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The Cryosphere Discussions

Interactive comment on "Extrapolating glacier mass balance to the mountain range scale: the European Alps 1900–2100" *by* M. Huss

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The reviews by Michael Zemp and J. Graham Cogley were both very detailed and helpful to finalize the manuscript. I'd like to kindly acknowledge them.
All reviewers' comments are repeated below (*in italic*), they are discussed (normal type style), and a suggested new text version is given in quotation marks.
The most important changes applied to the paper are as follows: A more in-depth assessment of the variables of the multiple regression is provided. The final choice of parameters used for multiple regression was narrowed to the most influential and straightforward variables. Thus, all calculations for mass balance extrapolation were re-done, and some of the results have slightly changed. An improved description of the extrapolation techniques applied is included, and further discussion of several issues

proposed particularly by Reviewer #1 is added to the paper. An additional validation data set (geodetic mass balance of glaciers in the Mont Blanc area, see Berthier (2005), between 1979 and 2003) was included which strengthens the extrapolation results for glaciers in the Western Alps.

Comments of Reviewer #1 (Michael Zemp)

... The comprehensive approach based on several other publications comes, however, with the price of being partly superficial and it is sometimes difficult to follow all explicit and implicit assumptions. As such, the paper probably has some conceptual mistakes in the sections about the extrapolation based on the glacier hypsometry (3.3) and in the calculation of stochastic and systematic uncertainties (5), and should elaborate further the sections about the extrapolation based on arithmetic averaging and based on multiple regressions.

I completely agree with the reviewer that not all methods are described extensively. Due to the variety of approaches applied in this paper (temporal and different spatial inter- and extrapolation techniques, modelling etc.) it is not possible to elaborate on every detail. It is however clear that some important issues require a better and more ample description (such as the points mentioned by the reviewer). The revised version of the paper addresses these points (see details after the specific reviewers' comment in this coverletter).

In view of the global relevance of this Alpine study, it would be great if the paper discussed the effects of a moving mass balance sample on the extrapolated mountain range estimate as well as the representativeness of the observation series for both the variability of the mean specific balance of all glaciers and the total mass change (i.e., contribution to run-off and sea level rise) of the entire mountain range.

Figure 7 already shows the effect of a moving mass balance sample on the extrapolated mountain-range balance and a discussion is provided. In addition, Figure 5 shows the overall mountain range ice volume change that can be interpreted in terms of water run-off and sea level rise. The representativeness of the observation series for mean specific mass balance is discussed. In the revised version this discussion is extended in order to investigate the mountain-range mass balance directly computed as an average from the moving sample of available WGMS mass balance observations (details see below).

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Also, the sections about the future scenarios could be better linked to the lessons learned from the extrapolation exercises which actually are the main focus of the paper.

Linking past (extrapolated measurements) and future (modelling) is not that simple. It is made clear in the introduction (and even the title) that the paper is split into two parts, one addressing the methodologies for extrapolating existing mass balance measurements, and a second with the aim to come up with a time series of Alpine glacier mass balance 1900-2100. The methodologies for calculating future mass balance (modelling) cannot employ exactly the same approaches that are used for extrapolating measured balances of a small sample of glaciers. I however agree with the reviewer that it is important to better link the two parts, and to stress common approaches. Therefore, some sentences were added in the revised paper (details see below).

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Furthermore, Figures 5 and 6 show some interesting features at the change from observations to scenario runs which might be illustrative to discuss.

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Additional discussion on this subject is included (details see below).

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Page 1118, Line 2, ”extrapolation of single glacier mass balance measurements”: In fact it is rather an extrapolation of a small (usually moving) sample of measurement series to the mountain range.

This is correct. The revised version now simply omits the ”single” which is actually redundant in this context.

”This study addresses the extrapolation of glacier mass balance measurements to the mountain-range scale ...”

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Page 1121, Mass balance datasets: The author uses modelled mass balance series (calibrated mainly with geodetic measurements) from Switzerland and glaciological mass balance measurements outside Switzerland for the extrapolation to the entire Alps. For some of these modelled Swiss series, there are glaciological mass balance measurements available too. It might, hence, be illustrative to show how good do these results compare?!

A series of individual papers could be dedicated to this topic... Therefore I prefer not to enter too deeply into this issue which is not directly related to the results presented in this paper.
As the bias between glaciological and geodetic mass balances is important nevertheless in international glacier monitoring (e.g. Zemp et al., 2010), four Swiss glaciers with long-term mass balance series (> 25 years) based on the glaciological method were

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compared to the geodetic surveys. The series of Griesgletscher and Silvrettagletscher have recently been homogenized; the new series (included in the WGMS data base) are now consistent with the geodetic ice volume changes (Huss et al., 2009). Whereas good agreement for Gries was found, a systematic bias was corrected between 1994 and 2007 for Silvretta. The series of Limmern and Plattalva compare well with the geodetic surveys (bias of the glaciological series of –0.01 m w.e. a^{-1} and +0.05 m w.e. a^{-1}, respectively).

Page 1122, Line 2: Is it correct that the 16 short-term mass balance series outside Switzerland are used together with the modelled Swiss series for the extrapolation whereas the 9 long-term series outside Switzerland are used for validation only? Please clarify.

