

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

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The manuscript represents a major step forward with regard to the reconstruction of humidity variability and as such is of great interest for the scientific community. Aside from the isotopic part of the methods chapter being rather long (two chapters from page 6-12), the chronology has several weak points. On page 2, line 4-6 Gemündener Maar (GM) is mentioned in line with Holzmaar and Meerfelder Maar. These sites with natural eutrophication since the onset of the Lateglacial formed and preserved annually laminated sediments (ALS), which provided a precise time control for lacustrine systems.

This is completely different for GM, a lake that remained oligotrophic until World War II and only in the 1950ies was culturally eutrophicated, still without any deposition of ALS. This should be clarified in the text.

The chronology is a mixture of one radiocarbon date, the age of LST tephra, a TOC increase assumed for the onset of the Holocene and a pollen-inferred age for the PB/BO transition. Except for the age of the LST all other ages are questionable. 1. radiocarbon age: the age of 11,950 BP is from charcoal and as such potentially can be linked to reworking. One radiocarbon age to date the Lateglacial is not enough! And in general, why is there only one radiocarbon age for the entire record of ~3500 years? At least during the early Holocene there should be enough terrestrial plant macrofossils for a proper dating. 2. How reliably is the TOC increase related to the onset of the PB? This is questionable but might easily be verified by pollen data. 3. The pollen age for the PB/BO transition (10,450 cal. BP) is in disagreement with published ages from Holzmaar (10,800 cal. BP) and Meerfelder Maar (10,740 cal. BP) - see Litt et al., 2009 (Boreas, 38: 679–690). Furthermore, the data to support this are not provided. And this is my major concern: in the manuscript reference is given to unpublished pollen data many times. Without pollen data being provided with the manuscript, not only the chronology remains unsupported by data.

Some minor issues: 1. On page 13, line 3ff the authors talk about clear evidence of carbonates. This is not at all evident from Fig. 2D. Moreover, where is the Ca coming from in this rather small catchment area composed of siliceous Devonian rocks? 2. The authors should explain, why they think that the record is representative for GM. Recovered from 20 m water depth (maximum water depth of GM is 39 m) in a small lake (diameter: 300 m), this implies a core from a relatively steep slope. 3. In Fig. 2H a threshold of 12 is used for C/N ratios related to autochthonous vs. allochthonous organic matter sources. The reference to Prahl et al. (1980) is related to estuarine sediments. Here the threshold of 10, e.g. Meyers (2003: Organic Geochemistry 34, 261–289), should be preferred for lacustrine sediments.

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