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Interactive comment on "Physicochemical changes in pyrogenic organic matter (biochar) after 15 months field-aging" *by* A. Mukherjee et al.

Anonymous Referee #1

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The authors present data on the impact of environmental weathering of biochar on the physicochemical properties. As the authors point out in the abstract this data is critical for the understanding of the both the mechanistic processes and the kinetics of the transformation. However, this manuscript contains a few shortcomings: 1. The design of the experiment includes a landscaping cloth cover – what was the mesh size? Would this impact the amount of water entering the column with the contained biochar? What was the temperature difference between the column of biochar on the surface and those buried in the soil? There would have been different availability to water for each column – the pure biochar and the biochar+soil. This is critical since water is one of the compounds that easily and spontaneously reacts with charcoal. This could lead to serious differences between the biochar with and without soil, thereby confounding the results of the experiment. 2. The presence of divalent cations are known catalysts

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for the abitoic reactions of charcoal. Yet these would not be present in the "biochar control" ; yet this is not accounted for. 3. If there was mass loss from the biochar with aging the comparison of solely the %C composition does not adequately account for the difference between the weathering profiles. There is no mention of sample preparation for this test. 4. In addition, the biochar + soil could have soil particles present in the pores of the material – Yet there was no discussion on this potential impact, both on the %O number determined (quartz) and the clay minerals and the biochar particle that was aged in soil. 5. The authors solely analyzed two points – initial and final. They missed an opportunity to look at the temporal dynamic of the processes, which would have made a more significant contribution to the literature. 6. Only two replicates – Samples were solely run in duplicate – no reason was given for this. This severely limits the strength of the statistical comparisons and there is no clear description given if only one piece of biochar was used for all the testing or how they collected a uniform sample from these artificial aged columns. 7. The SEM imagery: No indication how particles were selected or how many particles were used? Was there a difference with depth in the column? Or was this ignored? 8. Numerous statements in the manuscript (Starting just with the abstract→ example P 732 line 12: likely via leaching; line 14-15: Role of OM-microbe-biochar interactions during aging; line 21 "soprbed SOM; line 20 "colonization by microbes"; etc.) which are not supported by data in the manuscript. 9. There are critical details missing in the methodology: Was the dry soil was re-wetted prior to the installation of the columns in the field? Why wasn't the microbial activity measured since this was a major emphasis of the experimental design? Were the soil +biochar columns also covered? 10. How were the columns homogenized? Was there an examination of the changes with depth in the column? This could have shed light on the fundamental mechanisms at work... Especially if the sampling was done with time. 11. There was no mention of the analyses of the leached precipitation in the columns; or the fact that the rainfall could have contributed DOC/DOM as a potential source for the differences as well. The use of solely the differences in the CN analyses is a very weak method for illustrating differences from aging, since you can have a loss of oxidized material in charcoal with no change in the bulk chemistry. Without true knowledge on the variance of the values in Table 1 the theoretical combination of the independent materials is not statistically justified. 12. Calculation of oxygen by difference for grass materials is a source of significant error, due to the other elemental components: K, P, Ca, Si 13. The SEM imagery : How were the authors sure that these circular objects were microbes? There was no staining or other techniques mentioned for this conclusion. There are numerous inorganic substances that also have this spherical shape, such as combined metal oxides (e.g., $CoMn_2O_4$) as well as calcium phosphate crystals also have this circular shape and similar size. Recent work has also casts some doubt on the "microbial habitat" theory for biochar additions (Quilliam et al., 2013; Criscuoli et al., 2014; Jaafar et al., 2014). The authors need to purge or reword all the microbial findings to hypotheses since the claims are unsupported.

Overall, the manuscript does present data that could be of interest. However, without the statistical foundation of a properly designed experiment (3-6 reps); there is very little that can be confidentially transferred from this study. Regrettably, the manuscript lacks the innovative nature – since at the end of the manuscript there is very little new insights into the transformations or knowledge of the temporal scale of these biochar transformations.

Refs: Criscuoli, I., Alberti, G., Baronti, S., Favilli, F., Martinez, C., Calzolari, C., Pusceddu, E., Rumpel, C., Viola, R., Miglietta, F., 2014. Carbon sequestration and fertility after centennial time scale incorporation of charcoal into soil. PLoS ONE 9, e91114. Gurwick, N.P., Moore, L.A., Kelly, C., Elias, P., 2013. A Systematic Review of Biochar Research, with a Focus on Its Stability <italic>in situ</italic> and Its Promise as a Climate Mitigation Strategy. PLoS ONE 8, e75932. Jaafar, N.M., Clode, P.L., Abbott, L.K., 2014. Microscopy observations of habitable space in biochar for colonization by fungal hyphae from soil. Journal of Integrative Agriculture 13, 483-490. Quilliam, R.S., Glanville, H.C., Wade, S.C., Jones, D.L., 2013. Life in the 'charosphere' – Does

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biochar in agricultural soil provide a significant habitat for microorganisms? Soil Biol. Biochem. 65, 287-293.

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