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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 #1

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Review of Helfricht et al. 'LiDAR snow cover studies on glacier surface: significance of snow- and ice dynamical processes'. The Cryosphere Discuss. 7. 1787-1832, 2013.

Summary

Helfricht and co-authors present a comprehensive study of airborne laser scanning-derived surface elevation changes of four glaciers in the Otzal Alps, Austria, with corresponding field measurements of snow depth and density from ground penetrating radar, snow probing and pits. The study presents some novel datasets and has clear implications for end-users interested in the application of airborne laser altimetry data to estimating water storage within seasonal snow cover. The authors correctly locate their surface measurements within the context of glaciological theory in an attempt to highlight the significance of processes such as densification, ablation and ice flow on

their observed patterns of surface elevation change. However, the manuscript suffers in this respect as it instead serves to highlight both the authors lack of process-based measurements and the insufficient measurement accuracy of the techniques being used to infer these processes. With the data presented here, it is simply not possible to assess the 'significance' of snow and ice dynamical processes contributing to observed surface elevation changes. By 'significance' I take the authors to mean statistical significance, and thus (in this context) meaningful, quantitative process-based information.

Despite this criticism, I do not neccesarily think that the manuscript should not be published in The Cryosphere. The datasets alone are unique enough to warrant publication, and (as mentioned above) the paper will be of interest to those mapping seasonal snow cover and spatial distribution of accumulation. In order to warrant publication, however, I would recommend that the discussion of the 'significance of snow and ice dynamical processes' be toned down in the main text, and certainly removed from the title. A more suitable manuscript title would refer instead to lidar mapping of the spatial distribution of snow accumulation. Removing this emphasis on snow and ice dynamical processes will also allow some sections to be removed in the introduction and the paper to be shortened in general. I have several general comments and numerous specific comments and minor edits that I would like to see addressed in full in a revised manuscript.

General comments

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, 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. I refer to particular instances of this problem below in my specific comments.

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 organised sub-sections, examining in turn each set of results and each discussion point.

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 but I thought this deemed a general comment. Proof reading a couple of times probably would've caught most of these issues.

Specific comments

- 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.

- Lidar snow cover studies on glacier surfaces, surely? Plural, as you investigate more than one? However, this title should be revised, as the 'significance of snow and ice dynamical processes' is not really justified in this work (see comments above).

- 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.

- 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)?

- 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 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.

- 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.

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

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

- 1790, line 11: what ‘additional information’?

- 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.

- 1790, line 23: I see little use throughout this entire manuscript of the ‘example reference’ (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..).

- 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.

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

- 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 δh.

- 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’. 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).

- 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.

- 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.

- 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?

- 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).

- 1800, line 12: What interpolation scheme was used and how did that choice affect

resultant measurement accuracy?

- 1804, line 1: ‘supposed’, but unknown. Speculative. It could be measurement error, or something else?

- 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.

Minor comments / text edits

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

- page 1789, line 9: sources, not resources.

- 1789, line 12: subject to flood forecasting? Do you mean the subject of flood forecasting? Or something else? I don’t follow.

- 1789, line 20: glacier surfaces. And observed by who?

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

- 1790, line 13: operate at the point scale only.

- 1790, line 14: satellite remote sensing, not ‘extraterrestrial remote sensing’. Here are elsewhere, e.g. 1792, line 18.

- 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).

- 1790, line 21: Lidar delivers georeferenced surface. . .

- 1790, line 24: either ‘was applied by Author x (year)’, or ‘has been applied’ (e.g. Author x, year).

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

- 1791, line 3: delete ‘so-called’

- 1792, lines 4-5: Why does this sentence have its own seperate paragraph?

- 1793, line 13: not sure ‘were evolved’ in the correct wording here.

- 1793, line 20: a stake network has been surveyed..

- 1794, line 1: models were calibrated (not was)

- 1794, line 11: Redundant sentence.

- 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?

- 1796, line 5: Snow depth probing and pits were used to identify.. - 1797, line 15: measured vertically.

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

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

- 1802, line 9: taken alongside

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