

## ***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 #1**

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### Summary

The authors have shown convincingly that for continental stations north of the alpine divide in western and central Europe that there is a robust relationship between the winter NAO index and the stable isotope values of precipitation. The study represents a solid advance and the use of the statistics is convincing. The paper is data-dense and well written. Further, the observations of the continental gradients over time fits with the previously-published idea of looking at speleothem-based  $\delta^{18}\text{O}$  profiles of stalagmites or other proxies to reconstruct NAO in the past. The observations are also

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somewhat substantiated by isotope-enabled model results.

General Comments The NAO is a powerful mechanism to explain different Holocene isotope gradients. In this sense the main conclusion of the paper is sound, at least for essentially ‘modern’ conditions for which there are precipitation isotope measurements. Under late Holocene boundary conditions of restricted ice extent, near-modern insolation, and near-modern vegetation cover, the NAO-isotope relationship is likely valid. But for how long into the past? I would feel comfortable extending it a few millennia, perhaps five. But the early Holocene represents a significantly different climate than modern because of the ice sheets and a different orbital configuration. I would suggest that the assumption of time-stationarity back to the early Holocene of the wNAOi and  $\delta^{18}\text{O}_{\text{pw}}$  is possibly correct but currently unsupported. The two sentences around Line 460 do not do this point justice. The manuscript would be stronger by either supporting the assertion with additional evidence that, say, the NAO existed more or less as we know it know during the early Holocene NAO, when the presence of upstream ice sheets and different insolation and vegetation regimes were present, or by providing stronger caveats for extending the NAO discussion prior to the late Holocene. Perhaps using the term “NAO-like” instead of the NAO, while emphasizing that early-Holocene climate had distinctly different forcings and boundary conditions than the late Holocene, would be advisable.

One other conclusion is pretty easily testable but doesn’t seem to have been evaluated rigorously: that precipitable water is less during negative wNAOi states. The current study would have much stronger standing with readers if estimates of precipitable water (say from the NCEP database) and wNAOi were compared directly.

### Specific Comments

Line 122: change to “more strongly negative”.

Please give the altitudes of the >350m non-alpine stations. If they don’t differ much from the <350m stations (line 134), then why separate them out? Would it be better to

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include them with the <350 m stations because of similar response to the NAO?

Line 296: replace “confirmed” with “supported”. From a semantics point of view, observations can “confirm,” but models, being not real, can only support.

Line 337: change exceptions to exception, or provide another example.

Line 371-372: back trajectory analysis would be really interesting to do, also for the stations north of the divide, as it may explain some of the variable strengths of the d18O/longitude relationship for the NAO classes. I think this point deserves a little more emphasis either in the results section, or later in the discussion section. I could see it being relevant for high- vs. medium-latitude North Atlantic oceanic source regions for different moisture sources advected inland.

Line 418: Theoretical not theoretically

Lines 411-417: the point about modern relationships maybe not being representative of past conditions is important and requires some more emphasis.

Line 443: would read better as “situated in the Swiss Jura mountains approximately xxx km from the alpine divide. . .”

Line 446: equilibrium typo; line 446 “net” not “nett”

Line 457: There is no Figure 8 in the manuscript. Supplemental Figure? I would like to such a figure in the main text, as it is a crucial test of the current manuscript’s hypothesis.

Line 458-460: this is where I would suggest the assumption of stationarity of the wNAOi and d18Opw is not supported. Certainly not for the “entire Holocene”, but probably true for the past few millennia or so after ice sheets had decayed and land vegetation was established. One way around this problem is, for the pre-late Holocene, to refer to “NAO-like” behavior.

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