Not entirely. Extrapolation of the long-term mean mass balance (i.e. 100-year average) is *only* based on the Swiss series. However, all WGMS series outside of Switzerland are used for better constraining the temporal variations of the extrapolated series, i.e. they affect the year-to-year variability, but not the long-term mean. The mean of nine long-term WGMS outside of Switzerland is used for validation.
This issue cannot be clarified here (the Data-section presents the data used and not the methods), but is addressed later in the paper.

"Calculation of $\Delta\overline{B_{i,r}}$ is based on the 50 long-term mass balance series, and is supported by data of 25 additional WGMS glaciers for the years covered by these series. "

"(ii) Validation against 9 independent long-term WGMS mass balance series is shown in Fig. 3b. (iii) Extrapolated mass balances are compared to all available WGMS series (at least 3 yr) at the annual scale (Table 1)."

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Page 1123, Future climate: Please mention if you consider or ignore other parameters in the GCM-scenarios (e.g., radiation budget, cloudiness) and in your glacier model (e.g., albedo, lake formation, debris cover) that might be of importance for the energy and mass balance results. This might help to discuss if the current results are rather optimistic or pessimistic in view of how fast glaciers will melt away.

A sentence is added for the GCM-variables used. Additional information on the glacier model is given in the Methods-section.

”No additional variables such as the radiation budget or cloudiness were considered.”

”The model does neither include changes in supra-glacial debris coverage, nor positive feedbacks due to surface albedo decrease and proglacial lake formation.”

Page 1125, Lines 1-3: You should check your assumptions about the four climatic different regions against corresponding literature (e.g., Boehm et al. 2001, Auer et al. 2005, 2007).

In their Figure 8, Auer et al. (2007) show four statistically determined leading climatological subregions of the Greater Alpine Region (extending from France to Hungary and Serbia). In terms of temperature and precipitation variability, the southern flank of the Alps (i.e. catchment of the Po) is significantly different from the northern flank. Also a statistically significant east-west separation is found by Auer et al (2007), but it is situated close to the eastern-most glacier in the European Alps.
As observed glacier mass balance variability in the catchments of the Rhone, Rhine and Danube clearly show strong differences in individual years, and the four regions

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are relevant regarding runoff generation (main water divides of the European continent) I would like to keep the present separation of Rhone, Rhine and Danube into subregions with specific mass balance variability.
A discussion is added to the text.

”Four regions for the study of specific mass balance variability are defined by the drainage basins of the large streams Rhone, Rhine, Danube and Po separating the European Alps into a west, north, east and south section. The main water divides often correspond to borders of specific climatic patterns (Auer et al., 2007).”

Page 1125, Lines 16-18: Actually there is a forth Swiss Glacier Inventory available for 1998/99 (Kaeaeb et al. 2002, Paul et al. 2002, Paul 2004). Resulting area change estimates (1850-2000) for the entire Alps are given in Zemp et al. (2008).

Right. This is added.

”All glaciers in Switzerland (representing half of the glacierized area of the Alps) are covered by four inventories (1850, 1973, 1998/1999 and 2003, see Maisch et al., 2000, Mueller et al., 1976, Paul et al., 2004,2011). ”

Page 1125, Lines 18-23: Interpolating area changes between the available inventories using mass balance variations (cf. Equation 4) is straight forward but ignores the fact that area changes are a delayed, filtered, and enhanced reaction of a glacier to the energy and mass balance forcing at its surface. You should at least discuss the impact of this simplification on your results and might use available length changes measurements for the quantification of a corresponding estimate.

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Done.

”Interpolating area changes using mass balance variations is a first order approximation; glacier area and length changes are a filtered and delayed response to the surface mass balance forcing.”

Page 1127, Line 1 and Figure 5: Your density assumptions on this page (850 kg m-3) and in Figure 5 (900 kg m-3) are different and should be harmonized.

I do not agree with this statement although I am aware that the differences between these two density assumptions are subtle.
Here, a factor is chosen to convert volume change to mass change. This conversion factor has the unit of a density, and is normally referred to as the ”density of ice volume change”. However, it is not directly linked to the actual ice density. The conversion factor would only be identical to ice density if there are no changes in the firn density profile (which is highly unlikely in the case of glacier mass loss). Therefore the density of volume change (the conversion factor) is assumed to be smaller (850 kg m^{-3}) than a typical ice density as the volume change is also strongly influenced by changes in the firn density profile (leading to a volume change without a mass change).
In Figure 5, however, the density used to calculate ice volume from total glacier mass does not refer to a volume *change*, but to the total ice volume present in the European Alps. Therefore this density corresponds to the actual ice density and is assumed to be 900 kg m^{-3}.
For these reasons the densities were not harmonized as suggested by the reviewer.

