The study introduces a parsimonious soil organic carbon (SOC) turnover model for the soil profile that includes key processes controlling carbon persistence. The model specifically incorporates two crucial mechanisms often omitted in simpler models: (1) microbial energy limitation (a form of positive/negative priming where decomposition slows if substrate is scarce) and (2) physical protection via soil aggregation (which protects organic matter from decomposition). The aim was to test this model against long-term field data, identify how well model parameters can be determined (parameter identifiability), and analyze sensitivity of the model to its parameters. However, most of the parameters were not constrained and covariance between the parameters were used as an explanation to parameter unidentifiable. The manuscript is well-written and please find the specific comments as follows:

Line 34, 36: What soil depths represent topsoil and subsoil? Providing the depth ranges would be helpful for clarity.

Line 45-49: The study demonstrated parameter uncertainty and equifinality, despite having fewer parameters than complex models. However, this raises the question: how is the simple model used in this study different from detailed mechanistic models if parameters remain unconstrained? An explanation would help readers understand the trade-offs between complexity and parameter uncertainty.

Line 70: What does ICBM stand for? Additionally, the reference to Andrén and Kätterer (1997) is missing. A schematic diagram illustrating model development over time would help readers visualize how the model has evolved. Similarly, a conceptual diagram of the final model used in this study would be beneficial alongside the mathematical equations.

Line 233: Why were parameters in Table 1 fixed, while only parameters in Table 2 were used in the calibration?

Line 253-254: Why was the mean model efficiency (EF) across all three treatments used to identify acceptable parameter sets?

- Does this mean that the same parameter set was used for all treatments after calibration?
- Why not use treatment-specific parameter sets?
- Wouldn't taking the mean EF lose treatment-specific information that could be valuable for refining the model?

Line 257: 15 model parameters (Table 3?)

Line 320, 329,332, 346, 352: Graphs texts are too small and difficult to read.

Line 314: Given that most parameters were not well constrained, could parameter covariance be a model artifact or a coincidence?

- Could the current dataset be insufficient to constrain these parameters?
- Would incorporating additional data sources (e.g., isotope data, incubation experiments) help resolve this issue? If yes, how can this modeling work be robust?

Line 316: Why were only the 30 best parameter sets selected?

- What was the acceptance rate of parameter sets out of 12,000 simulations?

Line 317: The phrase “strong correlation” is used, but no statistical analysis (e.g., correlation coefficients, p-values) is provided to support this claim. Including quantitative analysis would strengthen this statement.

Line 338: Figures 5 and 6 are difficult to interpret.

- A more detailed explanation of what these figures represent would improve clarity.
- What key insights should the reader take from these figures?

Line 345: Minor inconsistency: (e.g., consistently use “Figure X” or “Fig. X rather than mixing “Fig. X” and “Figure X”).