Biogeosciences Discussions

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Comment on bg-2021-108

Anonymous Referee #1

Referee comment on "Theoretical insights from upscaling Michaelis–Menten microbial dynamics in biogeochemical models: a dimensionless approach" by Chris H. Wilson and Stefan Gerber, Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-108-RC1, 2021

In this study, the authors proposed a dimension-less reformulation of the scale transition method to analyze the effect of spatial heterogeneity on the mean-field description of SOC dynamics by the Michaelis-Menten kinetics. They showed that the ratio between mean microbial biomass and mean half-saturation parameter of the Michaelis-Menten kinetics can serve as the characteristic parameter to measure the strength of spatial heterogeneity and the non-linearity of the SOC dynamics of interest. They argue that such a result can help both theoreticians and empiricists to better interpret observed soil C decomposition dynamics, specifically respiration in their case.

Overall, I think this is a well-attempted study, and is clearly reported. I have some moderate to major suggestions for the authors to further improve the manuscript.

Idea-wise, the paper is very similar to the study by Chakrawal et al. (2020), except that the presentation here is simplified by introducing a characteristic parameter and normalization of the variances by the mean field approximation (making the variance corrections dimension-less in the analysis). However, I think the authors ignored the fact that some of the spatial heterogeneity can be considered in the nonlinear kinetics by recognizing that decomposition is at least a two-step process: (1) microbes approach the substrate (or vice versa), and (2) microbes assimilate the substrate. Consequently, the nonlinearity with the Michaelis-Menten kinetics emerge from the combination of two linear steps, and one thus should not be surprised to see that the mean-field Michaelis-Menten kinetics cannot upscale robustly. As shown in Tang and Riley (2019), conceptualizing microbial substrate uptake as a two-step processes enables the half saturation parameter

to incorporate the spatial heterogeneity to some extent. I thus recommend the authors to clarify this.

Additionally, in the "lessons for scientific inference", I would suggest the authors trying to discuss the relationship between their analysis and the parametric sensitivity analysis or uncertainty analysis that are very popular in the modeling community. Apparently, what the authors presented here and also in Chakrawal et al. (2020) are closely related to approaches like global sensitivity analysis that is used to understand the influence of parametric uncertainty on model performance. Has the authors here just rediscovered the global sensitivity analysis in a new context? And what can sensitivity or uncertainty analysis learn from the authors' study? If properly made, I believe the paper will become much more interesting to general readers. Particularly, I don't see an easy way to apply the authors approach to a model that is presented as numerical code, which technically could involve tens if not hundreds of equations, e.g. for a decomposition model that involves tens of microbial populations that I am work with, I don't see how I can apply the authors' method.

Finally, how should one analyze the temporal heterogeneity blended with the spatial heterogeneity using the authors method?

Reference

Chakrawal, A., Herrmann, A.M., Koestel, J., Jarsjo, J., Nunan, N., Katterer, T., Manzoni, S., 2020. Dynamic upscaling of decomposition kinetics for carbon cycling models. Geoscientific Model Development 13, 1399-1429.

Tang, J.Y., Riley, W.J., 2019. A theory of effective microbial substrate affinity parameters in variably saturated soils and an example application to aerobic soil heterotrophic respiration. Journal of Geophysical Research-Biogeosciences 124, 918-940.