Biogeosciences Discussions

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Reply on RC1

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-AC1, 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.

Response*: We greatly appreciated the reviewer's comments. We will modify the discussion to make the message clearer as the reviewer suggested. The fire impact is included because we represent grasslands which are impacted by fire. However, a full and detailed representation of fire impacts is beyond the scope of this study. Thus, in this version we adapted the fire representation from the DayCent model. During a fire event, plant biomass and litter are removed and a fraction of the biomass/litter C and N is returned to the soil as unhydrolysable C and mineral N. Since fire frequency determines the amount of C and N loss in fire, it impacts the amount of C and N input to soil. Our experience with DayCent modeling showed significant impact of fire frequency on grassland soil C and N. We acknowledge in the text that this is a simplistic representation of fire impacts, but we will provide more details to clarify how we represented it.*

Some major technical notes:

- 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.

***Response**: The user-defined horizons are multiples of 5 cm for the top 50 cm, and multiples of 10cm 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. The conceptual rhizosphere goes as deep as the root system so it is in every soil layer where roots go as deep. In the model, there is no separation between the topsoil and subsoil. We will modify the text to more clearly describe the soil horizons and rhizosphere structure.*

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.

***Response**: Both nitrate and ammonium are assimilated by microbes and there is no preferential uptake. The uptake of inorganic N is modeled explicitly. The model first estimates a CUE based on the availability of N in the organic substrate and in the inorganic N pool in the soil (Equation S18). Then it calculates the potential demand for inorganic N based on the amount of carbon assimilated and the minimum C/N ratio of microbes. The actual uptake is a result of competition with the plants (hourly time step calculation based on their demand). We describe this in the text (Lines 156 - 160), but we will revise the text to be clearer and will add the equation to estimate the potential demand of inorganic N to Table S2.*

- 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?

***Response**: The CN ratio data were published in Soong et al. (2015). The CN ratios are 10.8, 36.1, 52.8, 92.3, and 126.6 for alfalfa, oak, ash, bluestem, and pine, respectively. We will add these data to the Method section.*

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.

***Response**: We are sorry for this confusion. We will condense the description of soil OM fractionation, to make it clearer that the model includes five OM pools which comprise the bulk soil. However, our lab measurement did not separate sMAOM and eMAOM. As we discuss in the text, "one hurdle to producing these data is reaching scientific consensus around a reliable method for measuring exchangeable MAOM. We plan to verify and re-parameterize the partitioning of MAOM between exchangeable and stable pools in the future as these data become available." (Line 577 – 579 in the Discussion). The parameterization of the fraction of eMAOM and sMAOM is achieved through the Bayesian optimization, which we describe in the Discussion (Line 573 – 575): "A parameter (Frac_MAOMExchangeable) was used to define this fraction when both eMAOM and sMAOM were saturated. The value of this parameter was selected in our Bayesian optimization* *with a wide prior range because there is currently little data on which to base this partitioning." We now realize that we should have presented this earlier, and in more detail. We will add text to explain this in the Method section for clarity.*

- 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 compiling (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.

Response: *We appreciate the reviewer's suggestions to improve the structure of the Discussion. We will revise the discussion section as the reviewer suggested, adding sub-headings, and focusing on the new model key features. We agree that a robust model comparison requires that all models simulate the same sites. However, we feel a formal model intercomparison is beyond the scope of this initial model description and demonstration paper, and we are planning to perform these comparisons in subsequent analyses/publications. Since reviewer two is also interested in a model intercomparison, we run and present here a simple preliminary comparison with DayCent (see figure in the Supplement of this response). 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.*

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.

Response*: We are sorry about that. We will edit the text for improved readability by adding space between paragraphs and changing the format.*

I would also suggest releasing the model code for review.

Response*: Currently, we are filing for an invention disclosure, and after that we'll make the code available.*

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.

Response: *We agree that MEMS 2.0 is different from the typical microbial-explicit models. It uses first-order kinetics but the decomposition rate is affected by the C/N ratio of the microbial pool. MEMS 2.0 also uses dynamic CUE, which makes it different from the traditional SOC models. These features (CUE and C/N specifically) of the discrete microbial pools in MEMS 2.0 provide feedbacks to the fluxes and decomposition between and from other model pools. Although different from existing microbially-explicit models, we feel these feedbacks make MEMS 2.0 microbially-explicit in some ways. Additionally, as described in the paper, we developed and tested a stand-alone litter decomposition model that used microbial biomass as a decomposition rate modifier (as used in the CORPSE model). However, statistically that version did not provide better overall fit to the Soong et al. 2015 data than the current structure in MEMS 2.0. This all said, we take your point that we may not need to point out a comparison of distinctly different model structures because as you rightly state, we do not need all models to address all needs.*

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

Response: We agree – there are many reasons why bulk SOM measurements may disagree with model predictions. It was not our intention to say that partitioning was the only, or even major driver, but can be an important determinant in understanding where a model is misrepresenting the mechanisms/fluxes. We will add a sentence to this part of the text to emphasise this point, including adding the Sulman et al reference.

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)

Response*: We are sorry we missed them and will add these models to our Table S1.*

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

Response*: This is a very good suggestion. However, it is hard to address this without conducting an ecosystem scale modeling study. We could add this in our future work.*

Line 165: consider add N flow in figure 1

Response*: We are not sure what the referee is asking here. The N flow is in the figure and we feel extra arrows to call out C and N separately would make the figure too 'busy'. All the organic matter pools have N in them. Mineral N leaching, uptake by plant, immobilization and mineralization are in the figure.*

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

Response*: We will add a figure in the Supplementary Material to show the fitted* *function.*

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

***Response**: The suggestion of adding a second y-axis is good. However, we feel it could make the figure busy and affect the readability and would prefer to keep the figure as it is.*

Line 390: explain " diagnostic soil horizon"

***Response**: A "diagnostic" horizon is a horizon used to define a soil taxonomic unit. However, we see now that it is jargon, and we will delete "diagnostic".*

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

***Response**: We will add the saturation limit*

Line 484: "serval" should be "several"

***Response**: It will be corrected.*

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

***Response**: We intend to point to the limitation of the current model, as well as its potentials. We consider these limitations of the current version, and plan to address them in future developments. We actually already have projects in the pipeline to do this work, so we do not see it as far away as the referee thinks. Therefore, we would like to keep this section. However, we can certainly omit this part of the discussion if preferred.*

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-493/bg-2020-493-AC1-supplement.pdf