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Cover Letter and Responses to Reviewer Comments to accompany the manuscript:
"Unifying soil organic matter formation and persistence frameworks: the MEMS model"

Authors: Andy Robertson, Keith Paustian, Stephen Ogle, Matthew Wallenstein, Emanuele Lugato, and Francesca Cotrufo

Thank you for your correspondence concerning our manuscript and for giving us the opportunity to resubmit a revised version. All comments from the reviewers have been carefully considered and appropriate responses are made below.

Sincerely,

Andy Robertson

Responses to comments from Anonymous Referee #2 on "Unifying soil organic matter formation and persistence frameworks: the MEMS model" by Andy D. Robertson *et al*.

Reviewer comments in bold and our responses in normal text. Selected new text in the revised manuscript is pasted here in italics. Reference to the manuscript is given as line number (L).

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 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.

Many thanks for your constructive comments and praise. We have responded to each of your comments in detail below and hope to have satisfactorily addressed any concerns or queries you may have had. Regarding your points about the microbial pool please see our detailed response on those comments below. It is our hope that this publication and the resulting MEMS model can help to both stimulate a fruitful discussion and advance the practice of SOC modelling.

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.

Thank you for your point. We are fully aware of the limitations of this first version of our model and readily acknowledge that it is not an ecosystem model yet. It was never our intention to 'oversell' the model's capability but to rather highlight the possibilities for integration with other ecosystem model components (e.g., plant growth, hydrology, etc.) given the more realistic model structure. You are certainly correct that being able to simulate non-steady-state dynamics will be the true test of our model and to date it is more of a working proof of concept model than one to directly compare with conventional SOM models.

Throughout the revised manuscript we have tried to play down links or comparisons with true ecosystem models. However, we do maintain that the model is designed to operate the ecosystem *scale*. We have also added a few points to highlight the limitations of our steady-state comparative approach.

L318-320
These driving variables are external inputs of the initial model version but may be obtained from coupled climate and plant growth submodels in future versions, when incorporated into a full ecosystem model.

L575-582
MEMS v1.0 was designed to consolidate recent advances in our understanding of SOM formation and persistence into a parsimonious mathematical model that uses a generalizable structure which, after further development, can be implemented in Ecosystem and Earth System model applications

L665-667
In its current capacity, MEMS v1.0 is far from being able to simulate full ecosystems and is limited in scope regarding the land use scenarios it can simulate accurately.

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.

The focus of comparing patterns rather than absolutes was indeed our initial end goal, however after we ran the model and saw relatively good agreement with absolutes as well we felt it important to report these results. We agree that there are many reasons why our simulated SOC stocks would not match those measured but our choice to only look at grasslands and forests was a way to examine those sites that may be in, or close to, equilibrium. Your point is a good one though and we have tried to adjust some of our language in the discussion to focus more on comparisons with general patterns than on exact numbers. Several qualifying statements have been included when we do compare with absolutes.

L452-454
In addition to comparing measured values with those predicted at steady-state (which may not be an accurate assumption for many sites), a more general comparison was performed to examine groups of sites under similar site conditions.

L565-569
While the model's performance comparing absolute C stocks appears good, this is done with the assumption that these topsoil C stocks at forest and grassland sites in our analysis are at steady-state. This is unlikely to be true and therefore it is encouraging when general trends are as expected (as is the case for many of the land uses and for many of the different environmental divisions; Figure 6).

L606-608
There are also limitations of our approach given that very few of the sites will likely be under true steady-state conditions, leading to further discrepancies between model predictions and measured values.

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

Many thanks for your comments and time spent reviewing our manuscript. We certainly appreciate the opportunity to add the MEMS model to those currently driving progress in the field of SOM modelling.

Minor comments

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

We have now added this.

L80-82
Consequently, there have been several calls to represent this new understanding and re-examine how microbial activity is simulated in SOM models (Schmidt et al., 2011; Moorhead et al., 2014; Campbell and Paustian, 2015; Wieder et al., 2015).

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.

Thank you for the suggestion – we have now added this extra detail.

