Thanks to the three referees and M. Pelto for their very careful reviews of our manuscript that include many important points and will contribute significantly to the clarity of this paper.

One important issue which raised general concern with will be discussed separately before dealing with the other individual comments. The way we performed the comparison of area and volume changes between the different periods and the derived acceleration terms were critically judged, because of the special consideration of the mass gain period. The motivation in our first submission was the fact, that a direct comparison of mean annual changes in a period including a considerable glacier advance with a period consisting of a strong retreat only, will anyway lead to the conclusion that glacier retreat accelerated massively even though the first period may already have included a period of similar mass loss. Thus, we initially decided to deal with true glacier retreat periods, in order to obtain comparable parameters for area and volume loss.

Incorporating the referee's comments we saw the necessity to also offer a more objective comparison and thus introduced 'overall acceleration factors' where we include the whole periods between the glacier inventory datasets. We then compared these overall factors with acceleration factors of net-retreat periods and discuss their differences, potentials and limitations.

We therefore included Fig. 8a into the manuscript to demonstrate the way we calculate 'overall acceleration factors' (including all years of the advance as well) and 'acceleration factors of periods of net-retreat'. The differences and the potential of each method are discussed in the text.

Before answering the individual comments we like to discuss the objection related to the way we performed the reduction of the retreat periods of referee#1 since it is related to what we wrote above ('*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.'):

We agree that it is not straightforward to directly link the magnitudes of length changes to area changes and we did not do that. We used only the course of the length changes to identify the periods of net retreat. The comparison of the individual glaciers (Fig. 3) reveals indeed large differences. During the progress of the work we applied what referee#1 suggested and considered length changes in different size classes. However, it turned out that the sample number is too low (only 16 glaciers have continuous length measurements). In addition the length change measurements are unequally distributed within the classes (there is only one glacier smaller than 1 km² and none smaller 0.5km²; c.f. Tab. 1). The situation to derive volume change-periods is even worse, because only three glaciers with mass balance series exist in the investigated region. With the justification as formulated by I. Evans in his referee comment, 'to take the average so long as the duration of ‘non-wastage’ is approximately applicable to the regional total', we decided to perform this temporal reduction. In section 6 we also discuss and highlight the limitations of our approach: Long-term signals should be interpreted and not year-by-year changes.

In the following, the remaining comments are discussed in detail, with the referee's quotation given in italic. Language-related comments have been adopted in the revised version and are not listed here to keep the final author's comment concise.

Comments by M. Pelto:

'Page 421 10: This long sentence require at least one reference as to the response to climate change.'

We added a reference concerning the different response of adjacent glaciers within the study area (Kuhn et al., 1980).

'Page 423 6: What were the original sizes of the three glaciers lost?'

Information is added about the three glaciers that are not included in this study anymore due to their actual extent (<0.01km²). They originally belonged to the smallest class (0.01-0.1km²).

'Page 424 3: The contrasting change between Rotmoosferner and Wasserfallferner (Figure 5) indicates an important contrast between a glacier that has a healthy accumulation zone, equilibrium response to climate versus thinning over almost the entire glacier area indicating a potentially disequilibrium response (Paul et al., 2007 and Pelto, 2006).'

We agree, that this contrasting evolution is connected with these facts and modified this paragraph and added respective references.

'Page 425 22: Last sentence confusing. . . deceleration trend and acceleration trend of absolute area. . . The meaning can be discerned but not easily.'

We re-structured this paragraph in order to distinguish more clearly between the different evolutions of F_{A_retr} and $F_{A\%\_retr}$.

'Page 427 18: can be further noted that the greater relative percent reduction in volume than area indicates downwasting, which provides for future rapid area losses.'

We agree that our study supports this condition for the past decade and it is very likely to be true for the near future as well (as long as comparably thick glacier tongues are in low elevations).

