# 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

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**Introductory Remarks**

We thank the anonymous Referee 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 #2 by Anonymous Referee

*Klug and co-authors compare annually-resolved geodetic (airborne laser altimetry) and traditional mass balance records of Hintereisferner Glacier over the period 2001-2010. They find that for most years these two methods estimate similar mass change for the glaciers (within uncertainties). They note discrepancy between the methods for three years and attribute these differences to errors in the traditional mass balance data.*
*I found this paper (and their experimental design) to be well thought out and mostly well described. It should be published. Globally, the number of traditional mass balance records relative to the number of existing glaciers is vanishingly small. We thus require alternative approaches that could complement conventional methods of measuring surface mass balance. Klug and co-authors make a convincing case that estimating mass balance using geodetic techniques is appropriate and in some cases can identify errors in the traditional series. This is especially important when one uses these series for calibration of models (hydrologic and mass balance).*
*I would recommend that the paper be considered for publication, but only after the authors make a number of substantive changes. Most of these suggestions are minor, but several will require some thought and future analysis. Below, I expand on the major points the authors should address. In several places I found the writing to be muddled and fraught with logic gaps and grammatical/typographical errors that might arise if English was not the first language of the senior author. I would encourage all authors spend time going through the revised manuscript to ensure its presentation is up to the standards required for the journal.*

**GENERAL COMMENTS**

***A)*** *Streamline introduction - I found the introduction of the paper to be too long and lack appropriate focus for what comes next. While many of the points brought up in the paper are important, they have already been stated in many previous papers. The point (I think) is to see how well geodetic and traditional mass balance methods compare over a suitably long period of time (decade). Perhaps focus on the point that analysis over shorter intervals may miss important processes that reveal themselves for longer periods. At the top of page three we first learn where the paper is going. Please state your objectives earlier and reduce introduction by about 50%. A reader should know at about page 1.5 where we are heading.*

We widely agree with the argumentation of the referee. The revised introduction is significantly shorter than the original one. We changed the introduction section with the aim of clearly showing the background, motivation and starting point of the paper. This was reached by omitting passages containing information which is common knowledge within the community or which is not relevant for the reader in this part of the paper. Thereby the main objectives of the study are presented earlier in the paper. We also tried to sharpen the motivation of the paper by pointing out the research focus more clearly.

***B)*** *Reorganization required – I appreciate the detailed attention that the authors pay to processes that could make traditional and geodetic methods differ, but the current organization of these sections comes after key equations used to convert volume change into mass (w.e.) change. You really should present sections (4.1, 4.2 . . .) before you present equations (1) and (2). This is especially evident when one reads section 4.2 and then needs to consider whether equation (5) really differs from equation (2) – it doesn't really. This change would make your paper easier to read (certainly more logical).*

Indeed the structure of the paper was a point of long discussions between the authors. We also agree that the organization of the chapters still provides some challenges for the reader. Nevertheless, after detailed discussions and thorough evaluation of the reviewer's suggestion, we prefer to keep the structure of the manuscript unchanged due to the following reason
In section 4 the inherent differences between two methods are addressed in detail. It is impossible to start such a discussion before the two methods are introduced.

***C)*** *Spatial noise – On page 7 the authors discuss using SDz from stable control area to define spatial variability, but I don't understand how this would yield that information. These control patches serve as so-called 'check points' used in traditional photogrammetry. What would they tell us about spatial variability and how it might affect their results? Not much I'm afraid. What would yield that information, however, is the decorrelation length inherent in their data. The authors have gridded data where they can correct their sample sizes for* *spatial autocorrelation. You should assess the degree of spatial correlation of your data and reduce number of independent samples accordingly. There are several key papers on this topic, one of them (Rolstad et al., 2009) is cited below.*

We did not intend to show the spatial variability of the DTM errors, but to give a measure on the overall DTM accuracy affecting the geodetic mass balances. Since the errors are quite low and do not show large spatial variation within the DTM, this was deemed a comprehensible approach. In our case of ALS (> 1 point per m2) it can be assumed that the number of independent items is about the number of glacier pixels (cf. Joerg et al. 2012). However, since both reviewers criticized this, we changed the way in which we calculate the random error of the used DTM. This also leads to a much lower random error in the DTM.
Although we are aware 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.), we did not correct our sample sizes for spatial autocorrelation, but added a better discussion of this issue to the revised manuscript.

