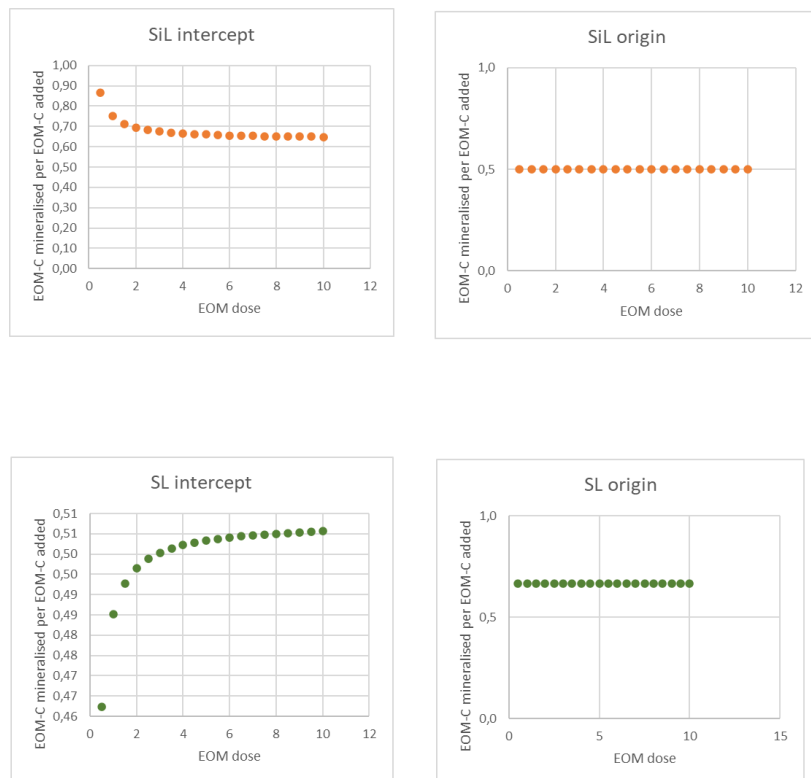


Mendoza et al. have addressed all reviewer's comments in their revision of the manuscript SOIL_2024-107. Now, the novelty of this study as compared to Mendoza et al. (2022b) is better introduced in the introduction. The new Figure 1 helps to provide a better overview on the hypotheses. However, I still highly disagree with the interpretation of Figure 2B and the following interpretations.

Relative EOM mineralisation (Figure 2).

I argued previously that the visualisation and interpretation of a non-linear decrease of the proportion of EOM mineralised at higher amounts of EOM added in SiL soils is misleading (Figure 1 in the original manuscript, i.e. Figure 2 in current version). This point is very crucial because this non-linear decline in EOM-C mineralised per EOM-C added is the fundament of further interpretation by the authors. In their reply, the authors have provided two examples to illustrate that the proportion of EOM does not necessarily need to be equal if cumulative EOM increases linearly with dose.

To further look into this problem, I extracted the data from their plots and fitted two linear regression lines. The first allowing for an intercept (*intercept*), the second one forcing the curve through the origin (*origin*). For SiL soils, this gave me the two functions $y=279.59x + 50.195$ (*intercept*) and $y=292.36x$ (*origin*). Then I projected the cumulative EOM for 20 doses ranging from 0.5 to 10 g to visualise for myself, how the regression would affect the proportion of EOM mineralised per total EOM added. The first fit, i.e. *intercept*, did indeed result in two non-linear curves for SL and SiL, with opposing trends depending on whether the intercept was negative or positive. The second fit, i.e. *origin*, did not. In the *origin* scenario, the calculation of the proportion of EOM mineralised per EOM added (Figure 2B) gives the slope of the fit of EOM mineralised over EOM added (Figure 2A), which is then equal for all doses. As mentioned in my initial comment, Figure 2B is then the derivative of Figure 2A.



I recognize that the authors' argumentation is valid from a mathematical point of view. An exponential fit of the EOM mineralised proportion over EOM dose is possible only if EOM mineralisation allows for an intercept.

I argue that it is not meaningful to assume that there is an intercept. Indeed, it seems much more logical to fit a model through the origin, assuming that the mineralization of EOM is zero, when no EOM is added. As mentioned before, the fit of the non-linear relationship in Figure 2B is weakly significant, and should not be overemphasised.

Furthermore, the question arises whether the observation of a non-linear fit in Figure 2B would really imply some underlying biological mechanisms or just point to uncertainties of the method, e.g. uncertainty of ¹³C-label at lower doses vs. higher doses? I would rather interpret the decline as caused by the intercept and the intercept being caused by methodological limitations.

