

Interactive comment on “Comparison of glaciological and volumetric mass balance measurements at Storglaciären, Sweden” by M. Zemp et al.

Anonymous Referee #1

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This paper presents a comparison of the new volumetric mass balance obtained in the study by Koblet et al. (Companion Paper) to the long-term glaciological mass balance records from Storglaciären. The authors apply several corrections to both data sets before comparison. Both data series agree within their uncertainties. They conclude that no adjustment of the traditional measurement series is required. In general, the paper is well written, and addresses the most important components of the homogenization of mass balance time series.

I have some comments on the methodological issues in the 'uncertainty assessment' section (details see below). Some components adding uncertainties are missing, and

C225

most importantly, errors of individual sources are not combined into one integrative uncertainty. A sound comparison of glaciological and volumetric mass balance (resulting in a potential adjustment of one series) can only be performed if the total uncertainty for both components is known.

The work, now split into two companion papers, would benefit from being combined into one comprehensive study that presents methods, results and interpretation together (see also my review of the Companion Paper by Koblet et al.). The methods applied in both papers are not new; comparison of glaciological and volumetric mass balance has been published many times over the last years (a reference list is provided on page 396, line 13-, in this paper). The present study does, in my opinion, not add substantial new methodologies or data to the older papers, but is rather an application of existing knowledge. Nevertheless, the comparison performed for Storglaciären is very valuable and interesting and should definitely be published. However, I recommend combination with the Companion Paper, where the newly derived volume changes are presented.

Detailed comments are listed below:

- **page 388, line 20-:** Density assumption: What is the authors' assumption of the density of glacier ice? 917 kg m^{-3} , as stated on page 388, line 25, or 900 kg m^{-3} , as stated on page 389, line 2? Be consistent. Moreover, I think that glacier ice only rarely reaches the maximum density of 917 kg m^{-3} . Secondly, I do not understand the reason for calculating the zonal average (page 389, line 1) of surface coverage of firn and glacier ice. The density of the volume change is determined by the quantity of melted high-density ice minus newly formed ice at the bottom of the firn layer in relation to the quantity of melted low-density firn. These are both volumes (three-dimensional!) that can not be approximated using

C226

surface coverage (2D). The final results of the density assumption seems to be quite reasonable, but I question the way to arrive at this estimate.

- **page 389, line 12:** Why do the errors depend on the 'glacier mass turnover'? This should be explained. Isn't it rather the quantity of melt or accumulation between photogrammetrical and glaciological field survey that matters?
- **page 389, line 15:** Does this mean that all exact dates of the field surveys except for the period 1980 to 1990 are lost? If yes this should be stated clearly in order to justify that assumptions have to be made.
- **page 389, line 25:** What lapse rate is used to extrapolate air temperatures to ELA_0 ?
- **page 390, line 2:** Degree-day factors relate positive air temperature to melt. Measured summer mass balance also includes summer accumulation which can be considerable, especially in the higher regions of the glacier. Therefore the degree-day factor cannot be calculated with a straightforward approach directly from summer mass balance. A correction of solid precipitation during the summer period is required in order to avoid systematically too low degree-day factors.
- **page 390, line 9:** Why are only 'melt corrections' applied? The authors implicitly assume that there was never any solid precipitation between the survey dates. If this is the case this needs to be shown using observational evidence. If potential accumulation is neglected, but melt is taken into account this leads to another systematic error in the correction.
- **page 390, line 12:** It would maybe be better from a homogenization point of view to correct/adjust the volumetric mass balance in the period, and not the glaciological measurement series.

