

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

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We highly appreciate the constructive comments of the Anonymous Referee #1. We agree that the readability and clarity of the paper will benefit from a reorganization and a clarification of the terms used. Therefore we will include basic definitions of mass balance together with a list of the terms used, which should make it easier to follow. Technical correction will be done.

The reviewer has the impression that we assume the direct measurements to be accurate, and the geodetic measurements to be erroneous. This is not the intention, and may have resulted from an unexact wording. The basic problem is that we have not done an complete analysis of the errors of the direct method. A study on that is currently taking place, comparing the spatial variability of mass balance measured at

C455

stakes and in holes in a small test field. At the moment, our knowledge on the errors of the direct method is based on several published and unpublished statistical analyses of stake measurements. The framework of this study does not allow a complete treatment of possible errors in the direct method and the presentation of the upcoming new results on that topic, so that I suggest to cite the existing work on that topic and go ahead with those quantifications of the accuracy of the direct method. I agree that deviations between the methods can not be interpreted as errors in one method, and will change the wording to avoid that impression.

‘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.’

The controls of mass balance are related to meteorological conditions at one location. The investigation of the controls of mass balance therefore needs comprehensive modelling of ice dynamics and meteorological conditions to separate the volume lost by melt in a specific pixel from volume lost by ice dynamics or gained by snow fall. Here I see the problem of investigating the controls of mass balance based on geodetic data only. I can not see a direct connection of this basic problem to the accuracy of direct measurements.

Mass Continuity and Emergence Velocities

The use of the term ‘emergence’ or submergence velocity can be done, but implicates the complication that those are, in most cases, defined vertically to the glacier surface and differ from the vertical component of the ice flow in cartographic coordinates by $\tan \alpha$. Since the surface slope is not uniform on the glacier surface, as shown in principle in Figure 9, it seems to be confusing to use this terms instead of a vertical velocity component which perfectly fits to the coordinate system used. Unfortunately, even the term in Cuffey and Paterson seem to use different coordinate systems, as indicated in Figs 8.5 and 8.14. If on page 286, the ‘upward’ component z (which would

be contradicting Fig. 8.5.) is meant vertical, and is used together with an x axis along the glacier in direction of flow, the coordinate system is not rectangular! I do not see an added value in presenting plots of submergence and emergence in addition to the vertical velocity component, differing by $\tan(\alpha)$ and not being comparable neither to the mass balance nor to elevation change. To avoid the term 'submergence' ensures to prevent that someone might mix it up with 'subsidence', as it was the concern of the reviewer.

Address densification

Densification is already addressed in this article, but this can be made more explicitly in adding the suggested part of basic equations to the introduction. The extent of the firn cover in the first and the last year is shown in Figure 1. Measurements near the surface will be added, the discussion presented in (<http://www.the-cryosphere.net/5/107/2011/tc-5-107-2011.pdf>) can be summarized, and literature can be cited.

Density: A part on the justification of the assumed density will be added, field data will be provided. Regarding the application of Sorges Law: I always wonder why Sorges law should be used for Alpine glaciers, instead of the available data from the 30 m firn pit dug on Kesselwandferner. Is there any indication that data from Greenland represents the firn density of Kesselwandferner and Hintereisferner better than data from Kesselwandferner, and why? Of course I could apply Sorges's law, but then I would like to have a good explanation for doing so, indicating why I prefer it to measured data at the site.

References:

Since this is the second part of the studies on geodetic mass balance, not all the literature cited in the first part (<http://www.the-cryosphere.net/5/107/2011/tc-5-107-2011.pdf>) is cited also in this paper. The reason for that was the intention to keep the reference list as concise as possible, so that not all the previous work, most of it

C457

in different temporal or spatial resolution, was also mentioned here. Of course, we are willing to include a larger list of previous work, and will include the list again, as done in the first article.

Other: The steps in some curves in Fig. 5 are clearly artefacts, created either by the processing of the DEMs or the subtraction. There could also be the result of specific data formats. The effect would not have been visible by choosing different classes in Fig. 5., but the sense of Fig. 5 is to show the general characteristics, and I had not the impression that this 'noise' changes the message of Figure 5.

It is not possible to investigate always the same sites for mass balance, since the absence of snow prevents the digging of pits. Apart from that, the location and number of stakes is basically the same as shown in Figure 1. The method and the exact locations are shown in an article for 2003 to 2006. I could add the maps for every year, but did not do so since the changes would not be visible at the first glance. 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? This research question addresses an important topic, but fits more in an article investigating the accuracy of the direct method. As the 'information overload' of this article is already a critical point, I don't think it makes sense to add additional material. I could cite additional, unpublished work done for Hintereisferner in previous years, which is basically summarized by Kuhn et al., 1999 (which is already cited, but maybe the information was overread).

Stake L9 is not included in Table 8, nor is it labeled in Figure 1, so it is difficult to verify its location

I will label the stake and pits in the Figure 1.

Interactive comment on The Cryosphere Discuss., 5, 565, 2011.

C458