We would like to thank both reviewers and the authors of the short comments for their careful and constructive reviews of our paper. In response to their suggestions, we did the following main changes to the manuscript:

1) we removed the term "glacierets" throughout the paper. Now we generally use "small glaciers" and clustered the 112 glaciers into two size classes for the correlation analysis. The threshold was set to 0.3 km^2, separating larger glaciers (with little scatter in their individual response) from smaller ones (with higher scatter in their individual response).

2) we provided definitions and additional information on the criteria which were used for distinguishing among different types of glacier (avalanche-fed, debris-covered, mountain glaciers and valley glaciers)

3) we used the suggested terminology and added the references which have been recommended by the reviewers

4) a proof reading for improving the English has been performed

5) we implemented the suggested changes and answered to the specific comments made by the reviewers, as detailed in the following of this document. The comments are reproduced in italic while the authors responses are reported right below. Line and page numbers are referred to the discussion paper.

Reply to the Interactive comment by M. Kuhn (Referee)

270/25 To the reference to Östrem and Brugman you could add the more recent publication of Kaser, Fountain and Jansson (2003, A manual for monitoring the mass balance of glaciers, Technical Documents in Hydrology 59, 107 pp.)

Ok, reference added.

275/21 Can evidence of motion be detected from Landsat images? 275/21 Is the difference between very small glaciers and glacierets well defined? Is it useful? Is it necessary?

We agree that this distinction is not necessary for the aims of our study and have removed the term "glacierets" throughout the paper. Now we generally use "small glaciers" and clustered the 112 glaciers into two size classes for the correlation analysis. The threshold was set to 0.3 km^2, separating larger glaciers (with little scatter in their individual response) from smaller ones (with higher scatter in their individual response).

275/26 Can you give a reference to the calculation of clear sky radiation?

Rephrased and reference added ("... which was calculated using the Solar Radiation Tools implemented in ArcGIS Spatial Analyst (Dubayah and Rich, 1995)."

277/27 Instead of "solar constant" I propose "Top of atmosphere irradiance" as its value changes over the course of the year.

You are correct, replaced with "top of atmosphere spectral irradiance"

278/16 What is avalanche fed, what is not?

To distinguish both types we used the source of nourishment codes as reported in the World Glacier Inventory (WGMS, 1989). The text has been revised accordingly.

282/11 Please define mountain glaciers, valley glaciers, glacierets.

The definition of valley and mountain glaciers has been added in brackets, glacierets are no longer mentioned in the manuscript.

282/23, 24 Can you define "avalanche fed" and "debris covered" in quantitative terms. I mean how large a fraction of a glacier needs to be debris covered?

Glaciers were classified as "avalanche fed", based on the source nourishment codes reported in the World Glacier Inventory (WGMS, 1989). Definitions reported in the "Glossary of Glacier Mass Balance and Related Terms" (Cogley et al., 2011) were used for discriminating between debris-free and debris-covered glaciers (i.e. glaciers that supports a layer of rock, dust or ash detritus on most or all of the surface of its ablation zone). Explanations and definitions have been added in the text, including the quoted reference.

283/14 Please give a quote for the calculation of clear sky radiation

A reference to the calculation of the clear sky radiation has been added in section 3.1 (see

comment 275/26).

285/6 "as found" – is this a positive statement (just as found) or negative (that was found) ?

Ok, reformulated and clarified.

286/3 How are the future losses calculated?

The losses are calculated using Equation 3. Clarified also in the text.

286/19 "is generally related to the avalanche activity" – how?

Via the avalanche ratio from Eq. 4. The text has been rephrased accordingly.

288/1 "+ 14 m" – how was this determined?

The calculation method has been clarified adding "area averaged" in the sentence.

292/9 "are slightly lower than previously assessed" - please quote the previous assessment

The previous assessments are fully reported in section 6.1. We would prefer to have the conclusions section without citations.

293/2 "occurred from 1965 to 1982"

We prefer to avoid mentioning specific years here, but we are now somewhat more specific.

Figures 9 and 10 are hardly readable, please enlarge to full page size.

We will check appropriate figure sizes when we have the layout.

Fig. 12 "white + red + blue = glacier area in 2009" ("glacier margin 2009" is not clear)

Legend for light blue changed to 'bare ice in 2009' (also added in the caption).

