Clim. Past Discuss.,
doi:10.5194/cp-2016-77-AC1, 2016

Climate of the Past Discussions
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EGU

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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Received and published: 1 October 2016

General Comments: I would suggest that the assumption of time-stationarity back to the early Holocene of the wNAOi and d18Opw 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

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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.

We will revise this section and extend the discussion to justify the points raised by the reviewer. For this we will include recent studies that investigate the stationarity of the NAO during the Holocene (Wassenburg et al., 2016;Walczak et al., 2015). Furthermore, we will extend the discussion on the stationarity of the NAO during the Holocene in general and highlight potential caveats of our approach.

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.

We will revise the manuscript as suggested. We will include the analysis of NCEP/NCER reanalysis data of precipitable water in the manuscript. We will evaluate the dependence of the precipitable water on the wNAOi over Europe and will add a new figure in the manuscript. The NCEP/NCER reanalysis data shows a similar variability as shown for the ECHAM5-wiso data. Therefore, the results and conclusions drawn from the ECHAM5-wiso data are also valid for the NCEP/NCER reanalysis and the overall conclusions don’t change. (See the attached figure 1.)

Specific Comments: Line 122: change to “more strongly negative”. We will revise the manuscript as suggested.

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

We will revise the manuscript as suggested. The response of the non-Alpine stations is indeed similar to the NAO as for the continental stations: this is indicated by the sen-

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sitivity of the proxy to the NAO as well as by the calculated slopes. The slopes of the continental gradient in response to the NAO calculated for all continental stations (including the non-Alpine stations) are similar compared to one shown in the manuscript and are within the range of uncertainties.

Line 296: replace “confirmed” with “supported”. From a semantics point of view, observations can “confirm,” but models, being not real, can only support. We will revise the manuscript as suggested.

Line 337: change exceptions to exception, or provide another example. We will revise the manuscript as suggested.

Lines 411-417: the point about modern relationships maybe not being representative of past conditions is important and requires some more emphasis. We will revise the manuscript as suggested (see above).

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

Line 446: equilibrium typo; line 446 “net” not “nett” We will revise the manuscript as suggested.

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. We will revise the manuscript as suggested.

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. We will revise the manuscript as suggested (see above).

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

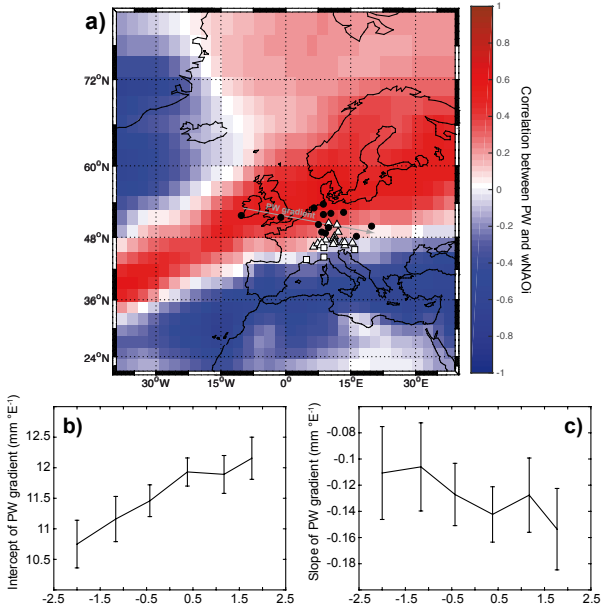

Fig. 1. a) Correlation map between the wNAOi and the amount of precipitable water (PW) for the month December to March based on NCEP/NCER reanalysis data for the period 1948-2016 and the results of the longit

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