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Climate of the Past Discussions

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Interactive comment

Interactive comment on "How dry was the Younger Dryas? Evidence from a coupled δ^2H-δ^{18}O biomarker paleohygrometer, applied to the Lake Gemündener Maar sediments, Western Eifel, Germany" *by* Johannes Hepp et al.

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This is a joint comment by D. Sachse (GFZ Potsdam) and F. Schenk (Stockholm University - Bolin Center for Climate Research)

The authors are applying an approach to quantitively reconstruct relative humidity from terrestrial sedimentary archives based on biomarker δD and δ18O values. In theory, this approach is elegant as it relies on two isotope systems and would represent a useful addition to our proxy portfolio, if it works. The underlying assumption is that

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Discussion paper

sedimentary n-alkanes and sugar biomarkers are equally sourced from the same vegetation, which is very unlikely to be actually the case. Earlier reviews and comments had already discussed some issues of this manuscript with regard to this assumption (see comment by E. Schefuß) as well as the chronological uncertainties due to the scarcity (or absence) of reliable age constraints (see comment by B. Zolitzschka), so we will not repeat those here.

In addition to those we would like to comment on some of the mechanistic data interpretations, which we think are not supported by the presented data and lack the context to earlier findings.

The authors argue their reconstructed relative humidity changes during the Late Glacial period and the early Holocene are driven by solar activity, which seems to be based on a perceived visual similarity of reconstructed 14C production rates and their relative humidity reconstruction.

In order to prove a mechanistic relationship between these parameters, two conditions have to be fulfilled:

1) objective demonstration of an actual covariation of these two parameters during the study period, e.g. a statistically significant correlation between the two parameters and 2) demonstration of a conceivable causal relation, i.e. a mechanism.

We think the current manuscript does not provide those, 1) is completely lacking and 2) is insufficient. We would recommend that a more detailed statistical analysis is presented than what is provided on page 27.

Page 27, lines 15-19: The way the Monte-Carlo-Simulations are conducted and the results should be explained in more detail here. The reported "maximum correlation coefficient of 0.37" does no tell much without providing the confidence limits of this test. If the test is based on smoothed values, the effective degrees of freedom may become very low and even r=0.4 may not be distinguishable from noise.

From visual inspection of Figure 6, there is no apparent consistent link between solar activity (IntCal13 14C production rate, Greenland 10Be flux) and RHdv. Based on the solar activity proxies, periods of negative activities are assumed for around 12.5 ka BP, 11.2 ka BP (roughly PB) and 10.2 ka BP (roughly BO). The low solar activity around the PB coincide with a period of very high RHdv while the low solar activity around 12.5 ka BP and 10.2 ka BP does not.

In addition, from Figure 6 it is not clear how Mg/Ca-based SST from South Iceland Rise should in any way be linked to RHdv. Neither the data show any convincing co-variability nor is there a plausible mechanism which should link these two regions. A more straightforward driver or humidity changes may be rather a southward migration of the North Atlantic sea-ice front in response to a weak AMOC state during the YD (e.g. Renssen et al. 2018). Overall, it is also odd to argue that "one possible driver for the unexpected Lake Gemündener Maar RHdv variations could be the solar activity" in the abstract without trying to explain why the same solar activity causes "expected" variations in other lakes including Lake Meerfelder Maar (which is a site closeby).

We note, that the chronological uncertainties due to the poorly constrained age model (see comment by B. Zolitzschka) make any correlation analysis difficult, so that 1) can possibly only be confirmed after the age model uncertainties are reduced.

Further, the authors suggest that their data show a wetter first and a dryer second phase of the Younger Dryas (YD) period, but provide no data on this, i.e. how much different those supposedly were. When looking at Fig. 6 it is difficult to actually see a change in reconstructed relative humidity (taking into account the error ranges) during the whole studied period, except for the apparent increase of values at around 11ka BP. If differences in a quantitative proxy are being discussed, these should be stated with actual values and uncertainties and be tested for actual statistically significant differences.

The interpretation of the derived relative humidity reconstruction, i.e. no change in relative humidity at the YD onset, a wetter early YD and a dryer late YD and early Holocene, also should be discussed in the context of earlier findings. This interpretation disagrees with the bulk of previous literature data, based on palynological and geochemical evidence, which shows evidence for a dryer first half of the YD (in particular compared to the Allerød) and a wetter (and more variable) second half of the YD (Brauer et al., 1999; Bakke et al., 2009) in Europe. The authors mention this disagreement but provide no explanation (except that the other proxies are potentially biased). Also, new modelling results suggest an overshoot of humidity conditions at the Holocene / Younger Dryas boundary (Renssen et al., 2018) – a feature frequently captured in hydrological proxy data from ice cores (Rasmussen et al., 2006) to lacustrine sediments (Rach et al., 2017) but not apparently in the presented record.

If the interpretation of the data (see above) holds after statistical tests have been made, then these disagreements with existing proxy and modelling data need to be discussed. If no relationship can be statistically proven and no agreement with previous reconstructions is found, the potential of the proxy as a quantitative recorder of relative humidity should be re-evaluated.

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