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

Anonymous Referee #2

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Overall Review

The authors present a new soil biogeochemistry model, MEMS v1.0, that explicitly represents biochemical complexity of litter pools, microbial biomass, mineral associated organic matter and particulate organic matter. The model has the capability of including variable CUE in litter decomposition and mechanisms leading to SOM stabilization and saturation of mineral associated carbon fraction. Four key model parameters are calibrated to reproduce soil fractionation observations of mineral associated and particulate organic matter fractions and the model is evaluated in reproducing topsoil SOC in more than 8000 sites across different land-uses in Europe with satisfactorily results.

Constructing models that are based on measurable carbon pools rather than on the old framework assigning turnover rates to a given number of unmeasurable carbon pools is

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a very important endeavor and the authors are definitely moving beyond conventional SOC modeling. It is especially important to have models that link litter decomposition processes and SOM formation processes, which is rarely the case, as stated by the authors (L 89-91). I am very much in favor of such a type of approach and supportive of the author's effort. The manuscript is very well written and clearly presented and the introduction frames very well the problem.

I would be happy to have a few clarifications on some technical aspects and about one important assumption related to the role of the microbial pool. These are written in a number of minor comments that hopefully can be addressed.

I would also invite the authors to tone down the role of MEMS v1.0 as "ecosystem model", since the current version is still far from being there. As a matter of fact, in several instances (e.g., Line 606) the authors state that the model is incomplete (e.g., lack of hydrological and nutrient cycle) and that these deficiencies will be addressed in future model developments. The model represents SOM dynamics at the "ecosystem-scale". However, for various reasons but especially because the temporal dynamics are not evaluated in this article, I would invite to use cautious statements in the link with ecosystem models. Only the steady-state conditions are tested. A correct representation of temporal dynamics is key for coupling with other models. At this stage, this is a quite significant limitation for application in ecosystem models. Furthermore, feedbacks between soil and vegetation cannot be considered.

Other simplifications are that NPP is prescribed from MODIS, the model does not account for temporal dynamics of soil moisture or for nutrient cycles, the root:shoot ratio is prescribed for various biomes. However, these are overall clearly described. I would also appreciate some additional discussion about the issue in comparing pools, which are spun up at the equilibrium with observed pools (Line 366-367). The authors are aware of the issue and they briefly discussed it. However, most of the description of the results and the calibration effort convey somehow the intention to match C-pools as closely as possible. Given the expected difference between actual SOC and "steady-state" SOC, I would have allowed more freedom to the model and focus on comparing patterns as in Fig. 5 and 6 rather than absolute quantities.

Despite these limitations, the manuscript is undoubtedly a novel contribution to the field and surely a step in the right direction.

Minor Comments

Line 75. It is cited later on, however, Wieder et al 2015 would fit well also here.

Line 96. Maybe one sentence with additional explanations for K vs r strategies (e.g., copiotrophic and oligotrophic microbial functional groups) is necessary, not all the "modelers" may be aware of these concepts.

Line 113-114. The issue related to the lack of inputs or information to derive model parameters and validate model responses, of course, is a very important one and may compromise practicality as written by the authors. However, modeling efforts in the direction of more mechanistic representations of the soil system can shed light on the importance of processes and interactions that were not accounted or quantified before, they may provide guesses for the magnitude of certain pools/fluxes and may motivate the collection of those data that are necessary to testmechanistic predictions. In other words, they can have a value in process explanation rather than a predictive value.

Line 174-178. In a certain way, also the CENTURY model, especially in more updated versions (e.g., Kirschbaum and Paul, 2002) accounts for nitrogen and lignin content of the litter, which are affecting the turnover rates of the various litter pools. Additionally, their subdivision in metabolic and structural litter pools is not far from the subdivision in the pools C1, C2, C3. This may be acknowledged in the manuscript or if major differences, which I cannot recognize, do exist, they need to be remarked.

Line 189-190. The assumption of considering a microbial pool (C4) for the litter component is probably the decision in terms of model construction, which leaves me more bewildered. This pool, presumably, is mostly located aboveground, even though is not stated explicitly, and does not have an explicit role in the turnover of soil organic matter. Now, if anything, I would have make the reverse choice. Because of accessibility constraints and relatively paucity of microbial biomass in the soil, the decomposition of SOM is likely controlled explicitly by microbial biomass, while the decomposition of litter, which is mostly located aboveground (especially for land covers different from grassland) and air exposed is unlikely limited by microbial biomass. Maybe, my understanding of the system is wrong, but it would be useful to have a clarification of the rationale of such an assumption and eventually of the potential consequences.

Line 200. Please explain better what do you mean "represents microbial metabolism processes implicitly"

Line 268-269. It could also be, simply, that microbial growth is stimulated and there are more microbes that can also degrade faster the chemically recalcitrant substrates. If I understood correctly, this is not an effect that can be captured by the model without an explicitly microbial pool acting on POM (C5, C10) and MAOM (C8) decomposition.

