Responses to Referees

Anonymous Referee #3
1 General comments

This study presents an analysis of the mass balance (MB) at glaciar Echaurren Norte over a longer period. Using a temperature-index MB model they first assess the sensitivity of the glacier MB to temperature and precipitation. Then they use the fact that regional streamflow time series are well correlated to the MB at ECH in order to build a simple linear regression model and reconstruct a MB up to 1909.

The manuscript is generally well written and gathers some interesting regional data.

We thank the reviewer for acknowledging that the paper is well written, and would like to stress the fact that that no other site in the extratropical Andes (~22°-55°S) contains such a unique combination of long and complete glacier and climate records located only 10-15 km apart.

However, the efficient writing style chosen by the authors also hides some flaws and simplifications in the methodology (see comments below). The chosen methods are extremely simplistic (a fact acknowledged by the authors) while their conclusions are not. The authors' argumentation in favour of these simple tools is often qualitative and rarely backed up by references.

The use of this simplistic modeling approach was partly forced by the lack of detailed on-glacier measurements at ECH (see above), and partly motivated by our interest in finding if it was possible to successfully capture this glacier's annual mass balance variations using a simple model that only relies on monthly temperature and precipitation as input. In this sense, the key reference mentioned in several parts of the text is Marzeion et al. (The Cryosphere, 2012a), who applied the same model and sensitivity analyses to a larger glacier mass balance dataset available from the European Alps.

I am surprised that the authors did not care to discuss the influence of changing glacier geometry on both the statistical and temperature-index model outputs.

Fixed. In the submitted manuscript we failed to indicate that we are reconstructing reference-surface mass balance. This is now fixed.

Altogether I am not convinced that the presented study provides enough new material or methods to justify a publication in TC. My recommendation to the authors is to address the points below and to extend the study by including more ambitious objectives, for example by discussing the climatic drivers of the MB variability.

We have now addressed these points including a) a discussion on the use and implications of a reference-surface glacier area in the mass balance reconstruction, b) the cross-validation and better estimation of the simple mass balance model parameters, and c) an improved estimation of the uncertainties in the streamflow-based mass balance reconstruction and the associated uncertainties in the cumulative values back to 1909.

Temperature index MB model

The authors use a variant of the well known "degree day" of "temperature index" model as described by Marzeion et al. (2012a). I am not familiar with this paper but I know the following study (Marzeion et al. 2012b) were they apply globally an extended version of the model. In both versions of the model Ben Marzeion used monthly solid precipitation, while the present study uses total precipitation. This difference can have strong repercussions on the presented results, since temperature influences the phase of precipitation and thus the MB. In that sense, the choice of the representative altitude of the glacier for the temperature index model is also very important: Marzeion et al. 2012b uses two altitudes ztop and ztongue to represent a glacier, while in this manuscript the representative altitude is not specified. This altitude also has a strong influence on μ.

Marzeion et al. (2012b) used two altitudes to represent their glaciers and differentiate solid from total monthly precipitation in their global study that involved many sites (255 glaciers) with different glaciological and climatological conditions. In the case of ECH and other high elevation areas in the central Andes of Chile and Argentina, it is well known that the bulk of precipitation occurs during the winter months and that the fraction of liquid precipitation is normally minimal compared to the large proportion that falls as snow.

To demonstrate this we have included a new figure that shows the seasonal cycle of temperature and precipitation at ECH extrapolated linearly from the nearby El Yeso meteorological station (Fig. 1D). This diagram shows that the peak in precipitation effectively occurs during the coldest months, and that precipitation during the warmest months (i.e. those that show mean temperatures above 0°C - December to March) only accounts for 4.8% of the annual totals at ECH. For this reason, and to avoid the additional complexity and uncertainties involved in differentiating solid from total precipitation in this glacier with such a short altitudinal distribution (see Fig. 1C), we estimate the winter balance at ECH using monthly total precipitation values from El Yeso.

The parameter μ of the temperature index model is in reality a statistical tuning parameter and must be seen as such[1]: it can efficiently hide model deficiencies and must be used with care, in particular for sensitivity analyses. The authors should use cross-validation to properly assess the real accuracy of the model. The temperature index model might be a good approximation on average, but the authors should provide arguments and evidence for the usability of such a model for sensitivity analyses at the ECH glacier.

Agreed. We performed a cross-validation assessment of the simple mass balance model. This assessment also allowed the optimization of the model parameters that were adjusted to minimize the RMSE of the model at each time step. See section 2.2 of the revised manuscript for details.

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[1] even if there are physical reasons for the temperature index model to be successful, e.g. Hock 2003

Changing glacier geometry

The term “mass balance” used in this study is in fact “specific mass-balance” (Cogley et al. 2011), i.e. the MB per unit area. On decadal time scales the influence of glacier dynamics cannot be neglected. This is why the version of the MB model in Marzeion et al. 2012b (and other global studies) explicitly take glacier dynamics into account (using simple scaling laws, but still).

There are important differences between the studies of Marzeion et al. 2012a (which is the approach we use in our study) and Marzeion et al. 2012b. The first study reconstructs Alpine glacier mass balance changes using *reference-surface mass balance* estimates, whereas the second study develops *conventional glacier mass balance* reconstructions to estimate the global contribution to sea level from these mass balance changes. The analyses in Marzeion et al. 2012b necessarily require estimations of volumetric changes associated with the reconstructed changes in glacier mass balance. Therefore Marzeion et al. 2012b included a number of additional equations and parameters to explicitly account for the changes in glacier geometry at each site.