Reply to the Interactive comment by Referee 2 (anonymous)

The authors should add error bars in their estimates, e.g. in the abstract, results/ discussion/conclusions and they are urged to discuss the accuracy a bit more, maybe as a paragraph in the discussion. Error bars are not needed in every single result, but could be more evident in the paper than now.

We agree and added error bars for the main results in various sections, but think that accuracy is already well discussed in Section 4. We will thus refer to this section.

A bit English proof reading is also recommended. I have identified some places in the in the specific comments, but the list are just examples and suggestions, many more could be added.

Thank you for the suggestions in the specific comments. The text has been revised accordingly, and a proof reading for improving the English has been performed.

Some specific comments: 269, line 6-7 does not fit. rewrite

Ok, reformulated.

Line 9 – 'a band ratio' could be replaced with 'the band ratio method'. Divide sentence in two, thus. '.... manual corrections. Snow...'

Ok, done.

Line 12, replace now with specific time, 2000s?

Ok, replaced with "2009".

13/14, similar, over the last decade, if you mean 2000s be specific.

Ok, replaced with "2000s".

13 mean remaining snow? I assume at one point all glaciers were fully snow covered also in 2009

Ok, Reformulated.

17 could add error estimate, ±...

Error bars added.

18 emphasized instead of confirmed?

Ok, replaced.

270, line 11, what do you mean specific with feedbacks, too general, sentence could be rewritten and merged with the following one.

Ok, rephrased and merged.

16 add 'of glaciers' after response

Ok, added.

25, could also add reference to the Mass balance glossary by Cogley and others (2011).

Ok, reference added.

273, 26, mean low % cloud cover. add percentage.

Ok, added % of cloud cover.

274, 10, add reference to the met.data provider

The provider of the meteorological data series is Meteotrentino, which is already mentioned in the Acknowledgements.

274, 17. Say something on orthorectification first, were the images orthorectified and was the quality of the orthorectification checked?

Ok, sentence added with the required information.

21, how was the threshold selected?

Rephrased and explanation added.

275, 1, when and how was it carried out 2, assessing -> determining

Ok, reformulated and integrated with the required information.

275, 14, mean divides (not outlines)

Replaced.

275, 15 higher spatial resolution – compared to what?

Information added.

275, 22 could discuss how easy it was to determine glacierets from glaciers.

Based on suggestions from the other reviewers, we removed this distinction. See also the response to comment 275/21 of Referee 1.

276, assuming then A87 was always the biggest? add or explain

Yes, explanation added in brackets.

277, 13, define ha

Replaced by km^2.

23, unclear method description, it is common practice to describe methods before the results

We replaced the GlobGlacier (2008) reference with the PhD of G. Bippus (available online) where the procedure is described in full detail.

28, has been -> was

Ok, replaced.

278, 8, How sensitive is the threshold chosen to the results?

This has not been investigated in detail in this study as the threshold values were rather clear for the scenes used here. By changing the threshold value within acceptable limits it is possible to add or remove some snow pixels here and there, but these changes will not alter the further analysis and general conclusions.

278, 18, heavy sentence, suggest to rewrite

Reformulated.

279, first paragraph, the choice of AAR value of 1 needs to be explained/justified better

We removed this section as we no longer use the class glacierets.

12, remove first 'individual'

Ok, removed (279, 8)

280, 6, explain how this test was carried out and why this subset was chosen? Some information is given later, but this is confusing and the paragraph should be reordered and written more clearly.

The test has been better described now.

280, 11, what about other glaciers in the figure, zoom in figure or refer to them also

It is not possible to further zoom in, given the peculiar shape of Solda glacier. This heavily debris-covered glacier was chosen as an example, because it clearly shows the limits of automatic, not post-processed classifications from Landsat for this kind of glaciers. The other glaciers in the figure have much less debris-cover and are similar to all others. They have thus not received a special discussion.

280, 20, can it be strictly confirmed, writing 'in agreement' is better

Ok, replaced.

281, 1-2, could comment on the weather in this period, on how much melting it was.

A short sentence has been added for commenting the weather.

281,17, explanation to what, be direct

Rephrased.

