

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

Anonymous Referee #2

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The authors present a well written and profound study on an extremely important issue: the comparison and assessment of two methods for glacier mass balance determination. Mountain glaciers are known to be key indicators for climate change and glacier mass balances represent the direct response to climate variations. Annual (or even seasonal) mass balance fluctuations can only be derived from direct measurements using the so called glaciological method. However, this method is subject to several uncertainties. The most important quality check is the comparison with geodetically derived mass balances which have their own sources of error. So far, there are only few glacier worldwide where long-term series (which are of essential importance for climatic interpretations) of both methods exist and the longest record is from Stor-

C232

glaciären. The authors do not apply novel concepts or new data, but they carry out a careful reprocessing of the raw data resulting in a comprehensive error analysis for both methods. Although their efforts do not result in a better fit of the recalculated data compared to the older series, this paper is state-of-the-art of mass balance methodology and involved error analysis. It should encourage other groups which hold and produce long-term mass balance series to re-evaluate their records and to detect and assess their main uncertainties in a similar way. The article follows all rules of good scientific practice. It is well structured and clear, the language is fluent and precise. In form and content I cannot find general points of criticism, but I have a number of specific comments:

382, ln. 5 and 386, ln. 21: is “glaciological map” really the appropriate term? In my understanding a glaciological map is a thematic map showing glaciological issues such as ice thickness or mass balance. The maps mentioned here are “topographic maps” showing glaciers, maybe we can agree on this term or on “glacier maps”?

383, ln. 20: considering the interval between the maps, a new geodetic survey is overdue. If you know anything about it, you may want to comment if this series will be continued or if any new technology (ALS?) will be used in the near future.

384, ln. 6: “were carried out at. . .”

384, ln. 7: is the term “Nordic glacier” really common?

385, ln. 13: why are stakes distributed across the entire glacier and not restricted to the ablation area? According to figure 1, they are used for summer mass balance. Don't they constantly submerge into the glacier in the accumulation zone?

386, ln. 25: Although you refer to Koblet (2010), you should at least name the basic photogrammetric techniques (e.g. analogue or digital photogrammetry?). This would increase the autonomy of the paper.

388, ln 2: the estimates of the stochastic errors for field measurements and interpo-

C233

lation do not exactly result from equation (1), which is used to calculate the combined uncertainty of the two before mentioned. The uncertainties of the two error sources are the two roots (not their square) below the big root. This should be clarified in the text to avoid confusing the reader who is not too familiar with statistics.

389, ln. 2: what is a “firn and ice” area?

389, ln. 3-4: Are 1998 and 1999 years with a low ELA or why were they chosen?

390, ln. 9-12: you should explain here how the uncertainties concerning survey dates in Table 2 were calculated from the melt corrections in Table 1. I cannot follow this procedure. In my understanding, Table 1 presents the absolute values of melt corrections. Given a year where the aerial survey is before the field survey (all years except 1959), melt needs to be increased if the date marks the beginning of a survey period (positive melt correction) and decreased (negative sign) at the end of the period. If mass balance is considered, the signs change (increased melt decreases the net balance). In years where the photo flight was after the field survey (1959), the opposite of the above mentioned is true. The mass balance uncertainties in Table 2 should be the sum of the mass balance corrections at the beginning and the end of the respective period. If I did not make any mistakes, most numbers in Table 2 are wrong (the resulting uncertainties are 0.026, 0.21, -0.146, 0.031 and 0.121). Whatever numbers are correct, you should describe in more detail how they are derived in order to make the results more traceable.

390, ln. 19: is “1949” a mistake?

390, 24-29: the whole statement about the ranking of mass balance series cannot be retraced since no numbers are presented. How was the agreement with the volumetric mass balances determined? Which of the Koblet (2010) data was taken as reference or is the ranking of glaciological mass balances valid for the comparison with ALL Koblet (2010) results?

C234

292, ln. 4-13: the chapter on superimposed ice is a bit misleading. First you are talking about the annual maximum of superimposed ice appearance. This value (0.1 m w.e. in the ablation zone) is not important for the classical glaciological method. Considering mass balance measurements, only the superimposed ice at the end of the ablation season needs to be accounted for. This should be formulated more clearly and maybe a range of typical values at the end of the mass balance year should be provided.

395, ln. 28: this should be the “best estimate” including only the corrections A, B and C (not D!)

396, ln. 12: “which utilizing” seems to be a language problem

396, ln. 17-18: Echelmayer et al. and Sapiano et al. are missing in the reference list

396, ln. 18: it is a bit misleading if “the latter three” refers to the last three citations within the last brackets (I know that this is what you mean) or to the last three of the four methods mentioned. Maybe change the wording here.

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

C235