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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.

Anonymous Referee #2

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General Statement

In this paper the results of a coordinated measurement effort are presented: Ground Penetrating Radar (GPR), Airborne Laser Scanning (ALS) and traditional snow probing and snow pits have been applied to evaluate the impact of glacier dynamics and snow densification on the accuracy of snow thickness measurements from multitemporal ALS. The authors use a comprehensive data set to investigate the accuracy of multi-temporal ALS for snow cover mapping. Furthermore the study contains figures of high graphical quality. I would nevertheless suggest revising the study for the following three reasons:

(i) The title is somewhat misleading. One would expect a detailed and quantitative

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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 ALS-GPR could be really the effect of ice dynamics.

(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.

(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.

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Detailed Suggestions:

1. Page 1790, line 6: This is general knowledge. I would suggest removing the reference or replacing it with a more original reference.

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

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."

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."

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.

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?

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

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

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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 ..."?

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

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.

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.

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.

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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.

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

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

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

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

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.

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.

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

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.

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.

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

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.

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.

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