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Geoscientific Model Development Discussions

Interactive comment on "Jena Soil Model: a microbial soil organic carbon model integrated with nitrogen and phosphorus processes" *by* Lin Yu et al.

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A: authors' response

General comments

Yu and coauthors present a conceptually robust model that looks at soil biogeochemical processes that explicitly represents microbial activity and CNP stoichiometry in a vertically resolved model. The work presented here does a very thorough job documenting the model configuration and performance at a well-studied site. What's less clear is why it matters? A few suggestions are described in the specific comments

below.

A: Thanks for the recognition of our effort.

My other major concern with the model is that it doesn't reach steady state equilibrium, instead soil C pools are accumulating at a rate that's roughly 5% of NPP (Table 1). It seems longer spin up times were tried, but since results aren't presented I'm assuming this issue persists, if so, what do soil CNP profiles look like after 10ËĘ4 years, do they still match observations well? If the model just has long-term oscillations this may be less of a concern than a constant drift (as I currently understand). The spin up issues, however, seems like a significant issue that has to be addressed if models that more explicitly represent microbial activity and coupled biogeochemical cycles are ever going to be applied in TBMs, as seems to be the aim of this work. The 'lack of plant feedbacks' argument seems unsupported. Moreover, I don't really understand why / how constant 'loss' of P into 'occluded pools affect the C dynamics simulated belowground? This spin-up issue is also one I don't know how to handle in review and my overall assessment of this work. For this reason I'm signing this review and welcome an open conversation with the authors on this concern. I appreciate all the effort that the authors have made do make a very interesting contribution to this line of work- but a model that never really reaches steady state seems very challenging to use for more than short term-studies and sites where the model can be adequately parameterized. This may be the aim of this research group, but it seems unlikely given the introduction, conclusion, and history of strong work from this research group looking at global scale C and nutrient responses for climate change projections.

A: Thanks for the reviewer to point this out. First of all, in our opinion, the soil system should not reach a real equilibrium due to the fact that soil has to develop from bare soil to certain SOC content, and this accumulation process should not stop as long as the soil is not C-saturated when there are continuous C inputs. However, we do agree in an ideal model simulation, the accumulation rate should be constrained within a very small rate. This is actually the case in the top as well as the near surface subsoil in our model after a few hundred years, while small accumulation continue to take place in the deeper soil. We chose the 200-year simulation length for the manuscript, because the surface soil has already reached equilibrium after 200 years, but the deeper soil continues to accumulate C. In our long-term simulation (5000 yr), the annual accumulation of NPP as C in the soil is only 0.07%, compared with 5% in the 200 yr simulation. There is no evidence of the model application to result in oscillations at longer time-scales, as seen in Fig.1 in which we present the top- and sub-soil C content for the 10000 year simulation. We agree with the reviewer that this has been unclear from the previous version of the manuscript, nor did we include the data in the results. To elaborate this, we will include a new figure in the resubmission to demonstrate how the SOC accumulates in surface and deep soil over a very long time period. We still believe that the results of our study can reasonably be interpreted, because the top X cm showed near equilibrium conditions already after 200 years (demonstrated in Fig.S1 and S2).

A general issue with the development of stand-alone nutrient enabled soil biogeochemical models is that the assumed plant uptake demand does not adequately reflect long-term soils development. When the model was ran for a very long time (e.g. 10,000 years), there were some cases in which the primary P in surface layers got depleted and a large fraction of the sorbed P got occluded. While microbes detect this change and as a result levels down its biomass because it takes up less P, the root biomass and associated plant P uptake in our model is prescribed at the level of mature healthy forest. That said, the root biomass does not change under P limited growth condition, nor does the root distribution over soil layers change. The lack of the phosphorus-root growth feedback implies that under such conditions fine roots become more competitive than microbes in taking up inorganic P, and there is always living roots trying to take up P even if they only take up little P for a very long time. The inorganic P cycling problem is a common problem for the community of terrestrial biosphere modelers, especially at very P-poor ecosystems.

Specific comments

In my opinion there's a bit too much emphasis in the introduction in playing up the novelty of this work. This is not the first model to think about vertical resolution, microbes, nutrients or ECA. It may be the first to do all these together, which can be stated, but then move on. The current review of the literature is nice, but I'd encourage the authors to avoid language that's unnecessarily dismissive of previous work. To address my first issue of 'why this work matters' I can three of three options to consider:

A: We are grateful for all the previous work that makes this model possible and we do realize our wording has caused misunderstandings. We'll carefully rephrase them in the resubmission.

