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Comment on bg-2021-108

William Wieder (Referee)

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-RC2>, 2021

Summary

Wilson and Gerber present a thoughtful analysis on challenges related to scaling microbial dynamics to ecosystem scales. Their work takes a deep dive into the mathematics of these transition across scales. The work is well reasoned, well supported, and well written. My chief suggestion with this paper is to encourage the authors to take a step back from mathematical rigor of their analysis to connect their ideas more broadly with theories and measurements related to SOM turnover, persistence, and vulnerability. My major suggestions are aimed at making these suggestions

Major Suggestions

The mathematical focus on variability (especially related to substrates and microbes) that this paper explores seems connected to the more theoretical ideas in Schimel and Schaffer 2012 and Lehman et al 2020. I wonder if revisions can reach a broader audience by connecting the quantitative depth of this paper with these broader concepts?

Water seems like the big unknown here. If the collocation of microbes and substrates is largely dependent on liquid water availability (as well as SOM-mineral interaction) then high heterogeneity of soil moisture within sites seriously complicates the feasibility of

actually capturing the local scale heterogeneity for which the authors seem to be advocating. This is not a deal-breaker for publication, but it seems like a topic that could be discussed? More details in the minor comments below.

Line 230, this statement seems values for results in 1c, which converges at 0.9. This seem relevant, especially if one take home message from the text as presented is that 'a first order model is good enough'. This may be true, but what are the implication for having a scale correction factor that does not equal 1, even at large lambdas? Are the conditions required for this to occur plausible in natural systems?

Minor and Technical Comments

Throughout, I'd encourage the authors to be precise and consistent in their terminology for things like "scale transition correction", 'scale correction factor', 'mean field correction', etc. This will avoid confusion and help clarify the ideas in the text.

Line 54, I might add Wang et al. 2016 to this list.

Line 78, can the author's just write out process-based models throughout? There are enough acronyms in the text, removing this non-standard one that's sparingly used will aid readability.

Line 80, Although not related to trace gas fluxes, Bradford et al 2017, 2021 raise similar concerns related to litter decomposition rates. These reference are also good ones related to the major concern about soil moisture, above.

Line 89, remove 'enormous'

Line 90 remove 'universal'

Line 111 remove this duplicated equation, or number it?

Line 175, and potentially in the introduction (see Buchkowski et al 2017).

Line 210. This does seem like valuable insight. Notably, the variation in soil moisture (which I'd argue impacts substrate availability) is very high (Loescher et al. 2014). I wonder what implications this has for looking at flux estimates within and among sites?

Should the y-axes on fig 1 be held constant so that it's more obvious that at the CV(MB) increases the magnitude of the scale factor changes?

Also, should Fig 1 & 2 have the same y-axis label, they're both showing a Scale correction factor.

Line 235 maybe replace 'virtus' with something like 'benefit'

Line 255, microbial biomass is commonly used in models, but really it's the 'active' microbial biomass that matters here, which seems to be much more variable (and harder to quantify).

Section 3.2. This is a nice example for temperature, but given much higher variance in moisture, (again see Loescher et al 2014) it seems like the real environmental variance we need to care about within sites is moisture. Maybe this doesn't need a mathematical proof, but a brief discussion.

Line 321, this sounds ideal, but I wonder where this is ever going to occur for all the variables of relevance, again see Loescher et al (2014)?!

Extending a bit on the comment above I have additional thoughts, listed below. These are NOT intended to be stinging critiques of the work presented, but I would encourage the authors to discuss some of the more practical challenges that would be involved with putting the research plan they outline into place:

- SOC does not equal available C and microbial biomass does not equal active biomass.
- How do these measurements integrate with depth?

- How do we bridge the jump between the variability in soil and trying to infer heterotrophic respiration fluxes from NEE measured in the flux tower.
- Even if all this could be measured with enough fidelity at a single site how do we extend such insights to resolution of a grid cell in an ESM (nominally 1 degree or 100x100 km).
- The logic and math presented here is fascinating, and it does help prioritize the measurements that need to be taken, but I do wonder if it's realistic to make the measurements given existing technology & infrastructure?

Line 335. I'd push back a bit on this statement, because if the aim of these models is to faithfully capture soil flux measurements, then I'd assume there's not much benefit in using anything but a first order model. If, however, the aim of these models is to more broadly explore our theoretical understanding of microbial and soil controls over SOM persistence and vulnerabilities, then microbial explicit models may be useful (see Wieder et al. 2015).

I don't love presenting a new figure in the conclusion of a paper, but this is more of a stylistic comment than a serious critique of the work.

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