In our study we calculate reference-surface mass balances for ECH (i.e. the mass balance that would have been observed if the glacier’s surface topography had not changed, see Cogley et al., 2011), and thus the estimation of the impacts of changing glacier geometry in the mass balance reconstruction were not included in the assessment.

Interestingly however, given the particular shape and hypsometry of this small cirque glacier (see Fig. 1C), the conventional and the reference-surface mass balance estimates at ECH were probably roughly similar over the relatively short periods of time evaluated here. Fig. 1C shows that over the 1975-2013 period, the glacier frontal elevation has not changed much but instead the whole glacier has thinned and seems to be disintegrating in place. In the first report on this glacier mass balance program, Peña and Narbona (1978) indicate that the main glacier body is distributed between 3650 and 3880 m asl, an elevational range that has not changed much until today. In addition, the ice mass loss over the 1975-2013 period has been around -15 m w.eq, which is well within the error bands used to calculate, for example, the elevation range and the surface topography of the glacier.

It is not clear to me how changing glacier geometry is compatible with the single linear regression model based on streamflow presented here. Interestingly, the regional streamflow time series could contain the signal of changing glacier geometry and volume, but this should be proven and discussed. Currently, it am more than skeptical about any of the absolute values of specific MB presented here, especially the ones without error bars (e.g. accumulated MB, see specific comments below).

The glacier mass balance reconstruction is also based on reference-surface estimates, and therefore the changes in glacier geometry were not considered in this case either.

We have calculated the uncertainties associated with the reconstructed cumulative series (see Fig. 4B), and have also added notes discussing these uncertainties and the implications for using these reconstructed cumulative series to derive mass balance change estimates over extended periods. This is an important point and we thank the reviewer very much for suggesting this addition to the paper.

2 Specific comments

Title
I find that the title does not reflect the content of the manuscript. In the end the ECH is the only glacier which mass balance has been reconstructed.

Agreed. We now refer to the reconstruction of ECH in the title.

Structure
the text is sometimes repetitive. Since there are no sub-sections the logical structure is difficult to follow.

Agreed. We have now removed some repetitive sentences and have included subtitles to improve the readability of the manuscript.

P4955 "we believe that the parsimonious approach presented here provides solid evidence for objective testing of the relative significance of temperature and precipitation variables to the year-to-year variability of this glacier's mass balance": this does not convince me. Where are these evidences?

Agreed. We have now removed this sentence and have tried to make clear the caveats involved with these simplistic exercises (see reply to a similar comment from Referee #2 above).

P4960 L7 "indicating that up to 78% of the variance in the ECH record can be accounted for by the minimal model presented in Eq. (1)" : here cross-validation should be used to assess the real R2

Fixed. We have now cross-validated this glacier mass balance model and discuss the details and results in the text. Thanks to this exercise we have modified slightly the model parameters α (from 4.1 to 3.91) and μ (from 92 to 90.1, see section 2.2). These new parameter estimates minimize the RMSE between the observed and modeled mass balance estimates over the 1977-2012 model calibration period.

P4960 L25 "The snowpack-based mass balance reconstruction is not shown (...)" : does it even makes sense to mention the snowpack model if it is never used? The streamflow and snowpack time series seem to be highly correlated anyway.

We believe it is important to mention and show in the table the results of the snowpack-based regression model to support our assertion that the annual mass balance changes are strongly associated to changes in winter precipitation in this region.

P4960 L29 : "68% of the variance" : again, cross-validated?

Yes, this value is reached after cross-validating the estimated values obtained from the linear regression model. This is mentioned in section 2.3.

P4971 L2 : “offering the possibility of reliably extending the information on glacier mass balance changes back to 1909”: and what about glacier geometry?

Glacier geometry changes are not included in the reference-surface mass balance estimates used here. See reply to related comment above.

P4961 L9 : “The year 1968 is the most prominent feature of this extended negative period and according to these results it likely constitutes the most negative mass balance year since at least 1909”: I see that MB observations show at least one more negative year (approx. 1998, El Niño?). Given the large uncertainties of this very simple statistical model and the well known property of linear regression models to damper the variability, such precise statements cannot be formulated.

Agreed. We have removed the reference to this extreme negative mass balance year.

P4961 L13 : “an overall negative trend totalling almost -42 m.w.eq. between 1909 and 2013 (Fig. 4b)” + all numbers listed afterwards. For the cumulated time series the authors forgot to take the uncertainties into account. The accumulated values are subject to the “random walk” effect and will have a much larger spread. This uncertainty has to be quantified, for example by computing the spread of a bootstrap of random realisations.

Agreed. We have removed the reference to these estimated values and only discuss the overall patterns observed in the reconstructed ECH series.

We should first note that we have included an improved estimation of the reconstruction uncertainty which takes into account the standard error of the regression estimate, and the standard error of the mean streamflow values used as predictors in the reconstruction model (see section 2.3 and Fig. 4A). The first error was calculated during the calibration of the model, and the second error is directly proportional to the number of individual river records used to calculate the regional average (it increases in the first decades of the record due to the decreasing number of runoff records that contribute to the regional composite).

The cumulative mass balance record now includes the estimated uncertainties calculated by propagating (adding) the reconstruction errors as we move back in time. See section 2.3 for details.

References

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