

# ***Interactive comment on “Modelling dynamic interactions between soil structure and the storage and turnover of soil organic matter” by Katharina Hildegard Elisabeth Meurer et al.***

**Katharina Hildegard Elisabeth Meurer et al.**

katharina.meurer@slu.se

Received and published: 25 June 2020

In this work, the authors propose a new framework to model soil organic matter turnover, which includes a two-way coupling between SOM storage and soil porosity. The model considers four pools of organic matter, with the dynamics described by four coupled differential equations. The novelty consists in using additional pools to divide the organic matter between micropore and mesopore soil regions, each one characterized by its own fluxes and decomposition rates. In my opinion such a model indeed can bring new insights about the dynamical feedback between soil physical properties and SOM decomposition, and can be an important contribution to the field. Although I find

Printer-friendly version

Discussion paper



the paper interesting, I have some concerns. In particular I would have appreciated a more detailed discussion of the advantages of this new model. My recommendation is publication of this manuscript subject to a revision based on comments listed below.

Response: We appreciate the positive feedback of reviewer#2. Yes, we will include some more text on the advantages of this model compared to models that do not include interactions with soil physical properties in a final discussion and conclusions section

1 - I find that the paper is in general well written, but the section with the description of the model is very confusing and needs to be improved. I would suggest to first write the full model including the feedback on porosity, and only afterward to follow with all the necessary derivations.

Response: Thanks for the suggestion, which we have considered carefully. However, after trying out different options, we decided we would like to keep the current structure of the model description and derivation, which we are convinced will be easier to follow and understand for the reader. We think the model derivation is already clearly presented. However, meeting some of the changes requested by referee 1 should lead to further improvements in clarity.

Also, it is not clear by looking at the equations which parameters are kept constant, one has always to search in the text.

Response: Tables 1, 2 and 4 give information on the parameters that were kept constant and those that were varied in the sensitivity analysis and in the calibrations.

One solution is to use upper case for functions and lower case for constants.

Response: We are not sure we understand this suggestion, but the difference between parameters and functions is apparent from the equations themselves.

Please also double check the notation, for example the density of mineral matter is  $\gamma_m$  on pg.6 and  $\gamma_{min}$  in all tables.

Response: Thanks for the comment. We will correct this inconsistency (and also similar ones for the density of organic matter and the minimum layer thickness). We have double-checked the notation and cannot find any other errors.

2 - The abstract states that the model successfully reproduces the soil water retention curves. I find this statement too strong due to the discrepancy of the curves for the year 1997.

Response: Yes, we will modify this sentence to make it clear that the model only successfully matched the water retention measurements obtained at the end of the experimental period in 2019.

3 - I would like to see an extended discussion on the  $k_{\text{mix}}$  and  $F_{\text{prot}}$ , since these parameters are at the core of the discussed feedback. For large values of  $k_{\text{mix}}$  and  $F_{\text{prot}}$   $n_{\text{sim}} = 1$  the soil structure properties have to become less important to the dynamics of SOM turnover. Could the authors comment on this transition to the regime where the soil porosity becomes less relevant for the model outcome?

Response: Yes, this is correct. We will include a brief discussion of this in the revised paper

I would also appreciate a short comment on the choice of the sampled range for the sensitivity analysis (and also the choices for calibration).

Response: For the sensitivity analysis, we justified the choice of sampled ranges at lines 244 – 249: The sampled ranges for calibration were based on previous experience with SOM models and some trial and error. The defined ranges for calibration could in theory influence the outcome of the calibration procedure if there are local minima in the goal function. However, we found that increasing the ranges shown in Table 3 had no impact on the results.

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-135>, 2020.

Printer-friendly version

Discussion paper