282, 14, the terms ice body, glacieret, mountain glaciers should be defined in the paper.

Ok, definitions added.

283, 5, elevation (not elevations)

Ok.

283, 9 Are negligible changes observed? No notable changes may be better.

Ok, replaced.

283, 9-11 seems a bit odd.

Reformulated.

283, 11. The mean slope had increased....

Ok, rephrased.

285, the geodetic estimates are sensitive to many choices, including the density assumption, give error bar and discuss the uncertainty, suggest to add a subchapter in the discussion for this

Error bars have been added. Section 4.2 should already provide sufficient detail on the error sources. We have also added a reference to a recent study by Huss (2013) that describes the issue in full detail.

Table 4. Do you refer to the 1987-size or 2009 size. Could be stated in the table text

Ok, statement added.

Figure 4. It would be easier to compare if the Landsat and orthophoto were side by side with the same extent and scale.

We prefer the figure with the inset, to have them as close as they are now. Putting both side by side does degrade the comparison in our opinion.

Figure 6. Referring to 1987-area (see comment to table 4).

Ok, statement added in the caption, as for Table 4.

Reply to the Interactive comment by M. Pelto (Short comment 1)

270-16: Changes in velocity also fit in with the thickness area and length changes.

Suggestion added.

270-22: This decoupling in terms of equilibrium condition is more appropriately noted as a disequilibrium response. Paul et al., (2004) identified glaciers in the Swiss Alps that are disintegrating due to massive down-wasting, as undergoing a non-steady state response. Pelto (2006) noted that North Cascade glaciers experiencing a disequilibrium response to climate had thinning in the accumulation zone that was approximately equal to that of the terminus.

We have added this terminology and the references in section 6

273-6: There is considerable hydropower on the Adda River, Noce River and Adige River. It would be worth noting the output if possible.

We agree that this potential exist but have not found any useful information on it.

277-20: Temperate alpine glaciers survival is dependent on the consistent presence of an accumulation zone. If a glacier does not have a persistent accumulation zone, all areas of the glacier will experience thinning, including the former accumulation zone (Pelto, 2010). Low mean AAR values indicate glaciers that lack a significant consistent accumulation (Pelto, 2011). These glaciers cannot survive and are undergoing a disequilibrium response to climate.

We agree and discuss this concept in Section 6 where it seems more appropriate.

278-15: What is the suspected error in snowline altitude (SA) given the patchiness of the accumulation zone for many of these small glaciers with low SCA?

The error in estimating snowline altitudes has been reported at the end of Section 4.1.

279-4: An AAR of 1 as the equilibrium is not realistic for glacierets. Such an AAR year after year would lead to expansion before long. Instead given the no motion criteria these glaciers have years where they are dominantly snowcovered and years when they lose almost all their snowcover. This is the only mechanism that allows a stable area and volume. 282-17: The transition from glacier to glacieret is an important observation here. This is a uniquely large such identified group. Are there any key characteristics of the glaciers than ended up in this group?

Thank you for these remarks. Given that the difference between very small glaciers and glacierets is not always well defined, and that the distinction is not functional to the aims of our study, we decided to treat glacierets as the other glaciers in the imbalance analysis (i.e. we assumed a balanced-budget AAR = 0.5).

283-8: Retreat of the head of a glacier was noted by Pelto (2006 and 2010) as a key characteristic of glaciers in disequilibrium since this occurs due to thinning of what was the

accumulation zone.

Thank you for highlighting this concept, which is discussed in Section 6 (288-6), with reference to the study Pelto, (2010).

283-10: Negligible changes in glacier slope suggest thinning along an entire longitudinal profile that is not significantly different. How negligible is the difference in thinning with elevation?

For the purpose of our study it does not play a major role. However, we have described elevation changes and different thinning rates with elevation on glaciers with/without snow cover in section 5.2 .

284-23: The similar thinning and lack of any snow cover across the entire glacier again indicates a glacier without a consistent accumulation zone and one that cannot survive (Pelto, 2010).

We think the discussion of these concepts (and of the difference between active retreat and disequilibrium response) fits better with Section 6, where we discuss them more explicitly (289-23).

287-20: Ranzi et al (2010) should be referenced for the Madrone and Adamello Glacier changes that are also similar.

