was revised to:

(i) What is the magnitude and the sign of the deviations between ALS derived surface

elevation changes and actual snow depths calculated from GPR measurements on the investigated glaciers?

(ii) What is the consequence of deviations between ALS surface elevation changes and actual snow depths on the application of multi-temporal ALS to derive snow depths on the investigated glaciers?

Comment 19: page 1797, line 22: Don't reference equation 5 before equation 5. Instead, '... were calculated following' then show the equation itself. Same applies to equation 8 and 9. Would a common mid point survey not have helped to determine the signal velocity? Why was this not used?

Response: Reference to equation 5 corrected.
Common mid point (CMP) surveys are practicable using a GPR with separate transmitter and antenna. The GPR used for measurements in this study were shielded antennas including the transmitter. Hence a separation was not possible. Further a multi-antennae multi-offset campaign on seasonal snow on a glacier is not feasible from the surface due to disturbances of the upper snow layers for the wide-offset configurations caused by the multi-antennae setup. A CMP setup above ground (even a simple one with just 3 different angles) requires either helicopters or grooming machines and it is not guaranteed that the geometry of the CMP-setup stays constant. A single snow machine is not adequate to allow reliable data analysis for such a setup.

Comment 20: page 1799, line 20: xyz accuracy information is affected by the position of the scanning platform and its orientation. The position is affected by the quality and processing of DGPS data, orientation is not 'roll and spin' but roll, pitch and yaw, as measured by an inertial navigation system or unit (INS, or INU).

Response: Corrected to: "Accuracy of the 3-D location of each point is affected by the

TCD
7, C1338–C1351, 2013

accuracy of the recording of the position of the scanning platform and its orientation (e.g. Joerg et al., 2012; Deems et al., 2013). The accuracy of position is affected by the quality and processing of DGPS data. Orientation in terms of roll, pitch and yaw are measured by an inertial navigation system (IMU)."

Comment 21: page 1800, line 12: What interpolation scheme was used and how did that choice affect resultant measurement accuracy?

Response: A simple mean interpolation scheme including all points within a grid cell was used to derive the elevation raster with a grid size of 1 m. According to Deems et al. (2013) the high point densities allows simple interpolation schemes with minimal error. Data were analysed along the profiles on glaciers, where the surface is homogeneous and less inclined.

Comment 22: page 1804, line 1: 'supposed', but unknown. Speculative. It could be measurement error, or something else?

Response: Following the advice of referee #2 to shorten this part of the results section, this part will be removed.

Comment 23: page Figure 6, 7, 8, 9: the scale label is misleading here. What does 'depth scaling' mean? This should be lidar surface elevation change (m), and ground penetrating radar derived snow depth (m). Also the red-pink for 4.51-6 m looks a lot like the red for 0.51-0.75 m.

Response: 'depth scaling' was replaced by Δz_{ALS} and h_{GPR}.

Minor comments / text edits

Comment 1: Abstract, line 16 and throughout: Landsat should not be capitalised (see landsat.usgs.gov). Also, state which sensor your Landsat data came from.

Response: 'Landsat' was corrected. The Landsat 4-5 image was retrieved from USGS http://glovis.usgs.gov.

Comment 2: page 1789, line 9: sources, not resources.

Response: corrected

Comment 3: page 1789, line 12: subject to flood forecasting? Do you mean the subject of flood forecasting? Or something else? I don't follow.

Response: Corrected to subject of flood forecasting.

Comment 4: page 1789, line 20: glacier surfaces. And observed by who?

Response: Corrected to "Further complex relations exist between the formation of glaciers and snow accumulation, and vice versa".

Comment 5: page 1790, lines 7-9: strange English in this sentence.

Response: Corrected to "However, annual glacier mass balances are monitored on a small number of glaciers worldwide only"

Comment 6: page 1790, line 13: operate at the point scale only.

Response: Corrected

Comment 7: page 1790, line 14: satellite remote sensing, not 'extraterrestrial remote sensing'. Here are elsewhere, e.g. 1792, line 18.

Response: Corrected

TCD
7, C1338–C1351, 2013

Comment 8: page 1790, line 15: MODIS is an acronym, so capitals is fine. But first define the acronym. Landsat is not an acronym, so do not capitalise (see comment above).

Response: Defined

Comment 9: page 1790, line 21: Lidar delivers georeferenced surface ...

Response: Corrected

Comment 10: page 1790, line 24: either 'was applied by Author x (year)', or 'has been applied' (e.g. Author x, year).

Response: Corrected to 'has been applied'

Comment 11: page 1790, lines 27-28: a limited area, not a restriced area.

Response: Corrected

Comment 12: page 1791, line 3: delete 'so-called'

Response: Deleted

Comment 13: page 1792, lines 4-5: Why does this sentence have its own separate paragraph?

Response: Added to following text.

Comment 14: page 1793, line 13: not sure 'were evolved' in the correct wording here.

Response: Replaced by 'developed'

TCD

7, C1338–C1351, 2013

Comment 15: page 1793, line 20: a stake network has been surveyed..

Response: Corrected

Comment 16: page 1794, line 1: models were calibrated (not was)

Response: Corrected

Comment 17: page 1794, line 11: Redundant sentence.

Response: Sentence deleted.

Comment 18: page 1795, line 10: at a uniform speed. What speed? Also, you don't seem to mention what the actual GPR sample shot spacing was. This would be helpful.. Line 20 – why not DGPS every shot?

Response: In the authors view the speed of the snowmobile is not the essential information. Mean trace distances along the profiles are given in Table 1. The DGPS could not be connected directly to the GPR. Hence the individual sections of the profiles were planned to be straight and not longer than 500 m. Most of the sections were clearly less than 500 m in length, which can be seen in Fig. 6.-9.

Comment 19: page 1796, line 5: Snow depth probing and pits were used to identify..

Response: Corrected

Comment 20: page 1797, line 15: measured vertically.

Response: Corrected

Comment 21: page 1797, line 22-23: this should have been defined first at 8-9. (twtt, not twt)

TCD

7, C1338–C1351, 2013

Response: Location of the definition has been corrected. According to (e.g. Hauck and Kneissl, 2008) the abbreviation TWT will be used.

Comment 22: page 1799, line 13: e.g. Geist – lots of other people have used ALS in mountainous regions..

Response: Corrected

Comment 23: page 1802, line 9: taken alongside

Response: Corrected

References

Deems, J., Paintner, T., and Fineegan, D.: Lidar measurement of snow depth: a review, Journal of Glaciology, 59, 467–479, doi:10.3189/2013/JoG12J154, 2013.

Hauck, C. and Kneissl, C.: Applied geophysics in periglacial environments, Cambridge University Press, Cambridge, 2008.

Joerg, P. C., Morsdorf, F., and Zemp, M.: Uncertainty assessment of multi-temporal airborne laser scanning data: A case study on an Alpine glacier, Remote Sensing of Environment, 127, 118 – 129, doi:10.1016/j.rse.2012.08.012, 2012.

Interactive comment on The Cryosphere Discuss., 7, 1787, 2013.