L103
A recent paradigm has emerged that emphasizes the role of microbial life strategies (e.g., K vs r, referring to copiotrophic and oligotrophic microbial functional groups) and carbon use efficiency (CUE) in the formation of SOM from plant inputs (Dorodnikov et al., 2009; Cotrufo et al., 2013; Lehmann and Kleber, 2015; Kallenbach et al., 2016).

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 test mechanistic predictions. In other words, they can have a value in process explanation rather than a predictive value.

A good point, well raised. We have added this to the introduction help bolster the points we made. Thank you.

See L60-65:
Structuring a SOM model around these known and quantifiable biogeochemical pools and processes has the potential to drastically reduce uncertainty by enhancing opportunities for parameterization and validation of models with empirical data. Furthermore, mechanistic models can have value in process explanation as well their value in predictive capabilities; such models can pinpoint the processes that have the greatest influence on a system even when they are not traditionally determined empirically.

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.

We feel that the MEMS interpretation of these divisions is different to those in CENTURY, but we do acknowledge the similarities. However, these alterations may not qualify as 'major differences' but rather different formulations of the same general ideas. For example, at this early stage the litter chemistry and N content of the inputs are fixed and therefore similar to the lignin:N effects in CENTURY, however when we include a discrete N submodel, N-availability will be dynamic and influence those processes differently through time.

With this first description of MEMS we do not mean to suggest that it is better or worse to any of the more conventional models (including CENTURY) but rather that it presents another way of addressing the same questions about SOM dynamics. In some respects, MEMS is very similar to other models, and in other respects it is quite different. A full model-vs-model comparison was obviously beyond the scope of this manuscript. Therefore, to avoid direct comparisons between the conventional SOM models and MEMS, we deliberately did not discuss specifics about how they differ. To hopefully address this comment, we have added a single sentence to help clarify our position.

See L186-188
This structure is similar to the LIDEL model (Campbell et al., 2016) and follows the hypotheses that both N availability and lignin content influence decomposition by affecting microbial activity (Aber et al., 1990; Manzoni et al., 2008; Sinsabaugh et al., 2013; Moorhead et al., 2013). Similar approaches have also been used in many of the updated traditional SOM models (e.g., lignin:N ratios in CENTURY; Kirschbaum and Paul, 2002).

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.

Your understanding of the systems is perfectly correct. However, our decision to explicitly represent microbes in the litter layer of MEMS v1.0 was based on their importance informing the relevant SOM formation pathways (i.e., direct *vs* microbially-processed), not their impacts on decomposition. Consequently, this is also why we deliberately did not limit the discussion of a microbial pool to aboveground litter only – our structure implies that there must be a microbial pool at each point of carbon input (e.g., the litter layer, rhizosphere, etc.) so that the model can account for the carbon inputs that are microbially processed, and the amount of DOM that results.

We have added some extra information in the main manuscript (excerpts below) but also wanted to include a little more detail here to help clarify our rationale of why we have a microbial pool. At potential different "points of entry", carbon inputs contribute to MAOM or POM formation in differential amounts depending on the microbial community (as per Sokol *et al*. 2018). This is represented by the MEMS model structure by having an explicit microbial pool when organic matter enters the system but not after it; belowground, microbial biomass and associated metabolic processes are implicit (i.e., we assume there is microbial activity and mineralization of the carbon within these soil pools but we do not represent these processes with discrete pools or fluxes).

Sokol, N. W., Sanderman, J., & Bradford, M. A. (2018). Pathways of mineral-associated soil organic matter formation: Integrating the role of plant carbon source, chemistry, and point of entry. *Global change biology*.

