The Cryosphere Discuss., 8, C1844–C1848, 2014
www.the-cryosphere-discuss.net/8/C1844/2014/

The Cryosphere
Discussions
Open Access

Interactive comment on "Surface elevation and mass changes of all Swiss glaciers 1980–2010" *by* M. Fischer et al.

Anonymous Referee #1

Received and published: 22 September 2014

General Comments

This paper provides an updated assessment of the changes in volume of Swiss glaciers. A new DEM is used which provides an opportunity for a reassessment of changes during the period 1980 to 2010. The authors investigate geometric controls on the mass balance distribution, such as mean slope and aspect.

This manuscript presents important information on regional patterns in glacier mass balance. I have two general comments followed by specific comments below:

DEM differencing / Error Assessment: The authors assume errors in each of the DEM products are uncorrelated, as implied by Eq. 4 where errors are added in quadrature, put provide no evidence that this is in fact true. Uncorrelated errors will be smaller

than those showing a correlation structure. A more robust approach is to quantify the spatial correlation lengths through an assessment of the difference map over stable terrain, following Rolstad et al, 2009 (J. Glac. 55/192), and applied for example in Motyka et al., 2010 (J. Glac. 56/198). The more sophisticated error assessment is needed because the author's current approach of taking the mean differences over stable terrain may mask spatial autocorrelation and its impact on the error distribution. In fact, Figure 4 seems to indicate the elevation differences over stable terrain do have strong autocorrelation structure.

Source DEMs and Validation data: The authors choose the dataset of Huss (2010a,b) for validation purposes. This creates some confusion because the Huss 2010 data relies on at least one of the same DEM products (the DHM25 Level 1 data) used in the present analysis. Therefore, the uniqueness of this validation product relative to the new analysis here comes into question. Furthermore, the Huss 2010 data utilizes the SRTM product, which the authors identify as problematic in their introduction. The authors should provide more justification for their use of the Huss 2010 dataset for validation purposes.

Another product used by Huss 2010 is a series of DEMs from aerial photographs, presumably the same ones mentioned here on P4585, and implied to have higher quality than the DEMs used for the present volume change assessment. If better quality DEMs exist than the Swiss ALTI product, it is not clear why they were not used here. The authors should justify their choice of DEM products relative to other work done in this area.

Specific Comments

P 4582, Line 20: Replace "The currently observed atmospheric warming caused striking. . ." with "Recent atmospheric warming has caused increased mass loss. . ."

P 4583, Lines 1-2: In addition to the mass losses reported since the mid-1980s, make a statement about the longer-term losses observed in this region.

Line 4: Change "...data is..." to "...data are..."

Line 9: Delete "also"

Line 25: "might cause problems" is quite vague. Specify that Berthier et al found systematic biases at high elevations.

P 4584, Line 16-17: "In number, small, thin..." is unclear. Instead provide some specific information from the inventory (e.g. ranges of glacier sizes and thickness found in this region). Also, "glacierets" is a standard term (according to the Cogley et al glossary), but "ice patches" is not. Unless you define a difference between these, just use one.

P4585, Line 3: Do these vertical accuracies vary with elevation due to poor contrast of snow covered areas in the accumulation area? Also, is there any information on horizontal accuracy of the DEMs?

Lines 16-23: Can these DEMs from airborne photogrammetry be used as a formal independent check on the DEMs you actually use, rather than the rough quality assessment (< +/- 1m) given here? If you do bring the independent DEMs into your analysis then state clearly what their accuracy is and how geodetic controls were applied.

P 4587, Line 18: The value of 850 +/-60 kg m-3 is from Huss (2013) directly, and so that reference needs to follow this sentence. It is true this compares well with data from these other references, but the primary reference should be Huss (2013). P4591: A histogram showing the elevation change distribution and standard deviation bounds would help the reader to visualize your results. Also, are the DEM offsets normally distributed? If not, the IQR is a more suitable statistic (see Larsen et al., 2007).

P 4592, Lines 8-14: DEM co-registration is an important step prior to DEM differencing, especially in mountainous terrain where small planimetric offsets can result in large errors. If you have done the co-registration following Nuth and Kaab, I suggest including this formally as one of your processing steps, instead of at the end of your error

assessment.

Lines 19-20: "Partly differs significantly..." seems contradictory.

P 4593, Section heading: The following sections include both results and discussion. Rename the section, or separate out discussion elements into a different section.

Lines 1-2: this is a run-on sentence

Lines 6-7: Reword this to state that the area distribution changed such that the maximum area is now at a higher elevation. In the current formulation, the sentence implies the ice moved upstream or that there was a thickening at that elevation.

Lines 8-9: The section heading is "changes with altitude" but this sentence presents your total delta V for this first time, which is not specifically a result to do with hypsometric changes.

P4595, Line 8: Report the significance level of all of your correlations (p value). A low or high r value is not an indicator of statistical significance.

Lines 11-15: Can the authors provide some statistics on the strength of the trends that emerge when examining mean values for 5% quantiles?

Lines 15-16: If the area/mass balance relation is statistically robust, it has important implications for regional mass balance assessments based on conventional mass balance data. See the recent findings of Gardner et al. (Science, 2013), who propose that the offset between modern geodetic and older mass balance assessments based on conventional mass balance programs could be due to the bias of small glaciers toward more negative mass balances.

Lines 16-17: This sentence is not formulated quite correctly. The work of Johannesson et al (1989) relates the glacier response time to a characteristic ice thickness and the rate of elevation change at the terminus. Clarify how this theory predicts a glacier's response to a shift in climate as a function of the appropriate parameters. Also, see the

later work of Harrison and others (macroscopic theory of glacier response to climate). P4596, Section 6: Some but not all of these comparisons have error bounds reported. Can the authors include errors on all of their reported values? This will aid in assessing if the differences are significant or not.

P 4597: Lines 26-27: "...implies that only ice would have melted." This is an incorrect statement. Assuming a density of ice is equivalent to the application of Sorge's Law, which assumes the rate of accumulation and firn densification are time invariant (see Bader, 1954). Snow still melts during the measurement period, but the net loss of mass is in the form of glacier ice.

Lines 27-28: The authors should assess the impact of the density assumption on their mass balance calculations by performing calculations over a range of density values (see for example Johnson et al., 2013, J. Glac.). Then you can say definitively whether the density assumption accounts for the discrepancy with other studies you cite. Fig 1: I assume the tick marks are in units of decimal degrees? If so place a degree symbol on the labels, here and throughout.

Fig 6b: Change caption to read "volume change" not "volume loss". Specify in the caption which x-axis relates to which plotted element. Shouldn't the zero location of the lower and upper x-axes be at the same horizontal location on the plot?

Interactive comment on The Cryosphere Discuss., 8, 4581, 2014.