1. Idealized experiments: While the justification for including nutrients and microbial feedbacks in a model like the Jena soil model is well established in the abstract and first paragraphs of the introduction I also fear it sets up somewhat unrealistic expectations for readers. Notably, none of the results presented illustrate how the model may respond to environmental perturbations. I'm not suggesting these have to be compared to results, but instead simple idealized experiments that illustrate how the different model configurations respond to increases in litterfall inputs, root exudates, warming, or changes in precipitation.

A: We really appreciate the suggestions by the reviewer for more interesting model experiments, and we are also interested in carrying out such experiments in the future. However, performing such model experiments themselves in a meaningful way would require substantial additional model evaluation and discussion to discuss whether the simulated feedbacks are commensurate with current understanding. Because established model benchmarks do not exist for this model behaviour, this would require an in-depth discussion of the available observations, which in our opinion is beyond the scope of a model description paper. Simply showing sensitivity study without comparing these to suitable observations would be fairly meaningless.

2. Model validation: Alternatively, it seems lots of data were needed to initialize the model. This is fine for development, but how well does the model do simulating other sites? Are there other well studied sites that can be used for independent model validation? I realize this potentially an objective for future work, but it seems like typical activity for model development papers (especially in GMD) that would help illustrate the broader generalizability of the approach outlined here?

A: Thanks again for the suggestions. Simulation on multiple sites, e.g. a gradient study involves specific biogeochemistry scientific questions to be addressed, and we believe that this is beyond the scope of this paper. Simply showing that the model could be calibrated in other sites will not give much additional value to this paper.

3. Sensitivity analysis: A third alternative would be to consider illustrating model sensitivities to initial conditions? Much like the idealized experiment suggestion (above), I kept finding myself wondering how sensitive the model behaves to initial conditions that are being input to the model (e.g. litterfall and microbial stoichiometry, soil texture / mineralogy, water fluxes and temperature profiles). The parameter sensitivity analysis is nice, what about other assumptions that are being made regarding inputs to what seems like a highly parameterized model? This would open up the discussion for consideration of how to run JSM in regional or global simulations (clearly the intent), where we have less certainty of how the define these characteristics (especially with multiple elements and with depth).

A:Thanks for the comment. We did test how model performs under different initial conditions, and we have also done some other experiments, such as how the model responds to different microbial carbon-use-efficiency and nutrient-use-efficiencies, plant/microbe uptake rates of mineral nutrients, DOM uptake rates etc.. The reasons for not showing them are very similar as the ones for previous two: first, there is a limit on how much we can try to include in a model description paper; second, some of the experiments are very interesting topic to formulate new studies, and we don't want to dilute the importance of them by including them into this paper.

We will include the model results under different initial conditions in the resubmission, and discuss it together with the spin-up, equilibrium state, and stability issues.

The authors have actually done #3 with the microbial stoichiometry section that squeezed into the discussion. Maybe the most direct path forward to satisfy this concern would be to actually flush out these findings in the methods and results (see technical comment below).

A: Thanks for the recognition and we will make it more visible in the resubmission.

Technical corrections

Page 2, Line 10, I might include Lehmann and Kleber 2015 here.

A: Thanks, included.

Page 2, Line 11, Vertically resolved models are becoming more common (McGuire et al. 2018)

Page 2, Line 18, I'm not sure the assertion (made here and in the following paragraph) that microbial explicit models don't represent coupled biogeochemical cycles is accurate (Averill & Waring 2017; Schimel & Weintraub 2003; Sistla et al. 2014; Sulman et al. 2017, 2019).

A: Thanks. We will carefully revisit related literature and revise the introduction, as both reviewer 2 and 3 have pointed out the problem.

Page 3, Line 4. I'm pretty sure the ECA approach is applied in E3SM land model, which I wouldn't call a prototype model.

A: Corrected.

Page 3, line 16. & Page 4. Where's section 5?

A: Should be Sect. S5, corrected.

Methods. I know COMISSION already has radiocarbon, but should there be any focus on documenting how JSM implements radiocarbon in the text or appendix?

A: We will include the 14C initialization in the model protocol section. But we don't intend to include the radiocarbon in the supplementary material since they are not the new development of this paper.

