

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2020-493-RC1>, 2021
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Comment on bg-2020-493

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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-RC1>, 2021

The authors present a new full ecosystem model MEMS2.0 by extending a previous soil biogeochemical model MEMS1.0, which was constructed based on measurable carbon pools. This extension includes model components of aboveground and rhizosphere processes, vertical transport /mixing in the soil column and N cycling. The model has the capability of including litter chemistry, dynamic CUE and CN ratios, as well as MAOM saturations. They first validated a subcomponent, the litter decomposition model using year-long laboratory incubation experiments and further validated the full model with long-term field observation data. Adding the fire frequency impact is interesting, but it remains unclear how it was done.

Overall, I appreciate the effort of extending a novel modeling concept using measurable pools. I think this manuscript would significantly benefit from more work that better structure its discussion, clarify its message with an emphasis on "novel" model capabilities it's providing.

Some major technical notes:

1. The model description section needs further clarification (section 2.1).

It is not clear to me how the continuous soil horizons are divided into layers (line 124-126), and whether the rhizosphere and bulk soil separation has been done for all horizons/layers. How deep the conceptual rhizosphere goes? Since the authors mentioned the differences between subsoils (> 30 cm deep) and topsoils, it would be useful to make clear separations in the model description.

The CN coupling part needs some clarification as well. Inorganic N (nitrate or ammonium or both?) will be assimilated by microbes if the substrate N could not meet microbial N demand. Is this modeled implicitly? I have looked through table S2 and S3 and wondering if that's represented through MicCNeff.

2. Description of observational datasets could be more balanced and condensed.

The leaf litter incubation experiments were used to validate the litter decomposition model, but there's no specific description of the CN ratios in the method or result section. Specifically Figure 3 and line 350, what are the CN ratios or the range of CN ratios in the experimental data?

The description of soil OM fractionation for modeling could be condensed (line 257-281). Currently the mix of data description (DOM, POM and MAOM) and detailed experimental approaches make it difficult to find key information. On a side note, I also find it is inconsistent with OM pool descriptions: here the author mentioned MEMS2.0 represents three bulk soil OM fractions (line 262), but in the model description there were five (line 181), and in Figure 1 bulk soil box, do you consider three or four OM pools? Additionally, it would be useful to explain how eMAOM and sMAOM are determined/parameterized.

3. The discussion section would benefit from more structured statements

Currently the section is a mixed discussion of simulation results, model performance, and model formulation. There's no clear trail describing the key features of this new model and how well each new component behaved. Alternatively, I suggest using a few subtitles to describe the key model features, within each subsection, maybe add a short summary of the model formulation, followed by model performance and sensitivity discussion, then model limitations.

Additionally, I do not find the model performance comparison very compelling (e.g. paragraph starting from line 479). Just like what the authors mentioned in the former paragraph, model input data have a huge impact on the final predictions. Vegetation, NPP, climate, soil texture, historical temperature and moisture, there are many factors that contribute to model uncertainties. Since all other models mentioned in the paragraph were validated using different data, there's no basis for accuracy comparison. Alternatively, maybe add an additional simulation with a selected dataset that one of these models used, so the climate and vegetation data driving SOM dynamics are associated with the same level of uncertainties.

A minor note: There's no space after each paragraph or each reference, which significantly impacted the readability of this manuscript. All equations were summarized in a table that is tightly fitted with a small front size, consider changing the format to increase

readability. Maybe add proper subtitles, like MAOM dynamics, Microbial dynamics, environmental variable etc.

I would also suggest releasing the model code for review.

Some minor comments:

line 40: The model comparison is a bit misleading. Many models included are microbial explicit models that are designed to capture the biological control on SOM dynamics. The modeling goals are different, and we do not necessarily need all the models to address all of the needs (line 48). In particular, MEMS 2.0 is not a microbial explicit model.

line 59: SOM pool partitioning is only one of the reasons for large model disagreement, see Sulman et al. <https://link.springer.com/article/10.1007/s10533-018-0509-z>

line 75: There're a few other CN coupled microbial explicit models besides MIMICS-CN, such as MEND-CN (<https://www.sciencedirect.com/science/article/pii/S0022169420302377>), ORCHIMIC (<https://gmd.copernicus.org/articles/11/2111/2018/>), BAMS2 (<https://link.springer.com/article/10.1007/s10533-019-00580-7>)

line 96-100: Sokol et al's GCB concept paper was mentioned several times through the manuscript, it would be good to discuss if this model can be used to validate this conceptual model at the ecosystem scale

Line 165: consider add N flow in figure 1

Line 308: consider presenting the fitted saturation function, because it was discussed as a key point --MAOM saturation

Line 360: consider adding a second y-axis to show the two data series more clearly

Line 390: explain " diagnostic soil horizon"

Line 456: need some justification, like adding MAOM saturation limit

Line 484: "serval" should be "several"

Line 605: dynamic pH and redox, microbial community structure etc sounds a little far away in terms of current model structure and focus