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

Anonymous Referee #2

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Fischer et al. compared digital elevation models of all Swiss glaciers from ca. 1980 and 2010 to measure their 30-yr mass balance. A temporal homogenization is performed to take into account the differences in acquisition dates of the DEMs. This accurate and temporally homogenized dataset will be extremely valuable to understand the variability from one glacier to another and also for modelers who will have here a unique opportunity to test the ability of their mass balance model to reproduce this very well-measured 30-yr glacier mass change. This is an important study. The paper is also well-written and well-illustrated. A few clarification are still needed.

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General comments

The authors could indicate that their error estimate (@ the 1-sigma confidence level, right?) for individual glaciers is probably conservative because they do not account for the reduction of the error that results from averaging over a large number of pixels on each glacier. But I believe their choice to stay on the conservative side of thing is best. At the scale of the Swiss Alps (Eq 5) the authors assumed that the error for each individual glacier is independent from the neighboring glacier (and thus errors are summed in quadrature) resulting in a very small uncertainty of 0.03 m w.e./yr. This is not so conservative. Can the authors justify that the errors for individual glaciers are uncorrelated? Also the authors should add an error for the temporal correction factor, apparently not included yet.

Difference with Paul & Haeberli, GRL,2008 (PH2008) 1980s-1999 mass balance estimate. The authors state P4597 L4 that those differences are "considerable" (-0.78 m w.e./yr in PH2008 versus -0.60 m w.e./yr in the present study for the same time period, error bar to be added on the latter value by the way). I would not describe those difference as "considerable", no need to "hammer" an earlier study this way. Unfortunately PH2008 did not provide some error bars in their study but their uncertainties would have been probably relatively high (compared to the errors bars from the present study) and thus the differences would lie within the uncertainty. In fact, I find those differences quite reasonable and one can even speculate more quantitatively about the origin of the systematic differences (as Fischer et al. already do). If all of the difference (PH2008 have a Swiss-wide mass balance more negative by 0.18 m w.e./yr for this 14 year time period) is attributed to the penetration of the SRTM C-Band radar signal into snow and ice (under the sept-1999 surface), one can inferred a mean penetration (0.18*14/rho) of about 2.8 to 3 m (depending on the density used 0.85 or 0.9). A value that makes sense if compared to recent papers on the topic (e.g., Gardelle et al., JoG, 2012; Melkonian et al., JoG, 2014) although unfortunately, to my knowledge, no specific estimate of the SRTM radar penetration depth has been made in the Alps. I leave it to the authors to decide if such a discussion would fit in their revised MS. My opinion is yes, it is worth providing those back of the envelope estimates of the SRTM

penetration (or improved ones taking into account the 24% difference due to the fact that the density used was different in the two studies + 17% due to the errors in time span).

A proposed addition to the paper would be to compare at the basin scale (Table 1) or at the individual glacier level, the relationship (or the lack of it) between area changes and mass balance. The authors have all the data to so.

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Specific comments

Title: I suggest adding "during" before "1980-2010"

P4582

L9. I do not think "resulting" is needed

P4583

L2. Authors could reference here some non-Swiss studies (from colleagues like Abermann, Carturan or Vincent)

L4. Maybe add here one reference for the Swiss Alps (Paul, GRL, 2004) and one for the French Alps (Gardent, GPC, 2014)?

L25. "cf." not needed before a reference (true in general not only here)

L27. "most accurate" is unclear. Do the authors mean more accurate than the SRTM DEM? It is difficult to define the accuracy needed for the DEM to be useful because it depends a lot on the time interval between the compared DEMs.

P4584

L8. "a consistent period, 1980-2010"

L9. "used" not needed I think

L10. "accompanying studies of this type" not needed (seems obvious that you will analyze the source of errors from your method)

L12. Is "comment" the right word? Maybe "analyze"?

L24. " at the time of the beginning of the observation period" does not read well. What about "The initial glacier surface topography"

P4585

L2. Clarify if you did or not the interpolation. Are those DEM available freely (the old and recent ones)? If yes provide the URL.

L3. "estimated" is not needed. If it is reported, it has necessarily been estimated. L5. Why only contour lines? Spot heights also I guess.

