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Interactive comment on “Priming and substrate quality interactions in soil organic matter models”

by T. Wutzler and M. Reichstein

Anonymous Referee #1

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The manuscript “Priming and substrate quality interactions in soil organic matter models” addresses the interesting question of how different models of priming effect impact the simulated dynamics of soil C. The topic is timely, as biogeochemical models strive to achieve a better mechanistic description of the decomposition process. This work may be of interest for a wide audience, and it is certainly within the scope of Biogeosciences. I have one main issue with the proposed work: priming is modeled (in some of the proposed variants) by adding nonlinear interactions between compartments, instead of focusing on the mechanisms of the priming effect, that is, stimulation of microbial activity resulting in faster decomposition. I also have several specific comments and suggestions. Moreover, previous works already addressing similar questions and using similar modeling approaches have not been discussed in depth (see specific comments below).

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The authors state that “In this paper we rather discuss the underlying interactions between SOM qualities instead of priming effects directly” (P 17181). This approach creates a bit of confusion, as priming improves decomposition thanks to the enhanced activity or turnover of the microbial biomass (Kuzyakov et al, 2000, SBB). I like the idea of assuming steady state biomass to assess long-term SOM dynamics. If the mathematical description of microbial biomass is sound, and priming is correctly modeled in the microbial explicit models, then I would expect that also when microbes are assumed at steady state, priming effects would be accounted for ‘naturally’, without adding nonlinear interactions (substrate limitations) to the model. I see the substrate limitation functions (e.g., equation 1) as ‘a posteriori’ modifications, and not the result of the simplifications of a process-based model. I would suggest examining instead the effect of priming based on different formulations of microbial effects on decomposition, with and without the steady state assumption.

Specific comments: P 17168, L 19: this sentence overstates the novelty of the ‘discovery’ of microbial biomass role in decomposition. In fact nonlinear models of decomposition that account for the role of microbes have been proposed since the 1970s (e.g., Parnas 1975, Smith 1979, McGill et al. 1981), borrowing concepts and theories from microbiology. P 17169, L 7: suggested change “they neglect substrate quality interactions.” P 17170, L 7: “lower” instead of “less” Section 1.1.1: the model described here is quite similar to previously proposed ones, which would be useful to cite (e.g., Schimel and Weintraub 2003). P 17171, L 19: based on the equation for assimilable OM, $u=\text{summation of } d_J+p_f*t$ (microbial turnover is missing in the reported equation) P 17173, L 27: using the term “microbial efficiency” in this context might create confusion. I agree with what is said, from a mathematical perspective, but it could be misleading. Please explain a bit more in depth P 17174: the equations here are explained very quickly, without hinting too much at how they have been derived. Basically, if I understand correctly, the limitation function “l” in equation 1 is defined as the ratio of actual to potential growth. Assuming microbial steady state, actual uptake equals $u_{\text{pot-a}}/A$, leading to the reported equation. I would explain a bit better the rationale

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leading to this formula. Also the following equation (not numbered) needs more explanations along the same lines P 17176, L 12: suggested change “In contrast, the model...” P 17177: the quasi-steady state assumption has also been employed in earlier models (Ågren and Bosatta 1998), and it is an implicit assumption for all first-order kinetics models of decomposition. Perhaps in this section the possible role of environmental fluctuations (e.g., soil water, temperature) could be mentioned. The proposed model implicitly assumes that these fluctuations will not keep microbes ‘too far’ from their quasi steady state. Is this a reasonable assumption? P 17177, L 20: “very effective” with respect to another model, not compared to data. Please be clear about this. P 17179, P 18: suggested change “In contrast, with the substrate interaction...” P 17179: simple models assuming that the decomposition rate depends on the product of biomass and SOM predict no changes in steady state C stocks with increasing input (Wutzler et al., 2008, Biogeosciences), without requiring substrate limitation functions on top of the multiplicative microbial-substrate interaction. Section 4.5: I do not see how this discussion fits in the rest of the manuscript P 17180, L 25: only if microbial turnover is slow... Section 4.7: most of the concluding remarks are not very novel. Much work on model structure (including data comparisons) have been done – though often not in a systematic way Appendix A: perhaps building a large table to report inputs and outputs of the various pools for the different models would be best. Models would be in the columns and fluxes, $d(\text{pool})/dt$ definitions and other parameterization information could be reported in the rows. This would make the comparison across models much easier for the readers. A similar approach could be adopted for Appendix B. P 17187, L 8: $1 (40 \text{ yr})^{-1}$ means $1/40 \text{ yr}^{-1}$? P 17187, L 16: why this choice – seems arbitrary? P 17188, L 11: ICBM means “independent” here?

References: Ågren, G. I. and E. Bosatta. 1998. Theoretical ecosystem ecology. Understanding element cycles. Cambridge University Press, Cambridge, United Kingdom. McGill, W. B., H. W. Hunt, R. G. Woodmansee, and J. O. Reuss. 1981. Phoenix, a model of the dynamics of carbon and nitrogen in grassland soils. Pages 49-115 in F. E. Clark and T. Rosswall, editors. Terrestrial nitrogen cycles. Processes, ecosys-



tem strategies and management impacts. Ecological Bullettins, Stockholm. Parnas, H. 1975. Model for decomposition of organic material by microorganisms. *Soil Biology & Biochemistry* 7:161-169. Schimel, J. P. and M. N. Weintraub. 2003. The implications of exoenzyme activity on microbial carbon and nitrogen limitation in soil: a theoretical model. *Soil Biology & Biochemistry* 35:549-563. Smith, O. L. 1979. Analytical Model of the Decomposition of Soil Organic-Matter. *Soil Biology & Biochemistry* 11:585-606.

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