Page 1127, Arithmetic averaging: I propose to (i) extend this approach of simple averaging with regional averaging, and to (ii) assessing the impact of a moving sample *(as used in most other studies) vs. a static sample (of long-term series as used in this approach).*

These suggestions are valuable, but – if I understand them correctly – exactly this is already done in the present version of the manuscript.
First, according to Equation (1) deviations of the regional mass balance in individual years are superimposed on the average long-term mass balance $\overline{B_{100}}$. Calculating $\overline{B_{100}}$ as a regional average as well would lead to a duplication of the regional signal. Moreover, the calculation of regional long-term averages is error-prone as the statistical basis for the averaging is strongly reduced and long-term series covering a representative fraction of the region are partly not available.
Second, the impact of using a moving sample of mass balance series for determining the mountain-range mass balance is discussed in detail in the uncertainty assessment section and results are presented Figure 7. An increasing number of series of randomly chosen series is included for calculating the mass balance of the European Alps and the uncertainty given by the choice of the number of series is evaluated.
Probably, the reviewers' comment rather refers to the WGMS mass balance series. Those data are however not focus of this paper and any specific analysis related to that data source would be beyond the scope of this study. The discussion however contains a paragraph that deals with the representativeness of WGMS series for the European Alps and also makes some statements about the impact of using a moving sample.

"When calculating Alpine mass balance by using the average of a moving sample including all available WGMS time series (between 8 and 28 since 1960) decadal means are between 0.07 and 0.13 m w.e. a^{-1} more negative compared to the present study."

Page 1127, Glacier hypsometry: Consideration of the glacier hypsometry is certainly a promising extension of the approach(es) using arithmetic averaging. And I agree that simple averaging over given elevation bands is a first but not necessarily best approach. Nevertheless, you should show its results and the corresponding improvement of your suggested correction to glacier median elevations. I think your proposed approach, shifting the averaged mass-balance elevation distribution to the glacier's median elevation, might work for years and glaciers with mass balances not too far from steady-state conditions. However, for stronger mass imbalances (as we have had for the past two decades) the enforced positive balance of half of the glacier area will lead to systematic biases in your results and might explain the low performance of this approach. I, hence, propose to extend this section with testing a third approach: use (regional) ablation and accumulation gradients instead of elevation-band averaging and shift these gradients to the median elevation corrected for ELA-deviations from steady-state conditions (derived from ELA0 or median elevations of your observation sample).

This section is reformulated, some issues are clarified and the calculations were repeated basing on the reviewer's comments. The approach suggested by the reviewer is interesting and its application was tested. A modified version replaces the previous results. The increase in performance is moderate and this extrapolation method is still behind the other approaches.
Several problems were encountered in the implementation of the reviewers' suggestion:
(i) How can ELA-deviations from steady-state conditions be determined for a sample of several thousand individual (unmeasured) glaciers? The aim is to infer 100-year mean mass balances. In order to calculate meaningful deviations of the long-term mean ELA from an ELA_0-value, both detailed mass balance observations and knowledge about the dynamic response of each individual glacier would be required. Although these variables can be estimated this makes the approach quite arbitrary. (ii) The available

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data base is not sufficiently large to calculate well-justified regionally separated mass balance gradients (as proposed by the reviewer). The approach of shifting the averaged mass balance-elevation distribution (as a more detailed surrogate for the mass balance gradient) to the mean ELA elevation of each glacier however widely corresponds to the methodology proposed by the reviewer.
The revised methodology for extrapolating mass balance by on glacier hypsometry does not rely on glacier median elevation anymore but uses an estimate of mean long-term ELA for each glacier. The uncertainties in this estimate are stated in the text.

"By considering the area-elevation distribution of individual glaciers, it might be possible to partly account for the effect of particular glacier geometries on mass balance. This concept is similar to extrapolating the mean of observed centerline surface elevation changes in altitude bands to unmeasured glaciers as proposed by Arendt et al. (2006). Here, 100-yr mean mass balance of the data sets presented by Huss et al., (2010a,b) is averaged in 100 m elevation bands in a first step. Five glaciers with debris-covered tongues were excluded as their locally reduced ablation at low elevation would induce non-representative average balance in these altitude bands. The averaging implies that all glacierized surfaces located for example between 3000 and 3100 m a.s.l. exhibit the same mass balance everywhere throughout the mountain range. However, the strong regional differences in ELA (between 2600 and 3240 m a.s.l. within the glacier sample) lead to balances in the same elevation band differing by up to 3 m w.e. a^{-1} – an effect which must be accounted for. Thus, the mean mass balance-elevation distribution is shifted so that the ELA in the observed mass balance profile corresponds to the mean ELA of each glacier. This procedure allows the correction of ELA differences and mimics the extrapolation of observed mass balance gradients to the hypsometry of individual glaciers. Estimating long-term ELA based on topographical information only is however uncertain and represents a major drawback of this method."

Page 1128, Multiple regression: This approach is nicely complementary to arithmetic and hypsometry-based averaging. In my view it would be worthwhile testing also *some parameters a bit more innovative than just area, slope, and aspect. It would be interesting to see if including parameters related to glacier hypsometry (e.g., difference between mean and median elevation or percentage of area above mean elevation) or continentality (e.g., Tmax-Tmin or the index by Gorczynski (1920)) are able to improve the explained variance?!*

Besides the parameters finally chosen for M3 and M6 a number of additional variables were tested. For example, several variables describing glacier hypsometry (as proposed by the reviewer) were evaluated without achieving any significant improvement in the explained variance. As pointed out by reviewer #2, autocorrelation between variables used for multiple regression is critical. Index variables for continentality are, for example, strongly correlated to median glacier elevation, as well as geographic location (northing / easting) and should therefore not be used at the same time as these to predict mass balance.
The revised manuscript includes a re-analysis of the variables used for multiple regression. The choice of parameters was narrowed to the most important (and most straightforward) variables (multiple regression using eight variables is replaced by regression with six variables, M6).

