

## RC1

Dear editor and authors, thank you for your invitation and sorry for the delay comments.

This manuscript presents an interesting investigation of deep soil carbon stability under warming conditions, based on the well-established field experiment at Blodgett Forest. The study is valuable, and the experimental design and methodological approach are generally sound. However, several aspects require substantial improvement before the manuscript can be considered further.

In particular, data quality should be carefully re-evaluated, as large standard errors are observed for several parameters. In addition, the figures and tables need to be reorganized, and statistical analysis results should be clearly indicated or labeled in figures and tables. The English language requires further polishing, and the citation format in the main text should be carefully checked and revised, as the current format occasionally impedes readability and comprehension.

Please see my detailed comments below.

Thanks a lot for your careful reading, insightful comments, and constructive suggestions. We really appreciate your time, and thorough review of the manuscript, which has greatly increased its quality. We will address your comments below accordingly.

### General Comments

#### Introduction

The Introduction would benefit from additional background on SOC functional groups, as these constitute one of the main parameters measured in this study. Moreover, free light POM and occluded POM are not sufficiently introduced and should be described more clearly.

Thank you for your comment, we will add further definitions and exploration in the introduction. Specifically, we will add:

1. A background on SOC functional groups.
2. A definition of occluded POM (oPOM).
3. Differences between fPOM and oPOM.

## Results

This section is generally difficult to follow. First, many results are described primarily based on tables and figures presented in the Supplementary Material rather than in the main text. The authors should consider integrating key results into the main text or consolidating them into clearer, more informative figures.

Second, numerous non-significant results are reported, which is not necessary and detracts from the main findings.

In addition, I strongly recommend conducting ANOVA followed by appropriate post hoc tests for parameters across different soil depths and indicating statistically significant differences using uppercase or lowercase letters in figures and tables.

Thanks a lot for your critical, constructive comments on our results section. We will address your comments as below:

1. We have added the SOC concentration data and PCA plot for fPOM and oPOM data to the main paper as a table/figure.

2. Second and third points will be replied together since they are all related to statistics.

We used linear mixed effects models (LMEs) rather than ANOVA test because: 1) the same statistical methods have been used in previous studies (Ofiti et al., 2021; Soong et al., 2021; Zosso, 2022) and we wanted to remain consistent; 2) ANOVA does not account for random effects such as block-based design in Blodgett Forest, does not fit for unbalanced data (for fPOM and oPOM), and most importantly does not account for autocorrelation (or pseudo-replication) due to the assumption of independence. The vertically adjacent depths are not independent from each other, which will violate the basic assumptions of ANOVA (independent observations). Our LMEs use a covariance structure (auto-regressive 1) to account for this lack of independence. However, thank you for your suggestion.

In general, we prefer conservative statistical analysis as a supportive tool for ecological, and biological interpretation of our data. Therefore, together with small sample size and high spatial heterogeneity, we also report p values that are between 0.1 and 0.05 as marginally significant. Also, as we mentioned in the M&M, we include interaction in the LMEs, and only run post hoc tests when the interaction was significant. We will remove

supplementary tables after reporting p-values to avoid potential confusion. One can also check the original data on [ESS-dive](#) website once this paper is accepted/published.

## Discussion

The Discussion requires further improvement. The authors should focus more strongly on their own results and novel findings, rather than extensively mixing their interpretations with results from other studies, particularly those conducted at the same site. This approach currently makes the narrative confusing and diminishes the perceived novelty of the study.

Thanks for your critical comments. We will focus more on the data we have, and reduce the part that related to previous studies as suggested. But the comparison is also important, regarded as one of the novelties of this study, because this is the one of the first field whole-soil warming studies that has been conducted for more than 10 years continuously, and focus on subsoils. This allows us to have a better understanding of time resolved warming effects on the SOC dynamics.

