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Interactive comment on "Long residence times of rapidly decomposable soil organic matter: application of a multi-phase, multi-component, and vertically-resolved model (TOUGHREACTv1) to soil carbon dynamics" *by* W. J. Riley et al.

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This manuscript presents a new vertically resolved soil organic carbon (SOC) model, explicitly representing aqueous transport, and decomposition of a range of organic compounds by fungi and aerobic bacteria. I very much enjoyed reading this paper. The new model is highly innovative and nicely complements recent developments in the field, related to the need for more realistic and process-oriented description of soil carbon cycling, including vertical SOC transport. The overall length is appropriate (although the model needs to be described in more detail), and the introduction and

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discussion are scholar. I particularly like the more philosophical parts of the discussion, e.g. about the trade-off related to parsimony and complexity. Such issues rarely receive much attention in modelling papers.

My comments are all fairly minor and focus mainly on the presentation, and less on the model and the analysis of the results.

General comments

1. One thing I'm missing is discussion (and possibly also model results) related to the priming effect, i.e. the enhanced decomposition of old, autochthonous material when fresh material is added due to simulation of microbial activity. This has repeatedly been put forward as grounds that first-order kinetics models do not fully capture the correct dynamics of SOC (cf papers by Wutzler et al. 2008, Fontaine & Barot 2005). Since the main aim is to derive a more "fidelitous" description of SOC cycling, I think this should at least be discussed. Furthermore it seems to me that the model is capable of simulating such effects so perhaps a simulation experiment would be interesting. This is up to the authors, however.

Methods

1. The model description is not sufficiently detailed to be fully understandable which makes it difficult for the reviewers to check the validity. I would like to see a list of the mass balance equations including all terms in an appendix or online supplemental material. I think the authors should strive to make the model reproducible by the reader as much as possible. To my mind a journal like GMD is exactly the place for a more exhaustive model description. As mentioned in section 2.1 some things are described in the technical guide of TOUGHREACT. That's fine, but I would at least like to know the boundary conditions applied to the soil carbon

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model.

2. I wonder if the equations related to decomposition presented in sections 2.3–2.5 were derived specifically for this model or if they are based on previous work. They are quite complex so I suspect the latter, but I don't see any clear reference. Some of the formulations are difficult to understand, particularly those related to depolymerization. In principle this is not a problem if a reference to a more comprehensive description is included. If the model equations are new a more comprehensive derivation should be presented.

3. As far as I understand only aqueous transport is considered, i.e. the solid and adsorbed pools are not subject to transport. However, I believe that on the time scales of the simulations in the paper also transport of the solid components is relevant, particularly due to bioturbation. I would expect that bioturbation is an important process in grassland sites from which the measured profiles were collected. Modifying the model and redoing the simulations is not necessary, but I would like to see it mentioned in the discussion.

4. Page 822; lines 19–23: the description of z is missing.

5. Page 824; lines 3–8: can you explain how each property used to group the compounds relates to the processes (decomposition, transport)?

6. Figure 1: It is a bit confusing that woody litter, leafy litter, and root exudates are represented by rounded rectangles while they are in fact not pools but input fluxes. I would suggest using labelled block arrows instead.

7. Figure 2: Can you please make the link with table 2 more clear, for example by adding the abbreviations in an additional column in table 2?

8. Table 1: several comments: (1) I think this table should be placed after table 3; (2) Please explain better in the caption what the columns mean; (3) I think "S1"

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in the last row should be “S10”

9. Page 824; line 23: “encapsulated in aggregates”. Elsewhere it is mentioned that the effect of aggregates is not considered in the model.

10. To my mind sections 2.4 and 2.5 should be placed before section 2.3. Sections 2.4 and 2.5 introduce the decomposition reactions for monomers and polymers, and 2.3 deals with the rates of these reactions, and how they affect the different species. In fact, some of the symbols used in section 2.3 (Y_i, x_i) are not explained until the later sections.

11. I cannot find the mathematical formulation for the production of the carbon pools. Fig. 1 shows that a part of the decomposition flux of donor pools flow to other pools, and Fig. 2 shows these partitioning fractions. However I can't trace this to the mathematical equations. I would expect the quantities displayed in Fig. 2 to show up in the mass balance equations somewhere.

