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Discussions

Interactive comment on "Quantifying changes and trends in glacier area and volume in the Austrian Ötztal Alps (1969–1997–2006)" *by* J. Abermann et al.

Anonymous Referee #1

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This paper presents changes in glacier extent and ice volume for a large glacier sample in the Austrian Alps over the last four decades. The authors rely on new monitoring techniques based on Lidar that allow to produce a glacier inventory not only providing accurate glacier outlines and ice extent but also geodetic mass balances. The results are sound and well described and presented. The methods applied open new perspectives for the generation of glacier inventories providing a wealth of information about glacier changes in the past. For these reasons the results presented in this paper should be published. They are of high interest for the scientific community. However, I would like to make one major point concerning data interpretation where I disagree with the authors, and some smaller remarks.

My main objection against the data evaluation and discussion concerns the method of "estimating mean annual changes" (pages 421-422). In order to calculate mean annual changes from observed decadal to multidecadal area and volume changes the authors perform a reduction based on length change and mass balance data, trying to erase the effect of phases of glacier advance. For computing annual changes, the observed decadal change is divided by the inferred number of years with glacier retreat. I have objections against (1) the motivation of applying this methodology and (2) the way the reduction is performed.

(1) The authors intend to compare glacier changes in two periods of different length. To allow comparison, total changes in a period need to be converted to rates (annual changes). In my opinion, a rate of change (e.g. metre water equivalent per year) can only be obtained by dividing the total change by the total number of years. The authors focus on retreating periods; this implies that their methodology does not take into account the effect of the magnitude of mass gain in positive years. For example: 10 years with a mass balance of +0.1 m w.e. have the same effect on the mass budget as one year with +1 m w.e.; but the total change would be divided by a different number according to the proposed method. When comparing the magnitude of total mass change projected only to years with negative mass balance throughout the two periods, the interpretation in a climatic context becomes difficult and conclusions might even be misleading.

(2) The authors use the "arithmetic mean" of length change data to detect retreat periods (Page 421, line 14). Given the strongly different length change curves of the glaciers the evaluation of the arithmetic mean is not well suited for determining the time period in which the glaciers retreated. If the evaluation and interpretation of the results is performed in size classes, also indicator 1 and 2 should refer to classes. I expect large differences between classes having a strong impact on the results and conclusions obtained according to the authors' methodology. The same point also applies to the mass balance time series.

It would be valuable to perform a more careful analysis of the uncertainties in the ice volume changes based on the comparison of aerial photogrammetry and Lidar DEMs (apparently this is as well missing in the Companion paper (Abermann et al, 2009)). The authors state a vertical accuracy of <1.9 m for aerial photogrammetry. What is the elevation uncertainty for Lidar DEMs? Into what overall uncertainty do these errors translate when calculating ice volume changes? As the overall mean elevation changes detected range between –5 and –10 m (Table 3) an assessment of these uncertainties is important in order to yield reliable results on decadal mass change.

Besides an error analysis, a comparison of the DEMs in glacier-free regions could easily show whether there are strong biases between the data sources and allow some quantification of the error. Furthermore, it would be very interesting to see if the geodetic mass balances derived for 1969-1997-2006 are in line with the glaciological mass balances of Hintereis, Kesselwand and Vernagtferner. This might give an indication on both the accuracy of the DEM comparison as well as the traditional mass balance time series.

Detailed comments are listed below:

- **Page 416, line 26:** "glacier resources" – unclear. A glacier is not a resource for itself.
- **Page 417, line 24:** Here and also later in the text: The authors refer to the inventory of 1998. There seems to be some discrepancy with the year 1997 mentioned in the title and abstract. Although it is explained later, it could already be clarified here.
- **Page 419, line 6:** "glaciated" refers to time, "glacierized" to space and would be the appropriate wording in this context.
- **Page 420, line 5:** "... avoid apparent accuracies" – unclear

- **Page 421, line 21:** Do the authors assume that length change is an indicator for area change? If yes, this should be mentioned. But are these variables really directly proportional?
- **Page 422, line 15-:** The application of the reduction for the second period is obviously not consistent with the definition of indicator 3 and 4 (which seems to be based on the cumulative mass balance curve). For period 2 the reduction is performed based on the annual mass balances; is it the same way also for period 1?
- **Page 424, line 4:** It would be interesting to obtain some more explanation of the processes of changed glacier dynamics proposed here by the authors. After separation I would rather expect accelerated thinning as the terminus is not supported any more and flows faster. Maybe thickening could also be explained with above average precipitation over the last years.
- **Page 424, line 5:** Is an Appendix required? The two Tables could also appear in the main body of the paper.
- **Page 426-428:** As mentioned earlier I disagree with the calculation of annual rates of changes. These results have a strong impact on the different ratios F that are used as a primary source of interpretation and discussion of the results. I encourage the authors to reconsider this part of their methodology and update the interpretation of their excellent results.

Interactive comment on The Cryosphere Discuss., 3, 415, 2009.