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Interactive comment on "Switchgrass ecotypes alter microbial contribution to deep soil C" *by* Damaris Roosendaal et al.

Anonymous Referee #1

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General Comments This paper describes differences in switchgrass ecotypes with respect to biomass production, rooting structure, and soil microbial biomass and community structure plus its uptake of labeled exudates. The study is scientifically sound, using proper methods and suitable replication. A prodigious amount of work is behind the data.

Specific Comments 1. P2, Lines 8, 15. Substitute "biomass" for "abundance". PLFA is measured in ng of lipid biomarker per mass of soil and is commonly converted to biomass. It is not known to be directly related to cell abundances. For individual groups, it is measured as mole percent of the total and thus is a proportion of the total biomass. 2. P2, Lines 16-19. Please provide P values. 3. P2, Line 19. Insert "in" after "excess" 4. P2, Lines 29, 30 and P3 line 1. I think this statement is reversed and should be "greater productivity in lowland....". 5. Introduction. This study appears

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Discussion paper

to be examining a potential mechanism for varying soil carbon sequestration according to ecotype, i.e., root amount, structure, and exudates that may differentially affect soil microbial communities could alter the amount of soil C sequestered. What would be good to know at the onset is: What defensible data are there on the influence of switchgrass ecotypes (maybe even these same ones) on SOC at depths? 6. P3, lines 8, 12. There are many studies preceding Fierer et al (2003) that examine distributions of soil microbes with depth. A few examples are Federle et al., 1986, Wood et al., 1993, Dodds et al., 1996, Bone and Balkwill, 1988. Surely there are many in earlier decades. Maybe a review, a text, or earlier reference would be better for these general statements. 7. P4. Lines 13-17. Have these properties been previously measured in these specific ecotypes? This should be clarified in Introduction. 8. P4, line 29. Please clarify the timing of the burn with respect to the plant and soil sampling which follows. After May? 9. P8, line 1. No mention of neutral fractionation was made in the methods. 10. P10, line 6. Define SRL before use. 11. P10, lines 19-20. Please clarify that this was a transient increase in an otherwise downward trend. This statement is confusing after line 18. 12. P11, lines 1-4. The wording around these P values is objectionable to some readers who reject entirely "marginally significant". One option is to always state the P value and not provide an acceptable alpha in the methods and let the reader decide for themselves. A very sticky area; although, I personally have sufficient confidence in these effects. 13. P15, lines 9-16. Much of this text is verbatim from p13. Please re-write. 14. P15, line 18. Endophytes don't appear to be targeted by the sampling scheme. Re-write accordingly. 15. P15, lines 24-30. This paragraph switches between AMF and all fungi and is confusing as written. Please re-write. 16. P15, line 24. AMF biomarkers can be difficult to reliably use, especially PLFA 16:1w5cis. Please see Sharma and Buyer. Appl Soil Ecol. 2015. My recommendation is to downplay conclusions regarding AMF in this study. You have already shown that fungal biomarkers preferentially took up labeled exudates under Summer which is a nice finding and can be linked to C sequestration processes. 17. P16 lines 13-16. This info might be good in the Intro, especially if joined by other studies on ecotypes x SOC interaction. 18.

P16, line 24. Was aggregation measured in this study? Maybe insert "and therefore may promote..."

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