

Interactive comment on “Modeling transient soil moisture limitations on microbial carbon respiration” by Yuchen Liu et al.

Anonymous Referee #3

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This manuscript describes the application of two models of differing complexity in an attempt to fit a soil incubation study. Overall, the scope of the work is suitable for biogeosciences, and the experimental analysis is robust. However, I'm not altogether convinced that the work is extensive enough to draw significant conclusions beyond that elucidated by the original Manzoni et al., study.

A significant problem with this (and other models expressly incorporating dormancy), is that measurements of the active vs. dormant vs. dead biomass are very difficult to make. Traditional measurements of biomass tend to measure anything that can be extracted through chloroform fumigation (regardless of the taxonomic identity), and molecular measurements remain open to interpretation. However, given the likelihood of equifinality in interpretation of this modeled experiment (and the limited data set

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presented here), it would seem critical to be able to understand whether the model fit improved because of dormancy within the experimental conditions. What about comparisons to other datasets that examine variably respiration in variably saturated soils?

Furthermore, I am find the manuscript quite difficult to follow. The unconventional layout is really quite confusing, and the discussion proceeds as an extended results section. The introduction is very good, but overall, I think a thorough reconsideration of the layout is in order.

Specific comments. Ln 112: This is quite an irrelevant description of microbial communities. It's an operational definition used for ease of characterization an generally derived for higher organisms. Yet microorganisms tend to be metabolically flexible, and realistically fall across a continuum from r- to k-strategist, with little accumulation at either end.

Ln 206: This is a pedantic point, but why write 'degrees celsius', as opposed to using symbols?

Ln 215: Why do the results appear here? Where is the model description?

Ln 262: These aren't really communities right? Just two 'organisms' with differing kinetic values for Michaelis-Mentoin terms and growth rates?

Ln 295: The comma after 'pathway' isn't required.

Ln 297: Why use this value for CUE? It seems far lower than that measured for microorganisms in soil (which I think is about 0.3).

Ln 328: should this be expressly?

Ln 377: A lot of this seems like results. There's very little discussion going on here.

Ln 392-4: i'd recommend rewriting this sentence, very difficult to follow

C2

Ln 900: It seems like this table would be better presented as a figure?

Ln 992: Where are the references for the values in this, and the following table? It's important to understand where these came from, and how similar the environment was to the soil under consideration here.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-10>, 2018.