This study mentions the cited study of Maragno et al., 2009.

289-20: These glaciers then do not have a persistent accumulation zone, are experiencing disequilibrium and cannot survive (Pelto, 2010).

This has been made more explicit now (see comment 284-23).

292-28: The 50% reduction assumes a collective response. Given that many glaciers will be lost completely since they have no accumulation zone. The percent area remaining would seem to be more accurate, if it was determined for the group of glaciers that have a significant SCA, assuming total loss of the other glaciers.

We agree that such a number would be most interesting, but its determination would require some intense modeling which is beyond the scope of the present study.

Figure 1: Need latitude and longitude.

The figure has been modified accordingly.

Reply to the Interactive comment by Z. Kern (Short comment 2)

My brief comment concerns only a minor reorganization with $\alpha r=AAR_{09}/AAR_0$ derivation. Authors wrote that "Field evidence from glaciers subjected to direct mass balance measurements in the Ortles-Cevedale group indicates an average value of 0.5 for the balanced-budget AAR_0". Kern and László (2010) have shown that there is a size-dependency in AAR_0. The relationship between glacier area (S) and balanced-budget AAR can be optimally described by a logarithmic regression (balanced-budget $AAR_0=0.0648*lnS+0.483$); or as a crude estimation, an AAR_0 of 0.44±0.07 is best applied on glaciers with area in the range 0.01-1 km^2, 0.54±0.07 for glaciers covering area between 1 and 4 km^2 and 0.64±0.04 for glaciers larger than 4 km^2. Hence, I believe that field evidence show 0.5 as a mean AAR_0, because these glaciers are quite small, only a few are larger than 1 km^2, so their AAR_0 is expected to scatter around 0.5. However, a more realistic approach is to estimate smaller AAR_0 for smaller glaciers. Adopting the size specific AAR_0 approach the estimated current degree of imbalance (section 5.3) is expected to decrease. Consequently, the difference between current (i.e. 2009) snowline altitude and balanced-budget equilibrium-line altitudes (in section 6.2) can be expected to decrease, too. Similarly, the estimated area loss (50%) of the Ortles-Cevedale glacier system to reach equilibrium under current climatic conditions can be also expected to decrease if not the uniform 0.5 value but the size-specific AAR_0 value is used.

Given the huge scatter of AAR_0 values for glaciers smaller than 5 km^2 in that publication (Fig. 3) we think it is not a good idea to work with a size-dependent AAR_0. We fully agree that our calculations would change in the given direction if the size dependence would be valid. But for the often debris-covered and very small glaciers in our study region (i.e. the mass budget can be strongly influenced by local effects such as topographic shading and avalanches), we think that other factors than size could be much more important for the variability of the AAR_0. We thus prefer to stick to our proposed value of 0.5.

In addition, similarly to prof. Pelto, I remark that $AAR_0=1$ for glacierets is questionable. Tiniest ice bodies, with strong topoclimatic influence, frequently experience inter-annual negative or positive mass balance over the entire glacier surface (e.g. Hughes, 2008) and defining accumulation area for them is theoretically problematic. Maybe glacierets could be excluded from those parts when AAR_0 estimates are used.

We have decided to remove the somehow artificial distinction of glacierets from glaciers. They do all have now a 50% AAR_0.

p276 (eq.1) I guess A_{87} instead of A_1 should be written in eq.1. Meaning of A_1 is not explained, however, if I understood well, then volume change is calculated as elevation change above the initial area. The reference date (i.e. initial area) of the study is 1987, and the glacier area from 1987 is written as A_{87} latter in the text.

Corrected. Thanks.

Reply to the Interactive comment by S. Galos (Short comment 3)

Generally the paper seems to be informative and concise in the first part (up to roughly chapter 4) where the methods and results of the geodetic procedures are described, but gets more and more unstructured in the second part where the changes are correlated to topographic parameters and sometimes interpreted in a very general and subjective way.

We fully agree that other possibilities exist to structure these sections, but think that our way of following the structure of the methods section in the results and then summarizing two of them (area changes with controls and accumulation with imbalance) is a feasible way of doing it. The mentioned very general and subjective interpretation of the analysis is another topic. We are well aware that not too many studies have analyzed the relation between glacier changes and topography yet, so there is not much we can our results compare to. This is likely also the reason for the general and subjective interpretation we present here. By no means our statements should be seen as exclusive and we are looking forward to future studies that might provide further evidence.

