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Interactive comment on "A deeper look at the relationship between root carbon pools and the vertical distribution of the soil carbon pool" *by* Ranae Dietzel et al.

Anonymous Referee #1

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In this study, Dietzel et al. used an agronomic trial to study the linkage between root C input and the vertical distribution of Soil Organic Matter (SOM). Using a soil under corn cultivation for more than a century, they measured the SOM profile of this soil as well as the root material input and quality (C:N ratio) along the soil depth for both prairie and maize vegetation. They found that maize allocates a higher proportion of root input in deep soil layers and that it as a lower C:N ratio compared to prairie plants, which is quite classical. Further, they found that root C:N ratio increases with depth for all the treatments. This result is interesting and quite new from what I know. This suggests that deeper roots are dominated by transport root with highly sclerified tissues and poor absorptive proteins content compared to surface roots. Finally, they conclude that

in moving from prairie to maize, a large, structural-tissue dominated root C pool with slow turnover, concentrated at shallow depths was replaced by a small, non-structural-tissue dominated root C pool with fast turnover evenly distributed in the soil profile, suggesting that maize may allocate more root C input to the soil than prairies at deeper depths. This constitutes the strong portion of this manuscript.

Based on the conceptual framework and the empirical results of the study of Cotrufo et al. (2015) about the formation of SOM, they also argue that their pattern of increasing root C:N ratio with depth could explain why an disproportionally large stock of SOM relative to root C inputs is found in deep soil. First, I found it quite tricky to conclude about the driver of such a global scale pattern from data of a case study like this. Beyond that, I am not convinced by this interpretation and I found their argumentation about this statement quite weak for several reasons related to logical contradiction and some misunderstanding about the work of Cotrufo as discussed into more detail below. If I consider these two paths of SOM formation and your results together, I would consider that shallow root of high litter quality would supply high input of DOC that can be efficiently processed by soil microorganisms (high Carbon Use Efficiency [CUE]) and supply larger quantity of microbial by-products that can be then stabilized in soil microagregates by mineral-binding, thus leading to higher C sequestration. In contrast, the deep root of poor quality (higher proportion of POM) will be least efficiently processed, thus leading to higher C lost by mineralization relative to SOM formation and ultimately lower C sequestration. This is thus not consistent with the pattern of the disproportionally large stock of SOM relative to root C inputs in deep soil. Further, Cotrufo et al. (2015) studied SOM formation over short-term scale whereas deep soil C is often hundreds to thousands year-old and highly microbialy processed. Fontaine et al. (2007) found that deep soil C mineralization is strongly limited by energetic constraints. This slow turnover together with the DOC input from surface to deep soil layer documented by Rumpel and Kögel-Knabner (2010) could more likely explain the disproportionally large stock of SOM relative root C input is found in deep soil. I also pointed several methodological issues detailed below. Finally, I fell that you did not so much discussed how the root system of your different plant communities (maize vs. prairie) could explain the vertical C profile of your studied soil though this constitutes the strong part of your study to the linkage between root C input and the vertical distribution of SOM.

Taken together, I think this manuscript will need important revisions to be acceptable for publication, especially by avoiding tricky extrapolation and misinterpretation and by refocused on the conclusion you can reasonably draw from your results. Clarify your scientific questions/hypotheses could also help to achieve this end.

âĂČ Detailed discussion of the manuscript P.1-L. 15. 'in moving from prairie to maize' If I well understood your design, you studied soil root allocation on restored prairies that have maize cropping historical of >100 years. Therefore, would it not be more correct to talk about moving from maize to prairie. P.1-L. 15. 'contribute' to what? To soil C stock? Please clarify. Alternatively, we could also talk about 'C allocation'. P.1-L. 21. Please clarify what you mean by 'aboveground process'. Is really soil disturbance (tillage?) an aboveground process? P.1-L. 26-27. Is this definition really necessary here? I think it will be better placed in the Material and Methods section. P.2-L.5. Please insert the Weaver citation P.2-L.17. Why did you used 'Carbon:N' though you used 'C:N' just before. P.2-L.19-21. I do not clearly see how your experimental design give you a 'unique perspective on characteristics of root inputs' Please clarify. It would also be useful to indicate here the number of year since prairie restoration at the end of the study (5 years not?). P.2-L.26-27. I did not understand the point of your second scientific question before to read the last extion of your discussion. Please be not explicit and precise on your purpose. P.3-L.1-2. What about soil N concentration? This could be importabnt P.3-L.1-2. How many replicates (blocks)? 4? This information is crucial! P.5-L.4-8. You used linear mixed models. Please state what factors are formulated as fixed or random effects in your models. P.5-L.15-23. Logistic model is used fit binary response variable. Therefore, I do not see the rationale to use Logistic model to fit root mass, which is a continuous variable... P.5-L.19. It is not so clear to me what you mean by "root