We will clearly state it in the revision that "the model explicitly traces 14C, see Ahrens et al. 2015 and Thum et al. 2019; the 14C values of litter forcing were generated using QUINCY, see Thum et al. 2019".

Page 7 and Fig 2 the model calculates its own bulk density?! That's pretty interesting, should this be described in the methods?

A: We will include it in the model description, together with the descriptions of some other processes, as mentioned in the response to reviewer 2.

Page 7, Line 10-15. It seems odd to jump from presentation of Fig 2 to 7. Should the display items reflect the order that information is covered in the text?

Throughout, display items should be numbered in the order they are introduced in the text.

A: Thanks for pointing out the problem. We will revise the order of our displayed items in the resubmission.

Fig 8 and Table 1 are never referenced in the results, should they be? I'd prefer these display items not be first introduced in the discussion of the findings of this study.

A: Thanks for pointing it out. We will reorganise the results and discussions according to the order of display items. However, we do think these findings are interesting enough given the fact it is a model description paper. More elaboration can be found in the response to comments of "Discussion".

Fig. 7 Bottom panels of should be % modern. I also couldn't help but notice that you just have 14C data for the site. Why not run the model for longer and show result, or put

the radiocarbon observations up on the plot shown here even if they're just illustrative for 14MOC (which should be most of what makes up the bulk 14C values at depth? Wait, the 14C data are presented in the SI (page 7, line 20- sorry I'm on a plane and don't have access to the SI material). It seems this would be a powerful constraint for the model to try and hit (and should be included in the main text). I'm struck that we can learn a good deal about the model, even if the model is not able to match radiocarbon profiles! If longer spin-up runs have already been done I can't think of any reason not to compare results to observations where they are available.

A: Since this study does not involve any development of radiocarbon calculation, we did not focus on presenting the 14C results. The main message of the 14C results in this paper is that, the inclusion of N and P cycling and other processes do not affect the capacity of the carbon core of JSM (i.e. COMISSION model) to capture/approach the soil profile radiocarbon.

Admittedly, we have stated in the paper that due to the uncertainty in initialization and P cycling processes, the model will have P depletion problem in the long-term simulation (>10,000 years). As a demonstration, we show the change of non-occlude inorganic P for 10,000 year below (Fig.2). The P content in top- and sub-soil fluctuates before 2500 years due to the combined effects of transport and immobilization/mineralization, but after that both of them decrease continuously. We did reach P depletion in other long-term (10,000 year) tests during the calibration processes although this one is not yet there.

We agree that a long simulation time is the perquisite to hit the 14C soil profile, but in order to run the model stably for such a long time, we might need to switch off some inorganic P cycling processes in the spin-up. We will discuss about this more in detail in the resubmission.

Figs 3-4, Page 7. From the text it sounds like there are observations of soil nutrient transformation (at least N mineralization). If so, can these be included on the appropriate panels, or am I misunderstood?

A: Sorry for the confusion, but we don't have observed nutrient fluxes that can be comparable to our simulations.

Page 8, line 10, what is TW in JSM? Section 3.2. Is the strong microbial competition for P (and not N) caused by the C:N:P ratios that are prescribed for the site (and notably skewed).

A: The content within the bracket in line 10 is a co-author's comment which should have been removed.

The reviewer 2 also has similar concern about the strong microbial P competition of the site, but as what we have seen in simulations of other sites (for another study) it is a consistent pattern in all sites. However, the C:N:P ratios of this study site is not far from other sites we have (Lang et al. 2017), but very far from the global average value. The scenario using global average microbial stoichiometry also shows that microbe outcompetes roots, but not as strong as the base scenario.

Page 8, line 24, the difference among models mentioned here regarding depth profiles of N-mineralization is not obvious, at least to my eye. Regardless, avoid using 'significant' when no statistical results are presented.

A: Thanks for the suggestion. It will be corrected in the resubmission.

Page 8, line 27, it's not clear from the methods how the actual and potential enzyme allocation curves are being calculated from the methods, or did I miss this description. I'm also still hung up on how or why this is being done if the model doesn't explicitly represent enzymes (by the way this decision not to explicitly represent enzymes makes sense to me from a purely practical / numeric standpoint)

A: The detailed processes descriptions are presented in the supplementary material. For the enzyme allocation, we made an assumption that the total enzyme is always proportional to the microbial biomass and used the enzyme richness in the Michaelis-

menton equation. Therefore we did implicitly model the enzyme production, and explicitly model the enzyme allocation. We will clarify this in the model description in the main manuscript.