The amount of references is very high and biased to the introductory part of the paper including many non-peer-reviewed reports (e.g. WGMS, CGI). Maybe the reference list can be upgraded by (i) focusing on the most important papers describing the methods used in detail and (ii) adding a few key publications where necessary.

We agree that the introduction has a high amount of references as the paper covers a wide range of topics and previous works should at least be shortly mentioned. As there are not too many journals publishing data records (e.g. mass balance measurements or glacier inventories) we have to cite the related reports here as well. But we would not say that they are so many. The reference list has been updated using the key publications suggested in the following specific comments.

All figures need improvements regarding the readability (e.g. font size) of labels, legends etc.

We will carefully check readability once we have the final layout.

3 Main methodological comments - 3.1 Decadal representativity of the snowline in late summer 1987 and 2009. Snowline determination and representativity of ELA-calculations from the Landsat images should be discussed more intensively. Field observations (e.g. Secchieri and Valentini, 1992) indicate that the hydrological year 1986/87 (2008/09) was a year with very low (high) winter accumulation in the Ortler-Cevedale group (second highest after 2000/01). This fact should be discussed in terms of ELA-representativity and possible bias in interpretation.

The mass balance series of Careser and Fontana Bianca glaciers show that the average AAR from 1987 to 2009 is not so far from the arithmetic mean of the two years (277/18-23) which are used in the correlation analysis (controls). Moreover, a low (high) winter accumulation is not necessarily linked to the annual ELA and AAR, which actually depend also on the summer weather conditions. The year 2009 was paradigmatic, since the

warm summer nearly completely offset the abundant winter precipitation, leading to AAR values which were only slightly larger than in the preceding years (Table 2). As can be seen from the mass balance series in the Alps (see for example in the following references), the winter accumulation in the investigated period has no trend, and more or less all the parameters (AAR, ELA, annual net balance) were controlled by summer ablation.

References:
http://www.cnrm.meteo.fr/icam2007/ICAM2007/extended/manuscript_226.pdf
Carturan, L., and Seppi, R.: Recent mass balance results and morphological evolution of Careser glacier (Central Alps), Geogr. Fis. Din. Quat., 30, 33-42, 2007.

The comparison of the AAR for 2009 for Careser (0.06) and Weißbrunnferner (0.24) with the decadal means (2000-2009, Table 2) shows that the accumulation area in 2009 was much larger than in the decadal mean (+500% for Careser, +100% for Fontana Bianca). The net annual mass balances for 2009 at Careser (-1268 mm w.e.) and Weißbrunnferner/Fontana Bianca (-622 mm w.e.) are by far less negative than the respective decadal (2000-2009) means of -1807 mm w.e and -1142 mm w.e. Similar is true for the 2009 mass balance of Langenferner/Vedretta Lunga which was the second least negative annual mass balance in the period 2003 to 2012. All this indicates that the derived snowlines for 2009 do not represent a decadal mean state of the accumulation area. This definitely results in a significant underestimation of the degree of imbalance of the glaciers in the region.

We are aware of this problem, which is more evident in the south-eastern (more snowy) part of the mountain group (where the four glaciers in Table 2 are located). Table 2 actually aims at highlighting this issue, and our degree of imbalance estimate is thus likely a lower-bound value. This is mentioned well upfront also in section 6.2 (page 290, lines 14-17), where we discuss the results of the imbalance analysis. Moreover, given the high variability of AAR values from glacier to glacier, the agreement in absolute terms is less important for our analysis than a good representation of the local variability.

3.2 Avalanches - By definition avalanching contributes to accumulation on the glacier surface (e.g. Cogley et al., 2011). Not considering this process may bias results. In the manuscript two main questions are not clearly explained: (i) How is avalanche snow distinguished from other snow? (ii) Why is it not considered in the accumulation area calculations? Of course, the high winter accumulation in 2009 also influences all kinds of avalanche related interpretations made in this paper. Thus, a focus on the representativity (comment 3.1) of the years of the satellite scenes helps to improve the manuscript and eases drawing conclusions.