L201-205:

Many of the biogeochemical processes represented by MEMS v1.0 are assumed to be microbially mediated (and therefore result in exo-enzyme breakdown and CO2 production), but only two lead to C assimilation into a distinct microbial biomass pool – from the water-soluble and acid-soluble litter pools (C1 and C2, respectively). In the mineral soil (i.e., pools C5, C8, C9 and C10), microbial anabolism and catabolism are implicit and considered part of the turnover of each pool. This ensures parsimony and allows model parameters to represent the differences in microbial community for each pool, as opposed to the alternative of explicit microbial pools. The C transferred from the C1 and C2 litter pools into microbial biomass is defined by a dynamic CUE parameter controlled by the N content of the input material and the lignocellulose index (LCI; defined as the ratio between acid-insoluble to the sum of acid-soluble + acid-insoluble) of the litter layer (i.e., lower CUE results when a higher proportion of the litter is acid-insoluble). Including microbially-explicit processes in the litter layer helps to determine the proportion of C inputs that result in MAOM vs POM formation (see Liang et al., 2017) and allows for future model versions to account for distinctions between different points of entry for inputs (Sokol et al., 2018). The lack of C transferred from other pools (e.g., C3) into microbial biomass implies their decay from co-metabolism with the more labile C sources (i.e., Klotzbucher et al., 2011; Moorhead et al., 2013). Once assimilated within microbial biomass, the anabolism of microbial activity results in generation of microbial products (i.e., necromass) that form tightly bound aggregates of biofilms and small litter fragments around sand-sized soil particles (Huang et al., 2006; Buks and Kaupenjohann, 2016), and dissolved organic matter (DOM). These contribute to the heavy POM (C5) and litter DOM (C6) pools, respectively. While these processes are well supported by relevant literature, to retain parsimony MEMS v1.0 represents microbial metabolism processes implicitly as per their description in LIDEL.

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

Apologies – the use of 'implicit' in this context was not correct. Hopefully the new sentence is clearer.

L210-217
Once assimilated within microbial biomass, the anabolism of microbial activity results in generation of microbial products (i.e., necromass) that form tightly bound aggregates of biofilms and small litter fragments around sand-sized soil particles (Huang et al., 2006; Buks and Kaupenjohann, 2016), and dissolved organic matter (DOM). These contribute to the heavy POM (C5) and litter DOM (C6) pools, respectively. While these specific processes are well supported by relevant literature, to retain parsimony and the generalizable structure required by an ecosystem scale model MEMS v1.0 represents microbial metabolism processes more generally (i.e., by linking them to a dynamic microbial CUE rather than specific community traits).

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.

As mentioned above, you are right for traditional SOM models. However, because our soil pools are physically-defined with a level of accessibility specific to that pool, our ultimate approach is to modify the parameters of processes for C-mineralization from each pool as the conditions (e.g., nutrient availability, input chemistry, point of entry) change. This would allow the different microbial community traits to be represented for each of the different pools. However, we acknowledge that this is more of a point for the next stages of model development and does not apply to MEMS v1.0.

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.

We have updated the terminology the refer to C-mineralization as the decomposition process which then results in CO2. We have added an extra sentence to the main text that states that microbial activity and the

resulting respiration is computed through decomposition estimates after other processes are calculated, and we refer the reader to the supplementary for more detail. Some information in the supplementary has also been made clearer.

L281-285
Thus, the decay rate constants represent total mass loss potential, embodying DOM-C generation as well as CO_2 emissions, as per a recent decomposition conceptualization (Soong et al., 2015). The total amount of heterotrophic respiration is the sum of CO_2 produced from the biotic decay of all model pools after other fluxes (e.g., DOM generation) are calculated (more detail can be seen in the Supplementary).

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.

We are aware and this is key to further development of the model. We have included the mention of pH here now.

L301-304
Simulating the influence of other important controls on decomposition, such as water, oxygen, pH and nutrients, are beyond the scope of this inaugural version of the MEMS model but are central to future development efforts.

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".

We have now added this extra information.

L312-320
Initializing MEMS v1.0 requires external inputs of basic site characteristics (climatic and edaphic conditions as well as land management information) and ideally measurements of daily C input. However, C inputs are rarely available at daily time scales. Consequently, for this inaugural version of the MEMS model we employ a simple function to interpolate daily C inputs from annual Net Primary Productivity (NPP), partitioning aboveground/belowground and to the simulated soil layer using land-use specific root:shoot ratios and a simple root distribution function (Poeplau, 2016). These driving variables are external inputs of the initial model version but may be obtained from coupled climate and plant growth submodels when incorporated into a full ecosystem model. Details of these approaches are given in the supplementary materials and all required driving variables are shown in Table 3.

