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Climate of the Past Discussions

Interactive comment on "North Atlantic Oscillation controls on oxygen and hydrogen isotope gradients in winter precipitation across Europe; implications for palaeoclimate studies" *by* Michael Deininger et al.

Anonymous Referee #3

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General comment:

Deininger et al. present an interesting manuscript demonstrating the influence of the NAO on the d18O and dD values of winter precipitation in Europe as well as the West-East gradient across Europe, i.e., the strength of the continental effect in dependence of the NAO. Comparison with the data of an isotope enabled climate model (ECHAM5-wiso) show a broad agreement with the meteorological data and enables to study the underlying processes in more detail. The evaluation suggests that the dependence of the d18O and dD values on the NAO results from both variable air temperature and

amount of precipitable water in the atmosphere. These are important findings, which should definitely be published. Finally, the authors discuss the potential to reconstruct past NAO variability (or at least persistent states of the NAO) based on changes in the gradients across Europe.

The paper is well written, and the complicated calculations (dependence of the slope on NAOi) are presented in a way that even non-expert readers should be able to understand the main messages. The results appear robust and most of the conclusions seem justified to me. Thus, I can definitely recommend publication in Climate of the Past.

I have a few editorial and specific comments (listed below), which should be taken into account prior to publication.

Detailed comments:

Line 40 ff.: The second paragraph of the introduction is relatively long and mainly summarises what will be shown and discussed in the paper. This section could be shortened substantially to make the paper more concise.

Line 46: "...to better evaluate the NAO-dependence on isotope longitudinal gradients." I thought the gradient depends on the state of the NAO and not the other way round? This should be clarified here and throughout the paper.

Line 56: 37 stations have been analysed. 28 are GNIP stations, 6 are ANIP stations. What about the remaining 3?

Line 126 ff.: "Comparison of the longitudinal δ18Opw and δDpw gradients derived from the ECHAM5-wiso with those from the station-based data show that slopes from the ECHAM5-wiso data reproduce the observed station-based slopes quite well (Figure 4)." I do not agree with this statement. All model data sets show a curve (i.e., the most negative slopes are shown for the 3rd and 4th NAO class) rather than a linear relationship with the NAO classes. It is, thus, misleading to state that the model data reproduce the station data “quite well”. It would be good to see the fit statistics (slope, r2 and p-value) not only for the station data, but for the model data as well (compare caption of Fig. 4).

Line 212 ff.: “Repeating the calculations using the vapour-ice phase change (snow) instead results in calculated differences that are still too small to explain the observed differences in δ18Opw and δDpw between the western- and eastern-most stations (not shown).” Please provide a bit more information on this. I do not request a detailed discussion, but in the present form, the reader would not be able to do the calculations themselves if they wanted to.

Line 217 ff.: “By contrast with the observational (GNIP) data discussed above, the ECHAM5-wiso simulated differences in δ18Opw and δDpw can largely be accounted for by model air-temperature differences alone.” ... “Overall, however, these results supports the conclusion above that the winter air temperature effect on the longitudinal winter δ18Opw and δDpw gradients is insufficient to explain the observed difference between the western and eastern GNIP stations.” This is not clear to me and even appears contradictory. If the effects observed in the model data can be explained by the model temperatures, this does not support the conclusion derived from the data. Please clarify.

In addition: “It is intriguing that the observed (GNIP datasets) and simulated (ECHAM5-wiso simulations) temperature slopes differ (Figure 6), while the slopes for longitudinal δ18Opw and δDpw gradients are apparently similar (Figure 4).” As far as I understood the text, the model temperatures in Eastern Europe are colder than those of the station data. If the d18O and dD values (i.e., the gradient and its dependence on the state of the NAO) are similar in the model and the data, but the temperatures are different, this either means that the dependence of model temperature on the NAO is too strong or that the sensitivity of the model d18O and dD values is too weak. In any case, this is an important difference, which makes it difficult to use the model data to interpret the station data. Based on the discussion following below, however, this statement is not

necessary.

Line 330 ff.: “The reason for the different strength of these two mechanisms (temperature gradient and precipitation history) on the longitudinal δ18Opw and δDpw gradients for the observed (GNIP) and simulated (ECHAM5-wiso) datasets remains unclear, suggesting that the ECHAM5-wiso simulations warrant further investigation.” I am not a climate modeller, but how representative are the precipitation data of the model for the 13 stations (still a relatively low number) considered here. As far as I know, simulating (high-resolution) precipitation patterns is still difficult. Thus, the model data (which represent climate variability in a larger grid cell) may be more representative for the dependence of the west-east gradient in precipitation on the NAO than the station data. In summary, it is not very surprising for me that the precipitation data of the stations do not show a dependence on the state of the NAO, but the model data do.

Line 334 ff.: I would suggest to strongly shorten the first paragraph of section 4.2. It only summarises results from the analysis, which has partly already been presented in section 3.2. I would move all these results in section 3.2, and briefly summarise the findings here in one or two sentences.

Line 401 ff.: I would remove the reference to tree rings here, which mainly record summer climate (and water isotopes).

Line 426 ff.: “Note that our assumption about the temperature change represents a limiting case, because it implies that the annual air temperature, to which the cave air temperature is usually equilibrated, also decreases by the same value.” As the authors state themselves, this assumption is not reasonable. It may be possible to find a correlation between winter and annual temperature (in the station and the model data). Based on that, one could try to estimate the dependence of annual temperature on the state of winter NAO. However, temperature is very stable in most caves, and an inter-annual change of 1.3 °C, as assumed in the example for the station Stuttgart, is almost impossible and may only occur in a strongly ventilated cave. In such caves, however, other effects, such as precipitation of CaCO3 under conditions of disequilibrium stable isotope fractionation or evaporation, will probably dominate the d18O values of the speleothem. The temperature effect may only be visible on a decadal or even longer time scale. Thus, the reference to persistent changes in the NAO on centennial to millennial time scales should be given at the beginning of the paragraph. However, I would rather suggest to remove the calculation because the caveats may not be present to many readers.

Section 5 in general: Since this section discusses the potential of speleothems for an NAO reconstruction based on speleothems, I miss a critical discussion of other potential "problems" of speleothems for NAO reconstruction (smoothing of the signals in the aquifer, contributions of different seasons than winter, disequilibrium stable isotope fractionation, dating uncertainties, etc.). I know that the authors are aware of these problems, so they should not be omitted from the discussion here. Mischel et al. (2015) have modelled some of these processes in detail. Their study could be referenced in this context.

Finally, as the two other reviewers question the stationarity of the NAO and the gradient in the past and, in particular, its dependence on the location of the pressure systems during the Early Holocene, it may be interesting to read and discuss the recent paper by Wassenburg et al. (2016).

References

Mischel, S. A., Scholz, D., and Spötl, C., 2015. d18O values of cave drip water - a promising proxy for the reconstruction of the North Atlantic Oscillation? Climate Dynamics 45, 3035-3050.

Wassenburg, J. A., Dietrich, S., Fietzke, J., Fohlmeister, J., Jochum, K. P., Scholz, D., Richter, D. K., Sabaoui, A., Spötl, C., Lohmann, G., Andreae, M. O., and Immenhauser, A., 2016. Reorganization of the North Atlantic Oscillation during Early Holocene deglaciation. Nature Geoscience 9, 602-605.

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