Line 270-273. Generally speaking, microbial respiration will be related to microbial activity and CUE. Being not considered microbial activity in the soil, it is not very clear without looking in detail at the Supp. Material how respiration is computed and which fraction of the decomposition is assumed to be. While you refer to CO2 efflux, "respiration" is never mentioned in the Supplementary Material, which is quite surprising.

Line 281. I would also add that pH controls are quite important. The authors are already well aware of this but neglecting soil moisture controls is a quite significant simplification.

Line 293. At this stage is not clear how NPP values are derived. Maybe, it is worth to state that this must be an external input to the model. This is actually what mostly separate a "soil organic matter model" from an "ecosystem model".

Table 3. The text-box with "site-specific values required" applies to all the site condition

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variables (e.g., from NPP to soil temperature). This is not clear from the current Table where site-specific values seem to refer to "rock fraction of soil layer" only. I would suggest to use some curly bracket to envelope all these variables.

Line 315-319. I am actually quite familiar with the global sensitivity analysis and I think I understood what the authors did. However, I am quite sure that the succinct explanation provided in these lines will remain unclear to most of the readers. I would suggest to either explaining it better (i.e., more extensively) or minimizing the explanation with a full discussion in the supplementary material.

Line 340. I know that this is probably the only option the authors had, but I hope they are well aware of the limitations of MODIS NPP product; maybe a sentence forewarning the reader would be necessary.

Line 345. The reference Cotrufo et al 2018 explaining the derivation of the POM and MAOM pools is not published. I guess for the sake of this article is fine, but of course, it would be a great contribution to the community if the values of POM and MAOM for the 154 sites would be provided as a part of the LUCAS database or somewhere as part of the article.

Line 368-369. This is probably more a philosophical than a practical point. However, I wonder if a rigorous numerical optimization for such type of models, where the model structure is very uncertain and difference between observed and simulated SOC could be related more to the initialization problem rather than to model structure or parameters is really needed. Given the fact that 4 parameters only were optimized and several replicates were made, this is probably an added value and unlikely a problem here, but still I wonder if is not giving too much weight to the data. How do the results look alike without optimization? This is briefly stated in Line 469-470 but it would actually be interesting to look at it in more detail.

Line 379. Maybe an explicit statement that optimized parameter values are reported in Table S2 would be useful here.

Line 386-387. How seasonal variability in C-inputs and temperature is accounted for? This is not very clear from the manuscript.

Line 407. The value for NPP and sand content differ from the mean value provided in Table 3.

Figure 2. What is the initial condition for the simulation of 1000 years depicted in Figure 2? Do you start from nearly steady state carbon pools or from carbon pools equal to "zero"?

Line 455. Why colder temperatures favor POM? Is this related to the sensitivity of decomposition?

Line 473-475. Table 2. Maybe I am missing something obvious but the units of decay parameters as "k1" to "k10" should be [gC gC-1 day-1], otherwise when multiplied by the pool (Eq. 1-11 in the supplementary material) you will get [gCˆ2 day-1] rather than [gC day-1].

Line 491. This is definitely expected given that variability in litter input, e.g., litter composition and stoichiometry root: shoot ratios are underestimated and soil moisture is not accounted for.

Line 496-497. For almost all of the analyzed sub-groups in terms of site-conditions of Figure 6, bulk SOC observations are mostly between 50-75 MgC/ha. I think this relatively narrow range complicates the identification of the control exerted by temperature, precipitation, soil texture or biomes and therefore also the model testing. A more reasonable test will require more distinguished values of SOC across different conditions, probably using other biomes and climates.

Line 521. I don't want to sound too pessimistic and overall I really like the approach of the authors but bridging the gap toward Ecosystem and Earth System Models still requires a considerable amount of work to test the reliability of temporal dynamics and plant-soil feedbacks. This should be stated in the manuscript.

Line 552. Also the dynamics of microbial pool in the soil is not explicitly simulated; however, the underestimation of variability is most likely due to underestimation of variability in the inputs and the steady-state assumption in the model, as you wrote in the next few lines.

Line 558-559. I am not sure why soil moisture controls should be so important at high-latitude, these sites are rarely water limited, I would expect lack of soil-moisture controls to be more important in South-Europe.

Line 621-622. This is a great point, and I am looking forward for further work of the authors along this line.

Figure 1. Just as a suggestion, up to the authors, it would be nice to have some of the parameters of Table 2 represented also in this plot to link the main fluxes to some of the key parameters regulating the flux.

References

Kirschbaum, M. U. F., and K. I. Paul (2002), Modelling C and N dynamics in forest soils with a modified version of the CENTURY model, Soil Biology & Biochemistry, 34, 341-354

Wieder, W. R., Allison, S. D., Davidson, et al, (2015). Explicitly representing soil microbial processes in Earth system models. Global Biogeochemical Cycles, 29(10), 1782-1800

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