”A number of parameters potentially describing mass balance variability was tested, and only those importantly contributing to the explained variance of the multiple regression were used. Multiple regression of mass balance is performed with parameter combination M3 consisting of the most important variables, and M6 including all variables with a significant influence on mass balance. Additional parameters such as the average slope of the glacier, the elevation range and other variables describing glacier hypsometry only marginally increase the correlation. Table 2 provides an overview of the variables finally used in the multiple regression. Indicator variables significantly correlated among each other are easting and northing, as well as median glacier elevation and northing.”

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Page 1128, Line 27; "So far, no study has attempted mass balance extrapolation using multiple regression.": This is not quite true. There is for example the paper by Schoener and Boehm (2007) using stepwise linear regression models for the reconstruction of LIA ice mass of glaciers in the European Alps.

Right. This study is referenced now in the Introduction, as well as in the description of the multiple regression approach.

In Introduction:

"Based on a regression model and homogenized climate data Schoener and Boehm (2007) derive mass balance time series since the Little Ice Age for glaciers in the European Alps. "

In Methods:

"Schoener and Boehm (2007) apply a multiple regression model that uses both meteorological input data and information about median and minimum glacier elevation for reconstructing long-term glacier mass balance series in the European Alps. "

Page 1129, Line 19: Any explanation for why south-exposed glaciers show less negative balances than north-exposed ones? Maybe, because these glaciers have built at higher elevations?!

Alternative explanation: South-exposed glaciers tend to be smaller (higher ELA, and thus generally smaller potential accumulation area), and therefore are closer to equilibrium than north-exposed glaciers.
Although discussions like this are interesting, I would not like to provide such speculative statements in the framework of this paper. The significance of this correlation is quite small, and any findings would need to undergo a more in-depth analysis which is

not the focus and beyond the scope of this paper.

Page 1130, Lines 7-14: Paul and Haeberli (2008) attribute glacier thickness changes to the period between 1985 and 1999 (not 2000) in order to account for the effect of C-band radar penetrated into the winter snow pack. Furthermore, a comparison of SRTM-based elevation changes against repeat DEMs based on aerial photogrammetry requires a careful co-registration process (cf. Nuth and Kääb 2011) before deriving glacier elevation changes. Please check your corresponding results in view of these two points and clarify the text in this section accordingly.

The comparison to the Paul and Haeberli (2008) study was performed for the period 1985-1999, but this was wrongly stated in the text. This is revised but the numbers and the corresponding conclusions did not change.
I agree that the co-registration process is an important issue when directly comparing DEMs. But here I validate the published elevation change numbers given by Paul and Haeberli (2008) against elevation changes obtained from an alternative and more detailed method (photogrammetric DEMs for individual glaciers, Bauder et al., 2007). Does the reviewer question the accuracy of the elevation changes (i.e. the co-registration) of the Paul-study, or of the Bauder-study? As both are published data sets, I am just evaluating the bias between them and cannot perform a re-analysis of the elevation changes found in these studies.

Page 1132, Line 2: Why are you now including all 50 glaciers? For reasons of consistency, it would be logical to use the same sample of 38 glaciers as before. Please clarify.

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It is explicitly stated why for the regression analysis only 38 series were used, i.e. why 12 series of smaller glaciers with slightly higher uncertainty of past mass balance were excluded (see below).
For the model calculations of 21st century mass balance the accuracy over the 20th century doesn't matter anymore, but a high number of modelled glaciers is important for a sound averaging, especially as future mass balances are evaluated in seven glacier size classes.

"The fitting of the relations used for mass balance extrapolation is based on 38 of the 50 long-term Swiss series (Fig. 3a). 12 rather small glaciers in the south-eastern Swiss Alps (Huss et al., 2010b) were excluded here due to slightly lower mass balance data quality (last DEM earlier than 2000) and a regional overrepresentation of these glaciers (Fig. 1)."

"Extrapolation of future glacier area and surface mass balance to the entire European Alps is based on the detailed modelling of 50 Swiss glaciers."

Page 1132, Lines 2-5: Why are you not using one of the extrapolation methods discussed in the sections before? This would much better motivate and link the observation part with the scenario part.

This is a good point. The paragraph is reformulated in the revised version. Actually, results from the multiple regression method M6 have been used for breaking down the modelled mass balance to individual glaciers, but this has not been explicitly described. Considering the mass balance response in different size classes is however indispensable for future projections as the disintegration of smaller glaciers is accelerating.

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”The extrapolation scheme employed is based on the results of multiple regression M6 but is extended for future conditions by considering glacier size specific changes in mass balance and area. The investigated glaciers are divided into seven size classes which are assumed to be representative in both their area change relative to the 2003 state and their mass balance. Due to the differences in dynamic response times between large and small glaciers, and consequently their different mass balance response to atmospheric warming, considering size classes for mass balance extrapolation over the 21st century is reasonable. Glacier-specific anomalies in $B_{100,g}$ obtained by M6 are superimposed on the mean size-class mass balance in order to calculate future mass balance of individual glaciers.”

Page 1132, Line 19-24 and Table 3: For completeness and comparison, you should include the measured total glacier area for the 1970s (2,900 km2, WGMS 1989) as well as the thereof derived areas for 1850 (4,470 km2) and 1998/99 (2,270 km2) by Zemp et al. (2008).

