Biogeosciences Discuss., 12, C6844–C6850, 2015
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Biogeosciences Discussions

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: 21 October 2015

General Comments

Wang et al. present a modeling study where two nonlinear models are compared under different scenarios and also against field data. Studies of this type are valuable and required for the improvement of the rapidly evolving soil carbon models. The analysis of the mathematical properties and the comparison against measurements are both helpful and add to the available knowledge that, as the authors say, will help in choosing the more realistic models and make experiments to test them.

The study looks at differences related to different forms of reaction kinetics and makes

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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.

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.

Since the analysis focuses on the MM equations, checking the shape of the response for the given parameter values (mainly of K) seems important.

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.

———————— Specific Comments

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]

P 14653

Eq. 1-2 μ 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.

P 14653

L15-22 Cm is not set to steady state. It is simply ignored and uptake is made equal to

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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.

P 14654

L1-2 Please explain how the models differ from that of Allison et al. (2010) since the enzyme pool has been excluded. 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. Using the regular or reverse Michaelis-Menten specifies how the relationship for each will be. 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 both equations are equivalent in practice (Here again we come to the fact that μ 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 KlR and KsR seem quite large and may not lead to a non linear response under normal conditions. KbR is probably in an observable range but should also be assessed. 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. 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.

P 14659

L9 maybe specify rather that the combination of low temperature and high input is not

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common (since many soils are under 10degC)

P 14662

L8 Soil moisture was used as input but no information is given as how it was used in the model.

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 re-write this section making it clearer and avoiding grammar/spelling errors. Also "values that were used'.

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.

———————— Technical Remarks and Corrections

P 14649

L8-16 Re-wording 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. 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"]

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P 14650

L19 why the decomposition L21 , known as priming effect L22 cannot be

P 14651

L18 kinetics, in which therefore

P14653

L 9 with the fraction a going to the soil and (1-a) going to the ...

L15 scales L 17 the dynamics

P 14654

L 15 the external environment L 18 > instead of < ?

P 14658

L4-8 This sentence is convoluted and has grammar errors. I could not follow the meaning. Please re-phrase. Fmax should be better defined or introduced later. L8 an analytic solution was L19 The response L22 of both models L24 an equilibrium state

P 14659

L1 case, L2 , but not forestry L25 After 20 years, both the . . .

P 14660

L2 dampens (not damps). And in the rest of the paper: dampens, dampened

P 14661

L27-28 Should mention the removal treatment first.

P 14664

L15 effect by measuring the L16 delete: from the primed treatment L24 delete: after

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t=0

P 14667

L18 The two models L25 a change in carbon input L26 and the sensitivities . . . soil temperature differ between the models, . . .

P 14670

L1 2x degC ?

P 14673

L14 . As a result, the second. . .

P 14674

L10 maximum

P 14675

commas and spaces missing between equations

P 14676

L12 that at t = . . . (?)

Please rewrite caption of figure 3. It has grammar mistakes and is generally not clear. Can a legend title be added in the plot?

In general, make the plot letter case (caps or not) be same as the reference in the caption.

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

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and readers have an easier time focusing on the science.

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