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The Cryosphere Discussions

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

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General Comments:

Andrea Fischer investigates direct glaciological and geodetic mass balance data of six Austrian glaciers over various multi-annual time periods. She aims at detecting potential systematic differences between the two methods and related processes. Thereto, she gives a methodological overview and discusses selected sources of potential errors. In a second part, the relevance of the findings from Austrian glaciers is investigated by a comparison to published data from seven glaciers in other countries. She concludes that geodetic and direct (glaciological) mass balance measurements are complementary, but differ systematically.

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Interactive Discussion
Discussion Paper

It is well established that glacier mass balance measurements based on the direct glaciological method are combined to volume change assessments from geodetic surveys (Hoinkes 1970, ZGG; Haeberli 1998, UNESCO; Fountain et al. 1999, GA). Over the past decades, it has become a standard procedure to check the (cumulative annual) glaciological with (decadal) volumetric mass balance methods. The majority of corresponding works, however, just compares the results from both methods with only considering a selection of potential errors, e.g., related to density assumptions, differences in survey dates, or to stake and pit readings. Meanwhile, it has become evident, that a sound validation ideally is based on consistent data and procedures (e.g., Holmlund et al. 2005, GA; Fischer 2009, GPC; Koblet et al. 2010, TC), and includes a quantitative assessment of stochastic and systematic uncertainties related to the direct glaciological and geodetic methods (e.g., Thibert et al. 2008, JG; Huss et al. 2009, AG; Zemp et al. 2010, TC).

The present study by Fischer is based on unique glaciological and geodetic datasets covering the past four to six decades. The present analysis has a great potential to make a major contribution to the current efforts in better understanding and quantifying the quality of the long-term glacier mass balance series. The paper does, however, only qualitatively discuss selected sources of (often worst case) errors and lacks a quantitative assessment of all potential sources of stochastic and systematic uncertainties. The terminology and calculation of corresponding (selected) errors are partly incomplete and erroneous. Without a sound and quantitative assessment, the direct glaciological and geodetic mass balances of an individual glacier cannot directly be compared; maybe with the exception of a statistical analysis of a large sample (cf. Cogley 2009, AG). For me, the paper reads like the author could not decide if the final work should be a teaching book (too many figures and explanations of basic knowledge), an error analysis (rather a collection of examples than a comprehensive assessment), or a statistical analyses of a “global” dataset (similar to but much smaller than the one by Cogley 2009, AG). Besides some more specific issues (see comments below), I do not agree with Fischer′s main conclusion that "geodetic and direct mass balance data [...]

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differ systematically“. Both the statistical comparison by Cogley (2009, AG) and the data of her own study show no systematic overall difference between the results from the two methods. In fact, the differences are much more likely related to some general differences in the two methods together with glacier- and dataset-specific uncertainties.

Overall, I propose to accept the paper only after major (re-reviewed) revisions. Thereby, I would like to strongly encourage the author to following up the careful homogenization work of the direct mass balance data of Hintereisferner (Fischer 2009, GPC) and to focusing a revised paper on the (laborious) quantitative uncertainty assessment of the great Austrian dataset including geodetic and glaciological mass balances. Please consider my specific comments below as a contribution to such a continuation of your highly appreciated work and feel free to contact me for further details and discussion.

Specific Comments:

Page 1152, Line 5: giving (only) the mean ANNUAL mass balance (e.g., -0.5 m w.e. a-1) and the mean CUMULATIVE difference (e.g., -0.7 m w.e.) is somewhat misleading. For reasons of comparability you should also give the mean ANNUAL difference.

P1152, L8: the given accuracy for LiDAR of 0.002 m w.e. seams rather a theoretical value than what is reported in glacier applications (see also your own comments on P1163). Indicate if these values refer to vertical or horizontal accuracy.

P1152, L19: I agree with your main conclusion that geodetic and direct mass balance data are “complementary”, but not that they “differ systematically” (see general comments above).

P1153, L5-6: direct glaciological (not “or”); geodetic (or volumetric)

P1153, L17-19: ...well, a complete WORLDWIDE dataset of direct glaciological methods would not show a bias either. And every real-world dataset might have some method-dependent bias...

P1153, L20-25: I think one should strictly differentiate between the use of geodetic

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methods for (i) the validation and calibration of direct glaciological data series at specific glaciers (e.g., by using photogrammetry or LiDAR) and (ii) assessing the representativeness of the few observation series for their entire mountain range (e.g., by using DEM differencing from SRTM and National DEMs for thousands of glaciers, cf. Paul and Haeberli 2009, GRL). Concepts, methods, accuracy requirements, and interpretations will differ strongly between the two tasks.