I agree that mentioning these numbers and comparing them to the glacier areas used in this study is important. However, this is rather an issue of discussion and should not be mixed up with the results of the present study. Therefore, a paragraph focusing on European glacier area was added to the uncertainty-section.

”The evolution of total glacier area in the European Alps throughout the 20th century was estimated by upscaling observed area changes between repeated Swiss glacier inventories to the entire mountain range and by interpolating them to the annual scale using mass balance variations. European glacier area inferred in this study (see Table 3) agrees well with previous estimates for 1973 (2900 km^2) and 1998/1999 (2270 km^2, Zemp et al., 2008). For the end of the Little Ice Age Zemp et al. (2008) find a total area of 4470 km^2. Our area estimate for 1900 (3350 km^2) thus indicates that a considerable fraction of European glacier area and ice volume was lost already in the late 19th century which is supported by Luethi et al. (2010).”

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Page 1133, Line 1: It would be interesting to compare your estimated Alpine mass loss to the results by Kaser et al. (2006), Radic and Hock (2011) as well as to the GRACE-based estimates by Jacob et al. (2012).

Very good suggestion.
An additional paragraph elaborates on the comparison to large-scale assessments. Instead of Kaser et al. (2006), which only gives European mass balance *including* Scandinavian glaciers, Dyurgerov and Meier (2005) is used.

"Long-term Alpine mass balances were compared to global-scale glacier change assessments in order to judge their validity for the European Alps. For the period 1961-2000, Dyurgerov and Meier (2005) calculate a mountain-range mass balance of -0.11 m w.e. a^{-1} from the measurement series which is significantly less negative than the number obtained in the present study (-0.25 m w.e. a^{-1}). Jacob et al. (2012) find a mass loss corresponding to -1.0 ± 1.3 m w.e. a^{-1} for 2003-2010 based on the Gravity Recovery and Climate Experiment (GRACE). Although this number agrees relatively well with the extrapolated mountain-range balance for the same period (-1.13 m w.e. a^{-1}), the error bars of the GRACE-estimate are too large for allowing any conclusive and independent statements about the rate of European glacier mass loss. Future glacier volume projections were compared to the global modelling study by Radic and Hock (2011). Their expected ice volume loss for the European Alps by 2100 (median of 10 GCMs) is –76% which is less than the –93% (both relative to the year 2000) obtained in this study (Fig. 5). This is probably related to the overestimate in area and volume that Radic and Hock (2011) used for initializing their model. "

Page 1133, Lines 17-19: The negative feedback due to glacier retreat into higher elevations might be at least partly compensated by a positive feedback due to the lowering of the glacier surface.

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Right. A sentence is added.

”This exerts a significant negative feedback on mass balance as glaciers retreat to higher elevations, and many smaller glaciers at low altitude vanish completely. For large glaciers however also a positive backcoupling effect due to surface lowering may become important.”

Page 1133, Lines 24-29: Here you should include a short statement about the significant uncertainty in remaining ice volume (cf. Page 1131) and its impact on the timing of glacier disappearance.

This section presents the results of the model. A discussion of uncertainties in the glacier mass balance projections is provided in the Discussion-section. There, an additional statement about the impact of ice volume uncertainty is provided.

”The considerable uncertainty in remaining ice volume also critically affects the timing of ultimate glacier disappearance in the European Alps. ”

Page 1134, Lines 1-11: In view of effects not considered in your modelling approach (cf. comment on Page 1123), are these results to be seen rather as best or worst case scenarios?

Here, a discussion of this issue would not be well placed. The uncertainty in the future modelling is addressed in the Discussion-section. This section (see below) mentions the backcoupling effects not included in the modelling and provides a statement on how to interpret these model results. It seems impossible to me to judge whether the presented results are best or worst case scenarios. In terms of climatic evolution the

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best and worst case is covered. Regarding the glacier model the relative importance of positive and negative feedbacks (both are present!) are yet widely unknown. As stated in the text, the model results represent best estimates given the current state of knowledge.

"Moreover, insufficiently understood feedback effects and processes not included in the applied model approach, such as a decrease in glacier surface albedo (Oerlemans et al., 2009), the thickening of supraglacial debris (Jouvet et al., 2011), or the response of polythermal ice bodies at high elevation in the Alps (see Hoelzle et al., 2011) might impact on modelled mass balances. Additional research is required to strengthen the process understanding for reducing these uncertainties. Due to the combined uncertainty of climate model input and the impact modelling, simulated future mass balance have to be interpreted with care and represent best estimates given the current state of knowledge. "

Page 1134, Line 27: where does the value for the stochastic uncertainty σg come from? Please clarify.

The reference for this number is given. For clarification some more details are added.

