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Discussions

Interactive comment on "Revisiting Mt. Kilimanjaro: Do n-alkane biomarkers in soils reflect the δ^2H isotopic composition of precipitation?" *by* M. Zech et al.

Anonymous Referee #2

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Zech et al. report precipitation and leaf wax n-alkane d2H values along an altitudinal gradient at Mt. Kilimanjaro. They find that precipitation d2H values decline with altitude while leaf wax n-alkane d2H values increase with altitude. The authors assign this discrepancy to increasing evaporative leaf water 2H enrichment with altitude. The data Zech et al. report here largely confirm the results of a previous study by Peterse et al. Yet, Zech at al. stress that this previous study needs "major re-interpretation". Unfortunately, they fail to tell the reader why this is the case . . .. .

In general the manuscript by Zech et al. reports interesting data. The findings are, however, not very novel and report water and organic hydrogen isotope data that can be expected in such environments. In fact, there are several previous studies that have

shown that hydrogen and oxygen isotopes in plant organic material becomes enriched with altitude in tropical mountain ranges, while precipitation d2H and d18O declines with altitude. There are no major flaws in this manuscript. I have, however, several comments related to the methodology used in this work, the inappropriate referencing of previous research and the style by which the work of Peterse at al. is criticized, that I feel should be addressed before this manuscript can be published.

1) The leaf water model that Zech et al. use is not the standard Craig-Gordon Peclet –modified model that is conventionally used to simulate evaporative leaf water 2H enrichment. While I have no objectives against this choice, the authors need to better explain why they used this model, how it works and cite papers that have shown this model can correctly predict leaf water d2H values.

2) The analytical procedure and quality control using the GC-C-IRMS is not entirely clear. The authors state that all peaks <750 mV were omitted because they do not withstand the linearity criteria. It is, however, unclear how these linearity criteria were assessed. I think that it is important to report this. Otherwise it is difficult to trust the data reported in this manuscript. How can the authors be sure that their analyses were in a linear range above 750 mV?

3) The authors interpret small excursions in their data in Fig. 3b (below 2000 m) as meaningful environmental signals. Without any information of the natural variability of the data, such interpretations are somewhat difficult to follow and should be omitted.

4) There is a very strong tendency in this manuscript to ignore previous original work of other groups in favor of citing the author's own research. I feel that the extent by which this is done in this manuscript is quite unusual or even blunt. For example, on page 7835 line 1 – 5 the authors state "like 18O in hemi cellulose . . . the deuterium isotopic composition of plant biomarkers can be expected to depend on three main factors . . .. ". Instead of referring to the original work that has postulated this for n-alkanes 10 years ago (Sachse et al. 2004 and Smith et al. 2006) they cite their own very recent work that

in parts does not even deal with d2h values in n-alkanes. Likewise, the authors argue that leaf water evaporative 2H enrichment is a key variable that determined the leaf wax n-alkane d2H values along Mt. Kilimanjaro. Yet, the recent and original work by several groups that have shown the effect of leaf water 2H enrichment on leaf wax n-alkane d2H values is largely ignored. Instead, the authors cite again their own work on 18O in hemi cellulose. There are many more examples throughout the manuscript with similar examples. I recommend the authors very carefully reevaluate their citations, and revisit the existing literature.

5) The authors make a very strong case throughout their manuscript that the work by Peterse et al. needs “major re-interpretation”. As mentioned above, it is not explicitly explained by the authors what this “major re-interpretation” should look like. In fact, I feel that in general Peterse et al. show the same patterns as reported in the present manuscript, although Zech et al. add some additional data (e.g. precipitation d2H values). In general, the manner by which the work of Peterse et al. is criticized in the present manuscript is quite blunt and for my taste a bit too aggressive. I almost have the impression that the authors try to make a stronger case of their own work by heavily criticizing these previously published data. I am not sure that this is the way we should communicate scientific results!

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