

Interactive comment on “Soil microbial biomass and function are altered by 12 years of crop rotation” by M. D. McDaniel and A. S. Grandy

Anonymous Referee #1

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I acknowledge the amount of work put into this study by the authors. This is a unique and comprehensive investigation of the effects of a single management practice (rotation) on microbial function, where many studies cannot study individual management decisions due to other, confounding management decisions. The introduction and discussion sections are very clear, with minimal jargon, and the data interpretation is logical. However, some issues need to be addressed (some very minor, others more crucial):

1. Throughout the manuscript: I noticed several problems with regards to the references, e.g. wrong year of publication reported in the main body of the text (Treseder et al. 2015; Anderson & Domsch, 1989), missing citations in the reference section (Ret et al. 2008; Guckert et al., 1996; Paul et al., 1999; Robertson et al., 2000; Schimel et al., 1989; Tibbits et al., 2002; Mou et al., 2008; Frey et al., 1999), references not cited in the

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main body of the text (Adviento-Borbe et al. 2010; Behnke-Ryser et al., 2012; Berard et al., 2015; Cambardella et al., 1999; Morillas et al., 2015; Plante et al., 2011; Thoms et al., 2010; Trivedi et al., 2015), and authorship misreported (e.g. “McDaniel (2014c)” instead of “McDaniel et al. (2014c)”; “Giller, K. E. N. E.” instead of “Giller, Ken E.”; “Van Der Putten, W. I. M. H.” instead of “Van Der Putten, Wim H.”). References were also not in alphabetical order in some instances. Additionally, there were spelling mistakes for some of them (see Doran et al., 2000; Franzluebbers et al., 1995; Hamilton et al., 2015), and incomplete references for Venter et al., 2016. Please make appropriate changes where needed.

2. L. 31: The authors may want to reconsider the statement as species richness in a rotated cropping system is one only if all weeds have been removed from the system, which is theoretically possible but not always the case.

3. L. 52-64: The authors introduce CLPP and how it works. Being unfamiliar with these measurements and how to interpret them, I think it would be helpful to other readers like myself if the authors added more information about this technique in the context of agricultural studies. How would a more or less even CLPP profile be interpreted in the context of agricultural soils? Does an even profile generally correlates with more efficient nutrient transfer to the plant or perhaps better C retention? Are there any previous studies that have looked into this that can be referred to here?

4. L. 83: 42° 24' N?

5. L. 96 & 98: It is *Zea mays* and *Trifolium pratense*. Please correct.

6. L. 101: Where in the field were the soil cores taken during the summer? Were they within or between rows? Likewise, were cores in the spring and autumn timepoints taken where previous rows or interrows had been? The rhizosphere effect can have a large impact on microbial communities and functioning and the summer collection is the only time point in which corn is actively growing. This might change interpretation of the data.

7. L. 114-122: The amount of sample used for specific measurements is omitted and should be included for reproducibility. Also, why specifically using a 50% water-holding capacity?

8. L. 127-129: What is the rationale for measuring PMC more frequently at the beginning of the experiment than the end?

9. L. 133: The acronyms MBC and MBN should be introduced here for later use in the manuscript.

10. L. 142: “Soils were

analyzed for 7 extracellular enzyme activities” should be changed to “8”. 11. L. 173: Please indicate what sort of transformation was applied to the data (remind the reader in the Figure titles as well). 12. L. 184: it is conventionally accepted to provide details about the version of R used. Later versions generally have bugs fixed and may explain discrepancies observed by other users (if they were to run the exact same dataset as the authors’). 13. L. 195: “There were no significant rotation or season effects on total soil C and N”. Table 1 says otherwise. Please correct and, if necessary, adapt your interpretation of the data and conclusions. 14. L. 198-212: Clearer language should be used throughout the results section. In several instances the authors average over the rotation treatments or the season treatments without telling the reader, making the percentage increases difficult to interpret (see lines 199, 200, 202, 204, 206-208 [why combining CSW and CS treatments here?]). Furthermore, I find different results for the DOC:DON mean in autumn (i.e. 17.4, five times that of spring and 13 times that of the summer). Report the standard deviation or standard error for these calculations. 15. L. 217-218: This is impossible to state without a post-hoc test. Please use the appropriate test and rephrase if necessary. 16. L. 221-222: I disagree. These effects seem strongest in the spring and autumn. 17. L. 228: This is not the p-value reported in the Figure. 18. L. 240-241: It should be “25% greater than autumn and 99% greater than spring”. 19. L. 256 and 272: I do not understand how the authors obtained these values. Could Table 4 be erroneous? 20. L. 263: Refer to table 4 instead of Figure S3. 21. L. 274: “Complementary” not “complimentary”. 22. L. 279: I agree that there is a positive relationship between CLPP and EEA but I would not say they are “strong”. 23. L.296-307: This is almost the same paragraph as l.273-295. Please remove. 24. L. 339-359: The authors discuss how drying and wetting impacted their results in the summer treatment. Why was this date chosen as a sampling date? Was the rain just bad luck? The fact that other timepoints were during dry periods may confound the comparison between summer and other seasons. The study may have benefited from another summer timepoint taken when a wetting period had not occurred for comparison. The author’s comments on this would be appreciated. 25. L. 347: It should be

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“Table 1”, not “Table 2”. 26. L. 365: Are the authors sure about these values? I was unable to find the same results. 27. L. 376: The authors refer to microbial biomass C:N as having a season \times rotation interaction. However, Table 4 does not show this interaction as statistically significant. Is the data in the table incorrect or is the text wrong? In the case that microbial biomass C:N does not have this interaction does the following interpretation (line 377-378) that these interactions are “indicative of the enhanced ability of soil microbes under diverse rotation to process, provision, and retain N” still hold? 28. L. 376-378: Regardless of what measurement showed season \times rotation interaction, the interpretation that these interactions serve as evidence for enhanced N cycling and retention in diverse systems could use strengthening. What specific functions of NAG and PER indicate that they can improve N cycling, and why is an interaction between season \times rotation meaningful? If microbial biomass C:N does show an interaction, how specifically does this serve as evidence for enhanced N provisioning? As it stands, these lines seem like a very important argument in the manuscript with little discussion to strengthen it. 29. L. 405: Repetition of “studies”. 30. L. 471-472: The authors may want to reconsider the strength of their statement. There is no proof rotation facilitate microbes in supplying more N to crops; there is only more potentially mineralizable N thanks to diversification (and the word “potentially” is important in this context). 31. Tables: 1-page Tables would be much more convenient to read and the number in parentheses/use of bold text/colors need to be explained in the title. Additionally, Tables S2, S3, and S4 are not reported in the text. 32. Figures: I would have liked to find post-hoc analyses for each panel in Figure 1 and 2. The statistics used for Figure 2 need to be briefly explained in the title, as well as what the error bars represent in Figure S2. The legend for Figure S3 is too vague; what are the lines and icons representing? Finally, there is an overall problem with Figure numbering in the Supplementary Material.

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