"Long-term mass balances of individual glaciers g are subject to an uncertainty of $\sigma_g = \pm 0.07$ m w.e. a^{-1} (Huss et al., 2010a). This number was estimated by analyzing uncertainties originating from DEM differencing, and also includes the uncertainty due to the density assumption for converting volume change to mass change. "

Page 1135, Line 4-27: At line 4 you start to mix up systematic and stochastic uncertainties and corresponding treatments! You cannot calculate a systematic uncertainty according to the law of error propagation. Please completely revises this section and *also better reference the values used for comparison.*

I have the impression that this comment is based on a misunderstanding related to the terms 'systematic' and 'stochastic' uncertainty. Obviously, the reviewer uses these terms differently than it is done in this paper. This is unfortunate, and the formulation of the whole section is revised in order to avoid ambiguity.
Concerning the method to calculate uncertainty, I however disagree with the reviewer. For example, σ_g is normally (independently) distributed among the 38 glaciers. Why shouldn't this uncertainty not be treated using error propagation?
As stated in the text, there is (i) one type of uncertainty affecting the long-term mean mass balance, and (ii) a second type related to the year-to-year variability. Also (i) is normally distributed among the glaciers, and does thus not refer to a bias in the mountain-range mass balance (as it seems to have been understood by the reviewer). All uncertainties are flagged by σ with a $\pm$. A bias (systematic error) however in the Alpine mass-balance estimate with independent data sets of ice volume changes is stated with a $+$ or $-$ sign.
The revised version avoids the term 'systematic' and is clarified regarding the calculation of uncertainties.

"This number was estimated by analyzing uncertainties originating from DEM differencing, and also includes the uncertainty due to the density assumption for converting volume change to mass change. As σ_g is assumed to be independently distributed among the $n = 38$ glaciers used for fitting the extrapolation relations, the error due to mass balance data uncertainty is reduced to σ_{obs}=$\sigma_g/\sqrt{n} = \pm 0.011$ m w.e. a^{-1} according to the laws of error propagation.

The uncertainty in mass balance due to the use of multiple regression (M6) for extrapolation is estimated by (i) combining results of the different validation approaches (Fig. 3, Table 1), and (ii) comparison of mountain-range mass balance obtained from different regionalization techniques. M6 has an average bias of 0.06 m w.e. a^{-1} relative to annual mass balances measured by the glaciological method (Table 1). This lies within the estimated uncertainty of $\pm$0.2 m w.e. a^{-1} in these Dyurgerov (2002) and could also be explained by a non-representative sample of monitored glaciers. Validation of M6 using geodetic mass

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changes (Table 1) shows a rather good agreement for different periods and regions with an error of about $\pm 0.02\,\mathrm{m\,w.e.\,a^{-1}}$. Note that the bias in the geodetic mass changes of the studies by Berthier (2005) and Paul and Haeberli (2008) can widely be corrected as indicated by comparison to photogrammetrical elevation changes. The 1900–2011 mass balance of the European Alps obtained by arithmetic averaging ($-0.33\,\mathrm{m\,w.e.\,a^{-1}}$), multiple regression M3 ($-0.29\,\mathrm{m\,w.e.\,a^{-1}}$) and M6 ($-0.31\,\mathrm{m\,w.e.\,a^{-1}}$) is similar indicating that the mass balance extrapolation based on the available data basis is relatively robust. Merging evidence from all these sources, an overall extrapolation uncertainty of $\sigma_{\mathrm{ex}} = \pm 0.04\,\mathrm{m\,w.e.\,a^{-1}}$ is estimated as an upper bound.
Thus, the total uncertainty in long-term mass balance extrapolated to the European Alps using multiple regression M6 becomes

$$\sigma_{\mathrm{tot}} = \sqrt{\sigma_{\mathrm{obs}}^2 + \sigma_{\mathrm{ex}}^2}, \tag{1}$$

with $\sigma_{\mathrm{tot}} = \pm 0.041\,\mathrm{m\,w.e.\,a^{-1}}$ defining the error bar of the 20th century mountain-range mass balance. ”

Page 1137, Line 11: What is the reason for using the 80% confidence interval? Confidence intervals are typically reported at the 90, 95, or 99% level.

Statements in the text and the caption of Figure 7 are changed in order to refer to a 90% confidence interval.

P1137, Lines 21-23: : : :which confirms the monitoring strategy as proposed by Haeberli (1998) and Haeberli et al. (2000).

Done.

”As a rule of thumb at least 5–10 series should be available to regionalize mass balance within acceptable bounds of uncertainty. This is in line with the monitoring strategy proposed by Haeberli et al. (2000). ”

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P1137, Lines 24-29: This would be a good section for discussing the impacts of major corrections in some of the long-term series or of a moving sample on the Alpine-wide mass balance estimate.

I have the impression that discussing corrections and the re-analysis applied to the set of WGMS data during the last years is too far off-topic. In this study, WGMS data are used for independent validation and not to directly generate results. Therefore, recent adjustments to WGMS data would not affect the results presented in this paper. I am thus reluctant to discuss this issue in the frame of this paragraph. This is beyond the scope of this study and would too much dilute the results.

P1138, Lines 5-8: You should specify what the term "representative" refers to. As such a sample of observed glaciers might be representative for the total glacier run-off contribution (that originates from the few large glaciers) but not for the average specific mass balance of all Alpine glaciers (dominated by the large number of small glaciers).

The term "representative" is clarified.
Please refer to Equation (3) which describes how the mountain-range mass balance has been calculated. The numbers given are an area-weighted average, the small glaciers are thus not dominating the overall specific mass balance as suspected by the reviewer. "Representative" means that the mass balance corresponds to the mean mountain-range balance which is also the relevant quantity for calculating the contribution of glaciers to run-off.

