Biogeosciences
Discussions

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Interactive comment on "Evaluation of bacterial glycerol dialkyl glycerol tetraether and ^{2}H$-^{18}$O biomarker proxies along a Central European topsoil transect" *by* Johannes Hepp et al.

Anonymous Referee #2

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GENERAL:

The topic of the manuscript is interesting and important as it deals with the evaluation of highly promising proxies used to reconstruct past environmental conditions. While the data produced are rare and are certainly worth publishing, the manuscript has severe flaws that prevent, in my opinion, its publication in this form.

MAJOR PROBLEMS:

A) While reading the manuscript, the connection between GDGT and the plant proxies (i.e. n-alkanes and hemicellulose) is not clear and seems disconnected as if from two separate manuscripts. Moreover, in the section 3.1 of the discussion, the GDGT

data are presented in a way leading the readers to believe that these molecules are produced by plants.

B) The other major point is that the authors suggest that it is "often" not feasible to disentangle between the evapotranspirative enrichment from the precipitation signal, but there is at least another well-established method to do so and published in Climate of the Past (see recent Sachse's group publications, e.g. A dual-biomarker approach for quantification of changes in relative humidity from sedimentary lipid D/H ratios, Climate of the Past, 2017). While this method should at least be mentioned, I also believe the method should be compared to help the readers understand the full set of tools available to study that issue. These two methods are very likely to be highly complementary.

SPECIFICS:

Line 298 to 303: This section is not clear due to some typos or mistakes, please reformulate.

Line 389 to 407: While the difference of ebio is reported at the end of the section (around line 477 to 487), the possibility that a variable ebio could explain the different signals in different types of vegetation, beside the damping effect, is evacuated of the discussion. This should at least be discussed.

Line 432: Is that referring to simply using isotope values of a single compound? What is that hitherto method (reference missing?)? I believe this brings us back to the problem B. The results would gain a lot to be compared with the updated tool box of proxies.

Line 444 to 458: The argumentation is not clear/convincing, please reformulate.

Line 483-484: The idea of a variable ebio is well expressed in general, but references to some recent works is missing that shows even greater variability in n-alkane dD values under different metabolisms (e.g. Cormier et al, 2018 – New Phytologist, Tipple & Ehleringer 2018 – Oecologia, Cormier et al, 2019 – Oecologia)

Line 490 to 494: Please reformulate, this section is not clear.

Line 550: If the author are really considering a variable ebio, the damping effect can only potentially explain the different signals observed in different types of vegetation. Again, ebio should be part of the points because standing alone, they can induce confusion even if mentioned afterward.

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