



Comment on soil-2021-56

Anonymous Referee #2

Referee comment on "Land use impact on carbon mineralization in well aerated soils is mainly explained by variations of particulate organic matter rather than of soil structure" by Steffen Schlüter et al., SOIL Discuss., <https://doi.org/10.5194/soil-2021-56-RC3>, 2021

This is an original approach to study drivers of decomposition and microbial respiration in soils, in particular soil microstructure. The work is based on clear objectives and a strong sampling design and overall clearly written. I therefore believe this work is worthwhile publishing. The authors find that soil micropore structure as determined with XR Tomography does not explain variation in microbial respiration or growth among different land use types, at least not in well aerated soil (NW European spring conditions). They also conclude that POM (particulate organic matter) mainly causes the differences in carbon mineralization among soil types (see title of the paper). My general concerns about this paper mainly relate to this latter conclusion, and in my opinion those concerns need to be addressed through major revision in order to make sure that the results are correctly interpreted and the limitations of the study are understood. Those concerns include:

- The interpretation of results based on correlations as proof of causal relationships (see for example the title).
- The authors make quite strong conclusions on the role of POM in explaining variation in respiration. However it is clear that the methodology used for POM fractionation did not separate root biomass from POM. So it should be very clear that all data and conclusions on the role of POM, in fact correspond with POM + ROOT biomass. This is an artefact of the method because these roots died as a result of sampling. Important to acknowledge this when interpreting your results and the possible implications of this for the interpretation of the results need to be explained clearly.
- Important aspect for interpretation of the results is that "in none of the soil cores the initial water saturation was high enough to induce a deficiency of soil aeration". This is a very important observation that requires attention when drawing general conclusions on the importance of structure for respiration (again, see title). The implications of this for the generalization of the results need to be discussed. At the moment this is only brought up at the end of the conclusions section.
- The authors should discuss the implications of the fact that the XR Tomography has its

limitations in terms of resolution (minimum pore size) and also discuss in a bit more depth the potential drivers that could explain mineralization but that were not measured (OM quality, nutrient availability, total SOM, microbial community composition, efficiency). Total SOC data seem to be available but this parameter not included in the regressions as a bulk soil property? Why not?

Other comments

Title:

The word "caused" is too strong here I believe, was a causal relation really tested? The word "explained" would reflect better the relationships obtained. Also this statement cannot be generalized beyond well-aerated soils. I suggest to change to: "Variation in carbon mineralization across land use types in well aerated soils is mainly explained by particulate organic matter content rather than soil microstructure".

Abstract:

Ln 15-16: Please be more specific: This paper is about respiration and microbial activity. Not about the other functions

The organization of the abstract can be improved from Ln 22 onwards. First, present the main results that were obtained, and then interpretation, followed by clear conclusions. At the moment results, assumptions and interpretations are all mixed and it is not clear what was actually measured and what was assumed to explain these results. Also be careful with the word "cause" where causal relationships were not established.

Ln 24: The word "absent" is too strong here. The effect of moisture cannot be excluded but in the current study these effects may be dominated by other factors such as total SOC? whether this effect is absent should be assessed by manipulating the moisture content of the same treatment and measure respiration. This was (unfortunately) not part of the current methodology.

Ln 25-26: How was this concluded from the data presented? No measurements were done in microbial hotspots. This is an assumption not a finding.

Ln 33: I don't think we can exclude that there would be an effect of land use through micropores although the smaller the structure the less likely it is affected by land use.

Ln 38-39: Again, mineralization in microbial hotspots was not measured; this explanation is assumed. Make sure this is understood as the abstract should be understandable as a stand alone text. Also make clear that POM is not only POM, but POM + root biomass

Introduction:

Ln 43- 51: This description does not provide a complete picture of the most recent theories on C storage and mineralization - biochemical and physical protection through interactions between organic molecules and reactive soil mineral surfaces is missing.