## Specific Comments

- **L26:** “warmed plots” → “Warmed plots”  
We will change the text as suggested.
- **L47:** What does “p.2” mean? The same issue appears at **L67**.  
Thanks, we will fix this.
- **L75–76:** Why would “multiple enzymatic steps for depolymerization” lead to lower carbon use efficiency (CUE)? Please clarify the underlying mechanism.  
It is generally agreed that CUE is lower when microorganisms use complex polymers as substrates when compared to labile, and simple substrates. We will make this clearer and add a specific citations (Manzoni et al., 2012).
- **L89–91:** This sentence is unclear and should be rephrased for clarity.  
We will increase the clarity of this sentence as below:  
As subsoil stores half of the SOC in the top 1 m of soil (Scharlemann et al., 2014) and is regarded as slowly-cycling and large C reservoir (Harrison et al., 2011; Rumpel and Kögel-Knabner, 2011; Sierra et al., 2024). Therefore, it is imperative to understand how subsoil carbon will respond to future warming.

- L149: This statement contradicts the description in the Abstract (Line 27).  

We will fix the term in the Abstract.
- **L132–133, L155–156, L159:** The citation format makes these sentences difficult to understand. Please revise.  

We will revise according to the comment.
- **L164:** What does “replicates” mean here? Are these biological field replicates? Please clarify.  

Analytical replicates. We will make this clearer in the text.
- **L192:** I suggest adding soil pH values here for reference.  

Previous study has measured the pH values at the same study site (Rowley et al., 2025). The soils are acidic and have a pH (H<sub>2</sub>O) of 4.9±0.1 (standard error of the mean), ranging between extremely and slightly acidic (3.7-6.2). We also measured the pH of our samples and will add the results in the SI.
- **L260:** The results presented in Table S1 are highly important for this study. Why are they not included in the main text? Additionally, the standard errors for some values (e.g., warmed soil at 20–30 cm depth) are relatively large. Please check data quality and consider whether outliers should be identified and addressed.  

For every sample, we have measured at least two analytical replicates, and for those with high standard deviation, we conducted more measurements to ensure data quality. For each run of measurement, we measured two different types of standard (Chernozem and Caffeine), and the standard deviations of both standards, and between each run are < 5 %. The larger errors could be derived from spatial heterogeneity between blocks in natural conditions.
- **L274:** According to Figure 1, fPOM at 40–50 cm depth appears to be higher under warming, which contradicts the statement made here.  

Well spotted, this should be fPOM from the 80-90 cm. We will modify this.
- **L293–294:** This sentence is difficult to understand and should be revised for clarity.  

We will improve the clarity of this sentence as such:

The proportion of oPOM was not significantly affected by depth or treatment, exhibiting consistent values.
- **L407:** “> 30 under warming cm” → “> 30 cm under warming”

We will modify according to the suggestion.

- **L422–443:** This section mainly discusses results from other studies and includes extensive speculation that is not directly supported by measurements in the current study. These parts appear redundant and should be substantially reduced.

Thanks for the critical comments. We want to explore the mechanisms to explain why some of the changes have happened. But we sometimes don't have direct evidence to support our assumptions, therefore, we referred to similar experiments in the same ecosystem or at the same study site. But as mentioned before, we will refocus the Discussion section on our own statistically significant observations.

## References

Manzoni, S., Taylor, P., Richter, A., Porporato, A., and Ågren, G. I.: Environmental and stoichiometric controls on microbial carbon-use efficiency in soils, *New Phytologist*, 196, 79–91, <https://doi.org/10.1111/j.1469-8137.2012.04225.x>, 2012.

Ofiti, N. O. E., Zosso, C. U., Soong, J. L., Solly, E. F., Torn, M. S., Wiesenber, G. L. B., and Schmidt, M. W. I.: Warming promotes loss of subsoil carbon through accelerated degradation of plant-derived organic matter, *Soil Biology and Biochemistry*, 156, 108185, <https://doi.org/10.1016/j.soilbio.2021.108185>, 2021.

Riley, W. J., Tao, J., Mekonnen, Z. A., Grant, R. F., Brodie, E. L., Pegoraro, E., and Torn, M. S.: Experimental Soil Warming Impacts Soil Moisture and Plant Water Stress and Thereby Ecosystem Carbon Dynamics, *Journal of Advances in Modeling Earth Systems*, 17, e2024MS004714, <https://doi.org/10.1029/2024MS004714>, 2025.

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Zosso, C. U.: Are we losing it? Exploring subsoil organic carbon dynamics in a warming world on the molecular level, University of Zurich, 2022.