Avalanche-fed glaciers were identified using the classification scheme from the World Glacier Inventory. This is now better specified in the text (page 278, line 15). Avalanche snow is considered in snow covered area calculations (SCA_{09} and AAR_{09} calculation, which serve for the imbalance analysis), while it is not considered for snowline altitude calculations (SA), for the reasons explained in Section 3.3. Avalanche snow has very little impact on the ELA as it does not cause a positive mass balance in the ablation region (at least for the larger glaciers). On the other hand, when glaciers are very small, avalanches might contribute substantially but the degree of contribution is generally not well known

(Kuhn, 1995) and therefore we excluded such topographically-controlled glaciers from the climate-related analysis. We thus think that the conclusions drawn are valid.

3.3 Controls of the observed changes - Three sections in the paper (3.4, 5.4 and 6.1) address the controls of the observed changes, but none is concise and easy to read. These chapters can be summarized into one clearly written part discussing controls of glacier area changes i.e. (i) changes of climate forcing (interesting data from Careser dam possibly including energy balance studies from Ortler glaciers (e.g. Senese et al., 2012)), (ii) glacier dynamics (unfortunately not mentioned at all) and (iii) maybe already including findings from the Ortler ice core. So far the discussion of the controls of the observed glacier changes is shallow and mainly consists of a few sentences presenting the long term data series of temperature and precipitation from Careser Dam.

Point i): Additional remarks on climate forcing and a higher level of detail concerning the energy balance over glaciers are beyond the focus of this study. We have included some references that analyze these relations in more detail. Point ii): We intentionally skipped to mention dynamic adjustment, as experimental or field data are largely lacking. A short sentence has been added for clarity in Section 6 (Page 289, line4). Point iii): The retrieved ice cores are still under investigation and cannot be used in this study.

4 Specific Comments - Eq. 1: explain A_1

We actually refer to A_{87}, corrected.

Eq. 2: Use ΔM instead of b to meet the Glossary for Mass Balance Terms (Cogley et al., 2011).

Ok, replaced.

Page 269, line 3: Responses to what. Please clarify!

Ok, added "to climate change".

Page 269, line 13: "...which fairly represents the extent of the accumulation areas over the last decade". We do not agree with this statement as explained in section 3.1 in this comment.

The small discrepancy between the actual extent of accumulation areas in the last decade and the extent derived from the snow covered area of 2009 is well presented (Table 2) and taken into account (Section 6.2) in the manuscript. As explained on Page 290, Lines 14-17: "The expected reduction (40% on average) rises to 52% if we take into account the small overestimation of the mean AAR in the last 10 yr by using the SCA_{09} (area-weighted mean difference of 0.06 in Table 2)". As mentioned above, representing the local variability correctly is more important than this bias.

Page 272, line 1: A reference to Kaser et al. (2006) would be appropriate here.

Reference added.

Page 273, line 6: Indeed, glaciers in the Alps are important for hydropower generation.

But in general glaciers in the Alps are insignificant for potable water supply and agriculture, otherwise give a reference.

Ok, rephrased.

Page 273, line 25: Give portion of cloud cover of satellite images.

Added in brackets at the end of the period.

Page 274, line 9: Add Verdetta Lunga to the list of glaciers with direct mass balance observations (since 2003/04). Data is reported to the WGMS and the Ufficio Idrografico (Bolzano) publishes annual reports.

Additional information added.

Page 275, line 23: Due to their small elevation range, the total surface area of glacierets is either accumulation or ablation, strongly dependent on the individual year of observation. Thus, a clear discussion of the representativity of the satellite images helps to interpret.

Based on this comment and on the suggestions of M. Kuhn and Z. Kern, we decided to skip the differentiation between very small glaciers and glacierets, which is not always clear, and is not necessary for the scope of our study.

Page 275, line 26 and throughout the paper: The term "clear sky radiation" is used in a misleading way. In a meteorological sense, "clear sky radiation" means global radiation on a day without clouds which already incorporates atmospheric transmission. Did you really account for that or do you mean a radiation potential, which is computed from the solar constant, an orbital eccentricity correction and the cosine of the zenith angle of the sun? In the latter case, we suggest using a less confusing term e.g. "mean summer solar potential".