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

This has now been done to the best of our ability given the formatting requirements of the journal. We will ensure this is done and clear for the final typesetting.

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.

We have now added further detail to our description in the main text. Hopefully this helps to make our methods clearer to all readers.

L325-358

*The default parameter values (i.e., those governing C turnover and fluxes between pools) used by MEMS v1.0 are informed by data from relevant literature (**Error! Reference source not found.**Table 2). However, different studies may suggest different values based on discrete site conditions, meaning a priori estimates may not necessarily be generalizable across all sites that the model could simulate. A variance-based global sensitivity analysis was performed to determine each parameter's relative contribution to the change in each state variable (i.e., determining which parameters have the largest influence on the size of each model pool). The sensitivity analysis was repeated for different simulation lengths (1 – 1000 years) as different fluxes operate at different temporal scales, thereby meaning that the relative importance of each parameter changes through time. Initial pool sizes were set to 0 and the model was initialized to simulate a steady-state scenario based on average site conditions (derived from ~8000 forest and grassland sites in the Land-Use/Land Cover Area Frame Survey (LUCAS) dataset ([Toth et al., 2013] – see Table 3). Specifically, this meant starting a model run with no C in the system and gradually building up the litter and soil pools until they reached equilibrium based on driving variables (soil type, C inputs, climate) that remain fixed over time. To evaluate how much each model parameter (e.g., decay rates, DOM generation rates, etc.; see Table 2) effects the amount of C in each pool (i.e., C1-C11; Figure 1) parameter values were changed to be higher or lower from their baseline and pool sizes are tracked over simulation time. Note that all temperature modifier parameters (T_{ref}, T_{opt}, T_{Q10}, T_{lag} and T_{shp}; Table 2) were excluded in this sensitivity analysis as the resulting T_{mod} has the same effect on all decay rates. Maximum and minimum values of all other parameters (n = 24**Error! Reference source not found.**) were defined as 50 % above and below the literature-derived (baseline) value (Table 2). Using Latin Hypercube techniques to sample within the full parameter space, a global sensitivity varying all parameters was used to determine total variance for changes to each model pool (i.e., how much each pool changes in size when all parameters vary up to 50 %). Then, in turn, each individual parameter was fixed at its baseline value while all others varied. This defines each parameter's contribution to a pool's variance, averaged over variations in all other parameters (Sobol, 2001; Saltelli et al., 2008) (i.e., how much each pool changes in size when all parameters, except one, vary up to 50%). When normalized over the global sensitivity variance, a contribution index provides the proportion of variance explained by each parameter. The analysis was run 10,000 times to define the total parameter space and the whole procedure was repeated annually for simulation lengths between 1 to 1000 years. Put simply, 10,000 different combinations of parameter values between the minimums and maximums were used to repeatedly run the model for 1000 years given average site conditions. The results showing changes in pool size correspond to the changes in parameter values (e.g., when maximum decay rate of MAOM is increased, pool C9 may decrease in size but others may increase). The impact that a single parameter has on pool size, compared to that of all parameters, is described by the contribution index, where the total effect of all the parameters is equal to the maximum change in pool size. Note that the results of a global sensitivity analysis of this kind are non-directional and do not indicate whether a parameter increases or decreases a pool size, but rather that it simply changes from the baseline.*

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.

We are indeed aware of the limitations of using the MODIS NPP estimates. We have also checked a 10-year average of NPP data for each site and noted the variability (and considered redoing the analysis). However, the variability for one site's 10-year average is considerably lower than the variability across Europe and therefore we concluded there was little value in redoing everything, given our limited expectations and reliance on the simulated absolute values.

