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Reply on RC2

Yao Zhang et al.

Author 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-AC2, 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 CO2 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.

Response: *We appreciate the reviewer's opinion but do feel that the scientific insights and new results provided in the paper are important and worthy of publication. MEMS 2.0 is different from other models in its design of flows and controls which is critical for a SOM model. We incorporated several recent understandings of the decomposition and protection processes (such as the MEMS theory, point of entry, saturation, in vivo/ex vivo pathway), which make the model unique. Additionally, MEMS 2.0 is a full ecosystem model with SOM simulated to deeper depths while some of the other new generation models only simulate one layer of soil and do not have a full plant module or water module (more practical to be used as a tool for scientific inquiry and real world decision making). In this study, we also present new and original measured and modeled SOM fraction data from several sites with deep depths.*

We also think a model description paper without a comparison to other models (using the same data to calibrate and validate different models) is highly valuable for publication. Numerous model description papers have been published and many are highly cited (e.g., Davidson et al., 2012; Parton et al., 1998; Robertson et al., 2019; Wang et al., 2013)*. Please see our response to the other reviewer regarding our plan of a full proper model comparison and a preliminary comparison with the DayCent model (Figure 1 in the*

supplementary of this response). The response to reviewer one is copied here. "We used a version of DayCent previously calibrated with grassland sites. Since DayCent only represents the top 20cm soil depth, the SOC observations of the six NEON grassland sites and MEMS 2.0 results were summarized to the top 20 cm. These preliminary results show that the MEMS model performs well on all six sites, while the DayCent model significantly under-estimates SOC on the OAES site. However, since DayCent was not calibrated with the same data set, we do not consider these data robust enough to be included in this paper, and will conduct a proper comparison at a later stage. Because DayCent only simulates the total SOC in the top 20 cm soil, more sites are needed to provide a good number of observations for model calibration and validation."

We will edit the text to make sure that the new scientific insights and results are more clearly presented and emphasized.

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.

Response: *The referee is right in that the microbial biomass does not have a direct effect on decomposition rate in MEMS 2.0. However, different from CENTURY model, the C/N ratio of the microbial pool in MEMS 2.0 is a modifier to the decomposition rate (the* $MicCN_{eff}$ *in Table S2 equations). We made a version of the stand-alone litter decomposition model that used microbial biomass as a decomposition rate modifier, as in the CORPSE model. However, statistically that version did not provide better fit to the Soong et al. 2015 data than the current structure in MEMS 2.0. The method of explicitly using microbial biomass has its advantage of quick response to carbon input (priming effect) compared with the first order method, but the first order equation is more stable in longer-term simulations. Luo et al.,* (2016) *has stated that the microbe-explicit models have "oscillatory responses to perturbation and insensitivity of soil C storage to C input, in comparison with classic models"; "such patterns may exist at the microbial reaction sites but have not been observed in litter decomposition and soil incubation studies.". The reasons are that first, the microbial biomass is very hard to predict (most models only use a constant death rate); second, there is a community of microbes that are functioning very differently (treating them as one uniform species is not correct); third, it is the amount of active enzyme rather than microbial biomass that is correlated with decomposition rate (using microbial biomass to approximate enzyme amount is not accurate)* (e.g., Li et al., 2018)*. A sound microbial-explicit model should model both the microbial community and enzyme activity but it will make the model very complex and hard for large scale applications due to the limitation of data availability. After weighing the different factors, we decided to use the stable first-order approach to make a parsimonious model structure while introducing new concepts (microbial C/N control on decomposition and dynamic CUE) to improve it from the traditional first-order models. We will add text to make it clear to the readers that MEMS 2.0 is different from the microbial-explicit model like CORPSE and also different from CENTURY.*

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.

Response: *We agree with the reviewer that a CENTURY-like model can be calibrated using measurable fractions of SOM. In fact, at least two studies have done that* (Luo et al., 2014; Skjemstad et al., 2004)*. However, a SOM model is not simply about the decomposition equation (no matter first-order or microbial explicit). The design of the flows and the controls are critical. In MEMS 2.0, we aim to model the real processes (as best as we can/understand them) instead of merely predicting variables with a black box. For example, the model represents two pathways of SOM formation, maintaining separate flows for soluble and structural inputs which is very different than the CENTURY model. Representing new understanding and theories helps the advance in biogeochemical modelling. In our opinion, MEMS 2.0 advances biogeochemical modelling also by allowing us to test new hypotheses. For example, MEMS 2.0 can simulate the impacts of litter quality on rate of MAOM saturation in a subsoil horizon with distinctly different soil texture than topsoil.*

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.

Response: *The reviewer is correct; the description of the rhizosphere is included in the litter decomposition section. We will modify the section title to avoid confusion as the reviewer suggested.*

Specific comments:

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

Response*: The other reviewer raised a similar question. As reported above, the user-defined horizons need to be multiples of 5 cm for the top 50 cm, and multiples of 10cm if lower than for below 50 cm. The model has fixed depths for layers: 0 - 2cm, 2 - 5cm, then 5 cm increments for 5 – 50 cm, and 10 cm increments for layers below 50 cm. For example, if a horizon is 20-35 cm, it will be divided into three layers with 5 cm per layer. We will modify the text to more clearly describe the soil horizons and rhizosphere structure.*

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?

Response*: When there is insufficient mineral N for immobilization, both CUE and C/N of the microbial biomass reduces. The reduced microbial C/N leads to a reduced decomposition rate. We will add this to L159.*

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.

Response*: We thank the reviewer for noticing this issue, and are sorry we had not noticed it before. We will make the changes accordingly. The parameter names in Table 2 were the names used in the input files thus different from the notation in the equations.*

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

Response*: We agree it is possible, and we will add this interpretation. Rooting depth is very uncertain according to the measurements. Studies have reported very different values at the same sites (methods were different). We used our best estimation based on NEON root biomass measurements and other studies conducted in the same areas.*

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).

Response*: We agree that the typical microbial-explicit models have the advantage of reproducing the trend of decomposition in some priming effect experiments. However, it has weaknesses as described above and in Luo et al. (2016). Studies showed the quality of substrate and N availability can drive priming effect; however, the mechanism of priming effect are still not clear* (Li et al., 2018)*, and studies found no robust correlation between soil microbial biomass and priming* (Liu et al., 2017)*. We will keep following advancement in the understating of the priming effect, and will modify the model to represent it, when the scientific community has reached a better mechanistic understanding of it.*

Line 485-486: Sulman et al. (2014) used MAOM measurements from the Duke and Oak Ridge National Laboratory elevated CO2 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.

Response*: Sorry for this oversight. We appreciate this comment, and we will add this reference.*

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

***Response**: We will modify the text with this reference.*

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

***Response**: We will add the units.*

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.

***Response**: We agree that the sites used in this study do not have soil temperature above 25 C. However, the summer soil temperature exceeded 20 C. The temperature curve started to flatten right after 20 C. We will simulate tropical and subtropical sites to better characterize the curve at high temperature in future studies.*

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

***Response**: Thank you for catching this. It should be "weak binding".*

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Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-493/bg-2020-493-AC2-supplement.pdf