

Interactive comment on “Unifying soil organic matter formation and persistence frameworks: the MEMS model” by Andy D. Robertson et al.

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Received and published: 6 November 2018

The study of Robertson et al. present a first version of the MEMS model, a parsimonious dynamical model of soil organic carbon (SOC) development at ecosystem scale, together with a validation across a many sites. I enjoyed reading the manuscript, which is well structured and succeeds in getting the fundamental ideas across in a concise way and provides the details in the appendix.

The proposed model is of similar complexity as classical pool-based models but better incorporates recent mechanistic understanding and is better comparable to measurable pools. Hence, it is of great interest to soil science, ecosystem research, and potentially also global change communities. It adds a complementary alternative in the

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suite of simple to much more detailed SOC models and the study should be published after revisions.

I liked the approach of directly modeling relevant quantities at the scale model purpose, the management scale. I liked the simulation time dependent sensitivity analysis, although Fig. 2 is hard to read.

The supplementary is complicated by already anticipating several mineral soil layers and sometimes is inconsistent with the main text. For example, there is explicit microbial assimilation in mineral layers in the supplementary, but the main text states that microbes are implicit there. Please, provide a version that matches the main text and the presented model structure.

The wording of “litter layer” and “mineral soil” are used in a fuzzy way. Also it did not became clear to me, how model-data comparison dealt with organic layers, which are neither part of the litter (in the model lacking particulate organic matter (POM) pools) nor the mineral soil (in the model simulating sorption to minerals). Maybe this partly causes the large model-data discrepancies for broadleaved sites with large POM stocks.

I have several more detailed comments or questions that are intended to help with clarification and setting the model into context.

Detailed comments for model structure:

Could you, please, elaborate a bit more why you (as well as the LIDEL model) choose microbes to not consume DOM?

In the LIDEL model there is a C5 microbial products pool also in the litter layer, why do you assume in MEMS that all microbial turnover is transferred to the mineral soil?

You choose decomposition to be independent of the size of biomass pool to avoid some problematic feedback. Then I suggest to simplify the model even more by replacing microbial biomass turnover by the sum of inputs to the biomass pool. Then you do not

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need to simulate this pool, save one state variable and several model parameters. If microbial biomass is required for data comparison, you can still compute it assuming near steady state with inputs (e.g. Wutzler 2013).

Detailed comments for model-data integration:

It should be clarified better also in the discussion, that the model performance was judged by comparing steady state MAOM pools to observations. I am still looking forward to a comparison where a model successfully simulated dynamics compared to observed changes decadal stock changes across many sites.

L376: averaging parameters is dangerous, because of nonlinearities. I suggest to use only one non-averaged parameter combination. You may pick the fold randomly.

L376: You can avoid the choice of one criterion among three data streams of MAOM-C, POM-C and bulk SOC by using a cost function based on the sum of squared residuals of all the data streams.

Fig 5: The classification to land use not particularly helpful, because variables are very similar with a high range across these classes, including the mentioned significant different of MAOM:POM (L 485). Furthermore, plotting the distribution of observations and distribution of predictions separately does not help to judge model performance (L488).

I suggest instead inspecting and plotting the distribution of model-data residuals of several variables and relating these differences to classes and other environmental conditions. This would indicate which variables and processes are most urgent to extend MEMS v1, as done with the discussing Fig 7.

Other detailed comments main text

Text in Figs 2 and 6 are hard to read. Can you provide a vector graphics of this figure? There are too many classes to distinguish by color, but I have no suggestion how to improve.

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Fig 4: Suddenly, pH is popping up, but was never introduced as a driving variable. I suggest to shortly state that sorption rate is pH dependent, and refer to the eq. 35 in the Appendix.

L 529: These are interesting effect of N in a C-only based model. While the microbially detailed models of Perveen 2014 and Wutzler 2017 attribute low litter N effects to N mining in older usually N-rich pool and accumulation of less processed material, MEMS attributes this to reduced microbial accessibility and reduced DOM production. Do you think that chemical and stoichiometric effects are two sides of the same coin, or are these competing hypotheses? I am looking forward to the version that explicitly simulates N fluxes.

Other detailed comments appendix

To me its difficult to always keep a list of meaning of pool 1 to 10 in my head. Could you come up with more expressive pool names?

L 70: I do not find more information on u_b and u_k in Table 2 in the main text. I suggest referring to eqs. 19-22.

L 110-112: The long sentence did not become clear to me. Is $L_{j_C5_C4_gen}$ really a combined flux of bioturbation, ..., and DOC leaching? I thought the latter one is covered by eq. 33.

eq 47: k_8 does not match the text before that states k_5 .

Thanks for this work. I suspect MEMS to be included in further model comparisons as a complementary model.

References

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Interactive comment on *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2018-430>, 2018.