P1154, L1: the higher (temporal) resolution of the direct glaciological method does not necessarily lead to greater accuracy! It might actually be easier to quantify the accuracy of geodetic data.

P1154, L5-9: A complete uncertainty assessment of direct glaciological and geodetic mass balances needs to consider much more potential sources of errors than just density assumptions and extrapolation of point measurements. For a comprehensive list of potential stochastic and systematic error sources see Thibert et al. (2008, JG), Huss et al. (2009, AG), and Zemp et al. (2010, TC).

P1154, L20-21: The calibration (i.e., adjustment) of the direct (cumulative annual) glaciol. mass balance to the geodetic (decadal) mass balance does reduce the systematic error but not the stochastic (i.e., random) one. Improve terminology and concept of the uncertainty assessment.

P1155, L4: Without giving the information on total change (i.e., signal) and corresponding time period, you cannot judge a mass balance difference (i.e., noise) to be "enormous“. Also it should be noted, that the comparison by Cogley (2009, AG) does ignore any systematic uncertainties in both methods (e.g., differences in survey dates) which might be OK for statistical analysis of large samples but certainly not for the interpretation of differences at individual glaciers.

P1156, L20-26: The density (e.g., of 850 kg m-3) used for conversion of the geodetic volume changes to mass balance is usually based on the assumption of a constant density profile in the accumulation area (under steady-state conditions for glaciers with

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constant accumulation rate and no melting in that zone; cf. Sorge 1935 and Bader 1954). I do not really understand how your concept of the "surface layer" does fit into this concept? Please define your term "surface layer" and explain your assumptions (and potential deviations from Sorge's law) including corresponding uncertainty estimates.

P1157, equation (2): Note that your way of calculating the specific geodetic mass balance (i.e. dividing the volume change by the larger area) is different than most approaches in the literature. Traditionally, the mass balance (Bgeo) changes are divided by the area AVERAGE of both survey years. With the introduction of GIS-based raster analysis, bgeo is sometimes calculated as the average thickness change of all rastercells. All three methods might lead to somewhat different results which might need to be discussed.

P1157, L26: How are the glaciological mass balances in Austria adjusted from the floating-date measurement to the fixed-date system?

P1158, L20-22: Your point regarding the issue of the projection of mass balances is certainly interesting. Please detail further. This applies, however, to both the glaciological and the geodetic mass balance and should not introduce a methodological uncertainty.

P1159, L11, and further down: Did the firn cover reduce more than the ice cover? For the conversion of the geodetic volume changes to mass balances it is not just the areal extension of the firn that matters. The density of the volume change is determined by the three-dimensional quantity of melted/newly formed snow, firn, and ice between the two surveys. You should try to quantify the related uncertainties.

P1159, L12-14: In Fischer (2009, GPC) you nicely show for Hintereisferener that the homogenization of the mass balance series is an essential step towards the comparability of (annual) mass balances within a glaciological data series. In my view, this should be done for the other glaciers and also for the geodetic mass balances too

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before any uncertainty assessment and further comparison!

P1159, L14-19: You correctly mention the uncertainty of horizontal (and potential other) shifts of DEMs due to changing datum and projection. Such systematic horizontal shifts may have a major impact on the thickness and volume change analysis and need to be quantified (e.g., Koblet et al. 2010, TC) and corrected before a comparison with the direct glaciological mass balance. Kääb (2005, UZH) for instance presents an analytical approach to analyse and quantify such horizontal shifts in DEMs.

P1160, Chapter on Results: Make sure that your values of mass balances and differences between the methods are comparable with respect of units (see also comments above, P1152).

P1161, L25: Note that above you used a value of 850 (not 900) kg m-3 for converting geodetic volume changes to mass balances. Try to be consistent.

P1163, L15: Are you sure that seasonal snow cover alone can explain the misfit? You may use the survey dates of both methods, the aerial images (on which the DEMs are based), and meteo data in order to check and quantify this effect.

P1162, L21-22: I can only agree with this first statement and encourage you to do so. ïĄŁ I would suggest that you first introduce a list of potential uncertainties and a concept on how to quantify these stochastic and systematic errors for the Austrian glaciers.

P1162, L22-23: Show why the estimates by Fountain and Vecchia (1999, GA) and Kuhn (1999, GA) should be valid for your glacier sample.

P1162, L25-27: Why should the accuracy from Svartisen Ice Cap (by Rolstad 2009, JG) apply to the geodetic methods used in your glacier sample?

P1163, Accuracy of DEMs: I like this chapter, but the estimates for stochastic uncertainties seem partly to be too optimistic and you are somewhat mixing horizontal and vertical accuracies (and precisions?!). Furthermore, systematic errors (between

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DEMs) should be analysed and discussed with respect to their influence on glacier thickness and volume changes.

