Clim. Past Discuss.,
doi:10.5194/cp-2016-77-AC2, 2016

Climate of the Past Discussions
Open Access
EGU

CPD

Interactive comment

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.

Michael Deininger et al.

michael.deininger@ucd.ie

Received and published: 1 October 2016

General Comments: 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,

Printer-friendly version

Discussion paper

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.

We will revise the manuscript as suggested. We will move the ECHAM5-wiso evaluation to the Supplementary Material and it will be discussed there in a new section and we will mention the ECHAM5-wiso results in the manuscript when it fits the discussion. We will additionally included NCEP/NCER reanalysis data of the amount of precipitable water to independently constrain the dependence of the amount of precipitable water on the wNAOi (see comment of Reviewer 1). The evaluations show that the dependence of the amount of precipitable water between the NCEP/NCER reanalysis data and the ECHAM5-wiso simulation are similar.

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?

We will include the findings of Wang et al. (2012) in the discussion section of this manuscript and highlight potential caveats that are related to the spatial stationarity. If this would be the case we would expect to see that any reconstructed longitudinal gradient based on speleothem δ18O records has a positive slope (towards the east). However, this is not the case as McDermott et al. (2011) have demonstrated for the last 12 ka: instead the reconstruction of Holocene slopes reveals that the slopes of the longitudinal speleothem δ18O gradients are steeper in the early Holocene and become progressively shallower until about 5 ka. This result demonstrates that if the pressure centres shift their location, as suggested from 9 ka to 8 ka by the recent study of Wassenburg et al. (2016) or throughout the Holocene as suggested by (Walczak et al., 2015), that this has only a minor influence on the orientation of the longitudinal gradient.

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.

We will revise the manuscript as suggested. We will include an additional figure in the supplementary material. The results emphasise that the maritime stations have only a minor influence on temperature gradients. The effect is stronger for the absolute values of the precipitation gradients but the NAO dependence is only slightly modified. Therefore, conclusions drawn from the original dataset are not hampered when the maritime stations are included. We will include the results of this evaluation in the manuscript.

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? We will revise the manuscript as suggested.

Specific Comments: Line 23: ‘analyzed extensively’ instead of ‘with great effort’, which suggests the analyses themselves were difficult. We will revise the manuscript as suggested.

Line 82: how was it determined that these stations had no Mediterranean influence?

We did not determine whether these stations have a physical influence of Mediterranean moisture, but labelled continental stations as non-Mediterranean influenced if they have a distance to the Mediterranean coastline >100km. We will include this information in the manuscript.

Line 447: 'nett' We will revise the manuscript as suggested.

Line 468: 'Central Europe' or 'central Europe' – be consistent We will revise the manuscript as suggested.

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-77, 2016.