mass accumulation". It is the difference in root mass between two sampling dates? Or is it cumulative root growth? But you did not measure it between all sampling dates, right? Please clarify. By the way, it not so clear what was the initial root mass stock and distribution prior to experimental set-up. P.5-L.29-30. We calculated root turnover constant as k = root loss / root stock. This computation is quite uncommon. Hence, Gill and Jackson (2000) calculated root turnover constant as k = root gain / root stock. In addition to be more standard method, I also found it clearer as root gains are directly obtained with the ingrowth core method while your root loss computation use root mass accumulation, which was not very well defined. P.7-L.9. Throughout the manuscript, we heavily use the 'pool'. Though I found this term appropriate for distingue different component of the global soil C stock, I found the term 'stock' more suitable when talking about quantitative estimate. P.6-L.6. There is no reference to Table 1 in the text. P.11-12. There is no reference to most of your tables and figures in this portion of your result section... You really need to clearly use reference to it for justify what you state in the text. In its current state, I do feel really difficult to follow your text. P.10-Table 3. Is this really useful ? Figure 4 already provide this information. This table should be place in appendix. By the way, I found that there is quite too much table and figure in the article. P.11-L.15. What you mean by input? Is it your root mass accumulation? Please clarify. P.12-L.6. What about soil N concentration and soil C:N ratio across soil depth and treatment? Isn't this information important is understand the root C:N profiles? P.13-L7-8. 'a physical-transfer pathway whereby plant tissue is processed by soil microbes to its fullest extent, and then remains in the soil functionally inert'. Really? Cotrufo et al. (2015) actually talk about physical transfer of Particulate Organic Matter (POM) from litter to soil. POM is not functionally inert! P.13-L.11-19. 'root decomposition in our study would have resulted in a gradient of microbially-derived to physically-derived organic matter from the top of the soil profile downward' Then this is not consistent with evidence that the contribution of microbial- and not root-derived C increases with depth (Rumpel and Kogel-Knabner, 2011) in contrast with what you stated L.15-16. I assume that DOC derived from soil surface can be mobile and move down the profile but a large portion can be stabilized in the surface and at least the SOM derived from deep root with high C:N ratio should be less microbial-derived given what you state. This point should be clarify. Moreover, the notion physically-derived SOM does not make sense, see my previous comment. P.13-L.12-14. 'Soil organic matter at the soil surface would be vulnerable to transport to greater depth as dissolved organic C whereas physically-transferred soil organic matter at depth would be relatively immobile'. If you read carefully Cotrufo (2015), she stated that DOC derived from litter is preceded by soil microorganisms and the microbial by-products are then stabilized in soil microagregates by mineral-binding. This mineral-stabilized SOM is thus actually less mobile than POM, in contrast with what you stated. P.13-L.16-19. Exsudates are highly labile compounds that are very quickly preceded by soil microorganisms. Once metabolized, they are much less mobile. Therefore, they probably represent a minor fraction DOC moving down the profile and that could form deep SOC. P.13-L.27-29. 'By the sixth year of reconstructed prairie establishment, root C pool equilibrium was reached and prairies began making substantial annual contributions to the soil organic matter pool above 30 cm, although the fraction of organic matter that remained in the soil is unknown' You have information on root litter decomposition and soil organic matter turnover, so cannot state anything about SOM formation or stock. All you can saw this you likely have higher root litter input that could eventually increase SOM stock. P.13-L.35-37. Probably, but this is quite speculative… P.14-L.10. 'contributed more C' This is unclear. âĂČ References Cotrufo, M. F., Soong, J. L., Horton, A. J., Campbell, E. E., Haddix, M. L., Wall, D. H. & Parton, W. J. (2015) Formation of soil organic matter via biochemical and physical pathways of litter mass loss. Nature Geosci, 8, 776-779. Fontaine, S., Barot, S., Barre, P., Bdioui, N., Mary, B. & Rumpel, C. (2007) Stability of organic carbon in deep soil layers controlled by fresh carbon supply. Nature, 450, 277-U10. Gill, R. A. & Jackson, R. B. (2000) Global patterns of root turnover for terrestrial ecosystems. New Phytologist, 147, 13-31. Rumpel, C. & Kögel-Knabner, I. (2010) Deep soil organic matterâĂŤa key but poorly understood component of terrestrial C cycle. Plant and Soil, 338, 143-158.

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