***D)*** *Dimensionless conversion factor K - I have a few problems with the introduction of this variable (K) into the literature. First, this is something that is routinely applied in sequential DEM differencing in many previous studies even though it isn't always stated as such. Second, unless I've missed something K should range between 0-1 yet it is state as ranging between 820-930 (line 267). Third, on lines 386-387 the authors state that their new dimensionless conversion factor K now has units of kg m-3. Many have used this conversion factor in past studies; it's not new, so please let's not re-invent the wheel and muddle the literature with new dimensionless numbers.*

There seems to be a misunderstanding. We did not want to introduce this factor to the scientific community, only to our paper. By giving units of kg m-3 we tried to make the factor more comparable.
We are well aware of the fact that this is not a novel approach and therefore we added some respective references to the revised manuscript to clarify this. Furthermore, we adapted the manuscript in order to assure a consistent use of the dimensionless K throughout the paper.

***E)*** *Clearer discussion needed for explaining discrepancies - One of the major conclusions of this paper is that based on the geodetic balance calculations the authors feel that the years 2002/3, 2005/6, and 2006/7 are biased in the traditional mass balance data. I think they are trying to state that the glacier lost most of its accumulation area and the bias was caused by having no stakes high up on the glaciers (in this case probing and pits would yield nothing). This point isn't as clear as it need to be in lines 410-444; they need to shorten this section, explicitly implicate the methodological factors that could account for the error and then* *implicate meteorological factors. As it stands they start with the latter without a clear discussion of the former.*

We agree with the referee. In the revised manuscript we attribute the differences between the mass balance methods more clearly to an insufficient measurement set-up and missing observations in the former accumulation area. Furthermore we shortened and restructured the section which results in a more logic streamflow of discussion which is easier to follow for the reader.

***F)*** *Avoid overly bold statements - A minor point, but it is best to avoid absolute statements in papers. The authors suggest that their study is the first to compare annually resolved geodetic and traditional mass balance records, yet a quick literature search indicates that this isn't correct. For example, Beedle et al, 2014 did this for a shorter period of record and Krimmel (1999) did this for a longer period of time. You should either modify your statements to reflect that your comparison exceeds those of other studies or simply drop statements like this. My preference would be to do the latter.*

Beedle et al., 2014 applied photogrammetry based geodetic mass balances for three years while Krimmel et al., 1999 used digitized maps and photogrammetry over multi-year periods. To our knowledge our study presents the first approach based on annual high resolution ALS DEMs over the period of one decade.. However we omitted the word“first” and also dropped down similar statements throughout the paper. Furthermore we explained the innovative aspect of our study more clearly in the revised manuscript.

## 2. Reply to specific referee-comments #2 by an Anonymous Referee

**SPECIFIC COMMENTS** (editorial and of minor/moderate substance)

**Title:** *A clunky title. I'd suggest. 'Geodetically corrected (or Homogenized) mass balance series of Hintereisferner Glacier, Austria for the period 2001-2011'*
In the revised manuscript the title has been changed

**Line 14:** *First sentence needs to be reworded. It sounds like you obtained 2001-11 mass balance(s) records.*
Done.

**Line 18:** Sentence needs revision (grammatically incorrect)
Done

**Line 23:** Replace 'as a substitute for' with 'superior to'
Done.

**Line 39:** Delete 'and within the snow' since the top of this layer defined glacier surface by definition.
Done.

**Line 40:** Replace 'subtracts' with 'differences'
Done.

**Line 45:** Full stop missing after 'glacier'
Done.

**Line 50:** *Beedle et al., (2014) is missing from this list*

We added this reference to the revised manuscript.

**Line 63-66:** Confusing and poorly worded sentences. Please revise.
Done.

**Line 67:** See major comment (F)
Done.

**Line 86:** Add 'an average' after 'with' and strike 'in average'
Done.

**Line 86:** What is a ‘totalizing rain gauge’ - bulk collector?
We use the term *totalizer rain gauge* as it is defined by the meteorology glossary from the American Meteorological Society: http://glossary.ametsoc.org/wiki/Totalizer_rain_gauge

**Lines 90-94:** Tangential to paper’s focus (delete).
Done.

**Lines 100-101:** Add ‘Annual’ at start of sentence, strike ‘annual mass balance’ and ‘have been started’ and replace with ‘commenced’ and strike ‘are carried out regularly since then’
Done.