L7. cf. not needed

P4586

L20. "identical coding scheme" is unclear to me

P4587

L14. "balcance" –> "balance"

P4588

L5. Why providing the equation for a year i and not the full equation for the 1980-2010 period? I think it would make the understanding of the equation easier and in closer agreement with Figure 3. In fact, it was not entirely clear to me how the adjustment was performed. Did the authors take into account the fact that glacier-wide mass balances can vary by a factor of 4-5 (and more from your figure 10...) from one glacier to another while temporal variations are known to be rather homogeneous at the scale of the mountain range? For the adjustment one could imagine a scaling factor for each glacier that would be the ratio of the individual glacier mass balance for the observation period

and the mountain range mass balance for the same period. This factor would then be applied to the correcting term. Maybe this is already what the authors did (according to Figure 3) but if so, it could be better described in the text.

L12. "Analysis of control". I wonder whether this is really "Method". the paragraph rather provide the background behind the study of the factor control the variability of glacier mass balance. This paragraph could probably be split between the introduction (background) and section 5.3

L13. What do the authors mean by "representative"?

L19. Another relevant reference is (Vincent et al., 2005) showing a factor of more than 4 for the cumulative mass balances in the French Alps

P4589

L14. "to be able in explaining" sounds a bit weird. To be checked. Maybe "to be efficient in explaining"?

L19. The "latter" is unclear because the previous sentence enumerated different variables. Make it clear what sigma_dz is (although obvious). The authors should also make it clear whether they discuss (as I believe) uncertainty at the 1-sigma confidence level.

P4591

L11. Was this mean difference of -1.7 m between the DEM corrected or not? Is this mean difference varying spatially? (it is varying with altitude and is stronger at altitudes where the glaciers are located by the way...). This value (or better its spatial and altitudinal variation throughout the study area) could be used as an estimate of the systematic error for the elevation difference, a source of errors not accounted for yet. It would result in a more reasonable estimate of the mass balance error (in particular at the scale of the Swiss Alps).

L17. Although I find it interesting, it is not very explicit why the authors calculated this stochastic errors (not used elsewhere I think). Also, in this equation "n" should be the number of independent measurements. It is known that there is some spatial auto-correlation in the errors on the elevation difference so that the number of effective sample is lower than the total number of pixel (Rolstad, JoG, 2009).

L20. "on average": was the error calculated for the stable terrain around each individual glacier and then the average computed? Not entirely clear to me.

P4592

L5. "a slight horizontal shift"

L9-10. Magnitude of the correction for individual glaciers?

L22. In Figure 5, I suggest that the authors add the magnitude of their formal error estimates calculated for each individual glaciers so that it can easily be compared to the mass balance differences and would confirm that this formal error estimate is sound.

P4593

L18. Still existing in 2010?

P4595

L10. I suggest replacing "good" by "stronger"

L11. The reader wonder why 25% was chosen and not, for example, 10% or 50% ? Can the authors justify their choice? Did they test different values and chose the one that led to the higher correlation?

L13. "5%-quantiles of the data" could be explained a bit more (Did the authors separate the whole sample into 20 bins with an equal number of samples in each bin and then compute the MB average in each bin, right?).

L17. " because of their longer response time" is not an explanation by itself. Clarify.

P4596

L9. Again not clear why a shorter response time means a less negative mass balance. Is it because those glaciers already adjusted rapidly to warming since 1980s (or since LIA?) so that they already reached a state (at higher elevations) where they are closer to equilibrium to climate and thus do not respond so strongly?

P4598

L21. Did a few glaciers exhibited significantly positive mass balances?

Conclusion: are this dataset available to others? e.g., modelers?

Table 1 sigma_B_ref is defined in the main text but I do not think B_ref is.

Figure 4. Author could add an inset with the distribution (histogram) of the elevation differences on the stable terrain with basic statistics such as mean, media, standard deviation..

Figure 5. The bold dashed line could be made more different (color? Bolder? Dotted?) than the 0.1 intervals. Did a regional pattern emerged in this differences (that would be related to regional biases in the DEMs)? It was not entirely clear to me if the mass balance that are compared here cover the exact same time period (the text say "we choose 31 glaciers from the datasets of Huss et al. (2010a, b) for which volume changes based on the independent, photogrammetrically derived DEMs show closest temporal accord with our respective measured period". Did the authors performed a temporal homogenization before this comparison. If not, Figure 5 should show the time period covered by the photogrammetric DEMs.

Figure 7. Are three digits needed? The fonts could be larger to improve readability.

Figure 8. Why adding the average mass balance for the entire Swiss Alps here?

Figure 10d. Define the whisker plot (because not all authors use the same representation)

Good luck with the revision of the paper.

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