Page 8 line 33, if P depolymerization is completely demand driven why is microbial P uptake so much lower in the ECA-off simulations (Fig 4a)? I thought these were supposed to be the 'demand based' simulations (methods)? Please clarify.

A: In the ECA approach, we do not calculate the demand, but the potential uptake depends not only on the uptake capacity per carbon roots/microbes, but also on the biomass of roots and microbes. The ECA approach mainly regulates the competition of uptake capacity per carbon, but eventually the total uptake still depends on the microbial biomass. That is why it looks like "demand-based" simulation.

To simply explain what happened when we turned off ECA: we initialize all the scenarios the same, but the microbes take up less P per biomass carbon than the base scenario, therefore the microbes develop less biomass than the base scenario. Both the lower microbial biomass and lower uptake capacity per unit carbon in the ECA-off scenario has caused the much lower microbial P uptake than the base scenario.

Page 9, line 5. Reference Fig 7 here?

A: Corrected.

Page 9, Line 25 these values are for soil stoichiometry? Also, what are N:P ratios for soils? Finally, to my eye it looks like the model may overestimate observed soil C:N ratios in upper soil horizons (Fig 2). Regardless, it's likely helpful to point to this display item to support claims made about soil C pools and stoichiometry made here.

A: Thanks for pointing out the problem. It is the soil stoichiometry we are discussed here, and the C:N ratio in the O-A horizon is indeed overestimated by the model. We will include it in the discussion when we resubmit.

Discussion: I have to admit I haven't thought much about the dynamics driving declines in soil C:P ratios with depth, nor am I very familiar with this literature. For everything the model is doing here, this text strikes me as an odd choice to highlight at the beginning of the discussion. That said, it. Is interesting. One detail I don't really follow is that to capture observations it seems like the P recycling term in the model has to be greatly reduced in model. It doesn't seem to logically follow that the community somehow shifts to 'nutrient rich' community that's also has lower nutrient use efficiency? Instead I think the findings of Rousk and Frey suggest that substrate quality determines the microbial communities in forest soils, but doesn't speak much to vertical distribution of microbes (or their stoichiometry) being. Discussed here?

A: Thanks for the comment. We think this finding is interesting and new, and should be stated early in the discussion. First of al l, the soil stoichiometry is a rarely discussed topic in the modeling community, and the fact that C:P ratio decreases much faster than C:N ratio with depth is also very interesting for us. Besides, we only have observations for the soil stocks but not flux, so it is natural for us to start with the finding that we saw in the soil stocks. However, as all the reviewers are concerning about the model spin-up, stability/equilibrium state, we will also include this topic in the first part of the discussion.

For the second part of the question, we found that the model has to be tuned in a way that the microbial residue becomes P-poor in the surface layer to reproduce the C:P depth profile. To us it means the microbes need to be more dominated by fungi in the surface soil, and it agrees with what Rousk and Frey (2015) presented in their results (Table 2) that organic layer has higher fungi:bacterial ratio than mineral soil. It also agrees with one of their conclusions that more litter input will lead to higher fungi:bacterial ratio. Although they did not mention soil depth specifically, it is an obvious fact that litter input to soil decreases with soil depth.

Table 1 should include soil C, N, & P pools of the model after spin up, as it's hard to assess total pool sizes from figures.

A: We only looked at the last 10 years' pool size change of the simulation. We will also include a new figure to demonstrate how the total pool size changes over 10000 years.

Fig 8 can colors of processes in the legend match the order they are displayed on the figure. As currently presented it's not easy for readers to interpret the figure. Introduction of the microbial stoichiometry part of the discussion seems like a nice sensitivity test of the model, but I don't like this being squeezed into the discussion and SI. Why not at least justify this experiment in the methods and describe findings in the results before discussing the findings? (It also likely makes sense to keep the figures in SI).

A: Thanks for the suggestion. We will include the microbial stoichiometry scenario in the methods section.

Page 12, line 25. What observations are the model able to reproduce? Can they be illustrated on the display items (* that also should be referenced here)?

A: We will rephrase the sentence to be more precise and relate to the display items.

