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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.

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We thank the Anonymous Referee # 2 for his/her constructive and very valuable suggestions to improve the paper. As suggested by Reviewer 1, the paper will be restructured as suggested, with a section on basics and definitions.

1) Theoretical considerations and formulation I agree that is important to stick to a clear terminology. Regarding the emergence velocity, the terminologies suggested by reviewers differ. In Bamber and Payne (2004), page 12 Fig.2.1, it seems that the emergence velocity is defined vertically. This is not the case in Cuffey and Paterson, page 337 ('the term emergence velocity refers to upward or downward flow of ice relative to the glacier surface'…'Emergence velocity differs from the vertical motion of the marker'…In any case, the z axis is defined in different ways even within Cuffey and Pa-

terson, chapter 8.1.1 page 286, Fig 8.14 and 8.5.5.8. Therefore, it is not exactly clear to me, how the reviewers define the z axis and therefore if the vertical component of ice flow velocity is equal to their definition of emergence/submergence or not, a footnote in Cuffey and Paterson says that it can be defined in both ways …. In any case, Fig. 2 was included in the article to make the definitions clear. My suggestion is to extend the introduction by an explanation of the basic concepts, reorganize the Fig. 2 and add a list of terms.

The calculation of volume change can be made clearer, as suggested by the reviewer, adding some information on the code used. The volume change is calculated on a cell by cell basis, I am not sure how this is covered by the article of Etzenmüller (2000), who cites at this point an other article. The ArcGIS tool used to subtract the DEMs does not come along with a citation explaining the code. I agree that the basic terms of specific balance, specific annual balance, geodetic balance should be defined better in the article, this will be done. The glaciers Hintereisferner and Kesselwandferner are non-calving, which I forgot to mention since I thought this would be evident from the map. I did not present the equations on mass conservation, since I am not sure if mass conservation (or better: density conservation) is valid in this case, but I will add that.

I avoided to present Figs 7 and 8 as maps of vertical ice flow velocity, since I can not quantify density changes, basal melt and the errors of surface mass balance as grids. Thus, I wanted to wait for the results of our estimation of i) the accuracy of direct measurements and ii) the estimation of basal melt, both ongoing studies. The reason to present this data set without declaring Figs 7 and 8 as emergence was that I think there is still a lot of information content within this study.

2) Elevation Change methodological description and implementation The GCP surveys were carried out to develop transformation paramters between the UTM WGS 84 system, the Austrian Gauss Krüger System and local coordinate systems to ensure the compatibility of old and new maps. Since the data were available, it was then a straight forward possibility to use them also within this study to evaluate the DEM ac-

curacy at those points (Why not?). Those GCPs were not used for the processing of the DEMs, and the DEMs and the GCPs were processed in UTM WGS 84, so that no transformations were necessary. The area shows a large portion affected by mass movements (mud flow, permafrost, permafrost degradiation). Stable parts (rocks) show steep faces, and as shown by Sailer's group, the vertical error of the DEMs increase with the surface slope. The GCPs are well known, on stable terrain, and many of them observed for more than 100 years. We decided to use the best points, not the most for the evaluation. Snow cover, surface roughness and imaging geometry cause small scale undulations in rough terrain (depending on the pulse used). We do not have information on snow cover in areas outside the glacier, and the snow there tends to fill the space between boulders and is patchy. I do not think that we could improve our estimate of vertical accuracy by adding a statistical analysis of non glacierized terrain. Work on general accuracy of LiDAR DEMs is ongoing within the LiDAR community, and I cited the results available so far. The differences between the GCP elevation also result from the different reflection points, as is evident from the two images I attach. The fastest fix point moves at a rate from about 10 cm/year, although it looks stable, so I hope you accept that I prefer to use the points were we know how stable they are for this analysis, and are able to exclude the points which are not stable or show erosion. We have no phase information of the return signal available, so that we can not investigate the coherence. The flight swath is not equal from flight to flight, so that the local incidence angles differ. The DEMs can be expected to differ on rough surfaces and in case of snow cover, so that I do not expect a possibility of a straight forward correction of DEM errors which are still in the order of cm!

3) Presentation Quality: I personally prefer the tables to Figures, but I could provide them as additional material if they are not needed within the article. Figures 3, and 4, and 6 show density variations, which were demanded to discuss by Reviewer 1, although shown here. I do not think that the point of the snow correction and its impact on the result will be clearer by skipping those Figures, I would prefer to improve the explanations of those Figures.

4) Errors

I understand the point and will improve the presentation.

