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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

This manuscript describes the Jena Soil Model, a new soil organic matter model that includes microbial processes, mineral sorption of organic matter, and vertically-resolved soil processes. I thought overall the manuscript was well-written, clear, and easy to follow, and the model integrates new methods for simulating microbial and mineral influences on carbon and nutrient cycling and will be a useful contribution to the biogeochemical modeling field. The introduction did an excellent job of describing the relevant issues and the context for the model. The description of the model was generally clear, although most of the details were left in supplemental material. I do have a few suggestions of areas where the clarity of the manuscript could be improved.

A: we thank the reviewer for the positive comment and recognition of our work

I think some additional detail about the sources of the measurements that the model was driven with and compared to would be helpful for understanding the results. The site description only covers the characteristics of the site itself (vegetation and soil types, and some soil profiles) and does not include what kind of data collections were available and the methods used to collect key data resources such as C, N, and P profiles and meteorological data. Some presentation of seasonally-varying factors such as soil moisture, temperature, and litter inputs would help with interpretation of the simulated seasonal cycles. While some of these data collections are presumably described in detail in other publications, a summary in the methods section (an expansion of section 2.2) would help make the measurement context of the simulations clearer.

A: we will include a summary of the measurements in the method section to give a bit more information on the data collections.

The description of model processes in the text is quite short and is very focused on a few details about stoichiometry and enzymatic processes. There is a lot of detail in the model equations (in supplemental material) that is not explained in the main text. I think some expansion of the process explanation would help readers to understand some of the results. In particular, the seasonal cycles of fluxes shown in Figures 3-5 are largely controlled by moisture and temperature functions , and possibly by the seasonal phenology of vegetation forcing in model simulations, which are not explained in the text.

A: we agree that the seasonal patterns are strongly controlled by the temperature and the seasonal variation of the litter forcing. Although the main focus of this paper is not to look at the causes of seasonal pattern, we do agree it is better to mention them in the method and discussion sections.

We will add some brief descriptions of other processes, such as the temperature and moisture sensitivities used, the microbial response to nutrient availabilities, the nutrient cycles parameterisation etc., in the model description to help readers better understand our results.

Specific comments:

Page 1, Line 5-6: Some microbial-explicit decomposition models have included nutrient cycle coupling for example, Abramoff et al., 2017; Sulman et al., 2017; Huang et al, 2018.

A: Thanks for the information. We will correct it.

Page 2, Line 31-32: Likewise, there are some TBMs that have included more mechanistic SOM cycling and there are some microbial SOC models that include nutrient cycling.

A: We will correct it.

Page 4, line 13: The “See Sect. 5” may be a mistake. Section 5 is the Conclusions. I think this should be SI section 5? Also, I would suggest explaining these processes in more detail in the main text rather than referring readers to the complex set of equations to understand how the model works.

A: Corrected. We will add a brief description of these processes to the model description, as mentioned in the previous response.

Page 4, line 21: "DFG" should be spelled out or defined

A: Corrected.

Page 4, line 27: "C content of SOM" is a bit confusing as it could suggest that SOM has been separated from bulk soil and the C content of only organic matter has been determined. Based on the numbers, I think this is C content of the bulk soil in those layers. I would just say “soil C content”

A: Thanks for pointing it out. Corrected.

Page 5, lines 26-30: It’s not clear from the description whether calibration was an iterative processes. Was this two-step process repeated until results were satisfactory? Was there a particular statistical method used to assess how well the model fit the data?

A: Thanks for pointing it out. No, the calibration is not done iteratively, but indeed, during the second step, we slightly revised some of the parameter values of the first step based on our previously experience.

Also, we only evaluated the model fit visually and did not use a particular statistical method. Because we did not run a Monte-Carlo type calibration, instead all the parameters were varied gradually between two selected values. By calibrating in this way, we learnt how the individual parameter/process would affect other processes/pools, and it also makes the visual judgment sufficient to choose the better model fit.

Page 7, lines 18-25: Since 14C measurements were an important part of the model evaluation , with some interesting interpretations, I would suggest moving the 14C comparison figure to the main text.

A: The 14C signal is indeed a very important feature of our model, but we did not include the comparison in the main text for two reasons: first, the main focus of this paper is to include nutrient cycles and discuss the features more relevant with carbon-nutrient interactions; second, we did not run the model long enough to match the 14C measurement due to the very high uncertainty in long-term inorganic P cycling and in model initialization. We did test the model for 10,000 years at two other sites with more extreme soil P content, and found out that current inorganic P cycling does not work well in long-term simulation, therefore we have no clue how to initialize the soil mineral P pools, such as primary P pool and secondary P pool, over such a long time. Please find more information in the response to reviewer 3.