**Line 129:** Strike ‘among others’ - meaningless in its usage here.
Done.

**Line 132:** ‘Further explanation. - Unclear why this statement is here. Reads like an orphaned one.
The respective sentence was deleted.

**Line 143:** replace ‘wrong’ with ‘incorrect’
Done.

**Line 148:** ‘For extrapolating ... ‘ - This sentence is linked to nothing (a single thread). Not sure why it is here.
The respective sentence was deleted.

**Line 154:** Replace ‘according to the law of error propagation’ with ‘by error propagation’ - There are few physical laws.
Done.

**Line 180:** I had commented in the paper margin ‘are density differences treated per elevation band’ and hence my suggestion for you to move sections 4.1 and 4.2 before equation (1). See major point (B).
We kindly refer to our detailed response in part (B) of the general comments.

**Lines 198-200:** See major point (C).
We kindly refer to our detailed response in part (C) of the general comments.

**Lines 212 (and throughout paper):** Try not to state things like ‘Figure 2 shows. . .’. State trend, observation and refer to figure at end of the sentence. For example, ‘Density increases

with elevation (Figure 2)'. This allows reader to digest your point and then refer to figure (it also reduces verbiage).
We avoid such throughout the revised manuscript.

**Line 220:** Move 'significantly' before 'influence'
Done.

**Line 228:** Add 'absolute' before 'vertical' and strike vertical lines as they are impossible to see in running text.
Done.

**Line 231:** Strike 'very' and avoid this vague qualifier at all costs.
Done.

**Line 265:** See point (D).
We refer to our response in part (D) of the general comments.

**Line 271:** Replace 'a multi methodical approach was applied incorporating' with 'we incorporated'
Done.

**Line 276:** remove (s) from extrapolations
Done.

**Line 280-281:** How does this standard lapse rate compare to one assessed with station data. Does this help to explain differences in the extreme melt years?

In the case of significant snow fall we assume the atmosphere to be saturated. Furthermore the analysis of station data shows that the use of the moist adiabatic gradient in such cases is quite a reasonable assumption. However, the large differences between the two mass balance methods are not sensitive to the choice of this lapse rates which hence do not significantly contribute to explaining those differences. We added a statement discussing this issue to the revised manuscript.

**Line 291:** Replace '5' with five. Write out all numbers less than 10 unless number has a unit. For example, seven stakes but 3 cm.
Done.

**Lines 305-212:** So how does this approach potentially affect your results? So if you simply ignored effects of crevasses what would results show?

This approach does not affect our results significantly; it is only included to show that the effects of crevasses are negligible to our results. We expanded section 4.4 to explicitly highlight this.

**Line 326:** Stylistic point, but ‘frictional dissipation’ I believe is the more precise term.
Done.

**Line 348:** Write out ‘E.g. - Never start a sentence with this.
Done.

**Line 353:** If it’s a small term at the annual scale, it’s small and within error at decadal scale. It can’t be significant for one but not the other. Suggest dropping last clause in sentence.
Done.

**Line 357:** ‘The 2001 to 2011 one step. . .’ - Not sure what sentence is trying to state.
The sentence was revised for clarity.

**Line 358-359:** See earlier comment about ‘Table and Figure shows. . .’
Done.

**Line 363:** What does ‘respectively’ refer to? The penultimate sentence? Revise.
The sentence was chanced for clarity.

**Line 370:** Sometimes last word before equation has a colon sometimes not, be consistent with journal standards.
Done.

**Line 371:** How does $\delta$ change if you incorporate effective degrees of freedom in the geodetic estimate of uncertainty (i.e. correct for spatial autocorrelation)?
We kindly refer to our detailed response in the general comments (part C).

**Line 376:** Replace ‘coherent’ with ‘similar’
Done.

**Line 378:** How is bias defined in this paper? Should be formally defined.
Done.

**Line 383:** Did you really explore the parameter space? This phrase is typically used with Monte Carlo sampling or Latin Hypercube sampling. Did you do that?
No we just explored the minimum and maximum random uncertainties. We added a clarifying statement to the revised manuscript.

