

## ***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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### 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.

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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  $10^4$  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.

### 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

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consider:

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. 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? 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 to define these characteristics (especially with multiple elements and with depth).

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).

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#### Technical corrections

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

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).

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.

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

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

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

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.

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.

Fig. 7 Bottom panels of should be % modern. I also couldn't help but notice that you just have  $^{14}\text{C}$  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

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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.

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?

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).

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.

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)

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.

Page 9, line 5. Reference Fig 7 here?

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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.

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?

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.

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).  
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)?

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

References:

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