L380-384
Complimented with geo-referenced estimates of annual NPP from MODIS satellite data (ORNL DAAC, 2009), and daily temperature data from the Climate Prediction Center's Global Temperature (CPC-GT) database (NOAA, 2018), this provided all driving variables required to run MEMS v1.0. The use of modelled/interpolated NPP and climate data is not recommended over measurement data directly collected from the site(s) being simulated, but for the analysis herein these measured data were unavailable.

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.

We agree and will make these available as part of this paper submission. The data will available at: http://esdac.jrc.ec.europa.eu/

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.

The pre- and post-optimized results did not differ significantly for some environmental divisions (e.g., hot, wet, sandy, grasslands) but did for others. We tend to agree with you that our optimization was a little more than what was needed given the early stage of model development, however we wanted to demonstrate how the parameter estimation approach could apply using real measured data. We performed several analyses to assess model performance before and after optimization, but we feel the manuscript already includes a lot of detail and this extra information would be of little value for the majority of readers.

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

We did refer to this table here already but have added an extra reference to hopefully make it clearer.

L422-426
The new, optimized parameter values (Table S2) were derived from a randomly chosen fold that minimized RMSE when compared to the MAOM fraction. This was chosen (instead of those optimized for POM or bulk SOC) since the MAOM fraction is typically the largest single soil C pool and using this approach led to the biggest overall decrease in RMSE when compared to all available data (Table S2).

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

The annual temporal dynamics of C-inputs are derived from a simple distribution function for this first version. We assume a normal distribution around mid-summer so that 75% of the C inputs are added between April and August (Northern Hemisphere). This is a very simplistic way of doing things and is the same for all land use types and locations in our analysis. However, we felt this was more realistic than the same amount every single day. Of course, because we are simulating a steady-state system the resulting difference in effect is minimal but in future versions these C inputs will be coupled to a plant growth model which will be much more accurate.

Regarding seasonal temporal dynamics of temperature, we simply use the daily values for each site and therefore we hope the values used are accurate and account for seasonal variability. This is already stated in the main text.

We have added a little information about this to the main text and point the reader to the supplementary for more detail.

L430-432
Driving variables of edaphic conditions and land-use type were extracted for each site from LUCAS and combined with daily estimates of C inputs and temperature (derived from simple interpolations assuming a normal distribution of MODIS annual NPP data [see Supplementary for details] and CPC-GT daily maximum and minimum air temperature data, respectively). Where these data were unavailable, the site was removed from further evaluation.

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

Yes, well noticed. However, the difference comes from the fact that in our methods (line 407) we refer to the median values whereas table 3 states the means. We felt the medians were a better way of describing the overall dataset we were using but the means in table 3 were simply a way of showing the average value – the actual values used in our analysis obviously varied with each site.

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"?

We start with the carbon pools equal to zero. We did repeat the process with several different starting condition scenarios, but the overall effects were the same. We chose this one simply because it was the easiest to interpret (although we acknowledge that it has so many colours it is still hard to interpret fully). We have added this information to both the new figure legend and in the methods section. See the new Figure 2 and fully colourized vector PDF of the associated Figure S5 in the supplemental file attached to these responses.

Figure 2 legend
Figure 2 - Global sensitivity analysis results showing the relative contribution of each parameter to a change in carbon stock of each pool in MEMS v1.0 (leached carbon to deeper soil layers [pool C11] is omitted for clarity) after simulation to steady-state. Details of each parameter and the abbreviations used can be found in Table 2. The sensitivity analysis was repeated annually for simulation times between 1 and 100 years, every 10 years after that to 400-year simulations and every 100 years after that up to a 1000-year simulation. Results are presented on a log scale in years. The four parameters that were optimized in our analysis (Table S2) are coloured to highlight their importance in the different pools (mid-point of logistic curve where nitrogen content of input influences microbial carbon use efficiency, Nmid, red; maximum decay rate of heavy particulate organic matter, k5, orange; maximum decay rate of mineral-associated organic matter, k9, blue; maximum decay rate of light particulate organic matter, k10, green). A fully colourised version of these results can be in Figure S5.