Page 7, lines 30-31: Were there changes in microbial growth rates over the season that could explain changes in microbial N demand? I also would suggest adding some explanation for the large spike in microbial N uptake in November. Is this something to do will autumn litterfall, like a short-term increase in N immobilization due to deposition of a large amount of fresh litter?

A: Corrected.

Page 8, line 10: What does "TW" mean?

A: Removed. It was a comment by co-author we forgot to delete.

Page 8, line 27-page 9, line 5: I had trouble following this explanation of the figure, particularly how the potential allocation curves were calculated and how they should be interpreted.

A: Will be revised in the resubmission.

Page 9, line 7-8: Microbial N uptake and N losses were not centered around the mean. And there is no Table S4, only S1 and S2.

A: Corrected.

Page 10, lines 4-9: This seems like an important part of the model structure and results, and should be introduced earlier than the Discussion section. I think this modification to the model should be described in the methods. And since making the parameter depth-dependent makes a difference to the results, it might make sense to include it as a separate set of model simulations (as with the SEAM-off and ECA-off simulations) so its effect could be shown.

A: Indeed, the depth-dependent microbial recycling of P is really important for this study site to yield the realistic C:P ratio and Po-to-Pi ratio, and is also what we expect to happen in reality (Rousk and Frey, 2015). We did run a simulation with uniform microbial P recycling along depth but excluded it in the final submission. The reasons

to exclude it is that, it is not a standard model feature as SEAM and ECA, which do have theoretical basis. Instead, we suspect that the depth-dependent microbial P recycling should be an emerging model feature if we separate bacteria from fungi.

However, we attach the comparison figure here (in the end) and hand it to the editor to decide if it needs to be included or not.

Page 10, lines 23-24: At steady state, plant N and P uptake would have to be close to litterfall inputs, unless there were large losses due to leaching or other loss pathways.

A: The major reason for not reaching a real equilibrium is, as we stated in the manuscript, the model does not have the feedback from vegetation. That said, we prescribed our litter forcing, and the plant uptake is only determined by the soil conditions regardless of how much plant really requires. As shown in Tab.1, there is no significant loss of N and P from the ecosystem, but N and P are accumulated slowly in the soil due to the fact that 5% of litter fall is accumulated in the soil as SOM.

Page 11, Lines 7-8: Is the fact that plants mainly take up N and not mineralized P specific to this ecosystem? In a more P-limited ecosystem, would the results differ?

A: We do not know the exact answer to this question. However, in our ongoing work where we run the model with multiple sites along a soil P availability gradient, this pattern still holds true. To our understanding, it is the very different stoichiometry of plant tissue and microbe that yield such a pattern, and it should be even stronger in P poor ecosystem than P rich ecosystem, as indicated by Lang et al. 2017.

Page 11, line 11-12: The global microbial stoichiometry simulations should be described in the methods.

A: Will be revised in the resubmission

Figure 1: It would be helpful if the notation in this figure matched the notation in the equations in supplementary material.

A: Will be revised in the resubmission

Figure 8: This figure is difficult to understand because there is not a clear explanation of what the different variables mean.

A: Will be revised in the resubmission

Fig 1. Simulated and observed (a) SOC content, (b) C:N ration in SOM, (c) C:P ratio in SOM, (d) organic P to inorganic P ratio in soil, microbial C, N, and P content ((e) to (g)), and (h) soil bulk density at the study site up to 1m soil depth. Black lines and dots: observations; Color lines and shades: simulated mean values and ranges of standard deviation by different model experiments. The microbial C, N, and P are only measured in top 30cm soil. Simulated means and standard deviations are calculated using data of the last 10 years from the model experiments.

Fig 2. Simulated seasonal and vertical distribution of (a) respiration, (b) net N mineralisation, (c) biochemical P mineralisation, (d) net P mineralisation, (e) microbial inorganic P uptake, (f) plant P uptake, (g) microbial inorganic N uptake, and (h) plant N uptake at the study site up to 1m soil depth. Points represent the mean values and error bars represent the standard deviations, both calculated using data of the last 10 years from the model experiments.

