Geosci. Model Dev. Discuss., 7, C265–C277, 2014
www.geosci-model-dev-discuss.net/7/C265/2014/

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

G. Xenakis and M. Williams

george.xenakis@gmail.com

Received and published: 4 April 2014

Dear Editor,

We would like to thank both reviewers for the positive and useful comments. We have updated the text accordingly, and below we respond in detail to each comment and explain any changes to the manuscript.

Response to Reviewer #1

We would like to thank the reviewer for his positive and useful comments

I suggest, that authors should amend their discussion section by comparison of their results with conclusions made by Wutzler and Reichstein.

We thank the reviewer for pointing us to this interesting paper; we have examined this paper in detail, and have now added the following text to the Methods:

"The chemical model was based on the concept that decomposition is dependent on the chemistry of the soil organic matter and temperature (Li et al., 1992, 1997; Liski et al., 2005; Metherell et al., 1993; Parton et al., 1988; Smith et al., 2007, 2010). The biological model was based on the concept that decomposition is dependent on microbial biomass and activity (Blagodatsky et al., 1998, 2010) and addresses the two challenges of Schmidt et al. (2011) outlined above. In the terminology of Wutzler and Reichstein (2008), the chemical model involves non-explicit representation of decomposer biomass in SOM decomposition, with the assumption that each pool of SOM has its own decomposer community in consistent equilibrium. On the other hand, the biological model includes a non-linear representation of a single decomposer community that determines decomposition of all SOM pools, with its microbial biomass and activity out of equilibrium with the substrate pools."

And we have adjusted the text in the Conclusions:

"...Our study suggests that the use of a chemical model is a simplification of the reality which does not match experimental warming observations. Likewise, Wutzler and Reichstein (2008) have noted that representing active decomposers in a nonlinear manner, as in DecoBio, is most suitable for describing long-term SOM dynamics. The main conclusion of the study, is that by excluding the impact of microbial community we miss key processes that introduce complex, often stabilising feedbacks."

Reply to specific comments:

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

Thank you for the suggestion. The title has been changed to *"Comparing microbial and chemical kinetics for modelling soil organic carbon decomposition using the DecoChem v1.0 and DecoBio v1.0 models."*

Additional information is needed in Material and Methods section: software version and programming language used to be described.

There is no software version to declare as none was explicitly used for simulations. The models were compiled with GNU Fortran 4.6.3 compiler available to all Unix based operating systems and inputs/outputs were all plain text files. The version of the models is given in the title and main text. We've amended the code availability section :

"The FORTRAN 95 source code for both DecoChem v1.0 and DecoBio v1.0 presented in this paper are freely available either through the supplementary material or directly by contacting the authors. The code was compiled using GNU Fortran 4.6.3 compiler freely available to all Unix based operating systems."

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

We add some extra detail to the abstract to respond to this request:

"The microbial model includes cycling of organic matter into and out of microbial biomass, and simulates the decay rate as a function of microbial activity."

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.

Thank you for the suggestion. We agree it makes more sense and the manuscript was changed accordingly.

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 previous version.

The titles were changed as suggested.

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).

Changed as suggested.

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

We have amended the text to make this clear:

"The decomposition rates for the chemical model and biological models were tuned manually and were allowed to spin up for 1000 years. The process was repeated until pools were in steady state, with inputs equal to outputs."

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

Glucose was added on an hourly time-step (the time-step of our model simulations) with a total of five grams in a year. We changed the text to make this clear.

"Starting from a steady state, we added 5 gC m-2 yr-1 (Blagodatsky et al., 2010), applied directly to Cfast every time-step i.e. hour"

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

It is only temperature variation that it was considered. We changed the text to make this more clear.

"For the biological model microbial biomass had a MRT 71% larger than Cfast. Including variable temperature reduced the MRT of the biological model by 5% for CLf, CLr

and CLw, by 1% for Cfast and increased MRT for Cslow by 0.17%. ..."

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.

We have clarified the text to say:

"First, we hypothesised different responses in the two models to increased litter inputs and glucose additions. In the microbial model we hypothesised that this perturbation would prime microbial activity and reduce soil carbon stocks; in the chemical model we expected this perturbation to increase C stocks."

P35, L9: Did you mean permanently frozen?

The phrase "perennially frozen" is more suitable to describe soils frozen for a very long time. In our opinion "permanently frozen soils" is something which indicates something indefinite and unlikely to change thus the first term is more suitable for soils which are long-term frozen but likely to change in the future.

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.

We apologise for not being clear on this: here we are exploring the effect of using a constant temperature for each experiment, versus including seasonal variation. We have clarified this by adjusting the text:

"In a second phase of calibration, we included diurnal and seasonal variation in temperature, using observations, and both models were allowed to spin up for another 1000 yr to reach steady state."

