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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.

Anonymous Referee #2

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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.

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.

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.

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.

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.

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.

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

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”

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?

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.

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?

Page 8, line 10: What does “TW” mean?

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.

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.

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.

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.

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?

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

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

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

References:

Abramoff, R.Z., Davidson, E.A. & Finzi, A.C. (2017). A parsimonious modular approach to building a mechanistic belowground carbon and nitrogen model. J. Geophys. Res. Biogeosciences, 122, 2418–2434.

Huang, Y., Guenet, B., Ciais, P., Janssens, I.A., Soong, J.L., Wang, Y., et al. (2018). ORCHIMIC (v1.0), a microbe-mediated model for soil organic matter decomposition. Geosci. Model Dev., 11, 2111–2138.

Sulman, B.N., Brzostek, E.R., Medici, C., Shevliakova, E., Menge, D.N.L. & Phillips, R.P. (2017). Feedbacks between plant N demand and rhizosphere priming depend on

type of mycorrhizal association. Ecol. Lett., 20, 1043–1053.

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