'Figure 4: This is an important diagram. What would increase the value to show the relative area change for an elevation band. That is divide the absolute area change by the absolute area. The paper has tended to emphasize relative changes. This is an important relative change. It is noted on Page 427 10, that area losses have increased more strongly at lower elevation than higher elevations. This is in terms of absolute area change, but would it be the case in terms of relative % area change? Does volume change follow this same trend? It is easy to imagine that the same volume change-thickness decrease would lead to more area loss at lower elevations.'

We found this an interesting idea that has been mentioned by I. Evans as well and we therefore included Fig. 5c,where the vertical distribution of relative area changes are displayed. These changes are now discussed in sections 5 and 6.

'Figure 7: Is this relative change 1967-1997 versus 1997-2006 with no trend being 1.0, correct? Or does the relative percent change reflect rates from selected years as discussed on Page 425.'

As displayed in Fig. 8a and Tab. 2, the factor for relative changes is not 1.0 but 1.11 (for the sum of all size classes).

Comments by anonymous referee #1 (excluding the comments on the acceleration trend extraction discussed above):

'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 <1.9m 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 –10m (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. '

The analysis of uncertainties concerning ice volume changes has now been extended in the companion paper (Abermann et al., 2009). Vertical accuracies of the LIDAR-DEMs have been added in the revised version (+/-0.1m). The main part of the overall mean thickness changes (ca. -5m to -10m, Tab. 2) originates from glacier tongues at low elevations (e.g. Fig. 6b), where elevation changes in the order of 10m to 100m between the inventory dates occurred and thus exceed the error values strongly.

'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.'

This has been done in another study where geodetically derived mass balances of Hintereisferner are in good agreement (Kuhn, unpublished data), whereas Kesselwandferner and Vernagtferner fit less well to the glaciological ones. Possible origins of the deviations may be different basal melt conditions as well as vertical velocities that can well partly balance surface mass balance values at times. Also interpolation errors are to be mentioned that can influence glaciological mass balance values. However, we think that this is a very interesting point and should be addressed in a separate study in more detail due to its significance for mass balance as well as glacier dynamic studies. This future perspective has been added to the conclusion-section.

*'• **Page 416, line 26:** "glacier resources" – unclear. A glacier is not a resource for itself.'*

We changed this expression to glacier extent which is actually meant in this sentence.

*'• **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.'*

We changed this in the revised version.

*'• **Page 419, line 6:** "glaciated" refers to time, "glacierized" to space and would be the appropriate wording in this context.'*

We changed the wording into 'glacier covered' in contrast to 'glacier free'.

*'• **Page 420, line 5:** "... avoid apparent accuracies" – unclear'*

We changed this to '...avoid errors introduced by interpolation...'.

*'• **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?'*

We assume that the sign of length changes is an indicator for the sign of area changes (e.g.: it is very likely that a glacier with a retreating glacier tongue is losing area). We clearly do not assume that the magnitudes of length and area changes are directly proportional and do not imply this information in our methodology.

*'• **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?'*

The authors think that this is a misunderstanding and, unfortunately, do not entirely understand this comment. Both periods are treated the same way (extraction of net-volume loss through cumulative mass balance series). It is only one year in period 2, where an overall positive value of mean mass balances occurred that had been nullified in the interpolated mean curve already before the next year's value. Therefore 8 out of 9 years in the second period contribute to an overall mass and volume loss.

*'• **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.'*

We changed this paragraph and included also the possible reason of positive precipitation anomalies.

*'• **Page 424, line 5:** Is an Appendix required? The two Tables could also appear in the main body of the paper.'*

The original reason to add the tables in an appendix is their comparably large size. In the revised version we added them to the main body and agree that it makes reading easier.

Comments by L. Braun:

'When talking about the Mass Balance measurements (p. 419, line 22) it would be helpful to name the institutes doing the mass balance measurements and the link, where the data can be found i.e. Commission for Glaciology, Bavarian Academy of Sciences, www.glaciology.de for Vernagtferner'

We apologize for this missing reference and added it.

