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Interactive comment on "Responses of two nonlinear microbial models to warming or increased carbon input" *by* Y. P. Wang et al.

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Since Dr. Moyano and Dr. Wutzler both mentioned my papers in their comments, I decide to reveal my name for this comment as well. Mathematically, I think the paper is correct. But I think more discussion is needed on why the authors focus on these two models, which was also pointed out by the other two reviewers. Specifically, the authors should elaborate more on the fundamental hypotheses that are built into these two models, particularly the assumptions that lead to the MM kinetics and the reverse MM kinetics. Are they close or consistent with the first principle based on reasoning? For instance, it should be mentioned that the reverse Michaelis-Menten kinetics is empirical

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and intuitively proposed (according to Prof. J. Schimel's comment on my ECA paper in GMDD). Therefore, like the Monod kinetics, which happens to be identical to the MM kinetics for a single-substrate-single-enzyme system, the relevance of their kinetic parameters to reality will be quite uncertain. Particularly, the two models as used in the authors' paper will assert the same microbe in different mineral soils as different microbes because of the different apparent half saturation coefficients one would infer through data-fusion. I would suggest the authors mention some of these caveats in their discussions.

Other comments: P14653, L22: Sulman et al. (2014) is not a correct reference here. I was lucky enough to have a discussion with him before his paper's publication. From my understanding, what he presented is not the reverse Michaelis-Menten kinetics. P14654: L 9-10: In explaining Eq. (8), I think it is important and also helpful to point out that Eq. (8) implies the soil carbon and litter carbon are taken in parallel by the microbes. However, I suspect this will be likely an incorrect approximation to reality given microbes usually have limited energy or surface area to support and carry substrate transporters. This has been the major motivation for our proposal of the ECA kinetics.

L11-L16: I also agree with the other two reviewers. The reverse is true. L18: $F_{npp} > 0$ should be used.

P14657: L3-5: please comment on the choice of exponential function to represent temperature sensitivity. Many studies indicate such temperature sensitivity is not exponential (Balser and Wixon, 2009), and we (Tang and Riley, 2015; NCC) and also Grant (2014) found this could make huge difference in model predictions.

P14658: L1-5: I found this sentence hard to comprehend, please consider breaking it down into two to make it more readable. L12: first time mention tropical forest site, please cite.

L15-19: This sentence also reads awkward.

BGD

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P14660: The section title “Minimum soil carbon temperature” is quite ambiguous. Please consider revision.

P14663: L14-15: I suggest replacing “Michaelis-Menten kinetics” with substrate kinetics to avoid confusion.

Figure 3: Please explain what do the black regions mean in the text.

Figure 6: Panel (c) is very hard to understand when combined with the discussion in the main text. Please clarify it is solely for model A or model B, which I think is for model A.

Reference

Balser and Wixon, 2009, Investigating biological control over soil carbon temperature sensitivity

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Interactive comment on Biogeosciences Discuss., 12, 14647, 2015.