The Cryosphere Discuss., 4, C221–C224, 2010
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The Cryosphere Discussions

Interactive comment on "Determination of length, area, and volume changes at Storglaciären, Sweden, from multi-temporal aerial images (1959–1999)" *by* T. Koblet et al.

Anonymous Referee #1

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This paper describes the generation of 5 digital elevation models (DEMs) based on repeated aerial photographs for Storglaciären, one of the glaciers with the longest mass balance records. These data are valuable, especially, as no homogenous set of surface changes existed for this glacier before. The data strengthen the confidence in the long-term glaciological mass balance series of Storglaciären, and are a good example of the benefit of careful re-analysis of previously acquired data. Uncertainty are assessed using different methods, and the paper is well written.

Besides of several minor comments on the presentation of the methodology and the results (see below), I have two major concerns:

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On page 354, line 11-14, the authors write that some information is missing to perform a detailed error analysis. However, they do not state which missing information would be required to complete the error analysis. I would for example expect, that there is additional uncertainty due to measurement accuracy of individual points in the DEM, and the interpolation of the irregularly measured points to a grid. These factors are not mentioned and not included in the error assessment. Related to this, I really miss one number for the uncertainty in each image. An uncertainty analysis is performed, but the authors do not come up with an error bar. Moreover, the results in Table 3 are highly worrying as the systematic errors relative to the dGPS points, and the pixels in non-glacierized terrain are not at all comparable. The authors should try to explain these differences, and combine them into one error estimate. It is interesting to obtain different numbers from based on different methods, however, for further analysis (e.g. comparison to the glaciological series) ONE number integrating all errors is required.

The evaluation of aerial photographs and the establishment of digital elevation models has now been performed in glaciological (and other) research for many decades. Therefore, the present study does not outline a set of completely new methods, but is rather an application of standard techniques (combined with some detailed error assessment), and the results presented in this paper are not very new and unexpected. This does not mean that I don't consider the data worth to be published, as I really appreciate the careful work that has been done. However, I do not quite see the point of splitting the work into two papers (see Companion Paper by Zemp et al.). There is already about a dozen of publications about the comparison of glaciological and volumetric mass balance (see also my review of the Companion Paper). So the matter presented in the Companion Paper is, as well, not completely new. I have the impression that separating the interpretation of the results obtained in the present paper is not a benefit.

Detailed comments are listed below:

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- **page 350, line 27:** The mean ice thickness could be provided in addition to the ice volume.
- **page 352, line 15:** Distribution of dGPS points: Almost all dGPS points seem to be situated in flat terrain lying quite a bit below the elevation of the glacier. This has certainly an impact on the representativeness of the measurements for determining the accuracy of the DEM on the glacier, which should be discussed, and, if possible, addressed somehow in the error analysis. Interestingly, Figure 4 implies that the plain with most dGPS points shows small deviations for most comparisons, whereas the errors seem to be increasing towards the elevation of the glacier.
- **page 354, line 28:** Kappeler (2006) is cited several times when it comes to length change determination. Is there an equivalent publication that is accessible more easily to the readership?
- **page 355, line 19:** Shortly outline the difference between the standard error (SE), and the standard deviation / root-mean square error. The equation for the SE is only given later in the text (equation 4). It might already be provided here.
- **page 355, line 25:** How does the uncertainty of the dGPS points (0.1 m is not completely negligible) enter into the error assessment?
- **page 356, line 19:** Equations (2) and (4) are formulated differently, although, to my understanding, the same is done.
- **page 357, line 24:** Which parameter is varied in each iteration step? The description of the Monte-Carlo simulation was not entirely clear to me, and could be enhanced.
- **Section 5:** This section is mainly descriptive and misses the link to changes in climate. In my opinion, as outlined above, the paper would greatly benefit if the

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comparison to the long-term mass balance series were presented here.

- **page 360, line 13:** The exact years do not need to be repeated.
- **page 361, line 1:** This statement is unsupported by what has been shown in the paper. When is the quality sufficient to derive a glacier mass change? You can still calculate mass change, when the uncertainty is huge. A criterion could be the uncertainty in comparison to glaciologically derived mass balances.
- **page 364, line 29:** The uncertainty assessment is 'sound', but in my opinion not complete, as no final error bars are provided (see above).
- **Caption Table 3:** Provide the number of data points for both statistical evaluations
- **Table 4:** Maybe related to my poor understanding of the Monte-Carlo simulation section I do not see the origin of the 'Signal' provided in the table. This might be explained in more detail in the text, or repeated in the caption.
- **Figure 4:** The figure would be much easier to interpret, when additional glacierized surfaces would be masked out. Now, large variations in elevation are visible in the North of the glacier. The reader has to know that another glacier is situated there.

Interactive comment on The Cryosphere Discuss., 4, 347, 2010.