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Interactive comment on "A dual-biomarker approach for quantification of changes in relative humidity from sedimentary lipid D / H ratios" *by* Oliver Rach et al.

Anonymous Referee #1

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Rach et al. present a mechanistic leaf-water isotope model to quantify for the first time past changes in relative humidity using lake sedimentary leaf-wax dD records. The model, which is suited for reconstructions from temperate regions where lake water evaporation is negligible, is tested using previously published proxy data from the type site of Meerfelder Maar (MFM) in Western Germany, with a focus on the period spanning the climate transitions into and out of the Younger Dryas stadial.

Personally, I'm glad to see this work finally coming along after several years of development. I think this study constitutes a long overdue leap towards a more quantitative estimate of lipid-based stable water isotope reconstructions, which will ultimately be of interest to a broad readership including organic geochemists, paleobotanists, paleocli-

matologists and climate modellers.

The construction of the model leans upon a large number of unavoidable assumptions involving both plant physiology and environmental conditions. While the majority of the physiological assumptions are reasonably satisfied through empirical evidence, I think some of the assumptions surrounding past environmental and climate conditions are less convincing. Nonetheless, due to the lack of sound proxies that allows reconstructing parameters such as local vegetation cover, atmospheric pressure, wind strength and seasonal air temperature (among others), the model formulation presented in this paper should be considered –overall– as good as it can get.

However, I think there is still much room for improvement and I have a number of serious concerns, primarily involving the methodological approach, the data-uncertainty treatment, and the contents of the paper that the authors should address before publication.

Main comments

1) I am very surprised the authors decided to design this study directly around down-core data without attempting validation of their model using core-top samples from MFM. I think the model should first be tested using lipid δD measurements from surface sediments in tandem with meteorological observations before a deglacial Rh reconstruction is discussed. Whether this unusual direction was taken intentionally or not, I think the authors should provide some explanations.

2) One of the main assumptions upon which the results depend is that pollen records are indicative of the local vegetation cover and that pollen counts scale linearly with waxes concentration in MFM sediments. If this were the case, one would expect to observe a clear correlation between the abundance of n-alkanes and pollen records. I would therefore like to see the distribution of n-alkanes plotted together with selected pollen data (e.g. relative distribution of trees, shrubs, herbs). Ideally, the authors should also present some correlation statistics or at the very least a visual correlation using

for instance the average n-alkane chain length (and other chain length ratios) to make a qualitative distinction between graminoids and woody plants that would confirm the authors' hypothesis.

3) The authors argue that monocots integrate a varying evaporative δD enrichment signal in response to changes in humidity conditions. Following this reasoning, the mixing ratio between leaf water and un-enriched xylem water in C3 grasses will change as a function of the aridity level in the catchment. Therefore, the weighing of the fraction of grass cover used to correct εterr-aq should also vary over time, whereby a much higher fraction of C3 grass integrated the leaf-water evaporative δD enrichment during the Late Allerød (AL) and Early Holocene, when climatic conditions were presumably wetter. The authors instead apply a constant weighing factor of 18% throughout the record in their **correction. This issue should be addressed and the weighing factor should vary over time according to the humidity conditions as inferred using an independent proxy (for instance Artemisia pollen percentages).

4) Despite the authors' effort to address all the potential errors that accompany their data, there is a large source of uncertainty that has been neglected and that has been allegedly circumvented by simply including the analytical uncertainty of the δD measurements. This source of uncertainty is the variability associated with the apparent isotope fractionation values of C3 dicots and C3 monocots as observed for modern plants (e.g. Sachse et al., 2012). This variability should generally be accounted for when estimating Rh. It should also be taken into consideration when calculating the vegetation corrected εterr-aq*, as the authors simply apply the mean εapp difference between dicots and monocots disregarding the related uncertainty, which can be quite substantial (as high as 30‰ at 1 sigma) for each plant group. While I understand that quantifying this uncertainty would disproportionally increase the error bounds of their Rh reconstruction, I think the authors should at least discuss this issue more openly in the text.

Furthermore, along these lines I believe that the authors should attempt at estimat-

ing how well the pollen records represents the local vegetation cover and include this source of uncertainty in the error propagation equation.

5) I am quite sceptical about the use of chironomid-based temperature reconstructions to infer air temperatures. Although I recognize that as yet lacustrine midge assemblages are one of the best proxies for summer air temperature, they have shown to incorporate a number of environmental signals apart from air temperature alone, most importantly catchment vegetation, nutrient levels, lake depth and seasonality (Eggermont and Heiri, 2012; Luoto, 2010). It is therefore likely that this biological proxy was very sensitive to the major environmental and seasonality shifts that occurred at the onset and termination of the YD. Especially, colder and longer winters during the YD might have resulted in relatively colder surface water temperatures during the growing season relative to the air (e.g. replenishment of the local aquifer via snow thawing), in contrast to the preceding warm AL phase. As chironomids are sensitive to water temperatures, it is hard to say to what extent the proxy is biased towards colder temperatures during the YD, not mentioning that the authors use a Dutch record thus making impossible to assess local versus regional factors. I think some of these issues should be openly discussed in the paper.

