

Response to Anonymous Referee #2 to preprint egusphere-2026-4029:

“Linking moss cover, soil organic matter fractions, and carbon stabilization pathways in temperate forest ecosystems”

We sincerely thank you for the constructive, careful and in-depth feedback to our manuscript. We also appreciate your positive assessment and helpful suggestions to further clarify and strengthen the manuscript. We are also grateful for the time and effort you have put for improving this manuscript. We have addressed each comment individually and will revise the manuscript accordingly. For clarity, the reviewer's comments are shown in blue while our responses (R) have been provided in black.

Comments and responses

Lines 55-56. I disagree with this way of simplifying what happens in a soil. According to these two lines, the sequestration capacity of soils depends only on the amount and characteristics of organic inputs, with little or no role of the mineral matrix. Later in the paragraph authors mention the association to minerals as a vector of SOM stability (lines 57-58), meaning they are well aware about these constraints. Please try to put this in a better organized way. The role of the mineral matrix must be mentioned from the beginning: actually, is the main constraint for SOM stabilization.

R: We agree that the C sequestration capacity of soils does not merely depend on the amount and characteristics of organic inputs. We have therefore, revised this statement to introduce the role of mineral matrix from the beginning, based on literatures of Jackson et al. (2017), Throckmorton et al. (2015) and Lützow et al. (2008) and incorporated into the Introduction for more balanced description as;

“The C-sequestration capacity of forest soils is governed not only by the quantity and quality of organic inputs and their transformation, but also by the interactions with the soil mineral matrix and soil structure. Organic inputs can enhance C retention through microbial processing and aggregate formation, but the mineral-organic interactions provide important mechanisms for SOM stabilization.”

Lines 65-66. These lines are somewhat redundant; essentially you say that SOM fractionation is essential to understand SOC fractions, which is somewhat a circular argument. Try to improve writing.

R: We agree on this and will therefore revise these lines to clearly explain the purpose of SOM fractionation in the revised manuscript.

Line 133. Specify the method used for analysis of total C and N, I assume an elemental analyzer (based on Dumas' method): ThermoQuest, Perkin Elmer, LECO or similar, but mention it anyway. I assume that soils were carbonate-free, and therefore total C = organic C: if so, please mention it.

R: We have clarified that the total C and N contents were determined by dry combustion method using an elemental analyzer (vario EL Cube, Elementar, Germany) in the revised manuscript. Yes, the soil samples were carbonate free and therefore, the total C = organic C which has been mentioned in the revised Methodology section in the manuscript.

Figure 2 (lines 173-174). I am very surprised by the results of the ANOVAs, specifically about the significance of the factors (forest type, moss cover Y/N) and the interaction between them. In the figures for SOC and total N, no problem with the high significance ($p < 0.001$) of the forest type: it is obvious. I am more surprised by the non-significance of the moss cover. But particularly, I am surprised by the non-significance of the interaction between them. The lack of interaction should mean that the behavior of the factor ii (moss cover) is about the same at both forest types: and this is not what happens. In coniferous mixed forests, SOC concentration is higher under moss cover, whereas in pine forest is lower. Same with total N. Thus, I would expect significance for both forest type, moss cover, and interaction between them. Revise the ANOVAs results, please.

R: Thank you for this valuable observation. We rechecked the ANOVA models, including forest type, moss cover, and their interaction. The results confirmed a significant effect of forest type on SOC, total N, and C:N ratio ($p < 0.001$), but moss cover and the forest type $\times$ moss cover interaction were not significant for SOC ($p = 0.201$ and $p = 0.158$) or total N ($p = 0.141$ and $p = 0.065$). For C:N ratio, both moss cover ($p = 0.043$) and the interaction ($p = 0.008$) were significant. We have revised Figure 2 and the corresponding Results text with the exact p values mentioned.

Page 8. Both figure 3 and Table 1 are good. But they refer to the same set of results. Thus, they are redundant. In my first years as researcher, I learned that results must be given either as a figure, or as a Table, but not both at the same time. The figure is nice, but actually Table 1 gives more information.

R: We decided to keep the more informative Table 1 in the manuscript and the figure in the supplementary file. We will then revise the Results text accordingly.

Figure 6 (page 12). Consider seriously homogenizing the scale of the Y-axis in the three sub-figures. This could add a relevant information to the reader, which is to show that the values of the Carbon Stabilization Efficiency are notably different between sites. Besides the effect of moss cover, no doubt there is a great effect of soil texture (abundance of fine silt plus clay) and soil mineralogy (presence of iron oxyhydroxides, aluminum, exchangeable calcium).

R: We agree and will update the Figure 6 with the same Y-axis scale across the three sub-figures. We also agree that soil texture and soil mineralogy may contribute to differences in carbon stabilization efficiency (CSE) between sites, although these were not directly measured in this study. We will therefore revise the Discussion to avoid directly attributing differences in CSE to the unmeasured soil texture and mineralogical properties and will acknowledge their potential influence on C stabilization.