'Figure 1 caption: rectangle: extent of Fig. 5 (not 4) '

We changed this misspelling.

Comments by I. Evans (excluding the comments on the acceleration trend extraction discussed above):

'To address the acceleration question directly, the authors define four "scaling parameters", F. I suggest replacing this broad term by the more precise 'acceleration factors'. '

We changed this accordingly.

'It is interesting that these are greater for thickness and volume than for area and length. Fig.7 shows that the four acceleration factors differ considerably for glaciers <100 ha, but are fairly similar for larger glaciers: comment on this could be extended. Also the notion that separation of two formerly coalescent glaciers affects the rate of loss needs fuller explanation (and critical thought).'

We emphasized on the greater variation of the factors of the smallest size classes and modified the paragraph dealing with the possible reasons for the two differently reacting glaciers (RMF and WFF).

'From Fig.4 I infer that absolute annual loss has accelerated below 3100 m altitude, but hardly above. I agree with Pelto that relative loss curves would be useful, and more consistent with the discussion here. Fig.6 shows that % area losses are more evenly distributed across glacier size classes in the second period, and that annual rates accelerated for all except the smallest (<10 ha) glaciers. I would emphasise that the deceleration for the smallest is from a very high rate of area loss (4.3 % a-1) to a still considerable one (-1.9% a-1). It would be useful to compare the changed situation for smallest glaciers, before and after 1997, with studies of glacier change elsewhere.'

We added Fig. 5c showing the vertical distribution of relative area changes per elevation band and included a paragraph describing this distribution. We now added a sentence to the discussion of Fig. 7, in order to emphasize that also small glaciers are still retreating considerably but, in comparison to the very strong retreat of the first period, there is a deceleration. We performed a comparison with Knoll and Kerschner (2009).

'The authors cite no climatic data. As this is one of the few mountain areas with decent data, an obvious addition to the value of the paper would be to give relevant climatic statistics for each of the two periods, 1969-97 and 1997-2006. Reference to the work of Schöner, Auer and Böhm might be useful, e.g. Annals Glaciol. 31, p.31-38 (2000) and 46, p. 161-9 (2006).'

We added a Fig. 2 with the annual, winter and summer temperatures of the valley station Vent for the period 1969 - 2006 and a respective paragraph introducing this data in section 2.

' I find it strange that other papers on Ötztal glacier change (1969-92) are not cited; Paul, F. 2002 Int J Rem Sensing 23, p. 787- 799; also Eos 83 (23), pages 253, 260, 261. As acceleration of wastage is the most important theme of the paper, some reference should be nade to other authors who have discussed this, notably Haeberli and colleagues, e.g. 1999 Geogr. Ann. A 81, p.585-591.'

We included references of Paul(2002) and Haeberli in the respective sections.

'p416 line 22 ‘past 40 years’ is 1969-2009. But the post-1973 mass gain, followed (as detailed later in the paper) by 12 years before the mass of Austrian glaciers fell back to that level, is not a ‘minor exception’! - not globally, and certainly not in Austria: so reword.'

'40 years' has been replaced by 'decades' and 'minor' by 'few'.

'line 24 replace ‘temporal course’ with ‘time series’?'

By using the expression 'temporal course' we want to emphasize that we do not use quantities but simply the trend (course) of the series. We suggest: 'temporal course of the available mass balance series' instead of 'temporal course of the available mass balance measurements'.

'line 5 Please state whether +- is 95% confidence interval, or standard error.'

This is an estimated standard error that is derived empirically. The part of accuracy-estimations has been entirely changed in the companion paper and an additional sentence now refers to the submitted companion paper.

'line 7 ‘annual length and area’'

Due to the changes in this section the headlines have been modified so this headline does not exist anymore.

'line 20 I made it 28-13 = 15, not 16...'