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

We thank the reviewer for highlighting this mistake. The text was not very clear and we have changed as follows:

"Total soil respiration increased by 6% for the warming and decreased by 6% for the cooling scenario but it only return to its original value towards the end of the simulation."

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

Changed as suggested.

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.

Changed as suggested.

Response to Reviewer #2

We would like to thank the reviewer for the positive and useful comments. Below we discuss these, and any changes made to the revised manuscript.

I went through their code quickly and found they used the first order forward Euler scheme. This makes me concern about the robustness of the paper's results. The forward Euler method has its pitfalls, especially when applied to nonlinear models thathas unstable or near unstable component. Also, it converges relatively slowly. Since the code is written in Fortran and there are quite robust numerical solvers written in Fortran, I would suggest the authors to confirm their results are robust. In addition, I think the linear chemical model can be solved analytically. If possible, I would recommend the authors to use the analytical solutions for the sensitivity analysis. That will be much more robust.

Figures 1 to 3 show that the DecoBio v1.0 model can reproduce the CO_2 respiration data and microbial biomass from Blagodatsky et al. (2010). Lines are model outputs for untreated (black) and glucose treatment (red) and points are the corresponding data extracted from Blagodatsky et al. (2010). Figure 1 & 2 show the model is capable of reproducing the results for different time scales from 5 to 60 days.

Finally, our study aims to explore key concepts of microbial organic matter decomposition and not to look at possible errors introduced by small variation of the state variables. For that reason we believe that Euler's method of integration is sufficient, and efficient, and there is no need to use an alternative integrator. We therefore request to leave the current text and approach unchanged.

In regard to the second major concern of the reviewer, s/he suggested an analytical solution to the chemical model. While we agree this is possible, the more complex biological model is not amenable to such analysis. Thus, performing a sensitivity analysis of the chemical model in an analytical form would not allow a direct comparison with a numerical analysis of the biological model. The aim of our manuscript is to explore

key concepts by directly comparing a biological and a chemical model. A comparison of the sensitivity of two models in different form is not consistent and thus we believe solving the chemical model analytically is not useful. So we again argue that our current approach is satisfactory and most applicable for the study in question, and request to leave the text unchanged in this respect.

Reply to specific comments:

Abstracts: I would suggest the authors spell out "numerical experiments" explicitly, because I have found myself got confused between "numerical experiments" and actual "observational experiments" when reading the abstract. Especially, the author also mentioned "experimental warming", which apparently referred to actual field experiments.

Throughout the text we now explicitly state "numerical experiments" and "field experiments" to clarify our presentation.

See:
P.34 L. 21 = numerical
P. 35, L. 26 = field
P. 41, L. 22. = numerical
P. 43, L. 18. = Numerical Experiments, L. 19 = numerical
P. 44, L 4 = numerical
P. 49, L. 11, numerical
P. 50, L. 14 = numerical
P. 53, L. 10, L. 24 = numerical
P. 56, L. 17 = field

P35, L12. Soils are likely a sink of atmospheric CO2 of approximately 0.4 Pg C(...) I think the authors misspelled the unit here. Author, would you mind providing the reference for your quote?

We have corrected the units to Pg C yr^{-1}. The value is a rate provided by McGuire et al. (2009).

P39: Eq. (5), did you use Einstein's summation convention? Similar problems are with Eq. (7), Eq. (11), and Eq. (12).

We are not quite clear about what this comment refers to, as we are using summations with different terms for each component of the right hand sides of these equations.

P39, Eq. 6: I think the equation is different from what was used in Blagodatsky et al. (2011), could you check that?

As we say in line 18 the variable is an adaptation of the variable by Blagodatsky et al. (2011) and not the exact variable. In Blagodatksy et al. (2011) what is called death rate is the Michaelis-Menten function mutliplied by how active the microbial community is and the size of their biomass. In our adapted model we call death rate only the Michaelis-Menten function multiplied with microbial biomass.

P45, L8. I would suggest replacing 'variable climate' with 'variable temperature' because you never included other climate variables.

We agree and have made this change throughout the text.

P45, L11: 'Slow organic carbon stocks ...', It is a little bit confusing here, you may want to put it explicitly what made the slow organic carbon stocks 10% larger.

In the steady state we aimed for soil C stocks that were similar in magnitude, rather than exactly the same. We have adjusted the text to note this:

"Slow organic carbon stocks at steady state were 10% larger in the chemical model than in the biological model (Table 2); while not the same, the difference was small enough to meet our criteria of approximate similarity in soil C stocks."

P47, I think it is better to make words like 'when litter was increased' and 'increasing litter inputs' more transparent. How much was the increase? Although

you mentioned in the designation of the sensitivity study, putting the increase into exact numbers here won't lengthen your explanation much but the paper is much more readable.

We are happy to make this change, and have added the litter increase amount (25%) to the text at various points through section 3.3, to provide better context.

P48, L12: Again, please use numbers to show how much extra glucose you added.

We have added these numbers.