Lines 155 to 163. How this analysis has been done is not obvious, and the paragraph 2.4 is not explicit enough. In a multiple linear regression, we try to fit an equation of the type $y = b_0 + (b_1 * x_1) + (b_2 * x_2) + \dots + (b_n * x_n)$, where 'y' is the numerical result, we want to explain (in your case, the amount or proportion of MAOM), $x_1 \dots x_n$ are the numerical values (experimentally obtained) of the 'n' variables that should explain 'y', and $b_1 \dots b_n$ the coefficients for each of these 'n' variables. The problem is that in multiple linear regression all values in the right half of the equation are numeric (both 'b' and 'x' values), whereas in your model the several components ('x' values) of the model to fit are factors (y/n). How did you calculate the model given in Figure 7? Did you replace these factors ($x_1 \dots x_n$) by numeric values (1/0)? Or rather did you actually apply a multivariate analysis, which is not exactly the same as a multiple linear regression? [I have seen often problems with the precise denomination of statistical tests, which may have different names depending on country: USA, Italy, France...]. At any rate, please give a bit more of detail about how this analysis was done, in order to understand well Figure 7.

AND

Lines 271-273. I have serious difficulties to take seriously the non-effect of parent material on MAOM. As I mentioned in my previous comment, soil texture (abundance of fine silt and clay, clay type), exchangeable calcium and Fe/Al oxyhydroxides are crucial to generate MAOM, and these properties are largely determined by the soil parent material. I am skeptic about the statistical analysis presented in Figure 7. Apparently the 'parent material' factor has not been studied altogether, since 'sand' and 'loess' have each one its own 'beta' value. Same happens with the 'moss cover' factor, which apparently has been split: what is the difference between the 'Moss cover' factor and the 'Moss effect in pine forest'? According to the figure foot (lines 275 and following) the figure represents a multiple linear regression model, whereas the several predictors (moss cover, parent material, vegetation type) are not quantitative but qualitative (Y/N for the moss cover, sand/loess, pine/mixed forest).

R: We thank the reviewer for these important comments. The above two comments related to lines 155 to 163 and 271-273 have been responded together.

We agree that parent material can strongly influence MAOM formation through its effects on mineralogy, texture, and organo-mineral stabilization processes. These comments made us to carefully re-examine the structure of the regression model and the coding of categorical predictors.

Figure 7 was generated using a multiple linear regression model, not a multivariate analysis. The response variable was MAOM-C concentration (g kg^{-1}), while forest type and moss cover were included as categorical explanatory variables. These categorical predictors were not manually converted into numerical variables; instead, they were included as factors and R automatically applied treatment (dummy/indicator) coding. Therefore, regression coefficients (β) represent differences between each category and the selected reference category (Coniferous Mixed Forest and No moss cover).

During the re-evaluation, we identified an important limitation in the original model: parent materials were completely confounded with forest types in our sampling design where sandstone and loess occurred exclusively within coniferous mixed forest whereas sand in the pine forest. Consequently, the effects of parent materials and forest types cannot be statistically separated in this dataset. Therefore, we removed parent material from the final regression model to avoid overinterpretation of its effects.

Regarding moss cover, the coefficient labelled "Moss cover" represents the overall effect of moss presence relative to no moss cover in the reference forest type (Coniferous Mixed Forest). The interaction term "Moss effect in Pine Forest" represents the additional change in the moss effect in pine forest relative to the coniferous mixed forest, and therefore tests whether moss effects differ between forest types. It does not represent a separate moss category, but rather the interaction between moss cover and forest environment.

We have revised Figure 7 and Section 2.4 accordingly. The revised analysis now includes forest type, moss cover, and their interaction as predictors of MAOM-C, while parent material is discussed as an important ecological factor that could not be independently tested because of its complete confounding with forest type in our experimental design.

Lines 284-286. 'The interaction between moss cover and pine forest was the strongest...' and 'the stabilizing effect of moss on SOC depends on forest type' are the obvious conclusions that arose from the mere visual inspection of figure 2 (page 7), which actually is very clear (and I congratulate authors for it). However, this is in gross contradiction with the non-signification of the interaction between 'Forest Type' and 'Moss Cover' stated in Figure 2. How does it all fit together? To me, there must be an error in Figure 2. Verify, please.

R: Thank you for pointing out this. We agree that the original wording was not sufficiently precise and could give the impression that a significant interaction between forest type and moss cover was detected for both bulk SOC (Figure 2) and MAOM-C (Figure 7).

Figures 2 and 7 addressed different response variables. In Figure 2, the forest type $\times$ moss cover interaction for bulk SOC was not statistically significant ($p = 0.158$), although the figure shows opposite moss-related trends between forest types. Figure 7, in contrast, evaluated predictors of MAOM-C concentration using multiple linear regression model where the forest type $\times$ moss cover interaction was significant, indicating that the influence of moss cover differed between forests specifically for the MAOM-C.

Therefore, the contradiction might have resulted from comparing two different response variables: total SOC vs MAOM-C. We have revised the Results text to clarify that the interaction effect refers specifically to MAOM-C stabilization rather than bulk SOC. We have also modified the interpretation to avoid implying that moss effects on total SOC were significantly dependent on forest type.

Lines 334-336. Replace 'microbial-derived compounds' with 'organic', simply. Another possibility is to say 'organic compounds, mostly plant-derived, more or less microbially reworked', or something similar. The binding of organic compounds to mineral surfaces, through a variety of possible bonds, is the main path to generate persistent SOM. But this 'persistent SOM' may come from microbially-transformed organic matter or from relatively fresh organic matter. Assuming that microbial-derived compounds are the only source of stable SOM is extremely debatable, and is an assumption with which I disagree.

R: We agree and replace the words “microbial-derived compounds” with organic compounds. We have therefore, revised the statement as, “Current SOM stabilization frameworks emphasize that persistent SOM can form through the binding of organic compounds with mineral surfaces, including plant-derived compounds and microbially transformed OM”.