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The Cryosphere
Discussions

Interactive comment on "A reanalysis of one decade of the mass balance series on Hintereisferner, Ötztal Alps, Austria: a detailed view into annual geodetic and glaciological observations" *by* Christoph Klug et al.

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Christoph Klug and colleagues present a detailed reanalysis of annual glaciological and annual geodetic balances at Hintereisferner, Austria, obtained between 2001 and 2011. This study puts an airborne laser scanner (ALS) dataset with exceptional spatial and temporal resolution over an entire decade at its full value. The comparison of these geodetic results with the glaciological balances from an extensive in-situ network have been long overdue but are now carried out in a very thorough way and including an error assessment according to best international practises.

Hence, I can recommend the paper for publication in The Cryosphere after consideration of the following two substantial points and a list of suggestions for minor corrections and clarifications:

(a) DTM-related random uncertainty of geodetic balances: The authors use the standard deviation of the DTM-differencing over selected stable terrain as random uncertainty for the geodetic balance (cf. equation 3, lines 196-207). I do not agree with this approach because it assigns a local DTM error to a zonal glacier change value. The standard deviation of the elevation differences on stable terrain indicates the uncertainty of the DTM differences for individual pixels. Instead, I propose to use the standard error, defined as the standard deviation divided by the square root of the number of independent items of information in the sample (cf. Zemp et al. 2013, The Cryosphere, Section 2.3). In the present case of ALS (> 1 point per m2) it can probably be assumed that the number of independent items is about the number of glacier pixels (cf. Joerg et al. 2012, RSE). Note that there is also the implicit assumption that the DTM uncertainty over stable terrain is representative for the DTM uncertainty over the glacier (cf. Rolstad et al. 2009, J. Glaciol.). Maybe that needs just to be mentioned somewhere in the paper.

(b) Geodetic method as substitution for the glaciological method: The authors conclude that the geodetic method (i) "can represent a valuable possibility to overcome shortcoming in the glaciological measurements even on an annual scale" (Lines 469-470) or (ii) "even as a substitute for the glaciological method". I can only partly support these conclusions for three reasons: (1) the geodetic and the glaciological methods are rather complementary in nature (than to substitute each other): the strength of the glaciological method is to capture the spatial and temporal variability of the glacier surface balance even with only a small sample of observation points but it is sensitive to systematic errors which accumulate linearly with the number of seasonal or annual measurements. The geodetic balance is able to cover the entire glacier but requires a density conversion, which becomes more challenging over short time periods because of meteorological influences on the elevation change. (2) the nature of uncertainties: typically, ten years of data are required for the detectable difference to become lower than the annual random “noise” of the glaciological balance (cf. Zemp et al. 2013, The Cryosphere). A validation at annual time intervals might actually miss a bias. (3) cost-benefit considerations: the costs of the geodetic method are one to two orders of magnitudes higher than the costs of the glaciological method.

I suggest adding a short section that discusses these issues and rewording the corresponding conclusions.

(c) List of suggestions for minor corrections and clarifications:

Page 3, Line 67, “first use of annual geodetic records”: At South Cascade Glacier, annual results from both geodetic and glaciological methods have been analysed by Krimmel (1999): Robert M. Krimmel (1999) Analysis of difference between direct and geodetic mass balance measurements at south cascade glacier, washington, Geografiska Annaler: Series A, Physical Geography, 81:4, 653-658.

P4, L119, “Results are submitted to the WGMS…”: you could add a reference to WGMS (2017, and earlier reports): WGMS 2017. Global Glacier Change Bulletin No. 2 (2014–2015). Zemp, M., Nussbaumer, S. U., Gärtner- Roer, I., Huber, J., Machguth, H., Paul, F., and Hoelzle, M. (eds.), ICSU(WDS)/IUGG(IACS)/UNEP/UNESCO/WMO, World Glacier Monitoring Service, Zurich, Switzerland.

P6, L178-184, Equation 2: the geodetic balance is usually calculated using the average glacier area of the two surveys (cf. Zemp et al. 2013, The Cryosphere, Eq. (5) and (6)). At annual time steps, this might not make a big difference, but for the decadal period with a surface area reduction of 15% it does become relevant.

