

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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Responses to review by Dr Tang

Many thanks for the constructive comments. We have responded to all points raised by the reviewer. All line numbers are those of the manuscript with tracked changes (attached as supplemental material in pdf)

(C1) 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

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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 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. Response: We have added some text in the Model description and Discussion. See L138-141, L191-198, L588-590. However we did not include the assumptions made for each kinetics, as this has been nicely done in Tang (2015), and discussion of those assumptions will likely need to include quite a few new concepts, which will distract the readers about the key message of this paper.

(C2) 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. Response: Agreed and the reference is deleted. See L179.

(C3) 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. Response: This is now stated. See L192-193.

(C4) L11-L16: I also agree with the other two reviewers. The reverse is true. L18: Fnpp

> 0 should be used. Response: Agreed and revised. See L204.

(C5) 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. Response: We have discussed this limitation, and cited the work by Tang and Riley (2015) (see L574-577) and Grant (2014) (see L572). While the work by Balser and Wilxon (2009) is very interesting, we think that their results have yet to be evaluated using a wide range of field observations. On the other hand, many studies found that the modified Arrhenius function can fit very well the observed temperature responses of soil respiration. Furthermore, the derivation of our analytic approximation does not depend on the exact form of the temperature dependence of all model parameters. We agree the quantitative differences between the two models as shown in this study can be different, those differences can be readily recomputed using our analytic approximation.

(C6) 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. Response: Agreed and revised. See L262-274.

(C7) L15-19: This sentence also reads awkward. Response: Agreed and revised. See L277-285.

(C8)P14660: The section title "Minimum soil carbon temperature" is quite ambiguous. Please consider revision. Response: We have revised the title to "Response of soil carbon to warming", and avoid using the term of "minimum soil carbon temperature". See revised text L362, L352-365.

(C9) P14663: L14-15: I suggest replacing "Michaelis-Menten kinetics" with substrate kinetics to avoid confusion. Response: Agreed and revised. See L423.

(C10) Figure 3: Please explain what do the black regions mean in the text. Response:

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The black region in Figure 3b represents Tx <0oC. This has been added to the legend.

(C11) 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. Response: Agreed. We revised the text (see L476-486) and figure legend.

Please also note the supplement to this comment: http://www.biogeosciences-discuss.net/12/C8258/2015/bgd-12-C8258-2015supplement.pdf

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