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Interactive comment on “Assessment of soil n-alkane δD and branched tetraether membrane lipid distributions as tools for paleoelevation reconstruction” by F. Peterse et al.

Anonymous Referee #2

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The manuscript by Peterse et al. evaluates two potential organic-geochemical proxies for the reconstruction of paleoaltitudes: the distribution of tetraether membrane lipids in soils and the δD values of n-alkanes from soils. The authors compare their response along two altitudinal gradients, on Mt Gongga (China) and Mt Kilimanjaro (Tanzania). However, both proxies are not directly recording the altitude, the MBT and CBT indices (describing the distribution of different tetraether lipids) have been shown to change with temperature and soil pH, respectively. As temperature usually declines with altitude, the MBT could potentially be an indirect recorder of the altitude the soil formed. Similarly, the δD of precipitation usually becomes more negative with altitude, so soil n-alkane δD values can potentially record paleoaltitudes. Part of the data (tetraether

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lipid distribution on Mt Kilimanjaro and δD values of soil n-alkanes from Mt. Gongga) have already been published and have shown relatively good correlations with altitude, based on the above described dependencies. The newly added data (soil n-alkane δD values for Mt Kilimanjaro and MBT/CBT data for Mt Gongaa) however, do not show such a clear picture. The reasons for this are discussed in the paper and it is argued that a combination of both proxies can provide a better assessment of paleoaltitudes. The manuscript uses state-of-the art methods to assess new tools for paleoaltitude reconstruction. Especially the combination of two relatively novel organic geochemical proxies which can deliver a complimentary set of information is innovative. Although part of the presented data are already published the comparison of two very different sites results in a potentially interesting dataset. However, I think the manuscript in its current form has significant weaknesses which prevent me from recommending a publication. My main areas of concern are:

- a) the concept of how a combination of δD soil alkane and MBT/CBT proxies could serve as a “more reliable” paleolevelation proxy (cited from the abstract) is not explained at all. The fact, that both proxies are indirect recorders of altitude (via temperature and the altitude effect on the δD value of precipitation) is not elaborated in great detail.
- a) the explanation of the pattern of soil n-alkane δD data along the Mt Kilimanjaro gradient is based on several weak assumptions and limited by missing, but essential data (isotopic composition of plant source water) and can therefore be regarded as very speculative at best – the absence of soil water δD values (as the water source of the plants) makes any sound interpretation of this dataset impossible.

Specific issues:

Methods section: The variability of the H3+ factor is quite large. Over what time frame did the H3+ factor vary by 2.5? Without that information it is hard to assess if ion source conditions were stable enough for a precise measurement of the alkane δD values. Was the offset in δD values between offline and GC-IRMS measurements corrected

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for? If the offset was within the mean reproducibility of the duplicate measurements, then they are not statistically different from each other and there is no need to mention the actual value of the offset.

In the methods there should be a discussion on the suitability of the OIPC derived estimates for precipitation δD in the study area at Mt Kilimanjaro. It is of course legitimate, in the absence of any soil water or precipitation δD data, to use the OIPC, but the data derived from it have some issues, which become especially apparent in areas like Africa: the OIPC derived precipitation δD estimates are modeled based on IAEA data and interpolated between the closest stations, largely as a function of topography. Especially in Africa there are very few of such stations (in contrast to N-America and Europe), and so the modeled data from the region will have larger uncertainties. Basically, the modeled data for Mt Kilimajaro are calculated as a function of altitude (plotting altitude vs δD precip will result in a linear relationship). So, this could be a valid assumption, if altitude is the only factor affecting δD precipitation, but in this case, its not, as you elaborate later.

Please introduce the correct terms for fractionation. What is discussed in the following is always the "apparent fractionation". In the manuscript sometimes the terms 'fractionation' or 'apparent fractionation' are used. I think it's important to stick to 'apparent' as this has become a somewhat commonly used term in the recent literature. So, the reader will not become confused.

page 8617 It should read "n-alkanes exhibit an odd-over-even predominance" or similar, as there are no "odd-over-even n-alkanes"

Page 8619, line 26: It is not strictly true, that the apparent fractionation depends on relative humidity only (even if this may be stated in some of the literature), it's a bit more complicated. The apparent fractionation should depend largely on leaf water enrichment. Leaf water enrichment itself mainly depends on relative humidity AND temperature (as well as the isotopic composition of water vapor), and to a lesser extent

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on plant physiological parameters. See the relevant literature on the causes of leaf water isotopic enrichment.

Page 8618, line25: Similarly, there is no consensus in the literature if gymno- and angiosperms show consistently different apparent fractionations, so that does not rule out changes in apparent fractionation due to differences in vegetation along the transect. But it is impossible to evaluate these (and their possible dependencies), in the absence of any data on the isotopic composition of plant source water.

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