Reply to D. Sachse and F. Schenk (SC4: Data analysis and paleoclimatic context)

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 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.

→ We thank D. Sachse and F. Schenk for their comment and the possibility to emphasize here again, that our coupled $\delta^2H_{n\text{-alkane}}$-$\delta^{18}O_{sugar}$ paleohygrometer approach does work. The approach is based on our current knowledge about (i) RH-dependent leaf water enrichment and (ii) the incorporation of the $\delta^2H/\delta^{18}O_{leaf\ water}$ signal into leaf-derived *n*-alkane and sugar biomarkers. Uncertainties and systematic offsets need, of course, to be considered (see e.g. our reply to the comment of E. Schefuß concerning grasses). We agree with the Referees #1 and #2, that the current manuscript suffers from a too detailed description and discussion of methodological issues and will therefore follow their recommendation to shorten our manuscript. Most methodological details are anyway already published. Please allow us to point here again to the validation study of Tuthorn et al. (2015). In that study, *n*-alkane and sugar biomarkers were extracted from topsoils along a climate gradient in S-America covering different vegetation types. Reconstructed RH values based on our coupled $\delta^2H_{n\text{-alkane}}$-$\delta^{18}O_{sugar}$ paleohygrometer approach correlated highly significantly with actual RH values ($R = 0.79$, $p < 0.001$, $n = 20$).
Concerning the chronological uncertainties, please note that they do not limit the take home message of our manuscript, i.e. overall pronounced dry climatic conditions during the YD are not corroborated by our results (see also our reply to the comment of B. Zoliztschka).

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 covariability 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.

Dirk Sachse & Frederik Schenk.

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Rach O., Kahmen A., Brauer A. and Sachse D. (2017) A dual-biomarker approach for quantification of changes in relative humidity from sedimentary lipid D/H ratios. Clim. Past 13, 741–757.
Rasmussen S. O., Andersen K. K., Svensson A. M., Steffensen J. P., Vinther B. M., Clausen H. B., Siggaard-Andersen M.-L., Johnsen S. J., Larsen L. B., Dahl-Jense D., Bigler M., Röthlisberger R., Fischer H., Goto-Azuma K., Hansson M. E. and Ruth U. (2006) A new Greenland ice core chronology for the last glacial termination. J. Geophys. Res. 111.
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→ Statistical data analysis: Following the recommendation of the Referees #1 and #2 we will shorten our manuscript during revision. The Monte-Carlo-Simulation-based correlation analysis will not be included any longer. Still, we are convinced that we should point our readers to the resemblance of the Gemündener Maar RH record with the solar activity records of Rasmussen et al. (2006) and Reimer et al. (2013), as presented by Muscheler et al. (2014).

→ Mechanism for solar activity/insolation effect on RH: Please see page 27, lines 28ff, where we explain that "It can be expected that the North Atlantic Ocean, the main moisture source for Central Europe, revealed already considerable higher temperatures during the Preboreal Humid Phase compared to the Younger Dryas, as indicated by a consistent ~ 2°C increase in planktonic foraminifera (*Globorotalia inflata*, *Globorotalia bulloides* and *Neogloboquadrina pachyderma*) derived Mg/Ca temperatures from a marine sediment core south of Iceland (Thornalley et al., 2009, 2010, 2011). […] This could lead to an enhanced moisture content of the atmosphere. When these wet air masses were transported to continental Europe, where low solar insolation inhibited warming up and drying of these air masses, more humid climate conditions were established." Please also note that the main focus of our manuscript is to establish an RH record for Central Europe, not paleoclimate modeling. Still, we would be very delighted, of course, if experts in the field of paleoclimate modeling such as Renssen and co-workers can include and possibly evaluate our findings in their models.

→ Paleoclimatic context / earlier findings and proxy evaluation:

First, please allow us to point again to evidence from pollen and biomarker results supporting a first wet Younger Dryas followed by a more drier Younger Dryas ending (see the section 3.4 of the current manuscript, which includes the references Isarin et al., 1998; Muschitiello et al., 2015).

Second, we highly welcome a critical evaluation of our proxy record as well as of all other proxy records established for the YD. For instance, the disagreement of our RH record with the one established by Rach et al. (2014, 2017) for the neighboring Meerfelder Maar might be explained with the latter authors using n-C_{23} as aquatic-derived n-alkane in order to reconstruct $\delta^2H_{lake-water}$. However, there is increasing evidence that n-C_{23} is also of terrestrial origin. Aichner et al. (2018) have recently shown for a lake in Poland that n-C_{23} is a variable mixture of aquatic and terrestrial origin in those Late Glacial and Early Holocene sediments. And birch as pioneering and one of the dominant tree species during Late Glacial reforestation of Central Europe is known to produce considerable amounts of mid-chain n-alkanes (Tarasov et al., 2013). Albeit not included in the latter publication, n-C_{23} concentrations of *Betula exilis* and *Betula pendula* reached 653 and even 2323 μg/g in that study. This is highly relevant, because as emphasized by A. Lücke in his comment, aquatic and terrestrial n-alkanes have different biosynthetic fractionation factors. Small changes in the contribution of terrestrial vs. aquatic n-alkanes will thus have a considerable impact on the reconstructed δ^2H (n-C_{23}) record.

Anyway, we wish both the 'dual biomarker approach' of Rach et al. (2014, 2017) and our 'coupled $\delta^2H_{n\text{-alkane}}$-$\delta^{18}O_{sugar}$ paleohygrometer approach' to be further applied and tested and are very much looking forward to learn how the discrepancy in the current state of research concerning RH history during the YD will be solved in the future.

Johannes Hepp & Michael Zech & co-authors

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