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Comment on bg-2020-493

Anonymous Referee #2

Referee comment on "Simulating measurable ecosystem carbon and nitrogen dynamics with the mechanistically defined MEMS 2.0 model" by Yao Zhang et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-493-RC2, 2021

This manuscript describes the MEMS 2.0 model, which combines a soil organic matter module based on measurable pools of POM and MAOM with vegetation, soil heat, and soil water components. The model is calibrated and validated using data from grassland sites, with pools of POM and MAOM along with eddy covariance measurements of CO_2 fluxes as the metric for evaluating the model. Overall, the paper is well written. The Introduction especially is an excellent review of current progress and limitations of soil organic matter models and makes a good case for the reasoning behind developing the MEMS 2.0 model.

In my opinion, the weakness of the manuscript is that the only results presented are the evaluation of the model itself. The manuscript makes a case for developing a model with measurable soil pools, but does not provide any new insights about the results of the model or whether the model provides any benefit for ecosystem prediction or process understanding compared to other current ecosystem models. The paper is a well written model description, but is very limited in terms of new scientific insights or new results.

In terms of the structure of the model, MEMS 2.0 is compared to other existing models including both first-order models like CENTURY and microbial-explicit models like MIMICS, CORPSE, and MEND. One key point that I thought was overlooked was the role of microbial biomass in MEMS 2.0. The model does include a microbial biomass pool, but all decomposition reactions are first-order. Thus, in contrast to microbial-explicit decomposition models, MEMS 2.0 does not include any interaction between microbial biomass and depolymerization or decomposition rate. I would suggest acknowledging this explicitly when comparing MEMS 2.0 to microbial-explicit decomposition models to make it clear to readers.

Given that MEMS 2.0 uses a first-order decomposition framework similar to other models like CENTURY, it’s unclear what the major conceptual advance of the soil model is, beyond being able to compare certain model pools more direclty with measurements. Is the model significantly different from a reconfiguration or reparameterization of a CENTURY-like model using POM and MAOM as calibration targets? Would this model produce different projections of SOM dynamics than CENTURY would if they were both calibrated to a similar dataset? The only information that the manuscript really provides is a demonstration that MEMS 2.0 can be calibrated successfully, along with an assertion that the measurable MAOM and POM pools represent an improvement on previous models. But apart from the a priori statement that it is better to have measurable pools, can the actual added value of the MEMS 2.0 model be demonstrated? Some more discussion or demonstration of how the MEMS 2.0 model is expected to advance the biogeochemical modeling field in concrete terms of actual model results or predictions (beyond the relatively abstract concept that measurable pools are inherently better) would help to provide better context for how the model developments represent a scientific advance rather than a purely technical advance.

In terms of the model description: Figure 1 shows the rhizosphere and bulk soil as separate model compartments. However, there is only a full description of bulk soil dynamics (Section 2.1.2). The dynamics of the rhizosphere also need to be described. If the description of the rhizosphere in the litter decomposition section is the full explanation, then perhaps that section should be renamed "Litter layer and rhizosphere" to avoid confusion.

Specific comments:

Line 126: Soil horizons are divided into "thinner layers" but the actual layer thickness is not provided

Line 159: The description should also state here what happens when there is insufficient mineral N for immobilization. In that case, is CUE reduced? Or does decomposition slow down?

Table 2: The notation in this table is different from the notation used in Equations 1 and 2 and the equations in Table S2, which have the same parameters but use different variable names. Using two different names for these parameters makes it difficult to tell which parameter in the actual equation is being referred to.

Line 467: Could the underestimation of POM C in deeper layers be related to rooting depth? If actual rooting depth is deeper than assumed in the model, the model would be underestimating fresh POM production at that depth

Line 470-471: There are multiple previous studies demonstrating that POM decomposition is slower in deep soils, and arguing that this could be due to limited microbial activity: e.g., Hicks Pries et al. 2018, Fontaine et al. 2007. These previous papers have suggested that the lower decomposition rate of POM at depth could be due to lack of fresh organic matter inputs to prime microbial decomposition. This is an area where microbial-explicit decomposition models have been invoked to explain the variation in decomposition rates via priming effects (e.g., Hicks Pries et al., 2018).

Line 485-486: Sulman et al. (2014) used MAOM measurements from the Duke and Oak Ridge National Laboratory elevated CO_2 experiments in model validation. So, this is one example of a model that used soil fraction measurements from more than one site. I do agree that this manuscript uses more fraction measurements from a greater number of sites than previous studies.

Line 495: The FUN-CORPSE model did use both C and N measurements for validation, including N mineralization rates and soil %N.

Line 532: The units should be provided for the two parameter values

Line 540-541: The reduction in sensitivity of decomposition to temperature at high temperature does seem to be justified. However, since the soil temperatures in these simulations did not exceed 20-25C (Fig. S2), it does not seem like the model calibration would have provided much constraint of that high temperature area of the response curve. The response only really flattens at temperatures over 25-30 C.

Line 572: “week boding” - should this be “weak binding”? Or “weak bonding”?

References:

Fontaine, S., Barot, S., Barre, P., Bdioui, N., Mary, B., & Rumpel, C. (2007). Stability of organic carbon in deep soil layers controlled by fresh carbon supply. Nature, 450(7167), 277-U10. http://www.nature.com/nature/journal/v450/n7167/abs/nature06275.html

Hicks Pries, C. E., Sulman, B. N., West, C., O’Neill, C., Poppleton, E., Porras, R. C., et al. (2018). Root litter decomposition slows with soil depth. Soil Biology and Biochemistry, 125, 103–114. https://doi.org/10.1016/j.soilbio.2018.07.002