P6, L188 & Fig 1, stable areas: I fully support the decision to complement the down-valley soccer field with stable areas near the glacier. Please add a short comment about the selection criteria for the stable areas A-E.

P8/9, L240-267, density conversion: the density conversion factor depends on changes in the three-dimensional firn body and is a function of (i) the additional snow layer incl. related densification and metamorphosis, (ii) firn compaction and metamorphosis, and (iii) sub/emergence velocity. From the text, I cannot fully comprehend how these factors are covered (or not) by the author’s approach combining differential DTMs, surface classifications, and density assumptions. Please clarify and discuss the opportunities and limitations of the used approached.

P9, L266-267 & Table 5, density conversion factor and related uncertainties: for a non-expert it is hard to follow how the density conversion factor and corresponding random uncertainties (together with the annual balance) relate to K.sigma and K.epsilon in Table 5. Adding a corresponding equation in Section 4.2 might help.

P9, L271, “stratigraphic year”: I think this should be “end of the hydrological year” or “fixed date system” (cf. P9, L275, “30th September”).

P10, L285-287, “elevation dependent mean ablation gradient”: do you use the same gradient for the ablation and the accumulation zone? Please clarify.

P11, L323-324: for comparability, convert the values by Thibert et al. (2008) to annual change rates.

P15, L448-251, “were the first and so far only”: consider rewording in view of earlier studies at South Cascade by Krimmel (1999, Geogr. Ann.).

Text, Figs & Tabs, “altitude” versus “elevation”: in most cases, you could replace “altitude” by “elevation” (cf. McVicar, T. R., & Körner, C. (2013). On the use of elevation, altitude, and height in the ecological and climatological literature. Oecologia, 171(2), 335-337.)

P24, Fig. 1: For clarification, you could write in the figure caption: “Note that in 2003, no accumulation measurements COULD have been carried out DUE TO THE STRONGLY REDUCED ACCUMULATION ZONE. HENCE, only ablation stakes were available.”

P25&30, Fig. 2 & 7: the two figures are redundant to a certain degree. On the other side, it is not fully clear, which differences and uncertainties are included. Please at least clarify in captions. In addition, you could consider merging Fig 2 & 7, showing bias corrections for both glaciological and geodetic results. Instead, you could remove the cumulative curves (=> shown in Fig. 8).

P26. Fig. 3: I would add a bar showing the intensity range (values) to the legend of the left image. In the legend of the right one, I would replace “perennial firn” by “snow and firn”.

P27, Fig. 4: In the caption, please clarify what you mean with “Corrected”. It might be sufficient adding a reference to the corresponding section in the paper. I would add the terms glaciolocial and geodetic to the label of the x-axis in the left and right figure, respectively. In addition, please add a note on the effect of the sub/emergence velocity.

P28, Fig. 5: you could add the data point(s) for the full period (glaciol.cum versus geod.cum, glaciol.cum versus geod.01/11).

P29, Fig. 6: please add a note on the effect of the sub/emergence velocity.

P31, Fig. 8: typically, one would calibrate the glaciological with the geodetic over the decadal period (i.e. 2001-11). Hence, it might be good to show that result here too.

P34, Tab. 2: you could add a column for the two dDTM of the full period, i.e. 01/11.

P35, Tab 3: please explain why the density given in the cation (900 kg m-3) differs with the one mentioned in the text (850 kg m-3, cf. P8, L249)

P36, Tab 4: in the caption, there are some problems with the symbol for average SC. What is the “mean acc. area"? Do you refer to the end-of-summer accumulation area?

P37, Tab 5: I would expect the annual uncertainties for the density conversion (sigma K) to be larger than for the (zonal) ones for the ALS-DTM (sigma DTM)... see also my comments above (substantial point (a) and comment related to density conversion, P9,

L266-267).

P38, Tab 6, caption: consider rewording “improved balance” into “bias-corrected balances”; consider rewording “statistical significance“ by “reduced discrepancy”. Use the same symbol for the common variance in caption (now wrongly epsilon.comvar) and table (=> sigma.comvar).

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