5) Steep areas show larger surfaces in reality than in map projection, which is available for energy transfer (normal to the surface, and not vertically). Therefore, the energy transfer into snow/ice should be higher than in flat parts, in addition to that, snow cover usually is lower and this mean albedo during the melt season. Surface runoff additionally causes erosion. In steep areas we can not operate stakes, and I think it is not straight forward to extrapolate the measurements from flat to steep areas. Vertically measured ablation in flat parts is therefore not equal to vertically ablation (not measured) in steep parts, but should be higher, as a result of the above factors. This is also evident from fast melt visible at non moving clean dead ice parts in the vicinity of rock outcrops, which I did not cite because I am not able to quantify the role of long wave emission of the rock outcrops. But is approximately the same for sunny and shady parts.

5) Manuscript Structure I fully agree with the suggestions to restructure the manuscript, and better work out the conclusions.

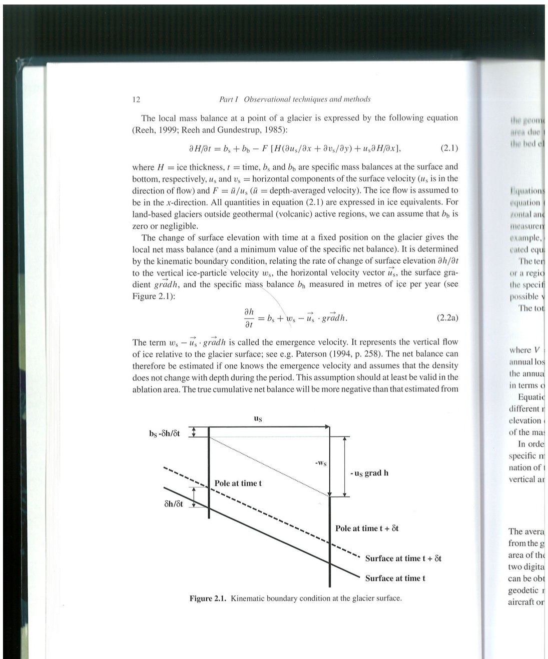
Technical Corrections

General: Sentences should not start with numbers. (E.g. pg 569, line 24; pg 581, line 1; and many more... ok âĂć pg 569: Numerous statistics are provided, but the reader has difficulty understanding what the meaning of it all is. Come clearer to the point, possibly stating first, and then back up with the relevant statistics. ok âĂć pg 569, line 21-23: Two time periods are given, and only one annual precipitation number(661 mm). Is the average the same in both periods? Yes, I can make that clearer âĂć pg 570: A very detailed description of the LIDAR is provided. Is it all necessary? I suggest removal and compacting some of the information as the paragraph is heavy and difficult to absorb. I can reduce it, and add relevant points later when the reader needs them for the interpretation of the GCP accuracies. âĂć pg 572, line 15-16: "The distribution of snow ... is monitored by webcam". How is this monitored? Or more specifically, how do you geo-rectify the pictures to produce firn maps? In the moment by manual mapping supported by GPS measurements.

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

Fig. 1.

Fig. 2.



12 *Part I Observational techniques and methods*

The local mass balance at a point of a glacier is expressed by the following equation (Reeh, 1999; Reeh and Gundestrup, 1985):

$$\partial H/\partial t = b_s + b_b - F\,[H(\partial u_s/\partial x + \partial v_s/\partial y) + u_s \partial H/\partial x], \quad (2.1)$$

where H = ice thickness, t = time, b_s and b_b are specific mass balances at the surface and bottom, respectively, u_s and v_s = horizontal components of the surface velocity (u_s is in the direction of flow) and $F = \bar{u}/u_s$ ($\bar{u}$ = depth-averaged velocity). The ice flow is assumed to be in the x-direction. All quantities in equation (2.1) are expressed in ice equivalents. For land-based glaciers outside geothermal (volcanic) active regions, we can assume that b_b is zero or negligible.

The change of surface elevation with time at a fixed position on the glacier gives the local net mass balance (and a minimum value of the specific net balance). It is determined by the kinematic boundary condition, relating the rate of change of surface elevation $\partial h/\partial t$ to the vertical ice-particle velocity w_s, the horizontal velocity vector $\vec{u}_s$, the surface gradient $\vec{grad}h$, and the specific mass balance b_h measured in metres of ice per year (see Figure 2.1):

$$\frac{\partial h}{\partial t} = b_s + w_s - \vec{u}_s \cdot \vec{grad}h. \quad (2.2a)$$

The term $w_s - \vec{u}_s \cdot \vec{grad}h$ is called the emergence velocity. It represents the vertical flow of ice relative to the glacier surface; see e.g. Paterson (1994, p. 258). The net balance can therefore be estimated if one knows the emergence velocity and assumes that the density does not change with depth during the period. This assumption should at least be valid in the ablation area. The true cumulative net balance will be more negative than that estimated from

Figure 2.1. Kinematic boundary condition at the glacier surface.

Fig. 3.