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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 #2 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 (i): The title is somewhat misleading. One would expect a detailed and quantitative investigation of glacier dynamics and their impact on the accuracy of multi-

temporal LiDAR snow depth. Glacier dynamics are mentioned as the source for certain systematic deviations but the manuscript does feature very few observations of ice dynamics nor does it include any quantitative assessment whether the observed deviations ALSGPR could be really the effect of ice dynamics.

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. We changed the title to "Lidar snow cover studies on Alpine glacier surfaces: comparison with snow depths calculated from GPR measurements".

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. However, only the summation of the processes leading to these deviations can be analysed from the measurements. 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. Available measurements of annual ice flow will be included in the revised discussion.

Comment (ii): The findings of this study appear sound to me but have a somewhat limited applicability. The authors have investigated glaciers that slowed down drastically over the last one or two decades. A similar study conducted in regions where glaciers are dynamically more active might come to diametrically opposed conclusions. I believe what is missing in the present study is a more detailed description of the dynamics of the investigated glaciers and an attempt to quantify the influence: How fast are they moving? How large are submergence and emergence components of the flow? How well does submergence/emergence from measurements and/or theory agree to the observed deviations ALS - GPR? Putting more emphasis on these points would add to the value of this study and make it easier to understand where the findings are applicable.

This requires that measurements of glacier flow dynamics are available (e.g. measured velocities at the stakes) or that flow fields could be derived using feature-tracking on the ALS DEMs.

Response: We agree with the statement of referee #2, that the investigated glaciers slowed down drastically over the last one or two decades (e.g., Span et al., 1997; Span and Kuhn, 2003; Abermann et al., 2007). Certainly the influence of ice dynamics would be different in regions with higher mass turnover of the glaciers. The motivation of measuring the deviation of Δz_{ALS} from actual snow depth was the existence of several ALS acquisitions in this region since 2001. However, deviations between ALS surface elevation changes and GPR derived snow depths have to be considered being predominantly in the present accumulation areas of the glaciers also under conditions of decreased ice flow. Hence, this study shows that only lidar measurements without ground-truth observations are not reliable to determine snow-depth variations for firn areas. We will include available measurements of ice flow in the discussion of the calculated deviations on the investigated glaciers and in the introduction of the study site.

Comment (iii): Comparing GPR and ALS on glaciers is an interesting and straightforward approach. However, the manuscript appears lengthy and contains a number of repetitions. I believe it can be significantly shortened and I made a number of suggestions below. In particular I want to encourage the authors to avoid discussions of numerous individual GPR profiles. Where possible shorten by making general statements, detail information could be subject to one or two additional tables.

Response: The whole section of the results and discussion will be reorganized and shortened in the revised manuscript.

Specific comments

Comment 1: Page 1790, line 6: This is general knowledge. I would suggest removing

the reference or replacing it with a more original reference.

Response: This paragraph will be drastically shortened.

Comment 2: Page 1790, line 10: "measured separately"

Response: Corrected to "analysed separately"

Comment 3: Page 1792, line 17: The calculated deviations between actual snow depths and? Δz_{ALS} were intersected with optical extra-terrestrial remote sensing data of a LANDSAT scene to differentiate between accumulation areas and ablation areas of the glaciers . I suggest shortening: "The calculated deviations between actual snow depths and Δz_{ALS} were intersected with accumulation and ablation areas derived from LANDSAT satellite imagery."

Response: Corrected

Comment 4: Page 1793, lines 19–20: Maybe the following would be clearer: "Mass balance measurements from Kesselwandferner using the direct glaciological method started in the hydrological year 1952/53 and geodetic mass balances are available from 1964."

Response: Corrected

Comment 5: Page 1795, lines 4–14: This is not entirely clear to me. Does a "section" reach from one black point to the next (Figure 7) or is it an entire profile? Maybe the average length of a section could be mentioned.

Response: Here section was mistakenly used for profile. Corrected. One section reaches from one black point to the next. This will be mentioned in the revised text. The average length of a section will be included in the revised Table 3.

Comment 6: Page 1795, lines 15-25: I assume the georeferencing was done the same way as above, or did you have a GPS connected to the GPR unit?

Response: Unfortunately the DGPS could not be connected directly to the GPR. Hence "First and last points of the individual sections were georeferenced by DGPS"

Comment 7: Page 1795, line 25: Do you refer to the wrong table here?

Response: Yes. Corrected

Comment 8: Page 1796, line 27 – page 1797, line 1: This is already described elsewhere in the manuscript. Maybe shorten?

Response: Sentence deleted.

Comment 9: Page 1798, line 7: ". . . signal velocities derived from GPR . . . " Do you mean ". . . signal velocities derived according to Kovacs et al. (1995) and measured densities . . . "?

Response: Corrected to "Signal velocities calculated from snow densities according to Kovacs et al. (1995) were compared to signal velocities derived from measured snow depths in the snow pits (Table 1)."

Comment 10: Page 1798, equation 8 : I would suggest using c instead of 0.3 and explaining the symbol in the text.

Response: Corrected

Comment 11: Page 1799, line 4–6: This is a bit puzzling here. You refer to quite narrowly defined "most frequent" slopes and snow depth but then mention a rather

broad range of corrections (0.01 m to 0.1 m) which, according to Figure 5, refers to almost the full spectrum of applied corrections.

Response: Corrected to "'Corrections, which therefore have to be applied, were mainly in the order of magnitude of 0.01 m."'