L332-336
Initial pool sizes were set to 0 and the model was initialized to simulate a steady-state scenario based on average site conditions (derived from ~8000 forest and grassland sites in the Land-Use/Land Cover Area Frame Survey (LUCAS) dataset ([Toth et al., 2013] – see Table 3). Specifically, this meant starting a model run with no C in the system and gradually building up the litter and soil pools until they reached equilibrium based on driving variables (soil type, C inputs, climate) that remain fixed over time.

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

Partially. The main reason why this relationship occurs in MEMS v1.0 is because the MAOM and POM pools reach different equilibrium amounts under different temperatures – under steady-state, MAOM will reach equilibrium at roughly the same amount in all temperatures (i.e., near to the saturation limit), however the POM pools do not saturate and so when temperature is low, decomposition is low, and they will accumulate more before reaching equilibrium. Ultimately you are correct in assuming that temperature is assumed to have a bigger effect on the decomposition of POM than on the decomposition of MAOM (*sensu* Benbi *et al*., 2014). Our early attempts to differentiate between these sensitivities showed exciting results but were not based on rigorously tested measurements. A key focus of the next stages in model development is in the different sensitivities for the different pools so we hope to include these explicitly in MEMS v2.0.

Benbi D K, Boparai A K, Brar K. 2014. Decomposition of particulate organic matter is more sensitive to temperature than the mineral associated organic matter. *Soil Biol Biochem*.**70**: 183–192.

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 [gC^2 day-1] rather than [gC day-1].

Thanks for pointing out this error. We meant to simply write day^{-1} and this results in the same effect. We have now changed throughout.

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.

Agreed. We have now added this extra information.

L602-610
While average agreement between measured and modelled soil C stocks was very good for MEMS v1.0, the model failed to capture the wide range in total POM-C stocks that were observed at the fractionated LUCAS sites (Figure 5). This may be because this first version of the model does not include several of the key controls on POM dynamics, such as water/oxygen limitations (Keiluweit et al., 2016), aggregation (Gentile et al., 2011), activity of soil fauna (Frouz, 2018) and nutrient availability (Bu et al., 2015; Averill and Waring, 2018). There are also limitations of our approach given that very few of the sites will likely be under true steady-state conditions, leading to further discrepancies between model predictions and measured values. Furthermore, the variability in driving variables of litter chemistry, N content and root:shoot ratios are underestimated when using our approach of grouping many different land uses into broad classes.

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.

We agree and accordingly we are currently in the process of fractionating soils from the NEON network of sites that includes a wide range of ecotypes and climates – see https://www.neonscience.org/field-sites/field-sites-map. Once available, it is our hope that these data will help to improve the ability of the MEMS model to simulate a much more diverse set of soils. The relatively narrow range in this analysis of the LUCAS sites results primarily from the very large number of sites. Our initial analysis here was to try and see if general trends looked good and now we are moving on to more site-specific comparisons where we have much higher quality input data.

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.

We agree. We acknowledge the limitations of this early model version and have down-played the point slightly. However, we do feel that the change in approach and model structure can pave the way for an easier link to existing plant growth models and ecosystem models.

L564-566
MEMS v1.0 was designed to consolidate recent advances in our understanding of SOM formation and persistence into a parsimonious mathematical model that uses a generalizable structure which, after further development, can be implemented in Ecosystem and Earth System model applications.

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.

We have added to this as shown above. Also, we hope the extra clarification of how microbial activity is simulated implicitly in the soil pools will help to clarify this point.

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.

You are right that these sites are not water limited but rather they are water saturated. In these situations, it is possible that anaerobic conditions persist and limit C-mineralisation. You are right that water limitations on the other end of the spectrum (too dry) will be prevalent in Southern Europe, and this is another source of high residuals when we do not include soil moisture controls.

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

Thank you. We are also keen to work more towards these goals.

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.

While we tend to agree that it could be nice to have a single figure with all the information on it, we chose to keep figure 1 as simple as possible so it can serve as a simple way of conveying the overall structure, rather than all the details. We appreciate the suggestion though and will look into including more details on future figures.