Page 12, line 28, why not cite a commission paper that's already published

A: We have taken the carbon cycling framework of the most recent version of COMISSION (Ahrens et al. 2019, in review), which is already different from the published version (Ahrens et al. 2015). We will update the reference once it is accepted.

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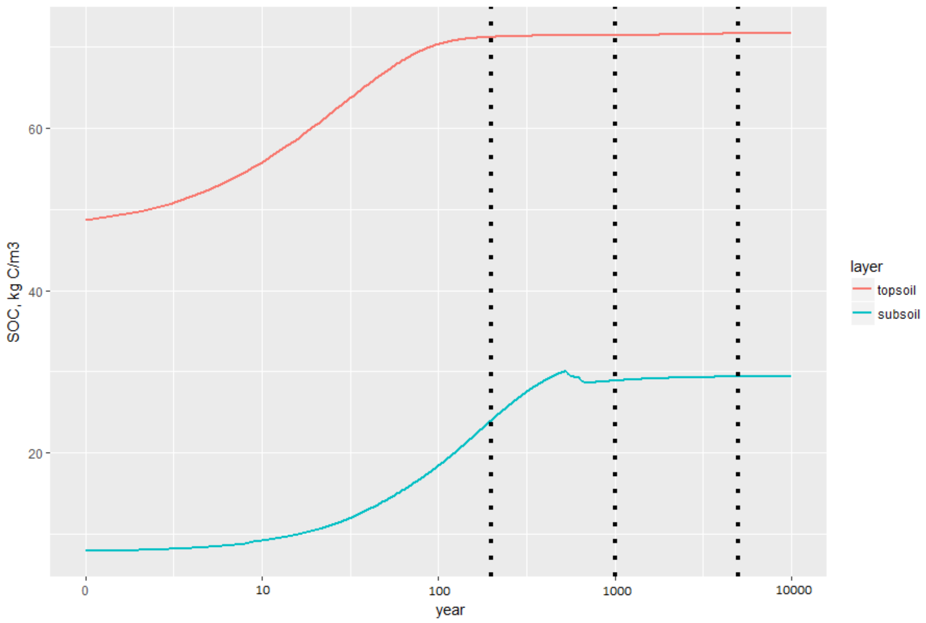
SOC, kg C/m3
60
40
20
0
10
100
1000
10000
year
layer
topsoil
subsoil

Fig. 1.

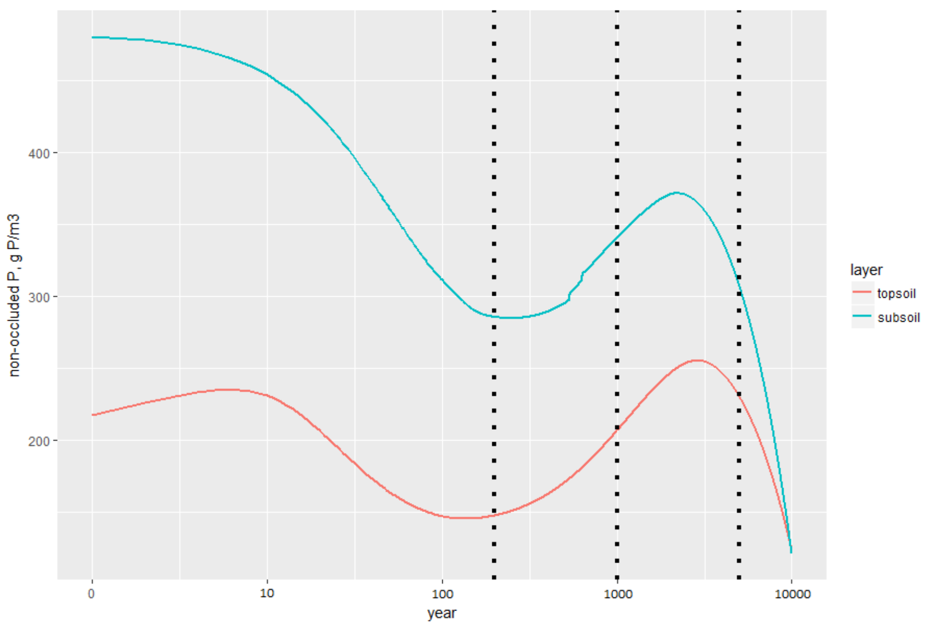
non-occluded P, g P/m3
400
300
200
0
10
100
1000
10000
year
layer
topsoil
subsoil

Fig. 2.