



## Comment on soil-2021-56

Anonymous Referee #1

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Referee comment on "Land use impact on carbon mineralization is mainly caused by variation of particulate organic matter content rather than of soil structure" by Steffen Schlüter et al., SOIL Discuss., <https://doi.org/10.5194/soil-2021-56-RC1>, 2021

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### General Comments

The authors present a study on the identification of soil properties which explain the differences in carbon mineralisation following a land use conversion. For this they apply classical experimental methods, the very promising X-ray CT method and statistical analyses. I see sound scientific work all over. Further, the manuscript is well-structured and written in a concise style. I made some suggestions with respect to the written English, however I am also not a native speaker.

Nevertheless, I identified two issues. I guess both could easily be resolved. I am not convinced that the PCA provides any new or fundamental findings that could not be determined via the extensive regression analysis presented in this study. Thus, I suggest to remove the PCA. The second issue is related to the conclusions section. It contains hardly any conclusion and is rather a summary. This should be entirely rewritten (see spec. comments).

Given this, I suggest major revisions.

### Specific Comments

title: What about 'Land use impact on carbon mineralization is rather caused by variations of particulate organic matter than of soil structure'? Just a little shorter...

17 replace 'farming' with 'agriculture'

27 'to' is missing after 'due'

29 please add 'the topsoils of' before 'croplands'

35 well, the fact that microbial biomass is correlated with substrate-induced respiration is not much of a surprise ...

60 replace 'moisture' with 'water'

62 I guess you should be more precise about the 'initial flush' as a consequence of soil disturbance. Is this initial flush hours, days or weeks? And in contrast Herbst et al. 2016 actually detected significant differences in basal C mineralization between disturbed and undisturbed samples incubated at the same water content for five days following a conditioning period of seven days after disturbance.

85 better write '.. to assess how these microstructural'

99 better write 'Each land use treatment is replicated five times...' cause you do not make use of the climate treatments in this study. As written now this would rather confuse the reader.

178 eq. 1 In the end this is a matter of taste, but I would suggest to write the formula as ' $p_{si}(t)=A+B*\exp(ut)$ ' and replace the 'e' with 'exp' cause otherwise the reader might

search for the variable 'Be'

194 The statistical analysis procedure is sound. I would just point to the fact that the error probability level of 0.05 is an absolutely arbitrary choice. You are doing a lot of sophisticated statistical analysis based on an arbitrary choice. I know it has been done for ages like this, which could also serve you as a justification ... Just be aware.

203 I would refer the reader to table 1 at this point, otherwise all the symbols might be a little overwhelming

217 well, 'manually selected' actually means something like 'variables selected based on expert knowledge', which may be a more precise statement

227 'no difference in average bulk density' don't you think this is related to the time since conversion? I would assume that it will actually take a long time. May be you find some literature data for the same type of land use conversion.

235 table 1 Units! There are some flaws in the units, please check. POM is probably  $\text{mg g}^{-1}$  (not  $\text{g}^{-3}$ ), there is a '1' too much for the basal respiration unit and the '-1' for TMP and AMB should be upper case.

254 remove one '.' at the end of the sentence

262 Well, I would not refer to a  $R^2$  of 0.53 as a 'strong' correlation. Just a little more than half of the variation is explained ... I suggest to moderate this statement. And strictly spoken: A coefficient of determination is not a correlation coefficient.

264 'independent of initial water saturation' well, there is actually not too much variation in the initial effective saturation. And the differences that may be resulting from differences in water saturation are probably counteracted by POM content: lower saturation but higher POM...

271 Generally, I do not think that SIR is very representative of what is happening under real-world conditions. The C source is totally liquid-phase and soil is almost entirely water saturated, which rarely occurs under field conditions.

276-278 so, this is all a measurement error?

286-287 I think that  $p_0$  and the  $t_{exc}$  are very redundant. A higher SIR will inevitably lead to an earlier excess of respirometer uptake capacity. Basically, the  $R^2$  should be 1...

296 For the C:N ratio this might be explained by the fact that the C:N ratio of the biomass of crops and grassland is rather similar?!

312 I guess this should be Fig. 6a and not Fig. 7a

320 I guess this should be Fig. 6b and not Fig. 7b

325 i do not think that the PCA contributes anything new or viable. I suggest to remove the PCA.

337 I guess this should be Fig. 7a and not Fig. 8a

347 I guess this should be Fig. 7b and not Fig. 8b

395 I do not think that those statements can really be validated with an  $R^2$  of 0.39. I strongly suggest to moderate the statements.

405 replace 'overlooked' with 'additional'

436-450 The conclusions section is rather a summary than any real conclusion. Basically, only lines 447-450 contain some conclusions and even those are rather common statements. I suggest to entirely rewrite the conclusions sections, simply refer to the objectives stated at the end of the introduction.