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Interactive comment on "Decay of a long-term monitored glacier: the Careser glacier (Ortles-Cevedale, European Alps)" *by* L. Carturan et al.

Anonymous Referee #1

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Carturan and others present a comprehensive analysis of long-term changes in Careser Glacier, Italian Alps. Since almost 50 years seasonal glacier mass balance is determined on this glacier. It thus one of very few such series worldwide and important for international glacier monitoring. Careser Glacier is an excellent example for the decay of glaciers under climate change, and the loss of this valuable time series to future monitoring is imminent.

I enjoyed reading this paper which is well written and clear. Although no new methods and only a limited amount of new data is presented the motivation for this overview study is well chosen, and the timing of this paper is perfect. The picture of the past evolution of Careser Glacier drawn by the authors is complete, and well suited for

publication in The Cryosphere.

There are however a few rather minor issues with the presentation of data and methods and their interpretation that should be addressed by the authors. I also have one more substantive comment – sorry for playing "devil's advocat" - that might trigger some additional discussion in this paper: Throughout the manuscript it is emphasized that the Careser series is highly valuable. But at the same time it is clearly shown that Careser Glacier is not at all representative for the region. Region-wide geodetic surveys showed that the Careser mass balances are more negative than for all other glaciers. This is explained by the authors with the particular topographic setting of the glacier. With the future decay of the glacier, the representativeness of the glacier will further decrease. For how many more years does it make sense to continue the measurements on Careser? And what might be the implication on the interpretation of Italian glacier mass loss in general, if this series exhibiting more negative balances than in the regional mean is abandoned? I suggest that the authors clearly comment on the issue of representativeness, and better point out the full value of the series in view of the regional glacier behaviour. In particular in connection with the projections of future glacier decay providing a detailed discussion and a time-scale for the potential continuation of the series might be very helpful. Also because several other mass balance series will be facing the same problems in the next years.

Specific comments:

- page 3299, line 14: It would be helpful, if the years for which maps are available were also mentioned in the text.

- page 3301, line 7: The uncertainty is given in terms of a RMSE. It would however be important to state both the RSME (stochastic uncertainty in the DEMs) and the systematic differences. If the mean error with the ground control point is significantly different from zero this might indicate that the whole image is shifted vertically, and thus the calculated geodetic balances are systematically wrong.

- Page 3302, line 15: Here and elsewhere: It is not quite clear what has been done / added in this study, and which results are already available from previous studies. In terms of the re-analysis of the whole series presented in this paper, it might be useful to re-compare geodetic and glaciological series (see e.g. framework in Zemp et al 2013, TC) and not just to qualitatively state published findings.

- Page 3303, line 12: Please be more specific here. What is the misfit at cross-over points?

- Page 3304, line 15:Is there a reason for the very strong retreat in a short time? Dead ice no longer counted as glacier? Topographic step?

- Page 3305, line 15: Why “constantly”? The DEMs do not cover decadal periods with glacier mass gain, but the long-term rate of loss is not at all constant.

- Page 3305, line 25: See also comment above. Please be more quantitative here. The meaning of the value 0.1 m w.e. is not entirely clear. Provide comparisons of geodetic and glaciological surveys besides the overall period also for the individual DEM periods, and state the differences.

- Page 3306, line 13: You probably mean “elevation loss” instead of “melting” here

- Page 3307, line 8: Is there actually an overdeepening in the bedrock? From the last Figure it is clear that there’s a confined area of maximum thickness, but with no clear “hollow” in the bedrock as stated here.

- Page 3307, line 21: Holocene

- Page 3308, line 28: I cannot quite follow this explanation – please clarify. Furthermore, I do not agree that cumulative / or mean mass balances are used for comparing glaciological and geodetic surveys. It would be much more robust to compare absolute volume (or mass) changes. As the glacier area changed quickly and probably non-linearly during the last period, the area over which the glaciological method is integrated is of major importance to calculate cumulative mass changes from the direct

glaciological method. If the area is kept constant (on the 2000 extent) it is not surprising the direct method is too negative.

- Page 3310, line 6: the multiplication of the glacier response time with the factor 2.9 given by Raper and Braithewaite (2009) is quite difficult to understand but is relevant for the conclusions that the authors draw from their results: only with using this correction factor the response time of Careser reaches the century time-scale (which in my opinion is exaggerated). The authors conclude that Careser is still dissipating its Little Ice Age thickness based on their very long calculated response time. Some additional argumentation is needed here: How robust is the correction factor? Is it applicable to different glacier types and sizes? How exactly is it motivated. In particular, it might be useful to compare the calculated response time to other published values for alpine glaciers. For example Oerlemans (2009, JG) / Oerlemans (2012, GA) finds much smaller response times for glaciers that are quite a bit larger than Careser (partly less than a decade!)

- Page 3311, line 2: The authors might consider citing Huss et al (2012, JG) here who discuss the two opposing effects mentioned here in some detail for a large sample of alpine glaciers with different morphology.

- Page 3311, line 10: When looking at the figures and tables it is really striking to me that in about 1981 a very clear regime shift occurred. Before the glacier often had an accumulation area, whereas after this date almost exclusively ablation took place. What has happened in this year? Several papers document this regime shift for other parts of the Alps, and might be discussed here: Marty, 2008, GRL; Eckert et al., 2011, JG; Thibert et al., 2013, TC.

- Page 3312, line 28: “series of SEASONAL mass balance measurements”. This seems to be important to me because very few long-term series are seasonal.

- Table 3: It might be good to provide the area that was used for evaluating the glaciological mass balance for every year. Only if this information is provided cumulative ice

volume changes can be calculated that can be compared to the geodetic surveys.

Interactive comment on The Cryosphere Discuss., 7, 3293, 2013.