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Interactive comment on "Crop residue decomposition in Minnesota biochar amended plots" *by* S. L. Weyers and K. A. Spokas

Anonymous Referee #2

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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.

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.

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. 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?

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?

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.

Table 1: What are "wheat mids"? 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?

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

Interactive comment on Solid Earth Discuss., 6, 599, 2014.