

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.

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We would like to thank both anonymous referees for their comments. Both referees agree on the necessity and usefulness of an assessment of the two studied organic proxies (soil n-alkane δD and branched tetraether lipid distributions) as tools to infer paleoelevation. Exactly for this purpose, we examined the relations of both proxies with altitude along two altitude transects (Mt. Kilimanjaro in Africa and Mt. Gongga in China). Although 3 out of 4 test cases yielded positive results (i.e. showed a clear linear relation with elevation), both referees' comments only concerned the single case that did not show a clear trend with elevation, i.e. the n-alkane δD along the slope of

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Mt. Kilimanjaro does not linearly change with altitude. They both find this the main reason to argue against publication. We think this a somewhat strange criterion to object to publication of our study, as the study was specifically designed to test absence/presence of these relations with altitude. We thus find it remarkable that the apparent absence of a relation, for which the reviewers supply good arguments why this could be the case, results in the conclusion that the manuscript is not acceptable. We feel that it is important to report that the relationship between the δD of n-alkanes and elevation is not always straightforward and that a multiproxy approach, attempted here for the first time, is much needed in future paleoelevation studies. Furthermore, both reviewers have little to no comments on the MBT/CBT proxy data, which comprises a substantial part of the new data presented in this study. Hence, in our view, our manuscript is, after appropriate revisions, still very much suitable for publication in Biogeosciences. Below we will discuss the joint comments of the referees on the δD study, after which we reply on some additional comments of referee#2.

Concerning the n-alkane δD values along Mt. Kilimanjaro, we would indeed have liked to compare our data with the δD of the precipitation along the same transect as both referees remark. Unfortunately, the absence of these data forced us to use δD values modeled according to Bowen and Revenaugh, 2003, with all its caveats, as mentioned by the referees. However, it was never the intention of our study to exactly determine the controlling factors on δD of n-alkanes along mountain slopes: we merely wanted to test if there was a straightforward relation between δD and elevation as observed on other mountains, and it was this observation that we wanted to report. Often, knowledge of the climate history of the region where one wants to reconstruct paleoelevation is limited, and one does not know whether past changes in e.g. precipitation or temperature could have influenced the material on which the reconstruction will be based. Therefore, even if we would exactly know all the factors influencing the δD of n-alkanes on Mt. Kilimanjaro, we still would not be able to reconstruct paleoelevation based on δD of n-alkanes. In case of Mt. Kilimanjaro it might have been possible to predict in advance that there would not be a strong relation between δD of n-alkanes and eleva-

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tion, but for paleoelevation studies of other mountains this knowledge is often simply not available. We feel that it is important to report weaknesses and strengths of the assessed paleoaltimetry proxies as they are being applied more often (see also recent paper by Polissar et al 2009, EPSL 287, 64-76).

Since it is clear that both reviewers have major problems with the interpretation of the δD n-alkane record we will considerably shorten the discussion on this data set in the revised manuscript, and constrain ourselves to listing possible factors that have affected the relationship between δD of n-alkanes and elevation.

Reply to additional specific comments by referee 2

Comments:

a) the concept of how a combination of δD soil alkane and MBT/CBT proxies could serve as a “more reliable” paleoelevation 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.

Reply: The relation between the discussed proxies and how they could be suitable for paleoelevation studies will be addressed in more detail in the introduction of the revised manuscript.

b) 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.

Reply: As stated above we will considerably shorten this part of the discussion and merely list potential factors affecting the δD of n-alkanes.

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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 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.

Reply: The H3+ factor slowly decreased from 8.5 to 6 over a period of a month and a half. We use Schimmelmann's n-alkane mixture to check the performance of the GC-TC-irMS on a daily basis and we co-injected our samples with squalane with a known isotopic composition to monitor the quality of the analyses throughout the day. We have reported the outcome of these analyses to show that both the performance was stable throughout the period of analysis and that the isotope values returned were correct and that, therefore, there is no need for correcting the isotope values for an “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 Kilimanjaro are calculated as a function of altitude (plotting altitude vs. δD precipitation 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, it's not, as you elaborate later.

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Reply: See general reply.

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.

Reply: We agree with referee#2, this will be corrected in the remaining part of the discussion.

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

Reply: We agree with referee#2, this will be corrected in the revised version of the manuscript.

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

Reply: As stated above we will severely condense the discussion on the n-alkane data and restrict ourselves to listing potential confounding factors.

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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Reply: The absence of consensus on whether apparent fractionation patterns of gymno- and angiosperm vegetation are different or not is already covered in the discussion section of the manuscript. However, the referee is right when saying that changes in vegetation can not be ruled out as an explanation for the different apparent fractionations along the transect when no data on the isotopic composition of plant source water are available. The statement that vegetation changes can thus not explain changes in apparent fractionation, made at the end of the concerning paragraph in the discussion, will therefore be rephrased more carefully in the revised version of the manuscript.

Interactive comment on Biogeosciences Discuss., 6, 8609, 2009.

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