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The Cryosphere Discussions

Interactive comment on "Comparison of direct and geodetic mass balances on an annual time scale" *by* A. Fischer et al.

Anonymous Referee #1

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General Comments:

This paper presents an important new dataset, and application, of remotely-derived glacier surface elevations. The study compares direct mass-balance measurements with annual geodetic mass-balance measurements made with Light Detection and Ranging (LiDAR) technology of two glaciers for the period 2001 to 2009. The two glaciers, Hintereisferner (HEF) and Kesselwandferner (KWF) in the Ötztal Alps of Austria, have long histories of research, and are two of the 30 glaciers used as global 'benchmark glaciers'. The nine resultant 1-m resolution digital elevation models (DEM) hold great promise for comparisons with direct mass-balance measurements, mass-balance error assessment, studies of emergence velocity, and contributing significantly to much previous work in these areas. However, in its current form, the paper fails

to appropriately address numerous salient issues, is difficult to read and understand, and neglects to reference many related studies. This paper, in its current form, is not ready for publication. Significant revisions need to be made, and I hope my specific comments, below, will help the authors in this process.

Specific Comments:

Terminology:

There are a number of issues with inconsistent and inappropriate terminology, which make the paper difficult to understand. I mention these at the outset in hopes that my following comments are clear and helpful.

In many instances it is unclear whether the paper is referring to specific balance (i.e. balance at a point) or average specific balance (i.e. average balance for a glacier). Mass-balance terminology varies widely; here I'm using the terms used in Cuffey and Paterson (2010, p. 94, 102). Regardless of which terms and associated symbols are used, they must be clarified and used consistently.

Emergence, submergence, or emergence velocity should be used where appropriate when discussing upward or downward flow of ice. Care needs to be taken so that the use of 'subsidence', for a reduction in surface height, is not confused with submergence.

The terms altitude and elevation are used interchangeably, where, in most cases, the authors are referring to elevation.

Accuracy of Direct versus Geodetic Mass-Balance Measurements:

In terms of average specific balance it appears that direct mass-balance measurements are assumed to be accurate and that, in comparison, the geodetic measurements are deemed to be in error. The accuracy (and precision) of the direct measurements needs to be addressed more completely, specifically with regard to spatial variability, and representativeness of the point measurements made at stakes and pits.

The geodetic method avoids these problems by sampling every square meter (in this case) of the glacier surface. Each methodology has inherent errors, and one method cannot necessarily be used to verify the other (e.g. Andreassen, 1999).

I am not convinced that the results for average specific balance point to errors in the geodetic method, but perhaps errors in the direct measurements. Much more work needs to be done to justify the accuracy of the direct measurements of average specific balance before concluding that high-resolution geodetic measurements should not be used for investigations of the controls of glacier mass balance.

With the objective of obtaining specific balance the paper concludes correctly that knowledge of emergence velocities is necessary in order to apply a geodetic methodology. However, this is not a new finding, and the paper would benefit from referencing past studies that have come to the same conclusion, or address this issue specifically (e.g. Hagen et al., 2005; and Gudmundsson and Bauder, 1999).

The paper finds the geodetic measurements of average specific balance to be more negative than those from direct measurements. Again, this is not a new finding, and the paper would benefit from references to previous work (e.g. Krimmel, 1999; Hagg et al., 2004).

Mass Continuity and Emergence Velocities:

The point should be made that, given mass continuity, the integration of the geodetic measurements of specific balance for a given time period should equate to the average specific balance – assuming no change in density and no sub-glacial erosion.

The maps of differences between the direct and geodetic mass-balance measurements in Figures 7 and 8 show annual emergence velocities for the entire glacier surface of HEF. How were direct measurements of specific balance inpterpolated across the surface of HEF? Summation of all values in these difference-DEMs should equal zero, given mass continuity and assuming interpolation from direct specific balance mea-

surements is reasonably accurate.

There is the potential issue of stakes self drilling, resulting in spurious measurement of emergence velocity on KWF. Steps may be taken to minimize or eliminate this, and it may be negligible, however this should be discussed as a potential error.

