Reply to comments by Anonymous Referee #1:

1) 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.

> Chapter 4 (accuracy assessment and validation) will be changed and extended as we will reassess the uncertainties of our methods and results according to all reviewers comments on these issues, including a re-evaluation of the question whether errors of the DEMs used are spatially correlated or not. We will try to apply the approach by Rolstad et al. 2009 and applications thereof (e.g. Motyka et al. 2010) to our study.

2) 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.

> The validation data relies on ice volume changes derived from a series of high-accuracy photogrammetrical DEMs for sub-decadal to multi-decadal time intervals (see section 2.3). Hence, the DHM25 Level 1 DEMs which we used for the glacier surface topography at the beginning of our observation period are not used for validation purposes. Also, neither for/in Huss et al. 2010a (GRL) nor for/in Huss et al. 2010b (Erdkunde), which we refer to for validation, the SRTM DEM was used. We have clarified this issue in section 2.3.
>
> "Time series of surface mass balance for glaciers of different type and size class covering the entire Swiss Alps over the last decades (Huss et al., 2010a,b) are used to validate the geodetic mass balances presented here. These series rely on ice volume changes derived from high-accuracy photogrammetrical DEMs for sub-decadal to multi-decadal time intervals (Bauder et al., 2007). By using a distributed mass balance modelling approach including comprehensive field data (winter accumulation, summer ablation and discharge measurements), annual mass balance series were calculated that agree with the observed geodetic mass changes."
>
> We clarified our choice of DEM products relative to other work in Switzerland.

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

Reformulated accordingly.

"Recent atmospheric warming has caused increased mass loss of mountain glaciers all over the world (e.g. Zemp et al., 2009; Radić and Hock, 2014), …"

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.

Now extended and rewritten accordingly.

"Glaciers of the European Alps showed general mass loss and shrinkage since the Little Ice Age (LIA) maximum around 1850 (Zemp et al., 2008), despite intermittent phases of positive mass and area changes around 1890, during the 1910s and from the late 1970s to the mid-1980s. Since then, pronounced glacier retreat has been reported again (Paul et al., 2011; Huss, 2012)."

P 4583, Line 4: Change "…data is…" to "…data are…"

Implemented accordingly.

"Mass balance data are available either from …"

P 4583, Line 9: Delete "also"

Deleted.

"… (DEMs) available worldwide and the fact that inaccessible areas and entire …"

P 4583, Line 24f: "might cause problems" is quite vague. Specify that Berthier et al found systematic biases at high elevations.

Now clearer.

"Furthermore, applying the medium-resolution SRTM DEMs to high-mountain areas is problematic due to the systematic biases reported for high elevations (Berthier et al., 2006)"

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.

Following these comments, the whole paragraph was rewritten.

"The study area covers the entire Swiss Alps, where glaciers generally showed rapid mass loss until today after a short period of mass gain between the late 1970s and the mid-1980s (Huss et al., 2010a). Overall, small, thin and rather steep glaciers dominate. In number, almost 90% of all glaciers were smaller than 1.0 km^2 in 2010. At that time, the total glacierized area was 944.3±24.1 km^2, corresponding to an area change of –362.6 km^2 (–27.7%, or –0.75% yr^{-1}) since 1973 (Fischer et al., 2014). If we apply the approach by Huss and Farinotti (2012) to all Swiss glaciers, the average estimated mean ice thickness was 63 m in 2010, and 92% of the estimated total ice volume of 59.9 km^3 was stored within only 10% of the 1420 glaciers recorded in the SGI2010."

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?

In order to answer these questions and give more precise information on the accuracy of the DHM25 Level 1 DEMs, we rewrote and extended the corresponding text passage as follows:

"The positional accuracy is reported to range between 2.5 and 7.5 m. The vertical accuracy was estimated by comparison of known spot heights with corresponding cell values of the DHM25 Level 1 DEMs and ranges between 3.7 and 8.2 m for rugged high-mountain topography depending on individual map sheets. Because spot heights are typically located at topographical extreme points like hilltops or depressions, the actual vertical accuracy over "average terrain", as for instance glacier surface topography, is probably considerably higher (Rickenbacher, 1999; swisstopo, 2000)."

P4585, 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.

The DEMs reviewer #1 is referring to here will be used as a formal independent check of the DEMs we actually use. – We will integrate this in chapter 4.

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).

Implemented accordingly.

"…is set as a constant of 850±60 kg m^{-3} (Huss, 2013), which is consistent with…"

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).