The atmospheric transmission is actually included in our calculation so it is clear sky solar radiation. Clarified in the text.

Page 277, line 6: How is old snow distinguished from firn of multiyear age?

Snow showed a very different reflectance compared to ice and firn in the images analysed, but we agree that this is not always the case. Clarified in the text (Page 278, line 9).

Page 277, line 15+16: See comment 3.1 and in line 23 this statement is contradicted by an error of >100% (AAR from direct observation on Fontana Bianca 2009 = 0.10 and from Landsat scene = 0.22)

We are aware of these differences, which are presented in the paper (Section 3.3 and Table 2). However, the discrepancies do not jeopardize the essence of the results of the study.

Page 277, line 25: Add Bippus (2011) to the quotation of the GlobGlacier report as it is based on this PhD-Thesis.

The quotation of GlobGlacier has been substituted with the suggested reference.

Page 278, line 16: How was avalanche snow distinguished from "normal" snow and why was it not considered for the AAR calculation? A local ELA-lowering does not seem to be a satisfying explanation. What is the criterion/threshold to define an avalanche fed glacier?

Please, see our answer to the comment 3.2 (Avalanches).

Page 278, line 24: "Field evidence from glaciers subjected to direct mass balance measurements in the Ortles-Cevedale group indicates an average value of 0.5 for the balanced-budget AAR_0". This statement is a central assumption for this paper and therefore needs to be better proved! How is this value estimated/calculated? Gross et al. (1976) and Kuhn et al. (1999) find values around 0.66 for glaciers in the Eastern Alps. An AAR_0 of 0.5 is an estimate for maritime glaciers, but the climate in the Ortles-Cevedale Group might not be considered maritime (Fig. 2).

We took into account the three glaciers with long-term mass balance measurements in the Ortles-Cevedale group and we calculated the mean of their AAR_0 values in the observation period. Rephrased for clarity. The mean AAR_0 for the glaciers in the Alps is 0.6 according to the WGMS Glacier Mass Balance Bulletin. Due to the high diversity of glacier shapes and hypsometries, there is a high variability from glacier to glacier. Nevertheless, consideration of glacier-specific AAR_0 values (e.g. derived by modelling) is beyond the scope of this paper. We are thus aware that our analysis is only a first step in resolving the issue.

Page 279, line 4: AAR0 of glacierets: a glacier/glacieret cannot be in equilibrium when its AAR = 1.

The differentiation between glaciers and glacieret has been removed from the paper. Now we treat all the ice bodies in the same manner for the imbalance analysis (i.e. we assume a balanced-budget $AAR_0 = 0.5$ for all of them).

Page 280, line 2: Use "changes in glacier extent and accumulation area" instead of "snow cover change" which can be understood as seasonal snow cover change.

Ok, replaced.

Page 280, line 25: Can this value also be understood as the error for the whole mountain range, or is it very specific to the mapped glaciers? Please discuss.

It can be regarded as representative for the entire mountain range. Explanation added.

Page 282, line 17: Define the objective difference between valley glacier and mountain glacier. "All 14 valley glaciers kept their shape" can be interpreted as no change in area at all.

The definitions of mountain and valley glaciers have been added in brackets. Shape is used here as a topological characteristic, meaning that the overall shape has not changed during area change (e.g. no separation of tributaries or disintegration was observed). The quoted sentence has been reformulated for clarity.

Page 283, line 14: "reduction of clear sky radiation" is misleading. In this case irradiation does not decrease, but the glaciers receive less due to topographic effects such as surface lowering which enhances shading through surrounding topography. Please clarify!

Ok, reformulated.

Page 284, line 13: It would be interesting to compare this volume change to other mountain ranges in the European Alps.

As absolute volume changes cannot be compared among different regions, we provided a comparison of area-averaged net (specific) geodetic mass budget rates in Ortles-Cevedale with the results from other studies in the discussion.

Page 285, line 11 and Fig. 11a: The vertical profile of elevation changes must be discussed with glacier dynamics, otherwise it is of restricted meaning over such a long time period.

A short discussion on that topic has been added in Section 6 (Pag. 289, Line 4).