C227

- **page 390, line 15:** The calculation of conventional mass balance requires an update of the surface area in *every* (!) year, and not periodically. It is for practical reasons that this correction is often applied only after a new glacier map is available.
- **page 391, line 1-:** I am unsure whether this procedure is justified here. When adding or neglecting a section of glacier surface area near the terminus, the effects are much larger due to high mass loss than for the accumulation area. Therefore, the relation between glacier-wide mass balance and glacier area is not linear, as it is implied by the multiplication / division on page 391, line 1. Local mass balance at the glacier terminus (which is available from the glaciological surveys) need to be taken into account!
- **page 391, line 13:** m w.e. a^{-1} ? (same also elsewhere in the manuscript)
- **page 392, line 2:** 'Annual accumulation' is not equal to the 'winter balance'! If this approximation is made, the authors should analyze by how much winter balance is lower than total annual solid precipitation over the glacier.
- **page 392, line 15:** Entire subsection: It is unclear to me how these general statements (I agree with them) are related to the uncertainty assessment. How would flux divergence impact on a bias in glaciological mass balance? As the effect was not treated here, this subsection should be omitted, or the effect be discussed in detail.
- **page 392:** I miss several important points in the uncertainty analysis of the glaciological mass balance data. What about the reading error at the stakes, and uncertainties in the measured densities in the accumulation area? More importantly: What about the spatial inter- and extrapolation from the point mass balance measurements to the entire glacier? This can add a significant uncertainty to the final estimate and definitely needs to be considered, and discussed.

C228

Furthermore, it is not clear, how the uncertainties of the individual sources are combined, or if this is done at all. Is the uncertainty in the density of volume change added to the error in the volumetric mass balance, for example?

- **page 392, line 24:** It should be stated (using an equation) *how* the uncertainties are cumulated over the periods. According to the laws of error propagation (as it appears from looking at the tables), or by linearly adding annual uncertainties?
- **page 393, line 9:** What is the number of ± 0.10 m w.e. based on? Was an analysis of the raw glaciological field data performed, or is this just an estimate? As already stated above, these error sources should also be included in the 'uncertainty assessment' section. For example, errors in the interpolation scheme might lead to systematic, and not only stochastic errors, as it is implied here.
- **page 393, line 10:** Unclear. Where do these numbers originate from?
- **page 393, line 13:** m w.e. per year, or absolute?
- **page 393, line 14:** The conclusion of the paper is that the glaciological and the volumetric mass balance agree, and no adjustment is required. However, here the authors write that they 'adjust' the glaciological mass balance. What has finally been done?
In general, it is problematic to perform adjustments (or not), if no integrative error estimate of the volumetric method is available (see my review of the Companion Paper). In addition, I also miss a final error estimate for the glaciological method. On page 395, line 6, the authors use the term 'statistical exercise' for explaining why this was not done.
For these reasons, I strongly suggest that uncertainty estimates for both methods including all potential error sources be presented. If this is missing, comparison of the series is vague, and it cannot be decided, whether an adjustment is necessary or not.

C229

- **page 394, line 11:** 'mass balance variations' – do the authors refer to the cumulative mass balance? In that case, the sentence should be reworded. Moreover, what do they mean with 'periodic'? Was there any analysis done?
- **page 394, line 12:** The unit of the change given here has nothing to do with glacier mass.
- **page 395, line 6:** As stated above, this argumentation sounds strange in a scientific article, and is, as such, not acceptable.
- **page 395, line 21:** What about the inter- / extrapolation of point mass balance to glacier-wide mass balance?
- **page 395, line 24-29:** This discussion only makes sense if the error bars of the volumetric mass balance are jointly considered.
- **page 396, line 8:** How were the mean annual deviations calculated?
- **page 396, line 21:** This statement should be put into context. If the errors in the volumetric mass balance are too high, such a correction does not make sense.
- **page 397, line 15:** The authors presented several glaciological series that were partially homogenized. To which one do they refer here? To the "official" one.
- **page 397, line 17:** This statement contradicts the findings on page 395, line 24-. In fact, it seems that these corrections increase the misfit, and do not reduce it.
- **page 397, line 21:** The authors explain the misfits with an overestimation of internal accumulation. The results even imply that there is no internal accumulation at all. This finding could be quite interesting, as internal accumulation is difficult to measure. I recommend to investigate this point in more detail by discussing the literature on this topic. Is it possible that NO internal accumulation occurs on Storgliären?

C230

- **page 398, line 1:** Related to previous comments: This statement is not possible without integrative numbers for the uncertainty in both glaciological and volumetric mass balance series!
- **Table 1:** Summer balance should be negative
- **Table 3:** This is a duplication of the Table in the Companion Paper. This numbers are per se difficult to interpret. They should be combined into one uncertainty estimate. Especially, the anticorrelated systematic errors E and F need additional explanation.
- **Figure 2:** The legend of this figure should be displayed in much larger size. Error bars including all uncertainties in the volumetric mass balance should be shown.

Interactive comment on The Cryosphere Discuss., 4, 381, 2010.