A reanalysis of one decade of the mass balance series on Hintereisferner, Ötztal Alps, Austria: a detailed view into annual geodetic and glaciological observations

Author's reply to referee comments#1

Christoph Klug and Co-authors
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Introductory Remarks

We thank Michael Zemp for reviewing our manuscript and the thoughtful and constructive comments. Our response letter is structured as follows. Section 1 provides detailed answers to general concerns raised by the referee, whereas section 2 offers a point by point response to the specific remarks.
To facilitate readability, the referee's comments are given in *grey italics* while our responses are in blue regular font.

1. Reply to general referee-comments #1 by M. Zemp

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:

GENERAL COMMENTS

1. ***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.*

We followed the suggestions of the reviewer for this point. We therefore calculated the standard deviation and divided it by the square root or the number of independent items. This of course leads to a significantly lower standard error. We of course assume comparable DTM uncertainties over the whole DTM, which therefore do not differ between stable areas and glaciated terrain. We also stated this more clearly in section 3.2.

2. *<u>Geodetic method as substitution for the glaciological method:</u>*

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.

We agree with the reviewer and changed the manuscript accordingly, especially regarding the wording and the complementary nature of the two methods. We tried to elaborate more comprehensively why a reanalysis based on geodetic data is needed for HEFs glaciological mass balance record. We also agree that the strength of the glaciological method is the ability to capture spatial and temporal (year to year) variability of surface mass balance and to extract the part of mass change which is a consequence of meteorological forcing. However, this is only given if the analyses follow a certain quality standard. In terms of unexplainable differences between the methods, a thorough uncertainty assessment has to be conducted in order to indicate that available glaciological balances are questionable and geodetic data can help in improving shortcoming in the glaciological measurements.

(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.

This is correct and the reason we included the comparison for the ten-year period. The annual uncertainties are not suitable for bias detection, but the entire period of our investigation is. We have highlighted and discussed this issue in the revised manuscript. The annual uncertainties are only used to identify years which differ significantly between the two methods.

(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.

Regarding the cost-benefits, of course those depend on the individual investigated glaciers and the methodological set-up. In the case of Hintereisferner the cost of an ALS-campaign is about 50 to 100% higher than the budget for the labour intense direct measurements which (if done properly) require highly qualified stuff. However, this point is discussed more critically in the revised manuscript.

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

Done.

2. Reply to specific referee-comments #1 by M. Zemp

MINOR CORRECTIONS AND CLARIFICATIONS

Page3, Line67:

"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.

The authors meant the first use of annual ALS data. However "first" was cleared.

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.

Done. Reference was implemented.

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.

We agree. Method of calculation for decadal period was adapted to using the time averaged area mean $(S_{t1}+S_{t2})/2$, as recommended in Zemp et al., 2013.

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.

Visual inspection and expert knowledge of the terrain (Sailer et al., 2012, 2013; Bollmann et al., 2011). Comment was added.

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.

The conducted density conversion consists of three steps within our approach. First, the dDTM was calculated. In a next step, the glacier surface was classified into two classes (firn and ice) by using the intensity images of the ALS campaign, resulting in surface grids for each year. By subtracting the classified intensity rasters and reclassifying the resulting new surface raster, we incorporated the changing extent of the perennial firn zones in a third step. This should answer point (ii) raised by the reviewer. However, we are aware that firn compaction and metamorphosis are not covered by this approach.

Point (iii) could not really be considered using the available data, which is why we already mentioned in the introduction that we will not incorporate glacier flow dynamics in the presented analysis.

Regarding point (i), the snow layer was incorporated by combining a maximum snow height at the time of measurement with in-situ measured snow-densities, to redistribute the mass according to the snow layer to the glacier surface. Nevertheless, we are aware that this type of spatial distributed density conversion is rather a best guess than a three-dimensional modelling of the firn body.

In the revised manuscript we tried to clarify and discuss our way of density conversion in a more comprehensive way and added a workflow chart (see Figure 1), which helps to better follow the steps within our analysis.

