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Interactive comment on "Leaf wax *n*-alkane distributions record ecological changes during the Younger Dryas at Trzechowskie paleolake (Northern Poland) without temporal delay" *by* Bernhard Aichner et al.

Bernhard Aichner et al.

bernhard.aichner@gmx.de

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The manuscript "Leaf wax n-alkane distributions record ecological changes during the Younger Dryas at Trzechowskie paleolake (Northern Poland) without temporal delay" by Aichner et al. uses high-resolution records of n-alkane distribution and pollen, and lower resolution d13C analyses, during the last deglacial period to address whether n-alkanes reflect plant community changes. This is a well-written paper describing an impressively detailed dataset, and the data support the conclusions. This paper is a step towards developing quantitative paleohydrological reconstructions using compound specific hydrogen isotopes, and as such is an important contribution to the literature. An assumption that is widely made regarding leaf waxes is that mid-chain n-alkanes reflect aquatic plant productivity and long-chain n-alkanes reflect terrestrial plant productivity, with longest chain lengths potentially being produced by grasses and herbs. These assumptions are mainly based on analyses of modern plants. Very little work directly compares n-alkane distributions with plant ecosystem composition as reflected by different proxies in sediment archives, so this study is a nice test of that assumption. In addition, if one can use n-alkane chain lengths to determine or control for plant community changes when interpreting leaf wax isotope measurements, then the analyses are simpler and less expensive than analyzing pollen or macrofossils in addition to n-alkanes. The authors use appropriate methods, which could be clarified with a couple minor text additions, detailed below. These methods and results support their two main conclusions: 1. Changing measures of chain length distributions may be influenced by multiple different factors, and should therefore be examined carefully, in terms of which individual n-alkane chain lengths are actually changing, and 2. "ACL and ratios of n-alkanes are suitable integrative proxies to track major and abrupt vegetation changes in a local setting."

RE.: we thank the reviewer for the constructive suggestions to further improve our manuscript. Responses to detailed comments are given below.

Substantive comments:

1. Changes in pollen and n-alkanes in the same sediment samples are indisputable. The authors should be a bit clearer about age uncertainties when comparing their record to proxy records from other archives. For example: p 13 line 5: the authors mention a lag here, but do not mention a lag for previous intervals. The age model uncertainty varies throughout the record, but at all times in this record, the uncertainty is enough to influence timing & interpretations/comparisons with other records. p 12 line 5: The discussion of the timing of the longer ACL at 13.2 to 13.0 ka: age uncertainty at this time period is _250 years or so (based on visual inspection of the age model in

Supp Fig. 1). The age model uncertainty should be acknowledged in the discussion, as the age model uncertainties in each of the records means that the events can't be assumed to be synchronous. p 12 line 9: Similarly, the conclusion about the timing of the YD onset 170 years after GS-1 onset: the age uncertainty at 12.6 ka in this record appears to be about _150 years, so the lag could mainly be due to age model uncertainty. This should be acknowledged in the text. If the authors have evidence to suggest that the lag at this site is real, this would be a good place to present that.

RE.: uncertainties within age-models are a critical point when comparing proxy data among different records. We critically discussed this already in the initial submission for the YD-termination. Based on the comment of the reviewer we also included a sentence about the 13.2-13.0 events, which was measured in the non-varved section of the Allerød and hence is characterized by higher age-uncertainty. Concerning the YD-onset we can rely on a highly precise age-control, due to the nature of the sediment (annual laminations) from varve counting and thephrochronological anchor points, like the LST) in this core section. This approach enables the identification of leads and lags on timescales of several decades (at least in the laminated section of the core). We emphasize this stronger now at the relevant passage of the text.

2. Carbon isotopes: Isotopic measurements can be influenced by instrument drift through time. In addition, isotopic measurements can be strongly influenced by linearity (i.e., the size of the individual peaks being measured) (Kornfeld et al., 2012). The linearity effect is stronger for smaller peaks. Because the authors report d13C data on n-alkanes of widely varying concentrations in the same sample, these peaks are likely subject to strong changes in linearity, especially the smallest peaks (C23, which differs the most from the other n-alkane chain lengths in its d13C value). How do the authors account for drift and linearity effects in their d13C analyses? Do they run standards at varying concentrations and correct for the linearity effect? This is important to state, as these effects can dramatically impact isotope values. If the authors do not control for the effects of drift and linearity, they should also state that, as that limits

the degree to which the values can be interpreted.

RE.: Linearity effects of the IRMS were assessed by running standards at different concentrations. We monitor stability of this effect by running a Schimmelmann B-mix, consisting of compounds with different concentrations, on a regular basis within the measurement sequence. Based on these data, we principally exclude peaks with an intensity of < 1000 mV for evaluation and samples were concentrated in order to achieve this minimum peak intensity for the lowest abundance compounds. We added the respective information to section 3.3.

3. I have a question about each conclusion that would be helpful to clarify in the text: Conclusion 1: As far as I can understand from these results, it seems as if taking this type of data the next step and making a quantitative interpretation of compound-specific hydrogen isotope ratios still requires an independent record of plant ecology (i.e., pollen or macrofossils). Simply knowing, for example, that ACL increased because the long chain lengths increased doesn't allow for an interpretation of the ecological shift, as this shift could have been caused by one of several different plants in an ecosystem. Is that correct? If so, it could be helpful to clarify that independent ecosystem reconstructions will still be required for future quantitative isotope interpretations. If not correct, then the opposite can be clarified in the text. Conclusion 2: This dataset is a really nice illustration of the similar timing of pollen and n-alkane concentrations. These data do not address whether ecological changes lag climate changes, correct? Perhaps the authors could point that out in the conclusion, as a reader may be inclined to interpret the data as such.

RE.: to conclusion 1: we think that our data show that n-alkane proxies can be a useful tool to reconstruct major ecological shifts in a local setting. While this could well help for interpretation of dD-data, multiproxy approaches i.e. including palynological data will always remain useful, but maybe could be conducted in lower resolution. Palynological approaches can deliver more specific information (i.e. which plant type appeared or disappeared) while n-alkane ratios rather reflect compositional changes (i.e. if certain ratios change, vegetation has changed, but we can't pinpoint this change to individual species). We think that this opinion is sufficiently represented e.g in the abstract "Our results demonstrate, that a combination of palynological and n-alkane data can be used to infer the major sedimentary leaf wax sources and constrain leaf wax transport times from the plant source to the sedimentary sink ". To conclusion 2: we think it is clearly described that we discuss potential time lags between different types of proxies. Lags between climatic triggers and proxy response are a different objective which – as we think – does not need to be treated in the conclusion. We employ proxies mainly sensitive to vegetation change (which of course is likely driven by a climatic change). Compound-specific hydrogen isotope analysis for example could then provide independent information about climatic changes in a next step.

Minor comments:

Fig 1 a: what are the blue lines? I'm confused by the legend: it seems like the white lines with triangles are the kettle holes, but they're listed in the key as subglacial channels? I don't see the symbol for a kettle hole anywhere on the map. Can the symbols be clarified?

RE.: we updated Fig. 1 by mainly removing unnecessary information in order to clarify the issues as listed by the reviewer

p 13 line 1: I think the authors mean 11,540 years ago

RE.: this was corrected

p 13 line 24: ACL is increasing through this Early Holocene interval, not decreasing, as stated in the text

RE.:this was corrected

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