

This article is interesting and very relevant for this special issue and I recommend publishing it provided some modifications are done. The use of a high number of field- weathered biochars is particularly relevant as these types of studies use a much lower number of samples and are not generally done under field conditions.

Thank-you for the positive comments and the suggestions that follow. We have used these gratefully to improve the manuscript.

However, it is my feeling that the authors should improve the characterization of the biochars to add more value to their results. With respect to the selection of materials, it is worth to mention that they always used plant materials and at temperatures over 500°C. It would have been interesting to use other kind of materials such as sludges or manures to prepare the biochars and also to expand the range of temperatures to 350-500° C. Maybe in this case, there would have been more different between the treatments.

We have added more information on the characterization of the original biochars that were applied to the field site in Table 1, including elemental analysis of C, N, O, H, and S, and proximate analysis of ash, and fixed C, in addition, molar O:C ratio, pH, surface area, and particle size. We do wish that we had a larger selection of feedstocks. To date, these have been the only biochars that have been delivered in large enough quantities to be applied to the field plots. Since we do not manufacture our own biochar, we are left with the various suppliers and their operating conditions. The lower temperature biochars have been produced in small quantities with laboratory scale units.

Abstract: There are some references to time here “2.5 years prior to start of this study”, “14 weeks”. I miss the date of the start of the study.

To improve clarity, we have added to the abstract and methods that the biochars were applied in the fall of 2008, and the litter bag study was initiated in July of 2011.

Abstract: Line 14-15 is contradicted with page 609, lines 16-17. Is the difference statistical significant or not? Moreover, when I see Table 2 I cannot decide by myself for the biochar. Definitely, wood pellets seem to increase soil microbial biomass. Please, add some letters to Table 2 to indicate significant differences between treatments.

Microbial biomass changes were not statistically significant, so we have corrected the text on page 609, lines 16-17 to read: “Although not statistically affirmed here, soil microbial biomass changes were the most likely drivers of the variability in the decomposition rates observed.” Letters in Table 2 are not necessary given there were no statistically significant differences.

Abstract: Is the last line a good ending for the abstract? Re-applications of biochar have not been considered in this study.

That is correct, we did not evaluate reapplication. We deleted the last sentence of the abstract.

Introduction: In the last paragraph I miss some mention to its potential to improve crop yield (Liu et al., 2013) and, as mentioned by the editor in a previous comment, soil quality.

Liu, X., Zhang, A., Ji, C., Joseph, S., Bian, R., Li, L., Pan, G., Paz-Ferreiro, J. 2013. Biochar's effect on crop productivity and the dependence on experimental conditions-a meta-analysis of literature data. Plant and Soil 373, 583-594.

We have added multiple references as follows to express improvements: in soil biological activity (Paz-Ferreiro and Fu, 2013), in agricultural production in dry regions (Blackwell et al., 2010; Kammann et al., 2011; Artiola et al., 2012; Ibrahim et al., 2013), in yield improvement for acidic coarse textured soils (Biederman and Harpole, 2013; Crane-Droesch et al., 2013; Liu et al., 2013), and in soil structure and in reduced erosion (García-Orenes et al., 2012; Stavi et al., 2012).

The author's state in the introduction that different biochars can have dissimilar effect on soils due to the range of different properties that biochar can exhibit. However, in Table 1 they provide little information of the biochars. I would add at least ash content and pH. Some available nutrient content analysis or pore size distribution would also be interesting and could be helpful to interpret the data, in particular the former are a better candidate. Also, what was the particle size of the biochar? Are there differences among them?

Additional characterization data has been added to Table 1; see statements above. Unfortunately, pore-size data is not available.

Page 601, lines 18-19: Do not repeat the word "different".

We have changed this sentence to read: "Different biochars can have unique effects on composition of the microbial community"

Page 601, lines 21-23: Not sure about why mentioning cascading effects here as they are not the subject of research in this article.

What is implied here by referring to Hendrix et al. (1986) is that as with tillage promoting bacterial based foodwebs, and no-tillage promoting fungal based foodwebs, the whole soil food web can be altered due to the potential for diverse impacts of the different biochars that were applied. Thus, decomposition processes may be regulated differently. Granted we were not evaluating specific microfloral or faunal components to decomposition, but differences in the rate, as compared to a control, would implicate that these components had been altered in some way. We have reworded the sentence to emphasize the point of how this relates to the potential longer term effects. The sentence now reads: "Altered microbial community composition in this sense could have cascading effects on higher levels of the soil food web that could result in significant functionality differences in later years, such as that observed under different tillage regimes (Hendrix et al., 1986)."

Page 602: I miss here the study of Zavalloni et al. (2011) and also Cely et al. (2014). Zavalloni et al., (2011) Microbial mineralization of biochar and wheat straw mixture in soil: A short-term study. Applied Soil Ecology 50, 45-51. Cely, P., Tarquis, A., Paz-Ferreiro, J., Méndez, A., Gascó, G. 2014. Factors driving carbon mineralization priming effect in a soil amended with different types of biochar. Solid Earth Discussions 6, 849-868.

Thank you for the suggested reference. We have added citations to these references in the introduction as follows:

“Zavalloni et al. (2011) also observed no significant difference in the degradation of wheat straw residues in the presence of 5% hardwood biochar.”

“These differences in surface and bulk chemistries can lead to various responses in microbial mineralization dynamics following biochar additions (Liu et al., 2013; Cely et al., 2014).”

Material and methods: Have the data of microbial biomass being corrected to account for biochar absorption? Would different values of K_{ec} be necessary for the different biochars used in the experiment? (Durenkamp et al., 2010).

Durenkamp, M., Luo, Y., Brookes, P.C. 2010. Impact of black carbon addition to soil on the determination of soil microbial biomass by fumigation extraction. Soil Biology & Biochemistry 42, 2026-2029.

