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The Cryosphere Discussions

Interactive comment on "Reconstructing glacier mass balances in the Central Andes of Chile and Argentina using local and regional hydro-climatic data" *by* M. H. Masiokas et al.

Anonymous Referee #3

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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. 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. 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. 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.

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 z_{top} and z_{tongue} to represent a glacier, while in this manuscript the representative altitude is not specified. This altitude also has a strong influence on μ.

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

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

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.

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).

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 sceptical 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).

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.

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

P4955 *“we believe that the parsimonious approach presented here provides solid evidence for objective testing of the relative significance of temperature and precipi-*

tation variables to the year-to-year variability of this glacier’s mass balance”: this does not convince me. Where are these evidences?

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 R^2

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.

P4960 L29 : *“68% of the variance”*: again, cross-validated?

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

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.

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.

References

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