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Biogeosciences Discussions

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.

J. Bijma (Editor)

jbijma@awi-bremerhaven.de

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I like to thank the reviewers for their excellent and critical reviews and the authors for their constructive reply. I agree with the authors that, despite the critical reviews, the "... manuscript is, after appropriate revisions, still very much suitable for publication in Biogeosciences". In my view it is just as valuable to show the shortcomings and pitfalls of paleoreconstructions as it is to show the success stories. However, in the revised manuscript it should be made clear that this study, as the authors put it in their reply, " was specifically designed" to test absence/presence of relationships between altitude and both soil n-alkane δD and branched tetraether lipid distributions. It should

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be clearly stated that ". . ..., 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."

If possible the revised manuscript could also benefit from a new paper on paleoelevation reconstructions that just come out: Michael T. Hren, Bodo Bookhagen, Peter M. Blisniuk, Amanda L. Booth, C. Page Chamberlain. 2009. δ18O and δD of streamwaters across the Himalaya and Tibetan Plateau: Implications for moisture sources and paleoelevation reconstructions. Earth and Planetary Science Letters, Vol. 288, Iss. 1-2, p. 20-32

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