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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.

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General and Specific 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. We will revise the manuscript as suggested if possible.

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. We will revise the manuscript

as suggested.

Line 56: 37 stations have been analysed. 28 are GNIP stations, 6 are ANIP stations. What about the remaining 3? We will revise the manuscript as suggested. The remaining three stations were also GNIP stations.

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). We will revise the manuscript as suggested. We will include the r2 and p-values of the evaluation of the ECHAM5-wiso simulations in the figure caption of new Figure S3 (the ECHAM5-wiso evaluation is moved to the supplementary material, as suggested by Reviewer 2). The fit statistics show that the ECHAM5-wiso data has a weaker relationship the wNAOi. We will include these conclusions in the discussion of the ECHAM5-wiso results in the new section in the supplementary material.

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. We will revise this sentence by clarifying what parameter is used and will state the values used. We will include the reference from which the values were used.

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.

We will revised this section and deleted the sentence that is commented by the reviewer. The statement of this sentence is already given in the previous paragraph and it was confusing in the paragraph discussing the ECHAM5-wiso simulations. Note that the discussion on the ECHAM5-wiso data will be put in the supplementary material as suggested by Reviewer 2.

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. This is an interesting point that the reviewer highlights here. However, it is beyond the scope of this study to evaluate the robustness of the precipitation data here. This would require a rigorous comparison between different models and model types (GCM vs. regional models) and observational as well as reanalysis data. This should be and will be investigated in depth by a subsequent study that is developed at the moment.

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. We will revise the manuscript as suggested.

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

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 âŮęC, 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. We will revise the manuscript as suggested. We will clarify the calculations and state some more words on the caveats, like disequilibrium and smoothing effects to justify the points mentioned by the reviewer.

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. We will revise the manuscript as suggested. We will include a discussion on these caveats subsequently to the discussion of the NAO reconstruction. The study of Mischel et al. (2015) will be cited right at the beginning of the discussion of the NAO reconstructions.

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). We will revise the manuscript as suggested. We will included the aforementioned study of Wassenburg et al. (2016) and extend the discussion on the stationarity of the NAO during the Holocene (see action on comments of reviewer 1 and 2).

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