Ln 70 and other places in the manuscript: The authors tend to mix up R and R². Normally the small letter r is used for correlation coefficient. R² for goodness of fit of regressions. See also Ln 262-263

Ln 80-82. The objectives are very clear. But it is not clear to me why you chose to measure basal respiration at field moisture content. To address the objectives wouldn't it have made more sense to test respiration at different moisture contents for the same structure? This would have permitted you to separate microstructural effects from environmental conditions (like soil moisture content).

Ln 85: Can you explain what kind of bulk properties you had in mind for this?

Materials and methods:

Ln 95: No need to mention the part of the experiment with future climate conditions. This is not relevant for the current study.

Ln 115: Rather than "initial water saturation" isn't "water filled pore space" a term readers may be more familiar with? I suggest to replace throughout the text and figures

Ln 121-125: The method used to measure POM weight needs better explanation. POM data are quite crucial for the main conclusions of the paper. Make sure it is understood how POM fractionation was done, how the soil was dispersed and if dispersion was complete. Also make sure it is understood that what is measured here as the POM fraction includes root biomass present at the time of sampling. Do you have any idea what was the contribution of root biomass to POM in the different land uses? Did you not

measure POM-C? This could provide a more accurate estimate of the amount of POM+rootbiomass?

Ln 146: Did the visual method also include root biomass in the POM fraction?

Ln 191-200: The PCA analysis is not mentioned in the statistical methods. Also please explain how you dealt with the psuedo-replicates in your models (2 and 3 cores per plot were analysed)?

Ln 201-203: Where are all these symbols clearly explained?

Results

Presentation of results is confusing; please make it easier for the readers to follow. I suggest:

- Present the results according to the research objectives in a coherent way.
- What is not directly related to the objective put that in supplement and vice versa.
- Too many symbols are used in text and graphs. Try to limit them or explain them more clearly.

Ln 230: Is the volumetric air content after glucose saturation a bulk soil property? What does it tell us? other than the conditions during which SIR was measured?

Ln 251: Did both POM measurement methods that were compared include the root biomass?

Ln 252-Ln 255: This belongs to the discussion or to the materials and methods

Fig 4: Please explain the codes. Graphs should be self-explanatory.

Ln 295-301: This fits better the discussion or conclusions section

Fig. 6 + 7: PCA model: It is not clear why the PCA was not used to inform the variables to be used in the multiple regression. Why not pick the strongest correlating parameters from PC1, and the strongest correlating parameter from PC2 as test model?

Fig. 6a: can the variation in respiration in cropland soils be explained at all? The model with only POM has a R² of +/- 0.1, so can you say that POM is such a good predictor for respiration across land uses? or only in grassland?

Fig 6 and 7 do not match with in-text references (fig 7 and 8).

It would be nice to have the rationale of the different model (e.g. all parameters minus microbial parameters) also explained in captions. Now hard to understand the different models and the ideas of the different models

Discussion

The discussion is well organized and generally clearly written, with some exceptions:

Ln 371: "ambient conditions". Please be more specific. Rather than ambient conditions it is better to call this field moisture level. For example temperature was controlled, not ambient.

Ln 376-377: Fertilization/nutrient availability could also be an important explanatory factor, or the C/N of POM.

Ln 386: "...showed that soil compaction reduced soil respiration (Liebig et al., 1995)" -> At what scale? And what moisture levels? In the current study rings were only 100 cm³ and quite dry.

Ln 391-395: Good observation about POM including root biomass. But this is a methodological artefact. What are the implications in terms of interpretation? Please discuss.

Ln 401-404: This is not a surprising finding, when considering structural effects. However you did not look at interactions with reactive surfaces so be careful you cannot make any conclusions on this not being a driver of carbon mineralization.

Ln 405: parameters overlooked could include nutrient availability, POM composition.

Conclusions

Ln 438: Be careful to suggest causal relationships where they were not tested.

Ln 446-450: Move this part to the Discussion. This is not a good way to end your conclusions section. Finalize with your main conclusion about your objectives, not bringing up a new issue for discussion/reflection.

Please also note the supplement to this comment:

<https://soil.copernicus.org/preprints/soil-2021-56/soil-2021-56-RC3-supplement.pdf>