

Interactive comment on “Colimitation of decomposition by substrate and decomposers – a comparison of model formulations” by T. Wutzler and M. Reichstein

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Review of BGC-5-163-2008: "Colimitation of decomposition by substrate and decomposers: a comparison of model formulations", by T. Wutzler and M. Reichstein

The paper describes a collection of descriptions of micro-organisms in existing soil organic matter models, grouped according to formulation depending upon whether the decomposer biomass affects decomposition, either described as non-explicit (substrate only), b) linear or c) non-linear. The formulations are then implemented in a simple decomposition model, and the behaviour of the formulations under different conditions is examined. The paper is clear and well-written, and the analysis thorough

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and informative.

I think the paper is useful and allows the consequences of different formulations to be examined free from the other confounding factors present in different models. A similar approach has been applied before using frameworks such as MOTOR (Whitmore, 2007). The only danger of such an approach, which I do not think is a problem here, is that essential aspects of model function can be lost when stripping out formulations of individual processes. Related processes in the original model may interact strongly with the process description being studied, and the effect of these related process could be missed by the approach taken.

The paper shows that certain formulations were unable to represent different processes, with non-explicit descriptions unable to simulate the priming effect and linear relationships arriving at steady state independently of litter input. The authors find that certain non-linear relationships between the decomposer biomass and decomposition rate could simulate a portion of SOM that was not decomposed when no fresh OM was supplied, consistent with the need for fresh organic matter to decompose older SOM (Fontaine et al., 2007).

The authors call for a collaborative effort by modellers and experimentalists to identify "appropriate and inappropriate model formulations". I would caution against labelling model formulations as inappropriate since appropriateness depends upon the function required of the model. Certain models may be adequate for the desired purpose without including all processes known to science. Model parsimony should be sacrificed only where necessary. In my opinion, fitness-for-purpose is a more useful criteria than one in which model formulations are labelled "appropriate"; or "inappropriate";. The authors conclude with the statement "if one accepts the assumptions that the priming effect is quantitatively important for SOM dynamics and that the steady state SOM stocks increase with litter input, then we argue that the formulations of SOM decompositions, where the active decomposers are represented in a nonlinear manner are most appropriate to describe long-term SOM dynamics". Given my fitness-for-purpose

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argument above, I think that for certain applications, it could be argued that one or both of these assumptions need not be accepted.

References

Fontaine, S., Barot, S., Barré, P., Bdioui, N., Mary, B., and Rumpel, C.: Stability of organic carbon in deep soil layers controlled by fresh carbon supply, *Nature*, 450, 277-281. 2007.

Whitmore, A.P.: Describing the transformation of organic carbon and nitrogen in soil using the MOTOR system. *Computers and Electronics in Agriculture* 55, 71-88. 2007.

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