That was a mistake: The periods of net retreat between the first two inventories in terms of area are: 1969/70 -1975/76 (7 years) and 1989/90 - 1996/97 (8 years) which makes 15 years in total. The calculations had been made with the correct 15 years in the original submission already

'line 20 state that area change is assumed to correlate highly with length change, over time.'

We added a sentence in this paragraph to clarify this from the beginning.

'line 21 ‘net area (and probably length) reduction.’'

We assume it was meant the other way round: 'net length (and probably area) reduction'; this is what we added in the paper. We hope not to have misinterpreted the referee's comment.

'line 24 is misleading unless space is inserted between 9 and the formula.'

Space is inserted between 9 and the formula.

'p424 line 3 begs the question, ‘how?’...Please explicate. I doubt if WFF received much support (buttressing) from RMF.'

We changed this part and included also the possibility that changes in the precipitation regime may have influenced the observed changes.

'line 19 etc. I suggest F = ‘acceleration factors’ rather than ‘scaling parameters’.'

We changed that accordingly.

'lines 21-25 These equations are clumsy; could be improved by using ‘annual change in area...’?'

Unfortunately the authors do not entirely understand what is meant. Should these equations be changed into a out-written form? We doubt that this would make it easier to read: annual net area change 1969-1997/annual net area change 1997-2006.

'line 10 usually we compare later period with earlier...'

This has been re-ordered.

'p430 line 1 insert page numbers, as this is now published'

This has been included.

'line 13 insert page numbers.'

This has been included.

'p433 Does Table 2 show overall change per size class (not average of changes per glacier)? - there is a difference...(If so, perhaps the 3 disappeared glaciers should be included?). Do the size classes refer to 1969, and do glaciers remain in the same class even if their area crosses a class boundary?'

Table 2 shows overall changes per size class. Since the size classes refer to 2006 what we also mention in the text ('For this purpose the glaciers are divided into size classes based on their area in 2006.') we disagree that the 3 disappeared glacier should be included. To avoid unrealistic area or volume changes within the classes glaciers did not change the assigned size class on the basis of 2006 (e.g.: a glacier with an area of 0.9 km² in 2006 but 1.1km² in 1997 is assigned to the class 0.5-1km² for the whole period.

'Usually I ask for more rounding, but in this case, annual rates in km2 a-1 might benefit from an extra decimal place (i.e., use ha a-1, losing the decimal point and initial zero...); acceleration factors too need one extra decimal place.'

We followed the referee's advise and changed the units of the rates of area changes from km²/a into ha/a and added this as a note to the caption. We also added one extra decimal place to the factors.

' Is -1.9% a-1 correct for glaciers <0.1 in 97-06? I make -11.8% in 9 years = -1.3% a-1.'

There was an error in the original submission. However, to account for the relative area change impact that changes with a changing glacier size, we calculated the mean absolute net-area loss and subtracted it year-by-year to calculate year-by-year relative area changes. Their mean would give the mean relative area change which is not exactly equal to dividing the overall relative area change by the number of years in which this change had occurred.

'p. 434 Table 3 As in Lambrecht and Kuhn 2007 p.181, thickness (height) change is related to initial area. I suggest that a 'wedge model' is more appropriate; a more representative

glacier surface height change is obtained by dividing the volume loss by the mean of initial and final areas. This gives the actual lowering over the surviving glacier surface, assuming that the slope of the deglaciated areas is linear (draw a cross-section!).'

We assume that this is a misunderstanding. As equations 1 and 2 reveal, we calculated mean thickness changes by dividing the overall volume change by the arithmetic mean of the area to account for the area having changed between the dates considered.

'p435 The caption implies that grey v. red shows changing glacier extent, but it shows mainly the more limited survey coverage in 2006, so please reword the caption.'

We reworded the caption; we agree that this could be subject to misunderstandings.

'p436 The point symbols are heavy (spreadsheet?) splodges; a more refined symbol (cross or open circle) is desirable.'

We changed this symbol.