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Interactive comment on “Integrating microbial physiology and physiochemical principles in soils with the Microbial-MIneral Carbon Stabilization (MIMICS) model” by W. R. Wieder et al.

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This manuscript presents an analysis of the consequences of including two types of microorganism physiology on an SOM decomposition model. This is an interesting theoretical exercise about the potential effects of carbon storage in a global biogeochemistry model (CLM). I found the analysis compelling and of interest for a general audience, therefore I recommend publication in *Biogeosciences* after some minor changes.

After reading the manuscript however, I was left with more questions than answers, which I believe is good to stimulate discussions about representing microbial processes

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in global biogeochemistry models. The general comments below identify some issues and give suggestions to improve the manuscript.

General comments

- *Equifinality.* Figure 2 shows the agreement of model predictions with litter decomposition data from two LIDET time-series. Other studies using linear donor-control models produce equally good predictions with this dataset (e.g. Tuomi et al. 2009, Forney et al. 2012), so the calibration and confirmation of model results with this dataset does not provide any additional evidence that the model presented here performs significantly better than other simpler models previously proposed. I believe that to show a better predicting capacity of this model, the authors should show additional data where this model performs significantly better than others. For example, for Harvard Forest, a linear model cannot predict sufficiently well the effects of warming and N addition on soil respiration (Sierra et al. 2012). This could be an interesting additional test for the MIMICS model, although other tests could also be implemented. My point here is that in the current presentation, the manuscript does not provide convincing evidence that this model gives better predicting ability than previous models.
- *Validation with microbial data.* One of the main contributions of this manuscript is the explicit representation of copiotrophic and oligotrophic growth strategies of microorganisms; however, the manuscript does not present any data showing how model predictions satisfactorily represent growth of these type of microorganisms. Is it possible to show data on biomass or respiration of these two functional types and how the model performs? Furthermore, what are your thoughts on calibrating and validating this model at the global scale using data on microbial functional characteristics.

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- *Oscillations*. Figure 2 clearly shows oscillations in the model predictions, but it is unclear whether these oscillations are due to the nonlinear nature of the model or the climate time-series used to run the model. It is very likely that the oscillations are caused by the nonlinear Michaelis-Menten terms included in the model, and I think the authors should discuss this property of the model in more detail. The manuscript would be highly improved if the authors perform a stability analysis showing that the oscillations arise due model structure (similarly as in Manzoni & Porporato 2007, or Wang et al. 2013). Alternatively, the authors can just show a simulation with constant climatic variables indirectly showing that the oscillations are due to the model structure and not due to the daily climatology used.

Recently, Wang et al. (2013) argued that oscillations in nonlinear models are an indication of unrealistic model structures. I was puzzled by the fact that the first author of this manuscript is also a coauthor of Wang et al. (2013), which gives a sense of contradiction. On one hand, the use of nonlinear models are discouraged in Wang et al. (2013), and on the other hand a model with even a larger number of nonlinear terms is advocated in this manuscript. I think readers on this topic deserve a more elaborated discussion about how realistic is the oscillatory behaviour of this model and why it somehow contradicts the ideas presented in Wang et al. (2013).

- *Moisture control*. I think it is now clear that soil moisture exerts a strong control on microbial activity. I would suggest adding a moisture term in addition to the temperature term in equation (2). You can take as example the models presented either in Davidson et al. (2012) or Moyano et al. (2013).

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Technical comments

- Page 1155, lines 22-24. Is there a control to physical and chemical protection? The maximum amount of SOM that can be stored in these pools is controlled by the V_{max} parameters, but more realistically they depend on pH, soil texture, and mineralogy (see Gu et al. 1994, Mayes et al. 2012).
- Page 1164, second paragraph starting at line 7. I do not agree with the statement that in traditional (linear) models increases in quality always lead to declines in C storage and larger partitioning to fast pools. This behaviour is not a property of the structure of the model, but rather a consequence of the parameter value used for the coefficients that determine the partitioning to respiration and transfers to other pools (Bolker et al. 1998, Manzoni et al. 2009). In other words, the behaviour discussed here is not a consequence of a special characteristic of MIMICS, but rather the result of the parameterisation used.
- Figure 4, panel b. The symbols in this graph look too close to each other. Can you use lines instead?

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