Comment 12: Page 1799, lines 10–12: Since you already mention that LiDAR is an active system, I would suggest removing the sentence LiDAR measurements do not require any external light source". Also you do not use TLS in this study but here you mention it the second time. I would suggest removing the sentence at least here.

Response: Revised and shortened.

Comment 13: Page 1800–1801, Section 2.4: You mention only briefly that you expect the firn areas to be an indication where submergence flow is present. However, in the remainder of the paper you strongly focus on the firn areas as the regions where you believe submergence flow is responsible for the differences between GPR and ALS. Firn densification receives comparably little attention. Then again, I do have a more general concern with respect to firn areas used as a proxy for ice dynamics: Is it correct to assume that a temporary ELA – not a longer-term ELA – can be used to delineate submergence and emergence flow? Are ice dynamics not the result of topography, longer-term pattern of mean mass balance distribution and a complex set of glacier properties (e.g. bed properties)? In the present study there seems to be a reasonable agreement between the autumn 2009 firn extent and deviations between the GPR and ALS measurements. However, to my opinion the authors do not provide satisfying evidence that these can be attributed to submergence flow.

Response: The change of the surface elevation in typical firn areas on glaciers includes both processes: the vertical component of ice flow and the densification of firn layers. In our view, a longer-term ELA is not necessarily an indicator of the transition of submergence flow or emergence flow. Processes of submergence are very likely

in the central parts of extended firn areas. In the authors' view a second advantage of using firn areas to delineate areas of certain submergence by Landsat is the transferability to glaciers, where no long-term mass balance measurements and hence no ELA information are available.

Comment 14: Page 1800, line 20 to page 1801, line 6: This is a bit unclear. Maybe first state that you expect firn densification for firn areas and that you consequently need to delineate them. LANDSAT is chosen as the mean to do that. A second question related to LANDSAT. how did you deal with the scan line error? Why does the August 2009 scene provide a minimum estimate for the ELA? Please briefly explain.

Response: This section will be revised. The Landsat data used here are from a Landsat 4-5 image, which has no scan line error included. The sentence concerning the minimum estimate of ELA was deleted.

Comment 15: Page 1801, lines 9,10: Incomplete sentence.

Response: Corrected

Comment 16: Page 1801,1802, Section 3: maybe this could be shortened by listing the snow conditions of the different campaigns in a table?

Response: Will be considered in the revisions.

Comment 17: Page 1802, lines 17–19: This is already mentioned elsewhere.

Response: Sentence deleted.

Comment 18: Page 1803, line 24: Figs. 6 and 7.

Response: Corrected

Comment 19: Section 4: I would suggest shortening the discussions of individual profiles, also because the results are often similar on the different observed glaciers. Instead I would prefer a more in-depth analysis/discussion of the influence of glacier dynamics.

Response: In the revised manuscript this will be considered.

Comment 20: Page 1804, line 26: Do you mean profile G1 here? Or is the label GP missing in Figure 7? See also page 1808, line 16.

Response: Corrected to G1.

Comment 21: Page 1805, line 5: I am not sure whether "significance" is the right word to be used here.

Response: Corrected to "Overall the distinction between firn covered areas and ice areas highlights the varying influence of the deviations between Δz_{ALS} and "ground truth" snow depths (here h_{GPR}) for different areas of the glaciers."

Comment 22: Page 1806, line 2: Do you refer to snow fall events and subsequent melt prior to the campaigns? If yes, I would suggest being more specific.

Response: The whole section of the results and discussion will be reorganized and shortened in the revised manuscript. Hence this statement will be revised, too.

Comment 23: Page 1807, lines 3–13: It is not fully clear to me which snow-probings were used for calibration according to equations 5 and 6 and which were used for validation.

Response: According to equations 5 and 6 snow pit data were used only. Hence snow probings performed at the beginning and the end of the GPR sections were used to validate h_{GPR}.

Comment 24: Page 1808, lines 3–13: Do you average over all measurements on all glaciers? Why is Vernagtferner mentioned in the following?

Response: The locations of measurements treated first, namely Kesselwandferner and Gepatschferner, were included in the text.

Comment 25: Page 1809, lines 7 to 15: To a certain degree this is true. But only because these glaciers nowadays show very limited ice dynamics. Elsewhere the same approach could result in large errors when no attempt is made to address the impact of glacier dynamics. Thereby I do not only refer to maritime areas where glaciers are subject to large mass-turnover but also to areas in the Alps where accumulation areas are located at higher elevations and glaciers remained dynamically more active.

Response: We tried to clarify that the findings are valid for the glaciers in the investigated region. We agree, that increased glacier dynamics will cause increased systematic deviations in Δz_{ALS} from actual snow depth.

Comment 26: Figures: The figures are of good graphical quality. I would nevertheless suggest enlarging the fonts in the figures 3, 4, 6, 7, 8, 9, 11, and 12.

Response: Fonts will be enlarged.

References

Abermann, J., Schneider, H., and Lambrecht, A.: Analysis of surface elevation changes on Kesselwand glacier - comparison of different methods, Zeitschrift für Gletscherkunde und Glazialgeologie, 41, 147 – 167, 2007.

Kovacs, A., Gow, A. J., and Morey, R. M.: The in-situ dielectric constant of polar firn revisited, Cold Regions Science and Technology, 23, 245 – 256, doi:10.1016/0165-232X(94)00016-Q, 1995.

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