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The Cryosphere Discussions

Interactive comment on "Spatial patterns of North Atlantic Oscillation influence on mass balance variability of European Glaciers" *by* B. Marzeion and A. Nesje

Anonymous Referee #4

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General comments
This paper describes the application of a 'minimal' mass balance model to all glaciers in the European Alps and Scandinavia, in order to derive correlations of the computed mass balance series with the NAO. The text is well-structured and written in clear English. However, I have serious concerns about the methods used for the mass balance reconstruction, as explained in the major comments below. In addition, this paper does not present sufficient new results or applications. I therefore do not think this paper meets the quality standards of The Cryosphere.

Major comments

- The influence of the NAO on climate in Europe is well-known and several studies relating European glacier mass balance variations to the NAO have been conducted in the past. Several are mentioned by the authors in their introduction, but also Pohjola and Rogers (1997) and Rasmussen and Conway (2005) provide correlation analyses for Scandinavia. Although these studies only included glaciers with mass balance observations, the spatial coverage is similar to the current study. The authors do not state the additional value of their work. Furthermore, I wonder what applications the results could have in further research; the authors do not provide any.
- The authors use a so-called minimal mass balance model to extend the mass balance records back to the beginning of the 20th century. I doubt whether such a simple model is suitable for the presented application. In this model accumulation is determined from estimated solid precipitation (multiplied with a factor in part of the model variants), while ablation is a linear function of positive air temperatures. The multiplication factors are derived from a calibration with measured annual mass balances or by assuming that the total area-averaged mass balance is zero. Subsequently, the modelled mass balance is correlated with the NAO. Since glaciers in both regions have distinct accumulation and ablation seasons, this principally comes down to correlating winter precipitation and summer temperatures with the NAO. Modelling the mass balance seems like a redundant intermediate step. The actual glacier mass balance is affected by an interplay of processes, depending on more meteorological variables than only temperature and precipitation. To demonstrate whether their model provides a good measure for the correlation between mass balance and NAO, the authors should first compare the correlations of the measured and the modelled mass balances with the NAO over the same period.

- It appears that the authors only use measured annual mass balances to calibrate the two multiplication factors (a and μ) in their model. This is an underdetermined problem, since one variable is used to calibrate two parameters. The parameter set with the smallest error may not reflect the real accumulation and ablation on the glacier, it is more a minimization of the errors in the model formulation and the climate input data. For a proper parameter calibration, the authors should calibrate a with the measured winter mass balance for the set of glaciers, which can easily be obtained from the World Glacier Monitoring Service.

- An even more serious problem occurs for the 'climatologically derived' model, where the multiplication factor for precipitation is not included. The authors implicitly assume that the CRU precipitation is representative of the precipitation falling on the glacier. The calibrated values for a_{opt} in Table 1 demonstrate that this is very likely not the case and from my own experience I also know that precipitation in glacier catchments is seriously underestimated in the CRU data.

- The calibration procedure of the three model variants gives unexpected results, for which the authors should provide explanations to convince the reader that these are correct. Most surprising is the result that the mass balance modelled with the least realistic model, the climatologically derived model (no precipitation correction, glaciers in equilibrium, no calibration with measurements) show the highest correlation with the measured records. I would expect that the individually trained model performs best. Does this imply that one should not bother about calibrating a mass balance model with measurements, but just use a parameter set that gives a zero annual mass balance? Secondly, the bias for the individually trained model is close to zero, but when the mean parameter values are used to compute mass balances, the bias becomes considerably negative for all regions. How can that happen? I would expect that mean parameter values would result in overestimated mass balances on one glacier and underestimation at another, so the mean bias would still be similar. Could it be that extreme outliers affect the cross validation? Last, the standard deviation of the biases is very large, in my opinion.

Detailed comments

- Section 2: This type of mass balance model is not able to incorporate effects of changes in glacier area and hypsometry on the mass balance. The authors should note that the mass balance they calculate is with respect to a reference geometry. Such a reference mass balance record is the correct choice when examining the relation between climate changes and mass balance variations (e.g., see Leclercq et al., 2011). However, most glaciers in the sample probably did not have a constant geometry over the period of observations. How might this affect the calibration and validation procedures?
- p8, 2-3: It is not surprising that the correlation between temperature and elevation is very high: the CRU temperature at every grid point has been interpolated from station records, taking elevation into account. Calculating vertical lapse rates from the CRU data practically comes down to determining the lapse rates used to interpolate the temperature records over the grid.
- p8, 3-7: The authors do not motivate their choice for a precipitation increase with elevation of 2% per 100 m. They only note that it is lower than vertical gradients derived from the CRU data. Why did they still choose this (relatively small) value? And is it necessary to apply a vertical gradient, is this effect not already captured in the parameter a?
- p8, 15-19: It is not clear to me how the solid precipitation fraction is determined. What do the authors mean with 'Within this range...'? Elevation range, temperature range, the range of the fraction [0,1]? If only part of the glacier receives solid

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precipitation, the glacier hypsometry is needed to compute the correct fraction, but I assume the authors assumed a constant distribution of area with elevation?

