

Interactive comment on “Modeling anaerobic soil organic carbon decomposition in Arctic polygon tundra: insights into soil geochemical influences on carbon mineralization” by Jianqiu Zheng et al.

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This paper examines aerobic and anaerobic soil organic matter decomposition in the context of iron, and pH. This is an important contribution to the understanding of soil carbon dynamics in permafrost regions which hold vast reservoirs of carbon that could potentially be released under future climate change. Unfortunately, this manuscript has flow problems with substantial logical gaps between a traditional correlative analysis and the process rich model. More concerning is a lack of documentation on how the process rich model was developed, making the simulation results of this study unreproducible as is.

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This paper tries to do both a traditional regression/correlation style analysis and a non-linear process rich simulation. From what I can tell the traditional analysis is solid, although the lack analysis scripts make it difficult to evaluate. However, the connection to the process rich simulation is tenuous at best. In addition, I'm not clear how the data was incorporated into the simulation and how the simulations were validated with the data. I would consider splitting this into two papers, one with the traditional analysis and a second with the model development, parameterization, and validation. While this is not required it would make the manuscripts easier to write. As is there remains work needed on the flow and connection between these two components.

I would like to see some discussion of scaling of these microscale processes to macroscale models.

This study needs a lot more detail to make model development reproducible. The link to github code is a start but documentation is completely inadequate and lack of permanent DOI on the repository means that the codebase might not be there for future studies. The code needs to be commented with major algorithms summarized in functions. README needs instructions on running codebase with a summary of the content of each file. Alternatively this could be submitted as a markdown file with input-function-output format with inline comments explaining approach. Include version number for PHREEQC. Right now, I would not consider this study to be reproducible and it is difficult to evaluate the model results without this context.

I'm concerned that the authors both use a simple correlation analysis to argue for inclusion of various dependent variables in the proposed highly complex non-linear model. In particular, I would not have expected a strong correlation between moisture and SOC given the typical non-linear sensitivity function used to describe respiration response to moisture (though this is possibly explained by the range of moisture conditions considered). In addition, low correlations could be explained by non-linear responses. At the risk of adding yet another analysis to an already confusing study, I would suggest that instead the authors use a paired scatter plot to visually show the relationships between

these variables. This will demonstrate that there is no strong non-linear relationship and that the correlation coefficients are sufficient to describe the relationship.

Line by line reactions:

P1L23 While anaerobic decomposition certainly is missing from many ESMs, I'm not sure I would claim that it is the main driver for model uncertainty. There are several processes which could improve model performance that are currently being investigated and this tripped me up reading through the abstract.

P3L5 Models traditionally do however consider O₂ limitation with increasing moisture saturation. I'm almost certain that the authors are aware that traditional moisture sensitivity functions are typically rationalized to have decreasing decomposition under high moisture due to limited O₂ diffusion (Orchard and Cook 1983). What this typically does not extend to CH₄ emissions, it does implicitly include anaerobic decomposition. A review of implicit vs explicit process representation in decomposition models may be more appropriate here than an outright claim that anaerobic decomposition is not included in ESMs.

P4L28 60days is a short incubation to try to fit a full soils model to. I want to see concerns about time scale addressed somehow here.

P4L38 Why was the 4C dropped from the Q10 calculation??

P5L2 These package citations are less useful without the associated analysis script. Could this please be included in either the SI or as a separate DOI citation?

P5L12 Please be go into more detail on the adaptation of CLM here. Figure 2 is extremely useful but this could use more detail here or in the SI. I would urge the authors to restate the model formulation (even when explicitly drawing on previous work) since frequently it is not clear what portions were modified for the current model. Please include a set of full mathematical equations, descriptions appropriate algorithms, and a fully commented code base used to run the models.

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P6L5 Well that is certainly creative model initialization.

BGD

P6L22 'further adjusted' Could the authors clarify? Right now it reads as an 'expert tuned' model which is not current best practices given the range of parameter fitting tools that exist.

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P6L25 This feels like a very limited sensitivity analysis. An a priori 50% uncertainty seems to be a relatively tight bound for a soil model, especially given the 3 orders of magnitude that was mentioned previously.

P8L7 How was the model calibrated?

P9L18 Was this perturbation analysis done independently of the previous perturbations?

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