

Interactive comment on “The added value of biomarker analysis to the genesis of Plaggic Anthrosols – the identification of stable fillings used for the production of plaggic manure” by J. M. van Mourik et al.

Anonymous Referee #2

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General Comment

Van Mourik et al. investigated Plaggic Anthrosols using pollen and n-alkane biomarkers in order to identify stable fillings that were used for the production of plaggic manure. The presented manuscript is basically well written and the presented study is worth publication. However, given the considerable overlap with already published papers, the manuscript will profit from a stronger focus on the novel biomarker results.

Particularly, the alkane patterns of the vegetation samples are the basis for the interpretation of the biomarker patterns of the soil profiles. However, respective results

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for vegetation (above ground and root samples) are not presented. Is it possible to visualize the VERHIB method somehow?

Furthermore, while genotypic plasticity of alkane patterns is discussed, aspects like possible degradation effects and different alkane production/concentration of plants/roots are missing in the current version of the manuscript.

I strongly encourage the authors to perform compound-specific $\delta^{13}\text{C}$ analyses on some of their alkane samples from profile Posteles. According to the authors' VERHIB method, several soil samples contain nearly 100% alkanes from *Zea mays* roots. Given that *Zea mays* is a C4 plant, this would result in a clear $\delta^{13}\text{C}$ signal of the alkanes and would thus be as very strong cross check validating (and thus convincing the readers) or falsifying the approach of the authors.

Specific comments

Subchapter 2.2 14C and OSL dating: This was not carried out within this study but is already published. I therefore suggest to delete this subchapter and to incorporate the information in the text that is preceding subchapter 2.1 Pollen. Furthermore, include source/citations in Tables 1-3.

p.10, l.18: I find this formulation inappropriate, because the radiocarbon age does not reflect that the Anthrosol soil development started/lasted 1400 years (ago). The same holds true for the identical formulation in the subsequent subchapters.

p.10, l.20: What do you mean with "post sedimentary pollen spectra"?

p.11, ll.1-4: Micromorphology is not listed in the Material & Method section. Either include there, or, if already published, then delete here in the Result chapter and include with citation in the Discussion chapter.

p.13, ll.17-19: You fail to tell your readers that you refer to biomarkers here and you fail to refer to the respective figure. Furthermore, please check, 80 is not the lowest spectrum, is it?

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p.13, l.20: If you can differentiate between leaf and root-derived alkane biomarkers, please show these results also in your figures showing the biomarker spectra. I would appreciate to see the respective alkane patterns and concentrations of your root samples.

p.14, l.24: This is not clear to me. Why should accumulation of organic matter result in a radiocarbon age overestimation?

p.14, ll.27 - p.15, l.2: Is this derived from radiocarbon results? If yes, this statement is not robust/correct due to the age overestimation

p.15, l.13: Evidence for root-derived alkanes is not traceable based on the presented results.

p.18, ll.15-17: delete, not listed in the text

p.20, ll. 23-25: delete or include in the text

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