"Mass balance series at the scale of the European Alps allow assessing the representativeness of existing long-term monitoring programs, i.e. how well they reproduce the mean balance of all Alpine glaciers (see Eq. 3)."

P1138, Lines 9-17: this might be a good section for at least mentioning issues related to down-scaling techniques and potential changes in climate variability.

Done.

"Additional uncertainty arises from the down-scaling of GCM data to the single-glacier scale and the potential changes in future climate variability. "

P1140, Line 6: ... at drastically reduced glacier size whereas others show a run-away effect with annual mass losses above 2 m w.e.

Done.

"Afterwards, some scenarios indicate a gradual stabilization of Alpine mass balances at drastically reduced glacier size, whereas others result in a run-away effect with mass balances lower than -2 m w.e. a^{-1}."

P1140, Line 12, Colgey 2011: or Bolch et al. (2012) for the same region or Zemp et al. (2009) globally. However, I would recommend leaving the conclusions without references.

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References are now omitted in the conclusion.

P1150, Fig. 1: the circles as well as the black triangles are hard to see against the dark grey of the hillshaded terrain. Plotting elevations > 1,500 m a.s.l. in light grey (instead of the hillshading) might do a better job.

Done.

P1151, Fig. 2: The range of the y-scale in the lower figure should be enlarged in order to cover the full range in precipitation scenarios. Also, the thin grey lines are difficult to attribute to the individual GCM runs.

Done. It is not important to attribute all thin lines to the individual RCPs; they rather give a range of the GCM results.

P1152, Fig. 3: In the left panel, there seems to be a switch of the glacier hypsometry based estimate from over- to under-estimation of the observed balance (from left to right which probably is from larger to smaller glaciers). Can you explain this feature?! According to the figure caption, the glacier area of 2003 is given – where?

With the new approach to regionalize mass balance using glacier hypsometry this skew is now much less significant.

Glacier area is given inside the bars (specifically stated now in the caption).

" Glacier area (by 2003) in km^2 is stated inside the bars."

P1153, Fig. 4: The example given shows a glacier system where processes not included in the modelling approach (i.e. cold ice temperature, summer accumulation regime) might lead to a different future reaction of Grenzgletscher as compared to Gornergletscher. When showing these two glaciers, it might be worthwhile discussing that issue.

Whereas the summer accumulation regime of this glacier should be captured by the modelling, the effect of the polythermal character of Grenzgletscher was neglected. A simple energy balance consideration however shows that the negative ice temperature of Grenzgletscher (one part of the glacier system) only has the potential of reducing melt by about 1% (given the same energy input). This effect is far from significant considering the other uncertainties in the modelling.
This uncertainty is already covered in the uncertainty assessment section with a citation.

", or the response of polythermal ice bodies at high elevation in the Alps (see Hoelzle et al., 2011) might impact on modelled mass balances. "

P1154, Fig. 5: I guess the dashed line refers to the year 2011? Having the concept of climate scenarios in mind, I think the scenario mean is irrelevant and should not be shown.

Meaning of the dashed line clarified.

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The scenario mean is removed.

”The dashed line indicates the onset of future modelling results (2011). ”

P1154, Fig. 5: Why do the scenarios in plot (a) not start at the same mass balance value? And why do RCP 2.6, 4.5, and 6.0 show a trend towards less negative mass balance until 2020? If this is due to the reference period of the scenarios starting earlier (e.g., mean of 1961-90), it might be illustrative to show the scenario runs from the very beginning and discuss the course of the observations within the scenario ensemble.

Each GCM run represents a possible meteorological realization of future climate (given a certain assumption about CO_2 concentration). So there is no reason why all of them should yield the same mass balance in the first year of the modelling; glacier area and volume must be consistent however.
It is an interesting feature that the modelled mass balance for the next decade is less negative than the observed balance since 2000. This is due to the fact that the GCMs did not manage to capture the early 21st century warming and are somewhat lagging behind. The next years will show if observed warming further diverges from the scenarios or proceeds at a reduced rate. A short discussion of this point is included in the results section.
The modelling was initialised with the last available DEM (between 1999 and 2008) and driven with scenario time series after 2010. Forcing the model with GCM series in the past does not make sense in the frame of this study that wants to come up with a best estimate for European glacier mass balance.

”Until 2020 modelled mass balance is even slightly more positive compared to the observations of the early 21st century. This is explained by the GCM scenario runs that have not succeeded to reproduce the air temperatures of the very warm last decade and are lagging behind. "

P1155, Fig. 6: It looks like the dashed line refers to the year 2009? For reasons of consistency, it should refer to the same year as in Fig. 5.

Thanks! This is changed.

P1155, Fig. 6: Again, there seems to be this trend towards less negative mass balances until 2020?! And what is the reason for the enormous spread in reaction between different size classes after 2020 which is not visible in the observations?!

First point – see above.
The reason for the diverging future mass balance in the different size classes and their relative similarity in the past is indeed a very justified question. I attribute this to the lacking ability of the glaciers to adapt to the fast rates of change in the future. A discussion is added to the revised manuscript.

"Mass balance shows considerable differences among the glacier size classes, and strongly diverge in the future (Fig. 6). Whereas mass balances of glaciers currently $<3\,\mathrm{km}^2$ remain above -1 m w.e. a^{-1} for RCP6.0 throughout the century, the mass balance of larger glaciers shows a strong decrease beyond historical levels. These differences are explained by the glaciers' dynamic response and their increasing disequilibrium as we move on into the 21st century."