In this study, Durenkamp et al. (2010) did observe differences in the K_{ec} for activated charcoal, but not in general for the biochars evaluated. Therefore, we hypothesize that the weathering of the biochar has reduced the impact of the sorption of DOC on the methodology. However, we used the fumigation incubation technique and measured respiration rather than extracted C and assumed no absorption of respired CO_2 occurred. We added the following language to clarify that absorption would not be an issue in our methods: “Some studies have observed impacts of high surface area biochars on the determination of biomass through chloroform fumigation/extraction procedures (Durenkamp et al., 2010). Though we hypothesize that this effect is minimized since the biochars used in the current study had low surface areas ($<86 \text{ m}^2 \text{ g}^{-1}$), and that the biochar was exposed to and sorbed DOC in the soil environment, we used the incubation technique to measure respiration instead of the direct extraction of liberated biomass from fumigation.”

Also, I miss some key information. When were the litterbags put in the soil? Is it referring to July 2009? When were the samples for biomass taken? It is uncertain to which Fall it is referring. How much soil per plot was sampled for biomass analysis? Were soil samples sieved prior to biomass analyses?

As mentioned above for the abstract, this information was added to the methods as well, specifying also that microbial biomass was assessed in October 2010, with three soil cores taken per plot, homogenized and sieved to pass a 2 mm screen.

Page 604, line 2: July of which year?

We have clarified the year as 2011.

Page 604, line 17: There is a mention here to respiration. However, those values are not reported in the manuscript.

This section has been reworded to clear up the confusion, the fumigation-incubation method was used and not the fumigation-extraction method that the reviewer was thinking about. See comments above. Fumigation incubation procedure measures the CO_2 respired and then converts it into biomass with a conversion coefficient.

Page 605, lines 22-24: This is a bit speculative.

Though confidence intervals are used for statistical inference, the statement counteracts our main attempt to focus on the non-significance, therefore this sentence has been removed.

Page 606, line 14: Shouldn't it be "over time" instead of "overtime"?

Yes, thank you, we corrected this to "over time."

Page 606, line 20: There are a few studies considering mesofauna in soils amended with biochar. See Marks et al., (2014) and Domene et al. (2014)

Marks, E.A.N., Mattana, S., Alcañiz, J.M., Domene, X. 2014. Biochars provoke diverse soil mesofauna reproductive responses in laboratory bioassays. European Journal of Soil Biology 60, 104-111.

Domene, X., Mattana, S., Hanley, K., Enders, A., Lehmann, J. 2014. Medium-term effects of corn biochar addition on soil biota activities and functions in a temperate soil cropped to corn. Soil Biology and Biochemistry 72, 152-162.

Thank-you for suggesting these additional references. We added the following statements to the manuscript: "This would be in agreement with other studies illustrating short-term impacts on macrofaunal activity observed in short-term laboratory studies (i.e., months) (Domene et al., 2014; Marks et al., 2014), but these short-term effects are not persistent in the field (Domene et al., 2014)."

Page 607, line 10: I find this sentence a bit confusing as it somehow implies that forest soils are always acidic (which is true in the soils analysed by Wardle et al., 2008). Although it is true that some forest litter can have an acidifying effect, please, bear in mind that there are forest over calcareous soils which have pHs over 7. An example of this would be the forests on calcareous soils in the Mediterranean region. Re-write to credit differences to different pH values rather than to the type of vegetation cover, as it is implied now.

Thank you for pointing this out. This sentence has been reworded as "an acidic forest soil" and "our more neutral Midwest agricultural soil" to clarify this point.

Page 608, lines 8-10: This is in contradiction with the results section.

We corrected this to specify the non-significance. This statement now reads: "Although not statistically significant, the somewhat higher decomposition of the wheat straw in the wheat middlings biochar (BC5) and pine chip biochar (BC6) treatments compared to the slow pyrolysis hardwood biochars falls in line with these evaluations.

Page 608, lines 17-19: The study of Liu et al. (2013) mentioned previously show differences with biochar type and, more interesting, uses a larger number of samples than the article mentioned here

Thank you for the suggestion. We have revised this statement for emphasis on biochar properties rather than feedstocks as evaluated by Liu et al. 2013. Our revised statements read: “This would be in agreement with current meta-analysis of the yield improvements of biochar in soil that cannot be directly correlated to any specific biochar property or characteristic (Crane-Droesch et al., 2013). This further emphasizes the need to understand the mechanisms and impacts before extrapolating any biochar impact to the field.”

Page 608, line 20: But it was not statistically significant.

The correlation between microbial biomass and decomposition rate was sound, though correctly, neither parameter separately were significantly different among the biochars and controls evaluated. We emphasis the correlation to avoid the implication or confusion on statistical significance. This sentence now reads: “The positive correlation between microbial biomass and decomposition rate was notable, particularly as relates to the low measurements in the macadamia nut biochar treatment (BC7).”

Page 609, lines 6-7: The particle size of the biochars has not been indicated at any point in the study. Please, report it in Table 1 or in the text.

This information has been added to Table 1.

Page 609, line 17: If this result is not statistically significant, then this cannot be affirmed.

You are correct, so we have reworded this as, “Although not statistically affirmed here, soil microbial biomass changes were the most likely drivers of the variability in the decomposition rates observed.”

Page 609, lines 19-21: This is a bit speculative. If some of the microbial/faunal communities in soil are redundant, then there could be changes in their composition without processes as decomposition being significantly affected.

We agree and this conclusion has been restructured to the focus of the study. This line now reads: “These observations suggest that a one-time biochar application has little potential for chronic influences on degradation rates of freshly applied organic matter.”