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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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Received and published: 4 December 2015

Responses to review by Dr Moyano

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)

(B1) The study looks at differences related to different forms of reaction kinetics and makes an analysis of mathematical properties mainly related to the oscillation and equilibrium behavior of the models. Although the analysis seems correct (trusting that

the mathematics behind it are correct), the justification of comparing the regular or inverse Michaelis-Menten equation as a focus of the study is not clear. Response: Two types of kinetics are most commonly used in nonlinear soil microbial models, this has been given in the introduction. In revision, we added new text to explain why a more general form of kinetic equation was not chosen (see L138-141). (B2) The authors should explain better why they chose to compare the two versions of the Michaelis-Menten equations, apart from it being used in past models. They should consider from a theoretical point of view what version may be more appropriate, and they should also use the conclusions to consider what insights with respect to using those equations the study provides. Response: Explaining well the theoretical background of both types of kinetics will require a significant amount of new text, which may distract the readers from the key message of this paper. Background of these two models have been given in Tang (2015). So we largely refer the readers to Tang (2015) about the fundamental assumptions made in each model (see L139). We have also stated the insight about two models from our analysis. See L532-557.

(B3) Since the analysis focuses on the MM equations, checking the shape of the response for the given parameter values (mainly of K) seems important. Response: I understand the reviewer's concern about the magnitude of K in Michaelis-Menten kinetics. In practice, the exact meaning of K is difficult to define when MM or reverse MM are applied to the observed soil carbon decomposition. The values of K we used are fully consistent with those used by Allison et al. (2010) and Hagerty et al. (2014) (see our Table 1).

(B4) Generally, some concepts and terms used in the model and the paper should be better defined and discussed. These are addressed in the comments below. Response: See our responses below.

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(B5) P 14651, L12 delete 'effect' in priming effect [priming can be seen as an effect or

as a mechanism, but the concepts should be separated. In this case the mechanism is being referred to]. Response: Agreed. "effect" is deleted. See L98.

(B6) P 14653, Eq. 1-2 ïĄ▪ in model A is defined as a turnover rate, which would be the case in a first order mode but not in this case where the turnover also depends on a second substance. As a reaction rate modifier, it seems to be conceptually equal to V in model B. This similarity should be better reflected in the model descriptions. Response: We now use rate constant for ïĄ▪ for being consistent with previous studies (Schimel and Weinstraub 2003; and Allison et al. 2010). See L167.

(B7) P 14653, L15-22 Cm is not set to steady state. It is simply ignored and uptake is made equal to the sum of decomposition fluxes, thus ignoring the saturation effect. This may be a justified assumption, but the paragraph is conceptually incorrect and should be corrected. Response: Disagree. Eqn (5) can be derived by setting $dCm/dt=0$. No change is made.

(B8) P 14654, L1-2 Please explain how the models differ from that of Allison et al. (2010) since the enzyme pool has been excluded. Response: The difference is now stated (see L181-182). We think the differences are small for the same parameter values at time scales >1 year, as stated in L172.

(B9) L11-16 In agreement with the comments by Wutzler, I find that the authors' interpretation is incorrect. In both model, both the substrate and the biomass are limiting the reaction, while the response is asymptotic for enzymes in model A and for substrate in model B. Response: Agreed and revised. See L191-198 and our response to A1.

(B10) Using the regular or reverse Michaelis-Menten specifies how the relationship for each will be. Response: Not sure what the reviewer meant here. No change was made.

(B11) It should be noted however, that the nonlinear response depends on the value of the parameter K, and if K is very large compared to the saturating substance then

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both equations are equivalent in practice (Here again we come to the fact that $\ddot{\imath}A_\blacksquare$ and V are comparable). Because of the focus of this paper on the MM equation and the dependence on the value of K, it should be clearer in the paper how these values are affecting the fluxes. For example KIR and KsR seem quite large and may not lead to a nonlinear response under normal conditions. KbR is probably in an observable range but should also be assessed. Response: Table 1 lists the values of model parameters. $\ddot{\imath}\breve{A}\ddot{a}\ddot{\imath}A_\blacksquare$ and v are very different in magnitude for both soil and litter carbon in our model, and values of Ks are much larger than the substrate pool sizes for litter or soil carbon, and nonlinear dynamics is simulated (see Figure 2 and Wang et al. 2014 for model B). Furthermore, we provided analytic approximations that can be used for any parameter values. No change was made here.

(B12) Finally, the assumption that a second order reaction is always linear with respect to one substance, as happens in both versions of the MM equation, will be unrealistic under certain scenarios. The authors should consider this in the discussion section and discuss under which conditions one or the other equation would make sense, if in any at all. Tang and Riley (2013, Total quasi-steady-state: : :, BG) discuss such reactions in more detail. (see eq. 14). This paper should be mentioned and cited. Response: See our responses to C1 and C3.

(B13) L11 The phrase 'from soil carbon decomposition' is confusing, as litter may also be considered a soil carbon pool. Here and in the rest of the paper 'from the Cs pool' would be better. Response: Disagree. I think that soil carbon is more compatible with the convention used in the published papers. No change was made.

(B14) P 14659, L9 maybe specify rather that the combination of low temperature and high input is not common (since many soils are under 10degC). Response: Agreed. Changed. See L302-303.