P52, L3-4: 'The sustained increased in microbial biomass ...' I think you were saying 'the sustained increase in microbial biomass'. Also, readers would be happy to see your assertion here is numerically robust.

Thank you for pointing out this error - we have removed the aberrant "d". We have also added a reference again to the figure panel that shows the robust shift in the microbial pool.

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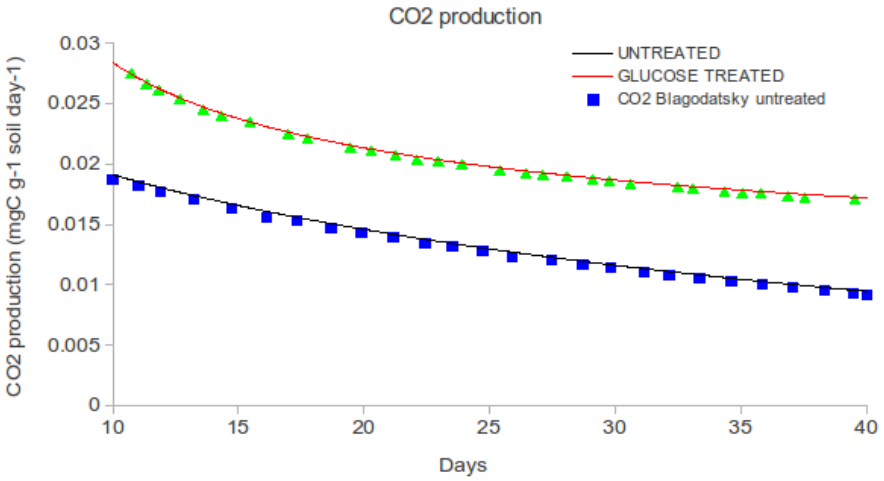

Fig. 1. CO2 production for 30 day period. Lines are the DecoBio model and points are data from Blagodatksy et al. (2010).

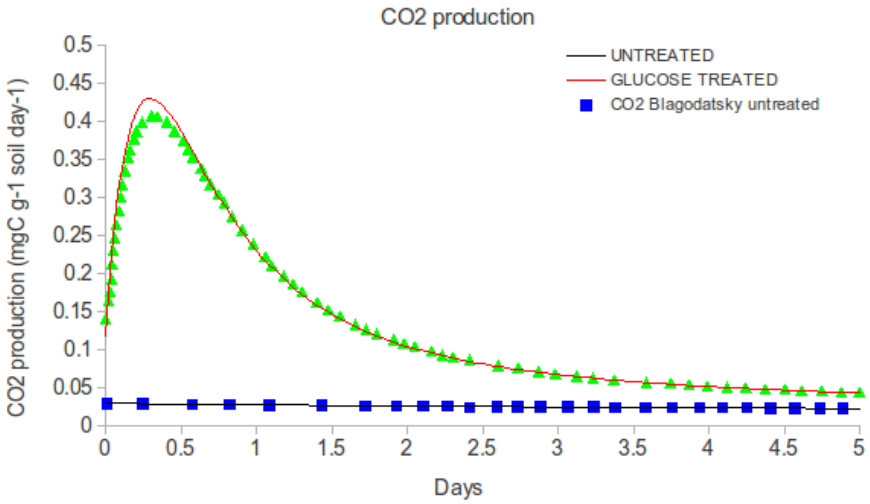

Fig. 2. CO2 production for 5 day period from the start of the experiment. Lines are the DecoBio model and points are data from Blagodatksy et al. (2010).

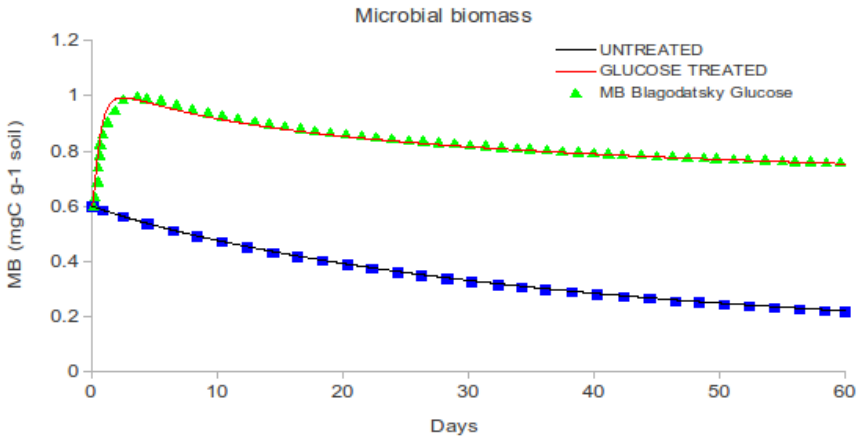

Fig. 3. Microbial biomass for 60 days from the start of the experiment. Lines are the DecoBio model and points are data from Blagodatksy et al. (2010).