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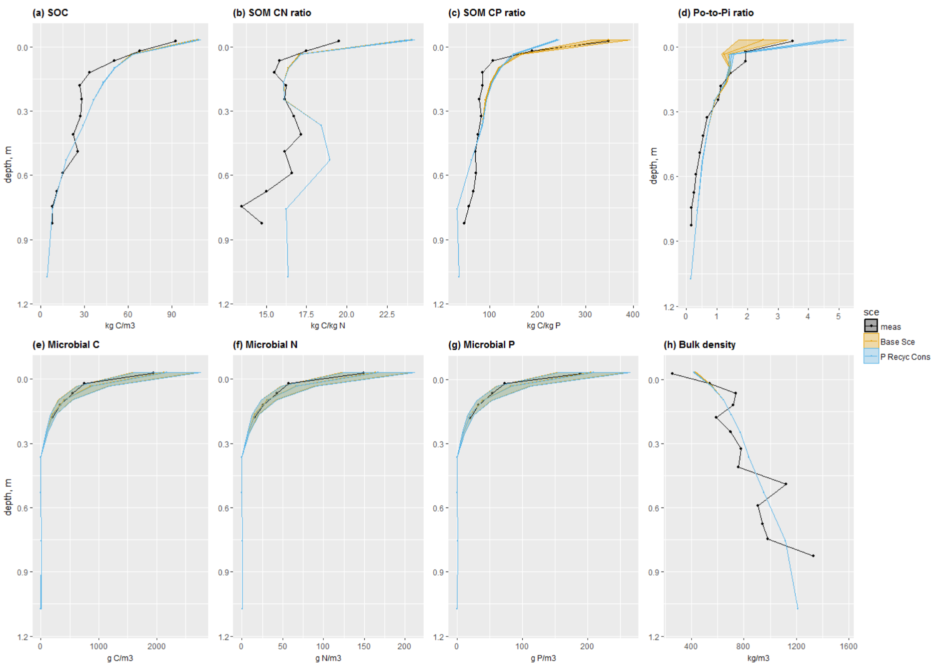

Fig. 1.

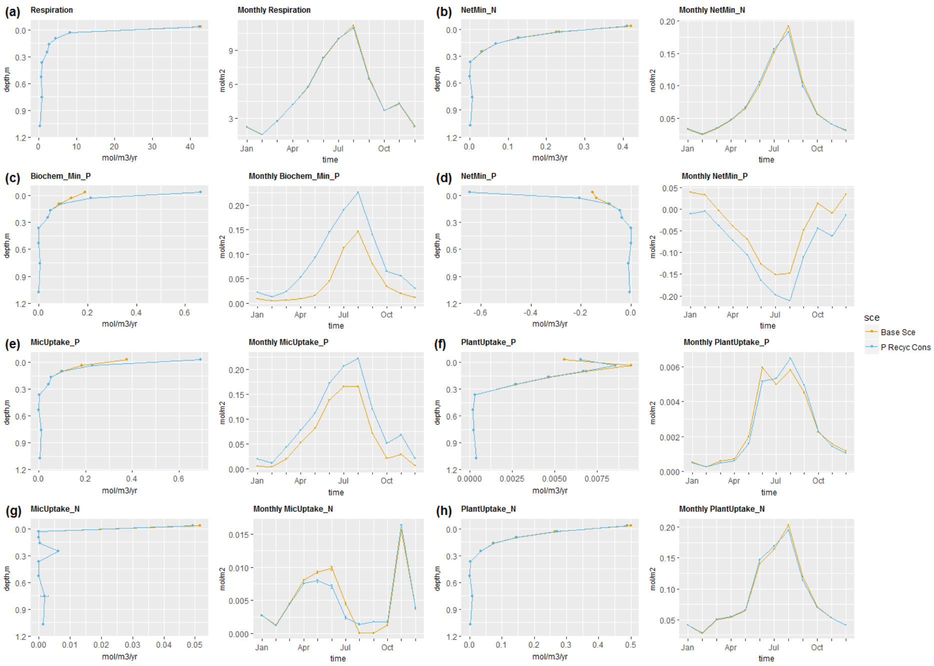
(a)
Respiration
Monthly Respiration
(b)
NetMin_N
Monthly NetMin_N
(c)
Biochem_Min_P
Monthly Biochem_Min_P
(d)
NetMin_P
Monthly NetMin_P
(e)
MicUptake_P
Monthly MicUptake_P
(f)
PlantUptake_P
Monthly PlantUptake_P
(g)
MicUptake_N
Monthly MicUptake_N
(h)
PlantUptake_N
Monthly PlantUptake_N
depth,m
mol/m3/yr
mol/m2
time
Jan
Apr
Jul
Oct
sce
Base Sce
P Recyc Cons

Fig. 2.