The authors' assumed, that EOM mineralisation would slow down at high dose due to O₂ depletion at the OM matter surrounding, limiting microbial activity. Assuming this to be the reason for slowed degradation, one would expect that the proportion of EOM mineralised per EOM added would further decline with dose, i.e. stronger limitation at higher O₂ depletion and stop of mineralisation. However, my little calculation exercise (i.e. *intercept*) implies that the proportion of EOM mineralised per EOM added will level off at a certain amount, reaching an asymptote at approximately 50% mineralisation. Furthermore, the authors' state that there they did not observe anaerobic conditions in L 380 "but still Eh remained at levels indicative of aerobic conditions".

Long story short, there is indeed little evidence that the proportion of mineralisation is slowed at high EOM dose in SiL and the authors need to revise Figure 2 and following interpretations.

Other points raised during the first review:

Novelty as compared to previous study published in *Biology and Fertility of Soils* is now better introduced.

Thanks, for the addition of the reasoning behind the selection of dose levels.

Relative SOC priming plot (Figure 3B). Revise changes in the paragraph added to L.446-453: "The slowed relative EOM mineralization" - see my argumentation above. Also revise L523-542: "...the slowed relative mineralization of EOM at increasing dose could be related to enhanced occurrence of local O₂ limitation surrounding EOM litter, even though its addition in fact also stimulated macroporosity. ..."

MBC. I agree with the revisions. Figure 4 becomes now clearer.

Statistics. Thanks for clarification. I had confounded GLMMs and GLMs.

Figure 1 I highly appreciate the new figure, which illustrates the expected outcomes and hypotheses of the study. I recommend to add a short description of these to the figure caption to provide a quick overview on the hypotheses.

Line-to-line comments

Abstract

L13: Consider to mention your hypotheses with regards to differences to soil texture already in the abstract.

L 15: Delete “economic”. Unclear what you mean by that. Do you mean “no increased microbial growth” or “no increased microbial efficiency” or “no changes in microbial growth efficiency”?

L 17-19: I do not agree with the interpretation that the percentage of mineralised EOM decreased with dose and the following hypothesised mechanisms. See detailed comment. Consider revision.

L 20: Delete “textured”. “In both soils” is sufficient.

L 21-25: With regards to the high uncertainty of slowed EOM mineralisation in SiL at high dose, I recommend to revise this part. It seems very speculative.

Introduction

L 88-97: I really like this new passage. The novelty of this study is now better introduced.

L 107: Helpful figure! Doesn't the green line show mineralised EOM instead of % mineralised EOM (curve would start high and become lower at high dose in the latter case)? I recommend to add a short explanation on the hypotheses with regards to differences to soil texture to the caption of Figure 1.

Materials and methods

L 161: Do you mean every 1-2 hours?

L 196: CFE-extraction was done in a 1:2 v/w ratio of soil-to-K₂SO₄. Why didn't you stick to a 1:4 ratio (Joergensen, 1996)? The ratio may affect the kEC. Can you add a reference?

Results

L 254-256: Revise result that relative EOM mineralisation in silt loam soil slowed down at high EOM dose.

L 279: “the extra amount of SOC mineralized vs. the unamended controls relative to the unamended controls” - delete and in caption of Figure 3.

Discussion

The discussion needs a major revision, given that there is no evidence for a slowed EOM mineralisation with EOM dose in the silt loam soil.

L 364: The revised sentence makes no sense anymore. Check.

L 380: "but still Eh remained at levels indicative of aerobic conditions" - this is another argument against the slowed EOM mineralisation with increased EOM dose.

L 385 "In conclusion, we could not identify the cause of these phenomenon, and further research is required to explore the potential mechanisms leading to a relative temporal stabilization of EOM when added at larger doses." - This further suggests, that the authors' may have misinterpreted their results.

L 444: Add a reference for the 1-3% SOC mineralisation in the field.

L 451-453: Unclear whether this conclusion is backed up statistically? There is no statistical analysis mentioned.

L472-474: Only one regression line was fitted for the relative SOC priming over EOM dose in Figure 3 and only one slope is provided in the Results section. Why are different slopes given here? You can not base your discussion on results you did not present and which are furthermore not significant. Please revise the discussion around this point.

Figure 8: Please revise the trend in % min EOC in silt loam. Unclear what primed SOC refers to (e.g. relative SOC priming, absolute, relative SOC priming per EOM added). Please clarify.

Conclusion

Needs revision with regards to slowed EOM mineralisation with EOM dose in the silt loam soil.