

Van Mourik et al. – The added value of biomarker analysis to the genesis of Plaggic Anthrosols – the identification of stable fillings used for the production of plaggic manure.

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The authors investigate to what extent biomarker analysis can contribute to identify what organic plant material was used as stable filling and contributed to the development of Plaggic Anthrosols, and how the land surface rise at early stages of plaggic agriculture can be explained. The study is based on a preliminary investigation at three sites in the Netherlands, and builds on existing information with regards to the chronology of the plaggic soil development, as well as the micro-morphology and pollen content of the soils.

The manuscript reads smoothly, and provides some interesting new insights into the development of the Plaggic Anthrosols. However, there are a few key issues that will need to be modified prior to publication:

- 1) The manuscript contains a number of serious mistakes that need to be corrected.
- 2) It should be made much clearer which part of the information (e.g. figs.) is original, and which parts were published before.
- 3) Emphasis should be on the novel aspects of the work (the biomarkers), and this aspect should be discussed in more detail.

Each of these issues is detailed below, and should be corrected before publication can be considered. I note that there is considerable overlap with earlier publications; whether this is acceptable I leave to the editor.

1) Essential corrections

The two thin sections from plaggic horizons of Valenakker shown in Figure 5 have been published before (Van Mourik et al., 2012 – QI) and were attributed to different locations in that original work. Fig. 5a was published as Fig. 12 in the original work (Van Mourik et al., 2012 – QI) where it was reported to be from Slabroek (*'Fig. 12. Micro photo of the distribution of intertextic organic aggregates in the 2Ah horizon, profile Slabroek. The distribution pattern of the aggregates is in line with the regular pattern, observed in Ah horizons of acid podzols.....'*). Fig. 5b was published as Fig. 15 in the original work (Van Mourik et al., 2012 – QI), where it was reported to be from Nabbegat (*'Fig. 15. Micro photo of the internal fabric of an organic aggregate in the 2Aan horizon profile Nabbegat with (in the centre of the photo) a pollen grain and a charcoal fragment.'* In addition, these same photos were used by Van Mourik et al. (2012-Intech – Fig. 31 and 32), where they were attributed to Valenakker. At least one of the attributions in existing literature and/or the present manuscript must be wrong, and corrections should be made.

In addition, OSL ages in Table 1 (Valenakker) and 2 (Nabbegat) are identical; those given in Table 2 are incorrect and differ from those presented for Nabbegat in the original work (Van Mourik et al., 2012 – QI, Table 7). The C-14 ages are also different to those published before, and the sample depth for the geochronological samples seems to be incorrect.

Table 6 from Van Mourik et al., 2012 – QI:

Table 7
¹⁴C and OSL ages of profile Nabbegat.

Horizon	Depth (m)	OSL age	¹⁴ C age hac	¹⁴ C age hum
C	0.70	1803 ± 12 AD	–	–
2An	0.80	1770 ± 11 AD	580–670 AD	400–610 AD
2An	1.05	–	90 BC–130 AD	177 BC–120 AD
2An	1.30	1676 ± 14 AD	960–760 BC	1370–1040 BC
3ABp	1.40	–	1390–1220 BC	–
3ABp	1.50	–	1490–1310 BC	–

Finally, the text and figures are not always in agreement; where Fig. 9 indicates dominance of Zea mays at 10, 20, 30, 50, 70 and 80 cm at Posteles, the text (p. 13) indicates dominance at 10, 20, 40 and 60 cm. Also the other attributes do not link.....

2) Originality material

All information in the Tables and the majority of the figures have been published before. The information is important for this paper, and hence there is good reason to present the information here. However, it should be made clear that the information is not original (in the text, but also in the figure caption). In addition, the editor should decide whether the amount of recycling is acceptable and whether the amount of new information is sufficient to warrant publication.

Fig. 1 – is very similar to Bokhorst et al. 2005; which states the original is from Pape

Fig. 2 – a) has been published in 2012 Intech, b) in 2012 QI, and c) in 2011 Catena

Fig. 3 – has been published in Van Mourik et al., 2012 – Intech (and possibly before?)

Fig. 4 – NEW

Fig. 5 – identically (incl. caption) published before in Van Mourik et al., 2012 – Intech; photo's published as well in Van Mourik et al., 2012 – QI (with different allocations, see above).

Fig. 6 – published before in Van Mourik et al., 2012 – QI (and there cited to be from Van Mourik and Ligtenberg, 1988)

Fig. 7 – NEW

Fig. 8 – published before in Van Mourik et al., 2011 – Catena (and possibly before?)

Fig. 9 – NEW

3) Novelty

The novelty of the work clearly relates to the investigation of biomarkers in plaggic deposits. This aspect should be discussed in far more detail:

- An input of leaf/root biomass ratio of 1.0 is applied 'in line with the exploratory nature of the present study'. How important is this ratio? What would happen if a different ratio is assumed (e.g. 1/2 or 2?). In the results, I don't see a distinction between root and leaf being made; is the ratio relevant at all to this work? In a previous paper by Van Mourik & Jansen (2013), they state that differences between pollen and biomarker presence can help interpretation: '*c) Biomarkers indicate the presence of a species at a certain interval, but pollens do not. This implies that the species was present, but not in this particular interval. Instead the biomarkers originate from younger root input from a higher (younger) soil surface of the sequence.*' Was such an approach attempted here, and if not, why not?
- The most detailed biomarker profile (Fig. 9; Posteles) shows huge fluctuations, which are not at all discussed in the text. How can biomarkers of Zea Mays completely dominate the spectra at 30, 50 and 70 cm depth, but not be present at all at 40 and 60 centimetres. Were the samples too small to be representative? What implications does this have for interpretation of the other profiles?
- How about decomposition of biomarkers; is it possible that Calluna vulgaris biomarkers degrade over time, and therefore are not present at depth?

In the final paragraphs of the discussion, the question is raised how the mineral component in the plaggic manure, responsible for the rise of the land surface at the earlier stages can be explained. This indeed is an interesting question, that should be discussed in more detail. The idea that the minerals may be from the stables (the floor?), is interesting. However, I presume that the area of the stables is much smaller than the area of the raised fields (1:100?). If so, raising the field by one cm of mineral material would require the stable floor to be lowered by a metre..... The second option (unleached sand dug on sheep walks and blown out depressions) is interesting; is there any evidence? Could you calculate how much material would be needed, and is the area of sheep walks sufficient to provide the material needed (similar to the argument above). Finally, could there be a direct input from drifting sand as well? Please expand this discussion.

References:

Bokhorst, M. P., Duller, G. A. T., and van Mourik, J. M.: Optically Stimulated Luminescence Dating of a fmic anthrosol in the Southern Netherlands, *J. Archaeol. Sci.*, 2005, 547–553, 2005.

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In addition; the recent thesis of Maika de Keyzer (Antwerpen University, Belgium) contains interesting information on the farming system in the Kempen. This work could/should be referenced here.