

## ***Interactive comment on “Soil Aggregate Stability of Forest Islands and Adjacent Ecosystems in West Africa” by Amelie Baomalgré Bougma et al.***

**Amelie Baomalgré Bougma et al.**

korodjouma\_ouattara@hotmail.com

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Before any response, We would like to thanks editorial board and the reviewer for the valuable and relevant comment.

Anonymous Referee #2 The scope the submitted manuscript ‘Soil Aggregate Stability of Forest Islands and Adjacent Ecosystems in West Africa’ is interesting as it has a great ecological significance. Introduction and research methodology is well described. However the manuscript significantly needs a critical attention especially the results-discussion section. Below are the key comments to authors. a). Major Comments: 1. In current study, did soil depth affect aggregate stability across sampling sites (zones)? This important information regarding depth effect on key soil indices is missing in cur-

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rent results and discussion section. For instance, the effect of land-use and mean annual precipitation on aggregate fractions has been depicted for only topsoil (0-5 cm) but why there is no information for second sampling (5-10 cm) depth (Figure 2 and Figure 3)? 2. Despite of the fact, physical fractionation of bulk soil was performed in current study, why the interaction of Organic carbon with only soil macro-aggregate is given in Figure 3? What about micro & meso aggregates, envisaged in this study? We put emphasis on the soil top layer because soil erosion starts on soil surface. The soil surface is the one in contact with rain drops, yet the stability of soil aggregates is an indication of its susceptibility to water erosion. In addition, the difference between land uses was higher on the top layer than in the sub layer. We did not find any significant interaction between meso aggregates, micro aggregates and soil organic matter content. That is why we don't have figures for these relationships

3. Besides land use, was there any influence of soil type on aggregate stability across sampled sites?

Probably, but we are exploring it in another paper.

4. Why the (depth wise) values for organic carbon, total nitrogen content, C: N ratio, bulk density, Cation exchange capacity (CEC) and pH of (bulk and fractionated) soil of sampling sites are not provided? It would have been more interesting if the C, N content of envisaged micro, meso and macro aggregates were also presented to get clear understanding which fraction sequestered more C content versus bulk soil under different land use systems! That is right but, the carbon fractionation is part of another study, this is why data were not show here.

5. Fe content was measured in this study and also inferred as key determinant of aggregate stability among sampling sites (regions) but why its impact on aggregate stability with respect to land use is discussed scarcely in current manuscript?

IT was not easy because Fe content is mainly inferred to soil type, climate condition and soil hydrology. Soil concretions formation, aluminum and iron oxides formation are

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rather pedologic processes than effect of land use.

6. At the end of this study, it is still unclear that what are the (ecological) implications if soil micro aggregate fraction remained unaffected despite of land use change but only soil macro aggregates were affected (Line 227-229)! Does it mean micro aggregates are resistant to land use change? What should be inferred from this phenomenon particularly in the context of climate change? Please also state which aggregate fraction was (ecologically) significant in this study?

Macroaggregation depends on temporary binding agents (fine roots, fungi hyphae, labile organic matter, etc.) and is considered to be sensitive to the changes in organic matter levels caused by ploughing. In contrast, microaggregates depends on strong binding agents (clay charges, colloids, ions, persistent organic matter, etc.) then show relatively high stability in response to physical disruption (Tisdall & Oades, 1982, Ouattara et al., 2008). There is a hierarchy order in aggregates formation; microaggregates binding gives mesoaggregates and their binding gives macroaggregates. Soil quality is best with the abundance of stable macroaggregates.

b). Minor comments: - Contrary to results section (stated content wise), why the discussion section lacks of respective captions rather discussed holistically? - The role of soil organic matter and organic carbon in aggregate stability has been redundantly discussed i.e. Line 187-191 and again in Line 203-208. Please check this.

It is just an option to have the discussion in its actual format. Thanks, we address the redundancy.

Line 27: 'Water sieving method' should be corrected as 'wet sieving method' We agree with your observation. This is took account in the manuscript Line 66: Which type of cultivation i.e. tillage type (conventional, reduced etc.) was applied to agricultural (AF) plots and up to what depth? Because tillage intensity greatly affects structural stability of soil aggregates.

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Cultivation is cropping, not tillage.

Line 69: Please state the date of soil sampling for the respective sampling locations

Line 71 (& 66): Instead of stating 'at least', kindly state exact number of samples taken (Line 71) and duration of cultivation (Line 66).

Line 183: Please briefly elaborate what 'wet aggregates' means here! Line 175-186: Please check the paragraph spacing Line 209-215: Please check the paragraph spacing

These remarks were taken into account.

Line 195-198: Please discuss current results especially of AF fields in the context of plowing intensity Line 197: Please briefly elaborate the extent of 'frequent plowing'! Does it refer to tillage intensity here! Line 200-203: It is proved, well understood that conversion of forests to arable lands affects micro-biochemical indices of soil then what this study particularly unveils new for us?

We did some changes in the statements

Line 227: Were the differences among envisaged land use systems non-significant? What presumably lead to lack of land use change impact on soil micro aggregate stability especially among FI and AF land use systems? Line 227: Does the phenomena of 'systematically increase' here means 'exponential increase'? Please briefly elaborate this. Table 1 has not been cited at all in current manuscript. To which section it belongs!

The conclusion has been revised and the Table 1 called in the text.

Please also note the supplement to this comment:

<https://soil.copernicus.org/preprints/soil-2019-87/soil-2019-87-AC2-supplement.pdf>

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Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2019-87>, 2020.

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