

***Interactive comment on “Soil carbon and plant diversity distribution at the farm level in the savannah region of Northern Togo (West Africa)” by M.-T. Sebastià et al.***

**Anonymous Referee #2**

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General comments The manuscript aims at elucidating relations between soil organic carbon contents and plant diversity in a demonstration farm in Northern Togo. This immediately highlights the main shortcoming of this paper: in how far is this model farm a representative agro-ecosystem for Western Africa and - in other words - how can the results be of any relevance to practical farming situations in the region? The title, hence, is promising more than actually produced. The subject of the work is however sufficiently relevant: soil organic matter is a crucial soil quality indicator in the depleted soils of West-Africa. The information that is critically needed, however, is on how an adequate level of organic matter can be obtained/maintained while the soil remains under production. It is not surprising to learn that higher values of organic

matter are measured in the so called 'sacred forests' where neither cropping nor any other disturbance occurs. Overall, the conclusions of this study are rather predictable. It is not particularly novel to state that soil organic matter levels are highly dependent on land use and management. About the established positive relationship between soil organic carbon and plant diversity, one could say the same. It would, however, be more interesting to reveal the underlying causality in this relationship. The authors also mention that there is room for improvement by changing the agricultural practices. How to do this in a practical and socio-economically acceptable way is not indicated and also probably not possible, based on data from a single 'model'farm. Specific comments - The first line of the abstract states that soil organic matter is 'a source of fertility for food provision&'and a 'tool for climate mitigation'. Both statements are rather unsubstantiated and need reformulation. - In the same abstract, line 17, the authors mention a strong influence of human activity on soil formation and distribution. While this may be so, one can not make these statements on the basis of this study in one model farm. Or do the authors mean something else? Besides, the soil type of this farm is not even mentioned, which makes it difficult to judge the representativeness of this study. Yet, on page 4111, lines 4-7, mention is made of soil mapping, soil profiles and so on... so the information must be available. - Page 4108, line 19: the potential carbon sink under forest is very misleading: it is not relevant to compare soils under arable farming with an undisturbed forest, as the main difference is that the former are continuously disturbed and biomass taken from them. I would argue that the forest soils are not useful as a baseline for potential fertility in the area. First of all, they can not be compared with a cropping situation and secondly - as the authors themselves indicate - the reason why they are still there (under forest) may be because the intrinsic fertility of the soils is low. The authors give the example of the rice fields, where already a selection towards the more clayey soils is made (page 4115, lines 4-8). - Page 4110, line 22: inconsistent: earlier the author states that fertilisation is very limited, then it is claimed that 150 kg ha<sup>-1</sup> of NPK is given. In the context, this is not limited, average doses for West-Africa are a few kg's only! Points one back to

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the earlier raised comment about representativeness! - A major and often occurring problem with studies like this where multivariate analysis is applied is that the original data structure is no longer presented. I would insist on also presenting the raw data of organic matter contents, cation concentrations and other parameters in the different treatments, before they are disappearing in the different axes of the CCA. - On page 4122, table 2. I have some problems with a soil organic carbon stock that changes with the season, as seems to be the outcome of the regression model presented here? I read on page 4114 that the carbon stocks are related to the number of plant species and that it is this relation that is dependent on the season. This I can understand, but I fail to see a direct link between SOC and sampling time. This may need clarification. - In the same table 2, p 4122, it is not clear to me that the factor 'No. species' has only one df? Technical corrections - Page 4123, fig 1: in the x-axis: G for January? - Page 4125, fig 3: no reps for the forest site, makes it hard to use it as a baseline! Conclusions For all the above reasons, I find insufficient merit in this paper to have it published in this journal. Not enough novelty, questions about representativeness, the lack of sound recommendations fine-tuned with socio-economic realities and a poor presentation of the data.

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