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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 #2

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This manuscript presents an interesting perspective on the modern NAO, and then uses this information to reconstruct its variability through the Holocene. The conclusions of the manuscript are important, and the manuscript is well written with only a few grammatical/spelling errors. The manuscript could almost be accepted as is, but I have a few general comments that I would suggest that the authors consider. It may be that I have missed some obvious points, in which case a slight clarification or simply a response to the comment would suffice. However, I think that a thorough consideration of at least some of the points will help improve the impact of the final published paper.

Comment #1: It is not clear to me that the ECHAM5-wiso simulations add anything substantial to the manuscript. They seem to me to interfere with the flow and the communication of the main points of the research. The research does not provide a thorough test of the model, or a comparison with other existing models (both are beyond the scope of the manuscript). Because the manuscript uses measured data (e.g., NAO data, GNIP data), there exists little uncertainty regarding the quality of the data or the synoptic conditions associated with the data. So why include the model results, which are considerably less certain than the actual data, and in fact are sometimes inconsistent with the other results, and presumably wrong? I would suggest either i) remove all mentions to the model or ii) move the model and associated discussion to the supplemental material. The strongest reason for keeping the model in is the independent analysis of the amount of precipitable water in the atmosphere - so maybe Figure S2 could be moved to the main text, with the 13 main GNIP sites marked on it, and the rest of the model discussion moved to the supplemental material. I think that a reference to the Supplemental Material would suffice as an explanation of how the new figure (the old S2) was constructed.

Comment #2: I wonder if a spatially stationary gradient can satisfactorily capture the complexity of the NAO. In particular, I suggest that the authors consider the 'Augmented NAO Index' of Wang et al., 2012, GRL within the context of their own work. The presumed stationarity of the pressure systems defining the NAO is an issue with most NAO studies, but because this study focuses on a spatial gradient perhaps it should focus more attention on this issue. Would the longitudinal gradient not change orientation through time as pressure centres shift location? Particularly in the Early Holocene, where more extensive sea ice would have relocated the pressure centers?

Comment #3: How dependent are the gradients calculated in Figure 4 on Valentia and Wallingford? Would the gradients be uniformly flat if these two sites were omitted? Even if this is the case, it does not imply a problem with the conclusions, though I suggest the authors investigate whether or not the gradients are driven by these two sites. Also, should these be referred to as ‘continental sites’ or as ‘maritime sites’ particularly since they do seem to behave very differently than the other sites?

Minor comments: Line 23: ‘analyzed extensively’ instead of ‘with great effort’, which suggests the analyses themselves were difficult. 82: how was it determined that these stations had no Mediterranean influence? 447: ‘nett’ 468: ‘Central Europe’ or ‘central Europe’ – be consistent

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