Now implemented as suggested by reviewer #1.

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.

We calculated the influence of co-registration according to Nuth and Kääb (2011) for the 45 largest glaciers. Because the co-registration of the source DEMs prior to the DEM differencing had only a negligible influence on resulting mass changes (changes inferior to uncertainty of the mass changes), we did not co-register the source DEMs prior to DEM differencing. We reformulated the corresponding text passage in order to be clearer.

"We assume this shift to originate from the creation of the DHM25 Level 1 source data and therefore calculate the influence of its correction via co-registration according to Nuth and Kääb (2011) for the 45 largest glaciers spread over the entire Swiss Alps and covering 650 km^2 at t_1. Because the effect of this correction on the average mass balance of individual glaciers turns out to be in the order of $\pm 10^{-4}$ to 10^{-2} m w.e. yr^{-1} and is always smaller than the uncertainty in the derived average mass balance from 1980 to 2010, i.e. smaller than ±0.03 m w.e. yr^{-1}, we consider the effect of the detected DEM shifts on calculated surface elevation, volume and mass changes as negligible and therefore do not co-register the source DEMs prior to DEM differencing."

P 4592, Lines 19-20: "Partly differs significantly…" seems contradictory.

Considering Figure 5 we do not think so. However, following reviewer #3s comments on P 4592, Lns 19f, we reworded this text passage.

"For individual glaciers, mean mass balance from Huss et al. (2010a,b) partly differs considerably from our results…"

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

Section renamed to "Results and first findings"

P 4593, Lines 1-2: this is a run-on sentence.

Text passage rewritten accordingly.

"The recent (t_2) area-altitude distribution was derived from the combination of the SGI2010 with the swissALTI3D DEMs. For t_1 the SGI1973 and the DHM25 Level 1 DEMs were used."

P 4593, 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.

Reworded accordingly.

"The most heavily glacierized areas were located at almost 200 m higher elevation in 2010 (Fig. 6a)."

P 4593, 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.

The section heading is now "Changes in surface elevation and area-altitude distribution"

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.

p values of all correlations are $<1.0*10^{-6}$. Therefore, the given correlation coefficients are significant. For clarification, we added p values in Figure 10.

P4595, Lines 11-15: Can the authors provide some statistics on the strength of the trends that emerge when examining mean values for 5% quantiles?

Correlation coefficients for 5% quantile mean values and corresponding p values of are now included in Figure 10.

P4595, 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.

Following our response to reviewer #1s comments on P 4595, Lns 11-15, we added information about the robustness of the trends for mean values of 5% quantiles. For average area 1973-2010, the correlation is negative. However, if the smallest glaciers (<0.1 km^2)are neglected, mean average mass balances did not vary considerably for different size classes (see 5% quantiles in Fig. 10a). On a global scale, our results would therefore rather not support recent findings by Gardner et al. (2013), who suggest that regional mass changes derived from conventional mass balance are biased towards more negative mass balances of smaller land-terminating glaciers.

P4595, 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).

Also due to our implementations of reviewer #1s comments on P 4595, Lns 15-16, we deleted the sentence reviewer #1 is referring to here.

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.

We did not report error bounds for the two reported values because they were not directly derived from the DEM differencing but from our time series of annual mass balance (see section 3.1, temporal homogenization via mountain-range mass balance data) in order to compare to reported values of other studies over the same time intervals. These values have an additional uncertainty component resulting from the temporal homogenization which we can not exactly determine.

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.

We rewrote the corresponding text passage.

"Assuming a density of the volumetric change of 900 kg m-3 implies that both the mean firn density and firn thickness and area would not have changed within the observation period. For glaciers in the European Alps, however, significant changes in both firn coverage and density are reported (e.g. Carturan et al., 2013; Helfricht et al., 2014)."

P 4597, 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.

Here we compare our results to the study of Paul and Haeberli (2008). As they did assume a constant density of the volume change of 900 kg m^{-3}, it does not make sense to further perform calculations over a range of density values. We assumed the same density of volume change of 900 kg m^{-3} here in order to find out how much of the disagreement in calculated average mass balance between Paul and Haeberli (2008) and our study is related to the difference in density of the volumetric change. We can therefore exclude effects as described in Johnson et al. (2013).

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.

Implemented accordingly.

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?

We replaced "volume loss" with "volume change". We argue that there is no real need to further specify in the caption which x-axis relates to which plotted element (stated in the legend). The zero location of the lower and upper x-axes are now at the same horizontal location.