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Interactive Comment

Comments of Reviewer #2 (Graham Cogley)

"eustatic" is better avoided. It refers to the globally averaged total sea-level change, not to the component due to ocean mass change. It can simply be omitted.

Done.

P1119 L13 I do not understand this. Does it mean that time series from different glaciers tend to be well correlated?

Reformulated.

"Temporal mass balance variations of individual series tend to be well correlated at the scale of a mountain range (Letreguilly and Reynaud, 1990; Vincent et al., 2004) ... "

P1119 L20-24 The accuracy of extrapolation from centreline traverses has been questioned by Berthier et al. (2010).

As the reference to Arendt et al. (2006) is given here for illustrating feasible *methods* for mass balance extrapolation, a discussion of the accuracy of the input data for these methodologies would be misplaced in this paragraph.

P1123 L21 Detrended data: if they were also normalized, say so.

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Data were not normalized.

L23 Clarify by ending the sentence at ”2100”, then saying ”The combination of RCPs and individual GCMs yielded a total of 35 such series.”

Done.

P1127 L7 These citations are rather odd because, although the sources present arithmetic averages, those averages are not the basis of the large-scale balance estimates.

References changed.

” ... used for large-scale mass balance estimates (e.g. Cogley, 2005; Dyurgerov and Meier, 2005; Kaser et al., 2006, partly including area-weighting schemes) ”

P1128 L1 Mention the effect on the 100-year mean of including the debris-covered glaciers.

”... is averaged in 100 m elevation bands. Five glaciers with debris-covered tongues were excluded as their locally reduced ablation at low elevation would induce non-representative average balance in these altitude bands. ”

L21-25 But it is also true that a rising ELA has less far to go to reach the maximum elevation of most smaller glaciers.

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Right. A sentence added.

”This is counteracted by a faster loss in accumulation area of smaller glaciers, and therefore a more important albedo feedback on glacier melt. ”

L27-28 The Lliboutry model (1974) almost qualifies as multiple regression. (See also Letreguilly, A. and L. Reynaud, 1990)

Done.

”Already Lliboutry (1974) has proposed the calculation of mass balance using multiple regression ”

L16 Something has gone wrong with Table 2, in which the sum of entries in the Rvar column is 104.9% and the variance explained by easting seems improbably large (it contradicts the ”slight decrease” mentioned at L21).

Thanks for pointing out this error. The whole table is now revised and the numbers are updated.
The percentage of explained variance does however not allow directly inferring on the rate of change (indicated with ’slight decrease’).

L1-22 This discussion needs further work. Nothing is said about the collinearity (i.e. non-independence) of the supposedly independent variables. For example I would expect easting and median elevation to be well correlated for reasons to do with

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continentality.
There should also be some discussion, if only brief, of the suitability of linear as opposed to nonlinear regression models.

This is a valid objection. All variables were now correlated against each other in order to detect potentially dependent variables. Results are stated in the text and the multiple regression experiments were redesigned resulting in small changes in all results.
A sentence relating to non-linear trends is added.

”A number of parameters potentially describing mass balance variability was tested, and only those importantly contributing to the explained variance of the multiple regression were used. Multiple regression of mass balance is performed with parameter combination M3 consisting of the most important variables, and M6 including all variables with a significant influence on mass balance. Additional parameters such as the average slope of the glacier, the elevation range and other variables describing glacier hypsometry only marginally increase the correlation. Table 2 provides an overview of the variables finally used in the multiple regression. Indicator variables significantly correlated among each other are easting and northing, as well as median glacier elevation and northing. The limited statistical basis does not justify the fitting of non-linear models to the data.”

P1130 L23 Should ”distribution” be ”distributions”? That is, are the distributions those of the glaciers individually, or of the two variables over all the glaciers?

Specified that thickness distribution must be known for every *individual* glacier to model future evolution.

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”The ice volume and thickness distribution of each glacier in the European Alps is crucial for modelling its future mass balance and area. ”

L2 Change ”normally” to ”independently”. Normality is not required as a justification of the upcoming calculation, but independence is.

Done.

P 1137 L21-24 This remark is entirely accurate, but not very helpful when (as is true of most of them) your mountain range has fewer than 5-10 series. Say something to indicate that help is coming (e.g. on P1139).

Statement put into context.

”As a rule of thumb at least 5–10 series should be available to regionalize mass balance within acceptable bounds of uncertainty. This is in line with the monitoring strategy proposed by Haeberli et al. (2000). For mountain ranges with fewer mass balance series a combination with new remote sensing techniques providing a regional coverage is promising. ”

L28-29 The mass balance of Stubacher Sonnblickkees has been reported for the past decade or more by relying on measurements of the accumulation-area ratio, the latter variable being calibrated against early glaciological mass-balance measurements.

”Vernagt and Sonnblick (AU) appear to be suitable index glaciers for Alpine mass balance (although data for Sonnblick are somewhat uncertain as balance is indirectly determined from accumulation area ratio since one decade)."

All stylistic comments were very helpful and were included as proposed.

Interactive comment on The Cryosphere Discuss., 6, 1117, 2012.