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Discussions

Interactive comment on "Intra-versus inter-site macroscale variation in biogeochemical properties along a paddy soil chronosequence" *by* C. Mueller-Niggemann et al.

G. Wiesenberg (Referee)

guido.wiesenberg@uni-bayreuth.de

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The current manuscript determined inter- and intra-site variability of organic matter properties in a chronosequence of paddy soils in China. Mainly intra-site variability related to a different time of rice cultivation was found to sustainably change 'conservative' soil organic matter properties. Also 'rapid' changes were observed to increase TOC contents in paddy soils within the first 300 years of rice cultivation. Afterwards, biogeochemical parameters do not undergo any further substantial change. This study is based on a sophisticated sampling design, using site and field replicates and application of various statistical approaches to determine clustering of individual sites. The combination of molecular, elemental studies with plot-related and field replicates has not been applied frequently and thus provides scientific innovations in that sense. However, the argumentation in many points and discussion of data with respect to available literature is not adequate in the current version of the manuscript. I.e. the reader of the manuscript might get the impression that numerous of the investigated parameters have been analysed for the first time or how the data is related to available literature (if there is some available). In some parts of the manuscript mistakes were observed and in other parts the meaning of the study is discussed in a very enthusiastic way, leading to conclusions, which are not completely supported by the data. Hence, some modifications are required before accepting the final version of the manuscript. But due to the overall improvements in combing several orders of scales, I would recommend acceptance of the manuscript after moderate changes.

Although it was not the main focus of the manuscript to simply determine SOM properties, a minimum of discussion is required, how the observed data fits literature results. This is completely missing and should be added. The definition of ‘conservative’ vs. ‘non-conservative’ parameters is not clear. How can the authors group some of the parameters without using any relevant literature arguing for their selection. Although this grouping seems to be logical in many points, it has to be supported by literature data. Even if such literature is not available for paddy soils, some data (14C dating and/or turnover times) should be available for other soils and sediments. At several points of the manuscript the ‘minerogenic composition’ is discussed to have an influence on the inter- and intrasite variability of SOM. Of course this is one potential factor, which somehow has to be confirmed. Otherwise such statements remain speculative. Hence, it should be indicated, whether this is proven or ‘likely’ as other factors cannot completely explain individual observations.

Abstract Line 17ff The lowering in CV from younger to older sites could be also attributed to degradation of some parts of OM and not only the longer usage. Paddy fields that are used for several hundred years should be more or less under steady state conditions, which argues against duration of usage as single effect.

1. Introduction In the introduction section, a consequent literature review of a couple of factors is simply missing. Numerous statements and assumptions have to be supported by adequate literature. Only few examples are given below. In general, the section has to be strongly improved in that context. Page 4 There should be some adequate literature be available, which supports the statements given in the introduction as e.g. the conservativeness of parameters like TOC, isotope and lipid composition, as well as for assumed labile parameters. This literature should be added. Similarly, the factors influencing soil heterogeneity should be supported by literature.

2.1 Study sites Page 6 As WRB was used for soil classification, soil names should be written according to WRB guidelines, i.e. with large letters for the main soil groups.

2.2 Sampling Page 6 It is unclear, how samples were collected, i.e. using a spade or an auger (‘roughly 20 cm’ is not precise enough in that sense). This is of special importance as it should be clear, whether the sample is representative for the whole sampling interval (auger) or is just a ‘mixed’ sample, which could yield more top soil than deep soil material (spade).

2.3 Laboratory analyses Pages 7-8 In Chapter 2.2 it was stated that samples were freeze dried and ground to fine powder. Is this also true for the samples amended to fumigation for soil microbial biomass estimation? If not, this should be indicated. But if it would be the case, one can doubt about the outcome of the analyses. Remove ‘hydrocarbon’ in line 23. Add static time for ASE extraction and provide details regarding GC-MS temperature program.

3.1 Soil parameters Pages 8-9 Chapter 3.1 is completely redundant. It just repeats parts of the M&M section without providing new information. Thus, this should be removed.

3.2 Macroscale intra-site variability Page 9 It remains completely unclear, how the au-

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thors could attribute the individual parameters as 'conservative' or 'non-conservative'. This must be supported by adequate literature and should be also added in the text body, but not only in Table 1. 3.2.2 Lipid and alkane conc and comp The authors state that alkanes contribute to total lipids only a rather short portion, whereas other lipids are much more prominent. So, why were the other lipid classes, which were also achieved during extraction (but probably not during separation), not analyzed? Analyses on terrestrial soils identified substantial differences in the stability and cycling of lipids in soils. Additionally, incomplete combustion has been described to sustainably contribute to specific alkane patterns in soils. In the current version of the manuscript it is questionable, whether these are assumptions in the current study, proven by comparison with literature results or simply speculations. The authors could simply face this by including relevant literature, which in major parts is completely missing.

3.3 inter-site variability Page 17 In Line 11 a 'significant change' was observed. This should be supported by the analyses of statistical siginificance.

3.4 Organic matter accumulation and sequestration trends Pages 18-19 This chapter in parts sounds a bit strange and should be re-written as some mistakes are included. - When referring to C sequestration and the IPCC report 'rapid' increase in TOC observed in the chronosequence (100 %) is not that surprising and common, whereas e.g. there are numerous examples, where cropping management or cropland to forest conversion can also contribute to similar carbon accumulation (or even more). Controversially, other conversions have been shown to reduce TOC in soil by 50% in less than 10 years. Hence, the manuscript in the current version suggests that rice production could contribute to sustainably sequester carbon in the short term (when using the word 'rapid'), which is not true. This should be changed. - The conversion of soils to rice cultivation cannot really taken into account for counter-balancing atmospheric CO_2 increase. As numerous FACE experiments clearly showed that biomass production is not strongly promoted under rising CO_2 and also TOC is not strongly increasing, rice production cannot solve this problem. In the current version the reader could get this

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impression, which is also not correct. Hence, some modifications should be done. - Marschner et al (2006) should be changed to Marschner et al (2008) - The statement of lipids being less recalcitrant than TOC is not true and not supported by the whole literature. In Marschner et al (2008) also alkanes have been described to be more recalcitrant than TOC, depending on cultivation. Additionally, some more recent and also older studies are available, showing that especially alkanes can be more recalcitrant than TOC (e.g. Bol et al., Wiesenberg et al., Feng et al.). It is speculated how n-alkanes are generated in soils, whereas there is numerous literature is available on this topic, which should be cited.

4 Conclusions Page 20 The environmental/ecological budget is not really supported by the data or is only related to a strongly degraded environment before conversion to paddy fields. The last paragraph of the conclusions is not really supported by the data as no real reference soils are regarded. Hence, the last paragraph should be modified in a way that could not lead to the conclusions that paddy soils are a global solution for carbon sequestration. As BIOGEOSCIENCES is a globally distributed journal, such statements as included in chapters 3.4 + 4 could lead some policy makers to the mentioned conclusions.

Fig. 4 'tricyclics' should be changed to 'tricyclic hydrocarbons'

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