Numerous studies have specifically addressed, or include detailed measurements of, emergence velocities in relation to geodetic mass-balance measurements, and should be referred to in this study (e.g. Meier and Tangborn, 1965; Holmlund, 1988; Gudmundsson and Bauder, 1999; and Cox and March, 2004).

Address Densification:

Densification needs to be addressed. I recommend introducing the potentially important role of densification in section 3.4 “Ice flow velocity data”. Refer to Meier and Tangborn (1965), who compare geodetic and direct mass-balance measurements, and nicely address emergence velocity and densification. Densification may (most likely) be found, or assumed, to be negligible, but it should be addressed (Bamber and Rivera, 2007). Also see Cuffey and Paterson (2010; p. 335-337).

Density:

In some places in the text it is unclear whether values are in water equivalent units or are presented as a surface height change (i.e. ice equivalent units). This needs to be clear throughout, perhaps including mention at the outset that both are included within the paper and defining how they are demarcated.

Justification for using 650 or 750 kg m-3 as the density of firn is lacking. It is mentioned that field measurements were used in justifying a density change (p. 580, line 20), but no field data is provided. References to previous work applying similar densities for firn are missing (e.g. Hagg et al., 2004, who use 650 kg m-3). Similar previous work invokes Sorge’s Law (e.g. Cox and March, 2004), is this deemed inappropriate for this study? How would the results differ if Sorge’s Law was used?

Regarding Planimetric Surface Area:

In general it is not made clear why this surface correction is made for direct measurements. Details on how the LIDAR measurements avoid the problem, justifying the need for a correction of the direct measurements, need to be included, perhaps in conjunction with errors in the LIDAR measurements on similar (steep) slopes.

The hurried presentation of the applied correction (p. 581, lines 9-13), at the end of the results section, is insufficient. It is unclear that this paragraph is related to the earlier discussion about planimetric surface areas.

Writing and organization:

The paper would benefit from significant revisions to the organization and content of a number of sections.

The introduction does not serve the purpose of introducing the paper, which is focused on the comparison of direct and geodetic mass balances. Much more of the introduction should be focused on past work that has looked at this same topic and where gaps in our collective knowledge exist. (See comments below regarding references)

The paper would benefit from the creation of a discussion section. Currently, much of the content within the results and conclusions sections belongs in a separate discussion section.

The abstract, and sections one and two, have an exceedingly large number of technical errors and are difficult to read and understand. Beginning with section three the writing is of higher quality. The quality and organization of the writing needs to be vastly improved. Technical writing errors inhibit understanding of the paper and judgment of scientific merit.

References:

This paper is lacking in appropriate references, specifically with regard to a wealth

of past work that has delved into comparisons of direct and geodetic mass-balance measurements. I have mentioned a number of these above, but other papers that compare the two methodologies include Tangborn et al. (1975), Hagen et al. (1999), Ostrem and Haakensen (1999), Rasmussen and Krimmel (1999), Cogley (2009), and Zemp (2009).

Other:

Figure 5 is a nice visual representation of the temporal variability of surface height change. I'm curious to know why in some years (2002, 2003, 2004, and 2009) the curves are smooth and in the others there are erratic variations. Is there a plausible, physically-based reason why this might be expected in some years and not others, or might this be an artifact within the DEMs or the DEM differencing?

Are direct measurements of specific balance always made at the same number of locations on HEF? How might the spatial variability and number of locations change from year to year, and how might this impact the accuracy of the direct mass-balance measurements?

The fact that the geodetic measurements are actually less negative than direct measurements in half of the study years seems a bit glossed over. Cumulatively, and on average (although marginally) the geodetic measurements are more negative, but the paper would benefit from further discussion of the methodology and errors on an annual balance that might lead to such differences.

I do not follow the logic on page 582, lines 10-13, which lead to your conclusion that "direct mass balance data in the firn area of HEF is likely to be reliable." Stake L9 is not included in Table 8, nor is it labeled in Figure 1, so it is difficult to verify its location, but I'm assuming it's in the ablation area of KWF. How does an emergence velocity from the ablation area of KWF validate the direct mass-balance data in the firn area of HEF? This is important to clarify as it is used as a lead-in to the following discussion of the primary conclusions.

Technical Corrections:

The paper is in need of an exceedingly large number of technical corrections.

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