(B15) P 14662, L8 Soil moisture was used as input but no information is given as how it was used in the model. Response: Soil moisture was not used in our model. Deleted.

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See L389.

(B16) P 14666, L10-17 'the minimum soil carbon temperature' I guess is the temperature at which soil carbon is minimum. Define this at some point and rather use a symbol or abbreviation afterwards, as the expression used is rather confusing. Also please rewrite this section making it clearer and avoiding grammar/spelling errors. Also "values that were used'. Response: Agreed. This section has been reorganised. Delete "that". See L507-517, L518.

(B17) P 14669, The third paragraph in conclusions seems to much like a recapitulation of the results. As in the abstract, the authors should try to shortly conclude on the differences without going into specific detail. Response: Agreed and revised. See L336, 351-352, and 355-373.

âĂŤâĂŤâĂŤâĂŤâĂŤ Technical Remarks and Corrections (B18) P 14649, L8-16 Rewording needed: "Using a combination of ... we find that: 1. after a small perturbation in the initial pool sizes, the oscillatory response of carbon pools has a higher frequency and dampens faster in model A than in model B; 2. in response to warming, soil : : : in model A but likely: : :; 3. after increasing litter input, : : : [Here from line 12 to 18 please change the abstract. The terms used and relationships explained cannot be understood before reading the paper and the abstract should be understandable by itself. Response: The abstract has been revised. See L25-47.

(B19) Fmax, "soil carbon" and the experimental setup could be more carefully defined here, but I would make the abstract less detailed, such as: "after an increase in inputs we found that the response of : : : depends on soil temperature, but this response differs between the models, being such in model A and such in model B"] Response: See our response to B18 and revised abstract L37-40.

(B20) P 14650, L19 why the decomposition L21, known as priming effect L22 cannot be. Response: Disagree. Kuzyakove et al. (2000) stated: "In studies of C turnover the definition is supported that the priming effect (Fig. 1a) is an extra decomposition of

organic C after addition of easily-decomposable organic sub-stances to the soil (Dalenberg and Jager, 1989).". on p1486, first paragraph, right column.

(B21) P 14651, L18 kinetics, in which therefore. Response: Here we simply stated that two different kinetics have been used in previous studies. No change was made here.

(B22) P14653, L 9 with the fraction a going to the soil and (1-a) going to the ... Response: Agreed and changed. See L166.

(B23) L15 scales Response: Agreed and changed. See L172.

(B24) L 17 the dynamics. Response: Agreed and changed. See L174.

(B25) P 14654, L 15 the external environment Response: This sentence has been deleted.

(B26) L 18 > instead of < ? Response: Agreed and changed. See L204.

(B27) P 14658, L4-8 This sentence is convoluted and has grammar errors. I could not follow the meaning. Please re-phrase. Response: Response can be any combination of stable/unstable, monotonic/oscillatory. Changed. See L297-298.

(B28) Fmax should be better defined or introduced later. L8 an analytic solution was L19 The response L22 of both models L24 an equilibrium state. Response: Agreed. See LL266-270.

(B29) P 14659,L1 case, Response: Agreed and changed to "in this study". See L294.

(B30) L2 , but not forestry Response: We cannot find word "forestry" in the whole manuscript. No change is made.

(B31) L25 After 20 years, both the : : : Response: Agreed and changed. See L320..

(B32) P 14660, L2 dampens (not damps). And in the rest of the paper: dampens, dampened Response: Agreed and changed throughout the manuscript.

(B33) P 14661, L27-28 Should mention the removal treatment first. Response: Agreed

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and changed. See L381-383.

(B34) P 14664, L15 effect by measuring the L16 delete: from the primed treatment L24 delete: after t=0 Response: Agreed and changed. See L451, L452.

(B35) P 14667, L18 The two models L25 a change in carbon input L26 and the sensitivities : : : soil temperature differ between the models, : : : Response: Agreed and changed. See L553, L554,L560, L562-563.

(B36) P 14670,L1 2x degC ? Response: This sentence is now deleted.

(B37) P 14673, L14 . As a result, the second: : : Response: Agreed and changed. See L840.

(B38) P 14674,L10 maximum Response: Agreed and changed. See L857.

(B39) P 14675, commas and spaces missing between equations Response: Agreed and changed. See L882.

(B40) P 14676, L12 that at t = : : : (?) Response: Agreed and changed. See L897.

(B41) Please rewrite caption of figure 3. It has grammar mistakes and is generally not clear. Can a legend title be added in the plot? Response: We rename the plots as (A) and (B) to avoid confusion with the parameter symbols.

(B42) In general, make the plot letter case (caps or not) be same as the reference in the caption. Response: Checked. They are consistent.

(B43) There were many grammatical errors and some missing or wrong wordings, not all of which I marked in the comments. There are several cases of wrong singular-plural. Articles are missing in many cases. Please note that correcting such mistakes not only makes the text look better, it makes it much easier to read also. Co-authors should take the time to go through the text and make those adjustments, so that reviewers and readers have an easier time focusing on the science. Response: Checked.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C8250/2015/bgd-12-C8250-2015-supplement.pdf

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