- p8, 21: As already mentioned in the major comments, errors in the absolute value of $P_{\text{i,clim}}$ directly affect the value found for μ_{clim}, how can the authors be sure that $P_{\text{i,clim}}$ is representative of the precipitation on the glacier? Optimized values for the parameter are close to 2 instead of 1; a doubling of $P_{\text{i,clim}}$ would also double μ_{clim}.
- p9, 16: What value is used for t_{lag}?
- p10, 6-8: It is not clear to me what mse_{ref} represents and how it is calculated, can the authors clarify this?
- p10, 15: I would like to see a comparison of μ_{clim} and μ_{opt}, the provided tables and figures do not allow for this. Are values for individual glaciers very different?
- p11, 13-16: I do not understand the explanation why the mean model performs better, what is the 'vastly increased data basis'?
- p12, 10-11: For a reliable correlation analysis, the modelled mass balance should not only correlate highly with the observations, but the mass balance variations should have a comparable magnitude as well. This is not the case for any of the three models as shown in Figure 5, especially not for the climatologically derived model. It suggests that the model does not capture all processes that determine the interannual variability in the mass balance. The worst performance of the climatologically derived model seems to be a direct consequence of not including a: accumulation and hence ablation are underestimated.
- p12, 15-16: Figure 6 does not allow for a detailed comparison of the three models' performance. I would suggest the authors to include an additional figure showing

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the measured and three modelled mass balances in one panel, only covering the period with measurements.

- p12, 19: As argumented above, I would think the climatologically derived model performs worst and therefore do not understand why the authors select it as the most reliable.
- p13, eq 8 and 9: With this notation, it is not clear which terms are anomalies and which are not. Please use a symbol like Δ, δ or ' to indicate the (two in this case) anomaly terms, also at other places in the manuscript.
- p13, 15-16: Please motivate why these years were selected, the measured mass balance is not captured well for the first year.
- p14, 1-15: Please discuss the results in Figure 7, clearly mainly variations in winter precipitation affect the mass balance on this glacier. Are similar results obtained for the other years in the record and for the other glaciers? Perhaps show a similar figure for a glacier with a small or anti-correlation with the NAO.
- p15, 3-4: The correlations in Figure 8 are very low, are they significant? One would expect the highest correlations for the most maritime glaciers in southern Norway, but this seems not the case. Could this be because also summer months are included in the analysis?
- p15, 10-11: Apparently, the authors expected to find the correlation to depend on the terminus elevation. Please elaborate on this, what would the terminus elevation represent, continentality or glacier size perhaps?
- p15, 18-24: Why is exactly this division of the year chosen? The period March-November covers three seasons and different phases in the mass balance seasonal cycle. This period is not suitable to examine the effect of summer temperatures on the mass balance, instead only the summer months or the main

melt season should be used. A more logical choice would for example be to use November-March as the winter season and May-September for the summer season. Then the mean NAO over both periods can be correlated with the cumulative mass balance over the same two periods, instead of averaging the values per month. This would practically come down to correlating the winter mass balance with the winter NAO and summer mass balance with the summer NAO.

- p16, 5-10: Do the authors suggest that in parts of Norway summer temperatures correlate positively with the NAO while in other parts there is an anti-correlation? Are summer temperatures that different between those regions or are other factors at play?

- p17, 2-5: Not only the increase with altitude, but also underestimation in the CRU data is not taken into account.

- p17, 9-12: The best measure for the altitudinal gradient in precipitation on the glaciers are measured winter balance profiles, which are available for many glaciers in Scandinavia and the European Alps.

- Table 1: What is the unit for μ_{opt}? Preferably, μ_{opt} (Table 1) and μ_{clim} (Figure 2) would be presented with the same unit, to facilitate a comparison.

- Table 2: Is the bias for southern Scandinavia negative because the mean measured mass balance was positive, i.e. as a result of the zero mass balance assumption? Please also include the calibrated values for μ_{clim}, to compare with Table 1.

- Figure 1: This figure is impossible to interpret, since different months are used for each grid cell. I would suggest to show panels for both winter and summer conditions.

- Figure 2: Please use smaller markers (like the green dots in Fig. 1) in this and similar figures, there is too much overlap to see the majority of the glaciers.
- Figure 3: Please indicate the value associated with the vertical line (12 years) and preferably extend the x-axis until 0.
- Figure 8: Please use the same colour scale and range in all correlation figures to facilitate a comparison between figures. Why does Ålfotbreen have such a low correlation, it is one of the most maritime glaciers in the sample?

References

Leclercq, P. W., R. S. W. van de Wal, and J. Oerlemans (2010): Comment on "100-year mass changes in the Swiss Alps linked to the Atlantic Multidecadal Oscillation" by Matthias Huss et al. (2010). The Cryosphere Discuss., 4, 2475-2481.

Pohjola, V. A. and J. C. Rogers (1997): Atmospheric circulation and variations in Scandinavian glacier mass balance. Quat. Res., 47, 29–36.

Rasmussen, L. A. and H. Conway (2005): Influence of upper-air conditions on glaciers in Scandinavia. Ann. Glaciol., 42, 402–408.

Interactive comment on The Cryosphere Discuss., 6, 1, 2012.