P1164, L5-8: see Kääb (2005, UZH) for a detailed discussion and analytical solution of this issue.

P1164, L10-15: I would suggest quantifying the influence of these uncertainties on the specific geodetic mass balances for each period of comparison.

P1164, Chapter on Seasonal Snow Cover: you should quantify the influence of these uncertainties on the specific geodetic mass balances for each period of comparison. See also comments above related to P1163, L15.

P1165, L4: above you mention a density of ice of 917 kg m-3... Try to be consistent.

P1165, L21-23: refreezing of melt water (i.e., superimposed ice and internal accumulation) is different from the issue of density changes and, hence, should rather be treated in separate sections/chapters.

P1165, L26: the potential influence of (changes in) crevassed areas on the density assumption, and finally on the specific mass balance, is interesting but needs to be quantified. You may assume a maximum depth of crevasses (e.g., 30 m) and use areal extent and crevasse frequency for a first-order quantification.

P1166: I agree that density changes of the "surface layer" is a potential source of error. You should, however, also discuss three-dimensional changes in that "surface layer". Is it plausible that your "worst case" estimates apply for all survey periods? Make sure that the values given are comparable to specific mass balances.

P1167, L11-19: What is the influence of these local (worst case) estimates on the specific mass balance?

P1167, Chapter 4.2 Comparison of the results to published data: What is the purpose of this comparison? And what does it show that we do not already know from the work

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by Cogley (2009, AG)? Any basic corrections (e.g., common density assumption) that are required for such a "statistical" comparison of the raw data? I think you should better motivate the discussion. As such you nicely show that the difference between the two methods (a) are overall not systematic, (b) do overall not depend on the length of the survey period, and (c) can be systematic for specific glaciers.

P1168, L10-12: What is the (statistical) threshold for the decision that the data are "in accordance"?

P1169, L1-12: the overall stochastic uncertainty has to be calculated according to the law of error propagation whereas systematic uncertainties are to be cumulated.

P1169, L5: Please clarify why the (stochastic) uncertainty of the density assumption is supposed to be 10% of the geodetic mass balance?

P1170, Conclusions: in my view, only a comprehensive uncertainty assessment (including corrections for systematic uncertainties and error bars for stochastic uncertainty) will allow to directly compare the glaciological with the geodetic mass balances and provide a statistical threshold of their (no) accordance. At present, the final remarks and numbers given seem to be rather arbitrary than thorough conclusions from the results and discussion.

P1170, Acknowledgements: I think it would be appropriate to acknowledge the work of the large number of glaciologists that have contributed to this great datasets over many decades.

P1172, 7-9: I believe the paper by Funk et al. was published in 1997 (not 1996), and the time period covered in the title is 1961-94 (not 1961-95).

P1174, L22-25: note that meanwhile the revised paper is published in The Cryosphere, 4, 345-357, 2010.

P1176, Table 2: when two months are given for one year – does this refer to two DEMs or to just one produced out of data from two surveys? How did you correct for the

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differences in survey dates, also when compared to the date of the field survey?

P1177, Table 3: give information on density assumption (bgeo) in table caption. Also, any corrections applied to the mass balances (e.g., adjustments to fxd-time system?) should be clearly stated.

P1178, Table 4: the values for Storglaciären are in the wrong column: bdirect should be in bgeo and vice versa. Note that for bgeo we changed our density assumption from the TCD paper (825 kg m-3) to the TC paper (860 kg m-3). Please indicate in the table caption if you recalculated the bgeo from other publications with your density assumption of 850 kg m-3.

Tables: All Tables 1-4 are relevant and should retain in the paper. However, Table 2 might be converted into a figure showing the timing of the different survey types per glacier on a time axis.

Figures: Reduce the number of figures. I would suggest to keep Figs. 3, 4, 5, 15, 16; reduce the Figs. 6, 10, 11, (13) which basically show the same issue; and reduce the "teaching book" Figs. 1, 2, 8, 9, 12, 14 to only the most relevant issues.

P1185, Fig 7: it is hard to compare bdirect to bgeo in this figure – vertical bars of bgeo and corresponding cumulative bdirect might be more appropriate and would allow to include systematic and stochastic error bars.

Table & Figure captions: give more details so that tables and figures can be understood stand-alone.

I would suggest to more strictly separate the sections on Data&Methods from Results, and to include a Discussion in order to improve the structure of the paper.

Last but not least, the paper would benefit from careful editing by a native English speaker.

Interactive comment on The Cryosphere Discuss., 4, 1151, 2010.