

General Comments:

This manuscript provides a helpful step in the process of transitioning from short-term lab studies of biochar's effects on soil to longer-term field studies. Such a transition is needed to answer if biochar engineering (i.e. tailoring biochar properties), and if biochar at all, is beneficial for soil, for agriculture, for sustainability.

Thank-you, we are grateful for your positive comments and additional suggestions below, which we have used to improve our manuscript.

The conclusions to be drawn from this study based on the results and the authors' discussion are not completely clear. On one hand, statistics indicate that there is no difference between amended soils and the control, and between biochar amendments. Such a conclusion would suggest that biochar is neither helpful nor hurtful with regards to sequestering carbon as soil organic matter over time. On the other hand, the authors suggest that there really are differences between the biochars (and the biochar vs. untreated biomass). This reviewer wishes that the authors would present an argument for one conclusion or the other, even if that argument, by necessity, contains specific caveats.

Yes, we have reworded the conclusions and restructured the manuscript to keep to the same conclusion – these biochar additions did not result in any long-term alterations in the degradation kinetics of the added residues.

There needs to be more information provided about the biochars; what is provided in Table 1 is minimal at best. At least some of these biochars have been used in previous studies and the authors are missing an opportunity to include that previous work in presenting a more complete characterization and history of "performance" for those biochars. Perhaps the authors could introduce each biochar in light of why they picked it and what possible differences they expected to see. Even if the results were not statistically different for this field study, the results would still support or not support previously described trends. Among the information that should be included are references to past studies where this char was used, the characterization methods used here (i.e. how was "volatile matter" measured?), biochar particle size distribution, ash content, pH, electrical conductivity, and H/C and O/C ratios.

Additional data has been provided in Table 1, including the particle size, ash content, pH, H/C and O/C ratios. Additional details have also been added to Table 1 to address the other questions (methods, feedstocks, etc.). We do not want to detract from the focus of this paper as an effort to determine if biochar applications in general can affect coarse residue decomposition by detailing previous research. We hope that our summarization of the large pool of research on biochar, now much improved upon in the introduction with additional references provided by other reviewers, and the detailed focus on relevant research, particularly on the macadamia nut biochar,, provided in the discussion are sufficient. As detailed in our response to Reviewer #1, the reason for using these particular biochars is not their specific characteristics, per se, but that these are the biochars that have been made available in sufficient quantity for large-scale field studies.

It would also be helpful if the authors provide more information about the plot study. Was this study the original reason for constructing these biochar-amended plots 2.5 years ago? Is there other data available from these plots such as corn yields, greenhouse gas emissions, etc. that could help understand the effects

(or apparent lack thereof) here? When the litter bags were installed, was biochar/wood pellet still visible? Had the appearance changed? Do you have any quantitative evidence that biochar/wood pellet amendment was still present in the soil? Would this presence have mattered or just the change in the overall soil microbial degradation community/environment?

No this study was not the reason for the biochar field plots. This study was added later to assess the potential differences after a period of time (2.5 years).

We have added a statement regarding corn-yield and clarified the dates of installation of the litter bags.

This study did not critically evaluate the mineralization rate of the biochar, but instead was focused on whether there were detectable differences in crop residue mineralization rates 2.5 yr after initial application. Thus we were not concerned with quantitatively measuring biochar residuals.

Specific comments (in addition to those provided by the previous reviewer that I will not duplicate here):

page 604, line 24: Why was a first order decomposition kinetic model chosen? Were any other kinetic models considered? Why or why not? If so, might such alternative models help clarify possible differences between control, wood pellets and biochars?

The justification for the first-order model was added to the manuscript. The first order model is the most commonly used model for litter decomposition studies. Given the length of time, shape of the curve and goodness of fit, we felt this model described decomposition kinetics sufficiently to determine potential differences. In another sense, the length of time was too short to require that active and passive components of decay be incorporated into the model, as that is more common in year-long or longer term analyses, and for comparing different types of residues.

page 606, line 25: The times of the macrofauna sampling appear to be the same as written ("the start of the litter decomposition study" and "the time of litter bag placement"). Please clarify.

This has been clarified to read: However, macrofaunal sampling established that earthworm abundance was not significantly different among biochar treatments at the time of litter bag placement

Table 1: What are "wheat midds"? Also please comment on the apparent inconsistency of the volatile matter contents with those reported in previous biochar characterization studies, i.e. how does a biochar made at a much higher slow pyrolysis temperature (BC6) have a higher volatile matter content than that of biochars made at lower temperatures? What was special about the ICM process? How does this relate to the ash content of those biochars and the original composition of the feedstock?

The definition of wheat midds is now given in the text and table 1, in that they are a by-product from the milling of wheat.

The inconsistency in volatile matter is due to the fact that these biochars originated from

different pyrolysis units. A statement clarifying the lack of clear relationships between biochar properties is now given in the materials and methods: "Since these biochars were produced in different pyrolysis units, they lack the overall relationship between properties and production processes (e.g. temperature and residence time) that have been correlated by previous studies when they use the same pyrolysis unit (i.e., Zimmerman, 2010; Mašek et al., 2013). These non-universal trends have also been observed in the chemical composition of volatile matter across different biochars (Spokas et al., 2011)."

ICM was the supplier and not the process.

A figure showing the relationship between decomposition rate constant and microbial biomass carbon would be helpful.

As the data for k value and microbial biomass have been provided in Table 2, we feel a figure with this data would be redundant. Regardless, we used a pairwise correlation procedure, to obtain a correlation co-efficient, which technically, is not graphically presentable. Use of a regression procedure might also imply that microbial biomass C could be used to predict the k value, an aspect we are not prepared to delve into. By standard assessment a positive correlation indicates that as the value of one measurement increases so does the value of the other. However, as a point of consideration, we can provide a graphical illustration of this relationship within this response.