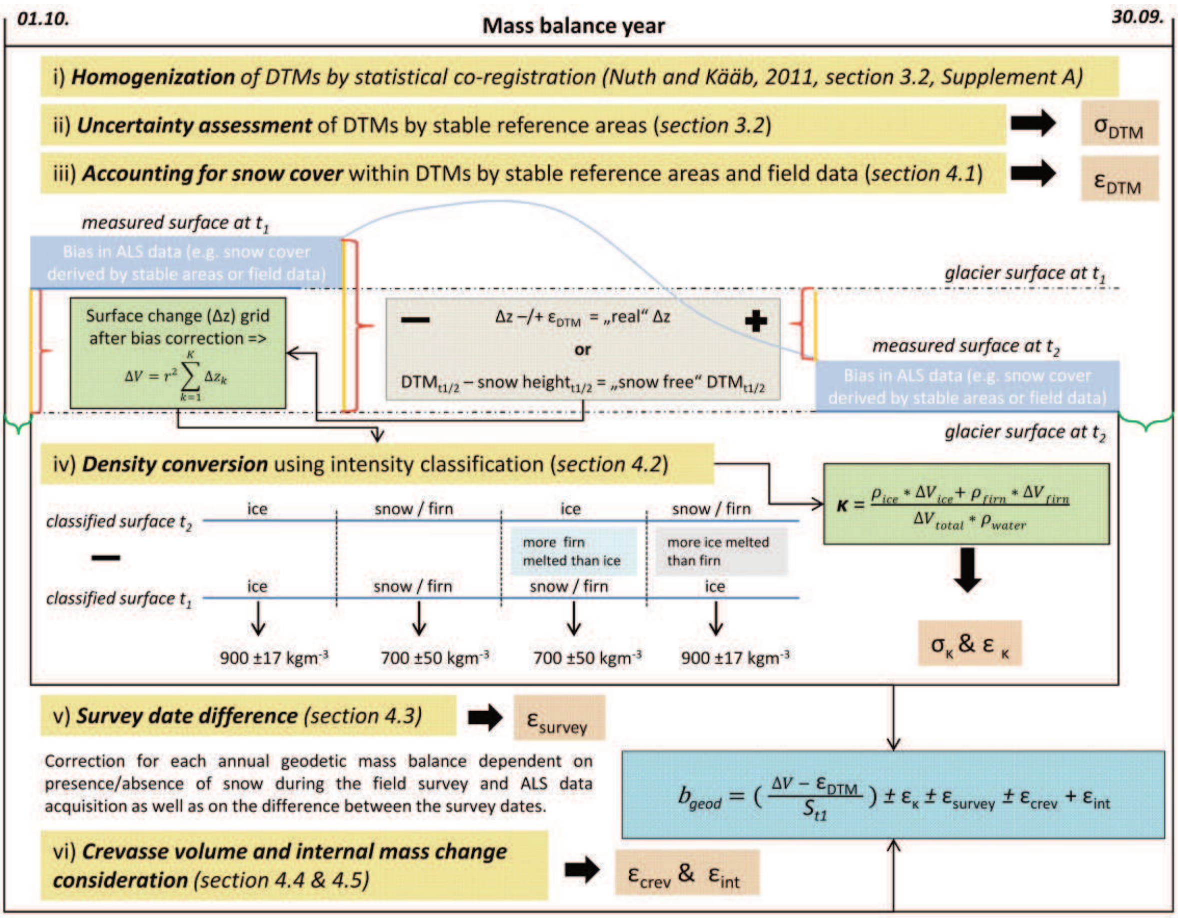

Figure 1: Workflow chart.

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.

We agree with the referee and added a corresponding equation.

P9, L271:

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

Changed accordingly to "end of hydrological" year

P10, L285-287:

"elevation dependent mean ablation gradient": do you use the same gradient for the ablation and the accumulation zone? Please clarify.

The ablation gradient is derived from in situ stake readings (L 278). The gradient is applied to correct the survey date difference between geodetic and glaciological survey in the years 2003 and 2008 (L 285-287). Table 4 shows that in 2003 (survey date correction 4 days) and 2008

(21 days) snow cover was present, varying with altitude. Thus, the ablation at each individual stake is a bulk information consisting of snow and ice ablation. We average the stake ablation per 100 m elevation zone and hence derive the elevation dependent ablation gradient along the elevation range of the glacier.
To avoid confusion with the word *mean*, we deleted *mean* (L 287) as it should refer to the mean of the elevation zone and not to a mean value all over the glacier.

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

P15, L448-451:
"were the first and so far only": consider rewording in view of earlier studies at South Cascade by Krimmel (1999, Geogr. Ann.).
Done.

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.)
Done.

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."
Done.

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).
We agree with the referee and will merge the two figures in the revised manuscript. Furthermore, we clarified in captions which uncertainties are included and removed the cumulative curves.

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".

Done

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 glaciological 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.

Done

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).

Done

P29, Fig. 6:

Please add a note on the effect of the sub/emergence velocity.

The following note on the influence of glacier dynamics was added:

Due to ice dynamics, an underestimation of the geodetic versus the glaciological mass balance is expected in the accumulation area (and vice versa in the ablation area). However, the surface height change due to the flux divergence is at least one order of magnitude smaller than the values presented here.

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.

Done

P34, Tab. 2:

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

Done

P35, Tab 3:

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

It's a typo and was clarified!

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?

Symbol problems have been revised. Mean accumulation area is the classified firn area (A_F). To avoid ambiguity it was changed.

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).

Since we changed the calculation of the errors for the ALS-DTM (sigma DTM), those are now lower than in the originally submitted manuscript.

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).

Done