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Biogeosciences Discussions

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

T. Wutzler and M. Reichstein

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General comments to Pete Smith

We thank Pete Smith for his encouraging and constructive comments.

P. Smith raised two main discussion points.

1. The danger of loosing essential aspects of model function with the approach of using a common minimalistic model.
2. The labeling of model formulations as appropriate and inappropriate.

The first point was already discussed in the discussion at P14 L19 ff. Our argument

is that we find qualitative differences between studies and we think that these differences would even increase when we were to include additional specifics of the various compared models. P. Smith also believes that this point is not a problem in our study.

For the second point P. Smith suggest using the term "fitness for purpose". We always had in mind the purpose and application of studying qualitative differences in long-term behavior between model formulations. In a revised version we will state this more explicitly in relevant passages. Furthermore, we probably will adopt the term "less fit" or "more fit" or more or less 8220;suitable8221;.

General comments to S. Fontaine

We thank S. Fontaine for his thorough and constructive comments.

S. Fontaine states that our work is important in order to identify and overcome our limitations in the ability to predict the future capacity of soil to accumulate carbon.

We agree that the differences between model formulations in our artificial example of "challenging models with experimental data" seem to be small in Fig. 3. This graphical impression is despite the fact that the inclusion of the isotopic ratio in addition to the amount of respiration improves the identifyability of model formulations. However, the addition of the isotopic ratio may be not sufficient. This is our basic argument for suggesting to infer additional patterns in the data and to tighten the collaboration between modelers and experimentalists before setting up experiments.
In a revised version, we will formulate this line of thought more clearly [17 second paragraph]. Especially we describe the experiment in more detail in the methods section [6] and the results section [13] and add a quantitative measure of the improvement of the model fit: the ratio of the likelihoods [6].

We indicated the classification of formulations according to the representation of decomposer biomass on the SOM decomposition rate a) non-explicit (substrate only), b) linear, and c) non-linear in Table 1.

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Thanks for noting the typing errors.

Specific comments to Pete Smith

[X] denotes page X in the revised version.

We do not readily see how the cited paper of Whitmore 2007 about the MOTOR framework relates to our study.

We agree with P. Smith that the two assumptions of first, the importance of priming effect and second, the increase of steady state SOM stocks with carbon input need not to be accepted for certain applications (P18 L12ff)[18 second last line]. However, for the applications where these assumptions are important, also our conclusion is relevant. We define more clearly under which conditions and in which applied contexts the state-of-the-art assumptions are still sufficient to describe the relevant carbon dynamics. However, we hold that if we want to move towards a scientific acceptable theory of soil carbon dynamics, it is crucial that models that are implementations of such theory are not already falsified by current data. This is basically a fundamental science versus applied science argumentation.

Specific comments to S. Fontaine

S. Fontaine asked why the classical formulation dC/dt=-kt can apply to the decomposition of fresh C and not to recalcitrant soil.
For the fresh C, the underlying assumption about a fast steady state of the amount of decomposers with the amount of substrate is approximately true, if the there is sufficient fresh C to be the primary source of carbon for the decomposers. The exception are the periods when the fresh substrate is nearly decomposed or when there is this much recalcitrant carbon so that the recalcitrant C supports a greater biomass than the fresh C. The turnover of the fresh C in this time is than presumably underestimated with first order kinetics. However, we expect this period to be very rare and to not changing the overall dynamics. The assumption of equilibrium with the substrate does not hold

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for the recalcitrant soil C, if they can also grow on the fresh C.
See also Marschner B, Brodowski S, Dreves A, Gleixner G, Grootes P, Hamer U, Heim A, Jandl G, Ji R, Kaiser K, Kalbitz K, Kramer C, Leinweber P, Rethemeyer J, Schmidt MWI, Schwark L B. WGL (2008) How relevant is recalcitrance for the stabilization of organic matter in soils? J. Plant Nutr. Soil Sci. Jan 2008
Because of we do not explicitly model the FOM decomposition but rather prescribe the influx of decomposed FOM to the decomposer pool as a model input, this discussion is out of the focus of the presented paper.

For giving reference support to the sentence "at daily resolution at plot scale" we will refer to the review section [3 second paragraph].

In the revised version we will move the equations close to the referring text instead of presenting them together [7-10].

In the revised version we will introduce eq. 10 with more rational [10 last paragraph].

Equation 11 is typed incorrect: The analysis was based on dS=k*(1-exp(-c*A))*S. We will correct this in the revised version [10, Table 1].

We will precise our statement about steady state of Eq. 5 according to the referee (P173 L9) [12 first paragraph].

We will again cite Ågren GI Bosatta E (1996) for supporting our statement of PP173 L26,27 [12 second last paragraph].

We will not pool the equations 4, 5, 7, 8, 9 together because Eq. 5 and 7 always exhibit unlimited SOM allocation (P175 L2ff). With the other equations both, a finite steady state and unlimited accumulation is possible [14 second pargraph].

We will precise the meaning of competition to "between different microbial functional groups" (P175 L6) and include a sentence of conclusion at the end of the paragraph [14 second paragraph].

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We will precise our wording on P177 L5-11 that the 8220;steady state8221; of SOM is independent of fresh organic matter input with the linear group of formulations. Formerly we referred to the 8220;accumulation or decomposition8221; of SOM, which is indeed not independent of fresh organic matter input [14 last paragraph].

We will rephrase the rather long conclusion sentence on P177 L13 [16 second paragraph].

Interactive comment on Biogeosciences Discuss., 5, 163, 2008.