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

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Thank you for this timely analysis of the properties of microbial versus conventional models. Such efforts are important for refining our models of soil carbon cycling at the global scale. This analysis emphasizes some underexplored features of microbial models that need to be considered before relying on their predictions at large scales.

I wanted to pose two questions regarding the oscillations and sensitivity to soil carbon inputs in the microbial models. It does seem unrealistic that the microbial models are not sensitive to inputs; I was perplexed by this result as well in dealing with these models. However, I wonder if the observational data require closer examination. You cite some examples of studies where inputs change and soil carbon changes, but aren't there also examples where they are decoupled? For example, several FACE studies

have found changes in NPP but little measurable change in soil carbon pools (e.g. Schlesinger and Lichter 2001). Perhaps not enough time passed to detect a change, but an alternative is that microbial priming effects decouple the response of inputs and soil carbon. Priming is another phenomenon consistent with microbial models but difficult to replicate in conventional models.

At the global scale, we have found some surprising disconnects between inputs and soil carbon. Todd-Brown et al.'s 2013 analysis showed that soil carbon in conventional models essentially increases with NPP and declines with temperature-driven decomposition. However, when we tried to use the same underlying model structure to re-create the observed spatial distribution of soil carbon around the globe, the conventional paradigm performed poorly, explaining only ~10% of the spatial variation (see Table 4 in Todd-Brown et al. 2013). In looking at these observations, there are many locations where soil carbon is high and NPP is low (arctic tundra) and vice versa (tropical rainforests). Part of the disconnect is due to temperature effects on carbon turnover, but not all of it. Clearly there must be some relationship between inputs and soil carbon stocks, but it appears this relationship could actually be quite weak at local to global scales.

Oscillations were an important and unique feature of the microbial models. Empirical evidence for such oscillations is scant, although it's possible that few investigators have thought to look for oscillations. I would also like to suggest that oscillatory behavior might weaken with greater pool heterogeneity in the microbial models. The oscillations arise because of tight coupling between microbial and soil carbon pools, yet tight coupling is likely to be rare in real soils. There are many organisms consuming chemically heterogeneous substrates on varying timescales. Such heterogeneity could smooth out oscillations. Of course, that means the existing microbial models are wrong because they are too simple. Making them more complex might fit the data better, but if they start to converge on the conventional models, the additional complexity may be unnecessary. These possibilities clearly require more empirical and theoretical attention.

References: Schlesinger, W. H. & Lichter, J. Limited carbon storage in soils and litter of experimental forest plots under increased atmospheric CO2. (2001). Nature 411, 466–469.

Todd-Brown, K. E. O., Hoffman, F. M., Post, W. M., Randerson, J. T. & Allison, S. D. (2013). Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparisons with observations. Biogeosciences 10: 1717–1736.

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