**Line 388:** Why does K now have units? You told us earlier that it was dimensionless. . .
We only introduced the units to make it more comparable for the reader. This has been amended following the suggestions by the reviewer.

**Line 400:** If you used results that weren't smoothed (removal of crevasses) how does this affect your results?
As already shown in table 5, and section 4.4, the effect on the results is negligible. We added a reference to table 5 and section 4.4. to the respective paragraph.

**Lines 410-440:** I found this portion extremely difficult to follow for reasons outlined in major point (E).
We refer to our response in part (E) of the general comments.

**Line 448:** See point (F). Several papers out there that do this. Your paper, however, does this for the longest series, and will be well received. But please don't oversell its novelty.
We refer to our response in part (F) of the general comments.

**Line 452:** Change sentence to, 'It neither include(s) a through. . . nor '
Done.

**Line 453:** Remove 'ed' from showed.
Done.

**Line 456:** Change 'a snow cover' to 'snow'
Done.

***References:***
I did not check these for typos, but suggest you add the ones in this review to the list.
Done.

***Figures:***
**Fig. 2** - A legend added to this figure would help reader. It would be nice in the figure caption to state level of uncertainty (68, 95%).
Done.

**Fig. 3** - What are units of Intensity (DN?). kg $m^{-3}$?
Intensity has no units. The used intensity rasters only show the backscattered energy stored in 8 bit or 256 grey values.

**Fig. 4.** - You don't deal with dynamics (flux divergence) so it is not appropriate to plot these data as 'Mass balance [m w.e.]' as a function of elevation. $\frac{dh}/{dt}$ and $\b{dot}$ are not equal due to dynamics. This plot must be redrafted showing 'Elevation change [m w.e.] and not 'Mass balance'.

It is true that we do not explicitly resolve ice dynamics. Nevertheless, the change in surface elevation can be used to calculate a mass balance which is the result of accumulation/ablation processes (surface, internal, basal) and ice flux divergence. In principle it does not matter if this is done for a point/column, an elevation band or the whole glacier (e.g. Cogley et al., 2011, page 5). However, we think that comparisons of geodetic and direct balances are only problematic if not done on the glacier wide scale. Hence, we did not redraft the plot but added a statement discussing the effect of ice dynamics on the local mass balance and the implications on method-comparisons on scales others than "glacier-wide".

**Fig. 6.** Remove titles from figures and simply use 'a)' and 'b)' . Avoid excessive qualifiers. Change 'The extraordinary mass' to 'Mass'
Done.

**Fig. 9.** Is this really the best way to show these data? Why not simply remove the figure and tell reader in text if homogenized Hintereisferner series correlates more strongly (or use of other statistic than Pearson) with nearby series.
This figure supports what we discuss in lines 436 to 448 of the original manuscript and was hence kept in the paper. However, we added a clearer statement on correlation with other glaciers to the revised manuscript.

**Tables:**
There are a lot of them and not sure if they are all needed. Any individual wanting your data would request them, no? Alternatively you could deposit them with the WGMS or other agency (or include as electronic supplementary data). They take up a lot of journal space and some repeat what figures show.

While it is true that a lot of tables are included in the manuscript, we do think that they add important information. However, we decided to move table 4 from the manuscript to the supplementary data. If required, we will move more of the tables to the supplements, but we would prefer to keep them within the paper.

**Table 5.** Replace ‘cum’ with ‘Sum’.
Done.

**References:**

Beedle, Matthew J., Brian Menounos, and Roger Wheate. 2014. “An Evaluation of Mass-Balance Methods Applied to Castle Creek Glacier, British Columbia, Canada.” Journal of Glaciology 60 (220): 262–76.

Krimmel, Robert M. 1999. “Analysis of Difference between Direct and Geodetic Mass Balance Measurements at South Cascade Glacier, Washington.” Geografiska Annaler: Series A, Physical Geography 81 (4). Blackwell Publishers Ltd: 653–58.

Rolstad, C., T. Haug, and B. Denby. 2009. “Spatially Integrated Geodetic Glacier Mass Balance and Its Uncertainty Based on Geostatistical Analysis: Application to the.” Journal of Glaciology 55 (192). http://www.igsoc.org:8080/journal/55/192/j08j136.pdf.

Those references have been added.