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Geoscientific Model Development Discussions

Interactive comment on "Comparing microbial and chemical approaches for modelling soil organic carbon decomposition using the DecoChem v1.0 and DecoBio v1.0 models" *by* G. Xenakis and M. Williams

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General comments:

The MS by Xenakis and Williams presents a theoretical comparison of two approaches in modeling of SOM decomposition (namely chemical, based on first order kinetics and more sophisticated, considering microbial biomass as a driving factor). The both approaches were published before: the first-order chemical kinetics is a basis for vast number of existing models designed for the description of SOM turnover; the biomass-

driven decomposition was also applied in several models describing SOM dynamics in short-term and at small spatial scale (e.g. Blagodatsky et al., 2010, Ingwersen et al., 2008). The comparative sensitivity analysis completed by the authors shows the principal differences in these alternative model approaches, when steady-state conditions are compared and long-term effect need to be considered. I believe, that this work is novel and useful, showing the importance of model structure and underlying assumption for realistic description of natural phenomena and, specifically, SOM decomposition. The MS by Xenakis and Williams makes a bridge between traditional SOM turnover models and soil biology models, by testing the latter at long term scale, so that they can be used at the ecosystem level. The results of modeling experiments completed by authors became extremely important in view of modeling of prospective climate change effect on soil C stocks. In fact, analysis of model structure effect on SOM decomposition at steady-state and in dynamic was done by Wutzler and Reichstein (2008, 2013), but they did not consider litter composition (leaves, root and woody parts) and temperature effects. They just mentioned these driving factors as a subject for consideration in future studies. Thus, the conclusions made in the current MS make a further important input in a model description of microbial biomass and SOM interaction during decomposition process. I suggest, that authors should amend their discussion section by comparison of their results with conclusions made by Wutzler and Reichstein.

Specific comments:

Title: "microbial and chemical approaches" sounds as a scientific jargon. Maybe microbial and chemical kinetics?

The used models are described in details, model code is provided in supplementary material, so the results can be reproduced. Additional information is needed in Material and Methods section: software version and programming language used to be described.

Abstract can be further improved by more detailed description of the difference between biological and chemical models.

Introduction: I find the research task of the paper very topical - it is really important which kind of model will be used for predicition of future climate change effect on SOC dynamics and C cycle feedbacks.

Sections 2.1.1 and 2.1.2. is better to place in inverse order, starting with more simple and traditional chemistry model and afterwards describing biological model, explaining the additional complications and differences. The same holds true for result presentation and discussion.

Sections 3.3 and 3.4.: I suggest to change the title, for example: litter quantity manipulation experiment 1 (or 2). Sensitivity is a misleading title, also it was described in previuos version.

P38, L21-24: Microbial activity concept was suggested by Panikov (1995), so in this place I would better cite the original work and only afterwards the article by Blagodatsky and Richter (1998).

P42, L3 - please describe how the rate coefficients were tuned.

P43, L.11-12: It is not clear how the soluble C (glucose) was added - daily or as ample amount once per year.

P45, L7-10: Not clear what climate effect is considered - temperature increase? Please specify this here or before in methods section.

Technical corrections:

P34, L12-14: modify the sentence - it is not clear which hypothesis you mean - litter increase the C stock, or prime microbial activity and decrease C stock. It is not clear which hypotheses was supported by experiments.

P35, L9: Did you mean permanently frozen?

P42, L.17-19: It is not clear which temperature forcing was included - 2 degree increase/ decrease? This is also need to be included in Table 2 heading.

P50, L10-12: Soil respiration decrease due to cooling, please correct.

Fig.2. Please include in the legend the reference for Eq. 15 for calculation of λ.

Fig.4. Axis legend is very small, increase the font size, name exactly what kind of experiment was presented (e.g., litter manipulation, temperature manipulation) - Figure should be self-expalining.

References

Blagodatsky S, Blagodatskaya E, Yuyukina T, Kuzyakov Y. 2010. Model of apparent and real priming effects: Linking microbial activity with soil organic matter decomposition. Soil Biology & Biochemistry 42(8).

Blagodatsky SA, Richter O. 1998. Microbial growth in soil and nitrogen turnover: A theoretical model considering the activity state of microorganisms. Soil Biology & Biochemistry 30(13):1743-1755.

Ingwersen J, Poll C, Streck T, Kandeler E. 2008. Micro-scale modelling of carbon turnover driven by microbial succession at a biogeochemical interface. Soil Biology & Biochemistry 40(4):864-878.

Panikov NS. 1995. Microbial Growth Kinetics. London, Glasgow: Chapman and Hall.

Wutzler T, Reichstein M. 2008. Colimitation of decomposition by substrate and decomposers - a comparison of model formulations. Biogeosciences 5(3):749-759.

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