

## ***Interactive comment on “Oscillatory behavior of two nonlinear microbial models of soil carbon decomposition” by Y. P. Wang et al.***

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We appreciate the constructive comments by reviewer 2. We have revised the manuscript by carefully taking account of all comments. (C1) Your analysis suggested that non-linear models are more sensitive to change in soil carbon inputs than occurs in nature. This made me wonder how environmental biotic and abiotic factors (e.g. seasonality, change in substrate composition and/or decomposer community, priming effects, etc.) would modulate the sensitivity observed in this highly sensitive model system. Although this was briefly touched upon in the discussion, I think that the authors could have expanded upon this substantially. Reply. Our analysis suggests that the pool size of SOC in the nonlinear soil model is insensitive (not more sensitive) to carbon input, and is unrealistic. We have now included evidence supporting the insen-

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sitivity of the nonlinear models, therefore a more balanced view is presented (see new text on L459-464). We also made it clearer that this study is focused on the intrinsic properties of the system and their causes for the oscillatory responses. We do not study the responses to periodic forcing here (see L84-87).

(C2) I would have liked to see more citations referencing studies connecting warming to empirically observed effects on soil carbon/microbial community/substrate effects; several times in the paper there were statements that I felt were not backed up with references or could have been more thoroughly discussed (e.g. “For example, it is well known that a system of nonlinear ordinary differential equations, such as a nonlinear soil model, can become unstable in response to a small perturbation to its initial pool sizes or inputs and can have multiple equilibria (Drazin, 1992), although there is presently no evidence that soil carbon dynamics exhibits such characteristics over interannual to decadal timescales.” Citation for latter half of the statement? Reply: We have replaced “inputs and can switch between different equilibrium states in response to climatic variation (Manzoni and Porporato 2007), although there is presently no evidence that soil carbon dynamics exhibits such characteristics over interannual to decadal timescales”. (see revised text on L77-80)

(C3) Also, what is the citation for: “The shifted microbial community toward more fungi and less bacteria under warming than control is expected to decrease carbon use efficiency.” Missing references occurred throughout the paper and grammar could be strengthened). Reply: This sentence has been rewritten as “The shifted microbial community toward more fungi and less bacteria under warming than control was found to be the dominant driver of warming acclimatization of soil respiration (Zhang et al. 2005)” (see revised text on L477-479).

(C4) A more comprehensive interpretation of how linear and non-linear models could be used to interpret why NPP and soil carbon accumulation are or are not decoupled from each other in different ecosystems would be a great addition to the paper. Reply: Agreed. We have added some text about the way forward in comparing the responses

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of linear and nonlinear models to carbon input (see L493-514).

(C5) Further, I think that the paper could be improved by more discussion regarding what empirical observations and/or additional processes would be useful in parameterizing/improving non-linear, microbially explicit models. Reply: Agreed. See the new text on L493-514.

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