Interactive comment on "Dynamic upscaling of decomposition kinetics for carbon cycling models" by Chakrawal et al.

Referee: Ali Ebrahimi (MIT, alieb@mit.edu)

Summary

The manuscript entitled "Dynamic upscaling of decomposition kinetics for carbon cycling models" by Chakrawal et al. deals with upscaling the role of soil micro-scale structure and heterogeneity in microbial and carbon distributions to macroscale carbon cycle models. The authors developed a novel mathematical model based on 2D mesh grid system to account for microscale heterogeneities and they used scale transition theory to provide an analytical framework for decomposition kinetics at macro-scale. The idea of upscaling is highly novel and the author's work on trying to show that microscale heterogeneities could play a significant role in large scale carbon dynamics is interesting and informative. To extend, I could re- derive, the mathematical model is formulated correctly and it should be accessible to audience with average knowledge on mathematical modeling. While the model is advanced, I do have some major concerns on the relevance of the analytical model for large scale carbon dynamics and the ability of the model to represent real soil with empirically measurable quantities. The lack of systematic parameterizations based on such quantities, makes the current conclusions rather speculative and hard to translate to relevant environmental scenarios in natural ecosystems.

Detailed review

Major concerns:

I) it is unclear to what extend the parameterization proposed in the analytical kinetic model could be experimentally validated. My major problem is that some of the quantities do not have real biogeochemical or physical meanings in which could be experimentally measured. For instance there is an emphasis on the second order moments as state variables used to close the model; however it is hard to think how such variable could be experimentally measured.

II) The type of model and scenarios proposed in this study are relevant and could potentially address some of the inconsistency in our field measurements but it could only be possible if the model could establish a systematic link to relevant abiotic and biotic factors observed in the field. While in the discussion authors have tried to relate some of the scenarios in the study to soil aggregation or pore connectivity and an entire subsection is dedicated for that, I still find that the modeling framework is too abstract that makes the explanations quite speculative and hard to think to what extend the decomposition rate may vary under realistic settings.

III) The model could potentially describe some of the underlying abiotic and physiological mechanisms that shape the decomposition dynamics but in the current form of the manuscript this has not been explored. For instance, I was wondering to what extend half saturation to substrate and decomposition rate constant (KM and Ks) are shaping the dynamics observed in the model. I would guess if lower KM or high Ks would have been chosen the heterogeneous scenarios would have converged faster to the homogenous one.

Minor comments:

- I was wondering to what extend the fluctuating environmental condition (for instance fluctuating in carbon distributions) could play a role in shaping the carbon decomposition dynamics. Do you expect to see faster convergence to homogenous scenario in high intensity fluctuations?
- The system size that is modeled in grid based network is rather small. The number of grids, or pores equal to10^4, is basically enough to model an aggregate with the size of ~0.5mm. I was wondering if this size is sufficiently large to capture heterogeneities in the soil? For instance inter aggregate pores or macro-pores?
- Following up on the results for negative correlations, I was wondering how much physical inaccessibility of the carbon to microbes could be relevant for the soil systems? For instance most of the carbon protections in soil are often driven by soil aggregation and creation of anoxic microsites. In a broader term, the counter gradients created by carbon and other necessary substrates for carbon degradation could lead to inaccessibility of the carbon for microbes and not necessary physical inaccessibility. This is a phenomenon that has been previously shown in soil aggregates that due to creation of anoxic zones, the carbon configuration does not play a role in carbon consumption (Ebrahimi and Or, 2018 GCB) and in other studies showing carbon protection by aggregation (e.g., Keiluweit et al., 2017 Nat. comm.).

Recommendations

The current study aims to take an important step to propose upscaling strategy of carbon decomposition rates from microscale. This study opens up a direction for follow up studies to provide more realistic parameterization of large scale carbon models. I think the current model could be used to study many aspects of soil microbial processes at micro-scale and help to create hypothesis that could be addressed experimentally. This being said, the manuscript should use more focus on what the model could offer at the current form and the limitations and avoid speculations. The manuscript should also provide incremental steps toward better connecting the current model to measurable quantities and field observations.