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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.

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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.

Thank you, we too were excited to quantify differences in prairie and maize root allocation and find root C:N ratios increase with depth.

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.

Yes, since this is only one study, we avoid making any strong conclusions, but do suggest that root C:N ratio plays a role in development of the soil profile. Finding increases in root C:N ratios with depth is significant on its own, but we feel it is very important to put this finding in the context of larger scientific questions.

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.

We feel we can do a better job communicating our proposed mechanism and we address these details below. We found your comments to be extremely helpful towards improving this manuscript and enjoyed getting a new perspective on many of the aspects we have wrestled with during writing. It seems we all have the same understanding of Cortrufo et al.'s conceptual models, but are used to thinking about these models under different environmental circumstances. One major assumption on which we do not completely agree is the likeliness of microbial by-products to be transported deeper in the soil profile. We hope the discussion below and additional references help to clarify the manuscript.

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.

Right! We find this inconsistency in patterns very interesting and a major motivation for the manuscript. Although the proposed relationship we describe between root C:N ratio and soil C profile development is not immediately intuitive, the combination of MEMS and dissolved organic carbon (C) transport leads to a very possible mechanism behind a disproportionally large stock of SOM relative to root C inputs.

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.

Yes, Cortrufo et al. have focused on short-term scales and their conceptual model is still hypothetical, however, what happens in the short-term is directly connected to what happens in the long term. The fact that deep soil C is highly microbially processed does not indicate where where the C originated.

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.

Both of these factors, and many more mentioned in the manuscript, contribute to the disproportionally large stock of SOM relative to root C input found in deep soil, but do not fully explain it and do not incorporate the role of root C:N ratio. The transport of DOC is especially important in our proposed mechanism and we spend some time on how roots at shallow depths vs. deeper depths contribute to this DOC.

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.

Thank you for the methodological questions below. We would have liked to spend more discussion on the root systems of maize vs. prairie, but felt that without measurements of the original soil C profile, discussion specific to change created by annual cropping systems would be challenged. However, we can strengthen this component in response to your comment.

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.

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.

We are referencing the historical shift from prairie to maize. I will change the wording for clarification.

P.1-L. 15. 'contribute' to what? To soil C stock? Please clarify. Alternatively, we could also talk about 'C allocation'.

Yes, soil C stock. We will clarify

P.1-L. 21. Please clarify what you mean by 'aboveground process'. Is really soil disturbance (tillage?) an aboveground process?

Will change 'aboveground processes' to 'soil management'.

P.1-L. 26-27. Is this definition really necessary here? I think it will be better placed in the Material and Methods section.

We included it here as we go on to use the definition in the introduction.

P.2-L.5. Please insert the Weaver citation

Thanks for catching this, we will insert the citation.

P.2-L.17. Why did you used 'Carbon:N' though you used 'C:N' just before.

We did not want to start a sentence with an abbreviation.

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.

We expect many of the characteristics reported here to be less detectable in well-established prairies systems, but you are right that prairie reconstruction in not entirely unique. We will replace "unique perspective" to "new perspective".

It would also be useful to indicate here the number of year since prairie restoration at the end of the study (5 years not?).

Yes, this will be added here.

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.

Thanks for this comment. You illustrate that we need to change some aspects of the introduction to make it understandable for an international audience. For example, in this instance we will simply say "perennial prairie" instead of "historical" and "annual cropping systems" instead of "current systems".

P.3-L.1-2. What about soil N concentration? This could be importabnt

We do have total soi N data and will include it here.

P.3-L.1-2. How many replicates (blocks)? 4? This information is crucial!

I'm sorry we did not include this, it will be added.

P.5-L.4-8. You used linear mixed models. Please state what factors are formulated as fixed or random effects in your models.

This information will be added.

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. . .

We will add more details here. Logistic regression is used to fit binary response variables. We used a logistic function to fit the data and then statistically compared the parameters of each fit of the function as described in the book "Mixed Effect Models in S and S-plus", Pinheiro and Bates, 2000. We will add this citation.

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.

"Root mass accumulation" refers to root mass gained. We did not measure it between all sampling dates, rather we used the model we fit to predict values during times we did not make measurements.

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.

We can and will easily replace "root loss" with "root gain" in our equation.

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.

We will change the text so that 'pool' is used only when discussing specific components of the global soil C stock.

P.6-L.6. There is no reference to Table 1 in the text.

Our apologies, we will correct this.

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.

We will add more references to make the text easier to follow.

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.

We will be happy to move Table 3 to the appendix and consider removing or combining

some of the figures.

P.11-L.15. What you mean by input? Is it your root mass accumulation? Please clarify.

Root mass accumulation is root mass input - root mass loss. Input is how much root mass went into the soil. We will clarify this in the text.

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?

Yes, this information is important and we can include total (organic + inorganic) N values for this soil. While root C:N ratio increases with depth, soil C:N ratio decreases with depth, and it may be useful to discuss this relationship.

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!

We will change this to better reflect Cotrufo's original language and refer instead to the "inherent chemical recalcitrance" of organic matter resulting from the physical-transfer pathway.

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.

The sentence following this one in the manuscript is very important. The microbially-derived organic matter would be mobile and transported to deeper depths, contributing to the relatively immobile pool of physically-derived organic matter. This is very consistent with Rumpel and Kogel-Knaber (2011), as we eventually conclude.

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.

Yes, DOC can be stabilized in the soil surface, but the proportion of C stabilized depends on soil type and level of C saturation. We will add a reference to Castellano (2015) to support this idea. These prairie-formed soils do not have C concentrations as high as historical levels, but total C concentrations are still at 2.8 percent, indicating a reduced capacity for additional C stabilization. In this environment, microbial by-products are likely to be part of the soil solution and easily transported to deeper depths with greater capacity for C stabilization. We will make this clearer in the manuscript.

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.

Microbial by-products are very mobile until they are stabilized. When and where they are stabilized depends on the soil conditions. In the mechanism we propose, microbial by-products reach the deeper profile and are stabilized there. This is consistent with findings that proportion of microbial-derived SOM increases with depth.

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.

Thank you, we should provide clarification that exudates quickly move into microbial pools. However, the fate of the C after that again depends on the environmental capacity for microbial by-products to be stabilized.

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.

You are absolutely right. This will be fixed by changing 'contributions' to 'inputs'.

P.13-L.35-37. Probably, but this is quite speculative. . .

It is indeed speculative, but the most reasonable answer given the evidence available.

P.14-L.10. 'contributed more C' This is unclear.

Will change to 'had greater C inputs'.

References

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