Page 285, line 16 versus Fig. 11d: "...stronger elevation losses were detected over slopes with higher radiation inputs." Rephrase or better explain the sentence (Fig. 11d shows minimum elevation change at maximum radiation inputs).

We meant "for classes of summer clear-sky radiation which were adequately represented in terms of area". Rephrased.

Page 285, line 19: What is the meaning of an averaged snow line altitude? Especially from these two years (1987, 2009) in which the course of accumulation and ablation possibly was not representative for decadal climate?

Please, see previous comments concerning the limits of the imbalance analysis (e.g. 3 Main methodological comments - 3.1 Decadal representativity of the snowline in late summer 1987 and 2009 and Page 269, line 13).

Page 285, line 26: Don't use "still" which indicates a stationary state of current climate to which glaciers will adapt. This is not the case as there is no reason to assume that the current long-term warming trend will stop in the near future.

We agree on your perspective that there should be a long-term warming trend. However, since about a decade temperature increase was strongly reduced, giving glaciers in the Alps the possibility to adjust to the past temperature increase (from the mid-1980s). The 'still' also means that we refer to this former period rather than to a new change.

Page 287, line 6: "This value is about half that of other recent alpine-wide estimates (-2% a^{-1} from 1984 to 2003, Paul et al., 2011), but it is similar to results of previous investigations in the European Alps." This sentence is confusing!

Agreed, we have reformulated it to be more clear.

Page 288, lines 3-5: We can support this finding from the winter and summer mass balance measurements from Langenferner/Vedretta Lunga.

A reference has been added.

Page 289, line 11: Please specify the "peculiar characteristics of the Ortles-Cevedale glaciers".

The peculiar characteristics of the Ortles-Cevedale glaciers are mentioned and discussed in the following sentence (Page 289, lines 12-16). Rephrased for clarity.

Page 289, line 26: "The lowering of the albedo and increased thermal emission from the growing patches of ice-free terrain likely act as positive feedbacks, i.e. its behavior might already be decoupled from climate change." Certain feedbacks or strong debris cover may make it more difficult to extract a climate signal from an individual glacier. Nevertheless, glaciers are never decoupled from climate change.

Ok, decoupled was clearly too strong. The statement has been rewritten.

Page 290, line 9: "Some of them, located in steep terrain at high altitudes, show little change and might in part be composed of cold ice". Is the existence of cold ice proven for any case except of Alto del'Ortles or is this just speculation? Anyhow, is it relevant?

No, it's not relevant and has thus been removed.

Table 2: In the text it is stated that observations on La Mare started in 2003 (page 277, line 12). This is not specified in Table 2, where an AAR mean for 2000-2009 (also for La Mare) is shown. Please clarify!

Mass balance measurements begun in 2003, but personal observations on the snow cover in Val di Peio begun in the early 2000s. This is implicit while presenting snow cover data also on Vedretta Rossa glacier in Table 2, which is not subject to mass balance measurements. To be more clear on these differences, we have added a short explanation in the table text.

Fig. 3: Is not linked to the text. The glacier shrinkage is already shown in Fig. 1. Maybe remove.

We think that a photographic comparison helps the reader to get a more complete picture of the ongoing glacier shrinkage and would thus like to keep it. However, we fully agree that the time series must be linked to the text when shown and have inserted a related citation of Fig. 3 in Section 2.2

Fig. 6: Does not provide any additional information compared to Table 4. Consider to omit one of the two.

We prefer to keep Figure 6 as it presents a visual comparison of glacier area and number for size classes at the beginning and at the end of the observation period.

Fig. 9: "Mean annual elevation change rates of the Ortles-Cevedale glaciers from the 1981–1984 period to the 2005–2007 period." The caption is misleading as the whole time span between date 1(1981-1984) to date2 (2005-2007) should be referred to as one period. (Do not use "period to period")

We reformulated the caption as follows "Mean annual elevation change rates of the Ortles-Cevedale glaciers in the period from 1981–1984 to 2005–2007, on a cell-by-cell basis".

Fig. 10: Same problem in figure caption than in Fig. 9: Depicted changes are means over ONE period from date 1 to date 2.

We have rewritten this to "Glacier specific mean geodetic mass balance rates calculated from the values shown in Fig. 9".