Moreover, if I remember correctly the chronology that underpins the record from Hijkermeer is based on 14C dating of regional pollen boundaries that have been dated elsewhere. This implies that the temperature record comes with fairly large age uncertainties as compared to the proxy records at MFM, where the chronology is much more accurate. Assuming that the alignment between Hijkermeer pollen records and the regional pollen stratigraphy is precise, I wonder if the author can include in their error propagation estimate the age uncertainty associated with the temperature reconstruction. Even though I understand temperature plays only a minor role in the DUB model, in my opinion, this would result in a much more rigorous estimation of the true uncertainties that accompany the reconstructed Rh.

In addition, I suggest that for reference the authors plot the temperature record together with the Rh reconstruction.

6) Since the authors decided to reconstruct Rh across the Younger Dryas, I think it would be appropriate to briefly present the status of the knowledge on this climate event (as well as to better frame their results into a paleoclimatological context). I suggest to mention the ocean–sea-ice–atmosphere mechanisms that would explain the climate variability observed in European climate reconstructions during this period (e.g. Brauer et al., 2008; Lane et al., 2013; Muschitiello et al., 2016; Rach et al., 2014). I would also recommend that the authors discuss the current understanding of hydro-climatological variability at the onset and termination of the YD in Europe based on the lake sedimentary δD and $\Delta\delta$Dterr-aq reconstructions available so far.

Specific comments

The DUB is based on a number of important assumptions that are discussed along the text (I counted at least 14 in the paper, some of which are "hidden" between the lines). I wonder if the author can provide a summary of these assumptions in the form of a table to facilitate rapid screening.

Similarly, I suggest that all the model parameters, fixed variables, and sensitivity tests are summarised in separate tables. As they are, these information are hard to piece together.

L170: The authors adopt a constant atmospheric pressure value in their model. I would first like to know what value they use and why? Secondly, I would like to know how sensitive their model is to this parameter. Climate modelling studies have shown that there were considerable summer sea level pressure changes over Northern-Central Europe from the late AL to the YD (Menviel et al., 2011; Muschitiello et al., 2015). I would therefore be inclined to apply different sea-level pressure values across the deglaciation. Perhaps the authors can comment on this and openly discuss these problems in the paper.

L176-182: Is there any empirical value that allows calculating Tleaf as a function of Tair? For the reasons I outlined in the previous comments, assuming that it is constantly (and equally) cloudy and/or windy at MFM during both AL and YD does not necessarily hold.

L487-488: A number of studies have shown a bi-partite structure of the YD with relatively drier conditions in Northern, Central and Southern Europe during the Early YD and relatively drier conditions during the Late YD (Bakke et al., 2009; Bartolomé et al., 2015; Lane et al., 2013). It surprises me that this mid-YD transition is not clearly captured in the Rh reconstruction at MFM. Although the authors claim that the record reveals "centennial scale excursions to higher ΔRh after 12.100 BP" I struggle to see any appreciable change in Rh variability. Critically, a marked shift in Rh after the mid-YD transition would support the reconstructed Rh, since virtually no significant vegetation shift had occurred during the YD and therefore the modelled Rh is independent of potential influences from local vegetation changes during this period. However, I must acknowledge that so far the mid-YD transition has been inferred only using qualitative or indirect hydro-climate proxies and thus a net shift from dry to wet conditions in Europe still requires conclusive evidence. Perhaps these issues can be briefly addressed in an apposite YD section of the paper (please see main comment on YD background discussion).

L491-494: How does the percentage of shrub pollen vary with respect to the percentage of tree and herb. If there is a strong covariance between shrubs, trees and herbs then it is not surprising that both the vegetation-corrected (using grass and tree+grass, respectively) Rh reconstructions correlate with the Artemisia pollen percentage (i.e. included in the shrubs pollen record) better than the uncorrected Rh. I would therefore like to know the level of covariance between the distributions of trees, grasses and shrubs at MFM. In addition, I would recommend that the authors include the relative shrub pollen percentages in Figure 3 for reference (please note that in the same figure either tree or herb distributions have not been plotted).

I also wonder whether it is possible that the improved fit between the corrected Rh and Artemisia data (Figure 4) merely stems from subduing the Rh series variability when applying the vegetation correction.

I believe that the paper would benefit from including some selected pollen diagrams (as supplementary material for example). Analogously, I think the original δD and $\Delta\delta$Dterr-aq records should also be presented in Figure 2 or 3.

I also recommend that the author consider to include as Figure 1 their conceptual overview model of the hydrogen-isotopic relationship between source water and sedimentary lipids (Figure 6 in Sachse et al. (2012) and Figure S6 in Rach et al. (2014)) to illustrate the initial formulation steps of the DUB model.

Line-by-line comments

L77-78 and 87-88: In equations (1) and (2) please specify that the terms εbio refer to terrestrial and aquatic components, respectively (i.e. εbio (terr) versus εbio (aq)).

L159: The term esat “Saturation vapour pressure” should be introduced at line 148.

L172: Missing “of” after “function”.

L243-244: Please provide reference for this statement.

L392: The line in brackets should start with small letters.

L416: “. . .low humidity treatment”: how much?

L462: “. . .provides are more. . .” should read “. . .provides a more. . .”

Figure 1: The data plotted in the upper-right panel are not in scale with the data presented in the upper-left panel. Please adjust.

Figure 3: Either the tree or shrub relative distribution is missing from the figure.

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