12. I believe that the notation $\frac{dC_i}{dt}$ in equations (2)–(6) is not completely correct. $\frac{dC_i}{dt}$ should denote the overall net change of concentration for species i due to both loss and gain, but in the equations $\frac{dC_i}{dt}$ refers only to the loss due to decomposition. This can be somewhat confusing. I would suggest choosing a different symbol or writing it as $\left.\frac{dC_i}{dt}\right|_{pr}$ for process pr (as done in eq (1)) to indicate that it refers only to the effects of a specific process. A similar point holds for $\frac{dCO2}{dt}$, $\frac{dO2}{dt}$, and $\frac{dB}{dt}$ in equations (3)–(6): these species are also affected by decomposition of other compounds so at least an i should be added to indicate that these equations refer to the effects of decomposition for one specific compound.

13. Eq. (7): $R_{O/C}$ varies per compound, right? Please add an i to indicate this.

14. Eq. (9): Please replace R_0 with R_O

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15. Page 828; line 3: can you please explain more clearly what x_j denotes?

16. Section 2.6: The representation of ad- and desorption is described in insufficient detail. It is only mentioned only that "forward (adsorption) and reverse (desorption) rates are imposed". However, this doesn't show up in any of the mass balance equations. Furthermore, it is not mentioned how adsorption affects decomposition. I assume that species adsorbed to minerals are protected from decomposition, but this should be clearly stated.

17. Page 829; line 26: Can you please explain by "characteristic lengths"? Also, it would be nice to see the root input profiles in a graph, e.g. in Fig. 3 or 4, or in supplementary material.

18. Page: 830; line 2: A minor comment: to my mind checking convergence to steady state based on the first derivative over time is risky. If pools accumulate very slowly it my seem as if they are close to steady state, while in fact they are not. Better is involve also the second derivative. But I trust that the authors made sure there was true convergence.

19. Section 2.8: I agree with the comment of Bernhard Ahrens regarding the delta-notation for 14C. Furthermore, in section 3.3 and 4.1 the model results for 14C are discussed in the context of observations, but the latter are not shown anywhere. It would be helpful to show several 14C profiles (possibly in supplemental material) for readers who are not very familiar with such data, also in view of the unrealistically low delta-14C values predicted for the topsoil.

20. Section 2.9: The two experiments described in lines 10–20 are not fully clear to me. For the first experiment it is stated that "we doubled all chemical species initial concentrations from those at the end of the 10000 yr simulation, and performed a 500 yr simulation". For the second experiment it is stated that "we performed pulse carbon input experiments by doubling the steady-state concentra-

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tion of all compounds in seven depth intervals (0–10, 10–20, 20–30, 30–40,40–75, 75–125, and 125–200 cm)". I don't really see the difference between the two experiments.

Results

1. Could you perhaps also give some numbers to the average predicted DOC fluxes (or show a graph in supplementary material) and compare with observations from previous studies? A good reference for the latter could be Kindler et al., 2011 (GCB). It is mentioned that since DOC concentrations are very variable in time, they is not a good metric for comparing with predictions. However, I believe that time-averaged fluxes (such as those presented by Kindler et al) could be a good reality check for the model, at least in terms of order of magnitude.
2. Page 833, line 13: the part starting from "where most of..." is a bit confusing. I would suggest writing "which receives most of the input..." or similar.
3. Figure 6: Please consider making this figure bigger and omitting the errorbars for the observations.
4. Page 834, line 3: "0% microbial biomass below 40cm depth". I assume the biomass it's not actually zero since this would mean there is no decomposition and carbon stocks would grow very large. Or is everything removed by transport?
5. Page 834, line 28: Please replace "-50cm" with "0-50cm". Same for page 836, line 27.
6. Page 835, line 18: The sentence starting with "Using..." is difficult to follow. Consider revising.
7. Figure 7: Like Fig. 6, please consider making these figures bigger, possibly by moving some to supplementary material.

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Discussion and conclusions

1. Section 4.1.1: near the end of the section it seems as if the aim is to fit the observations as close as possible (“mostly outside standard deviations”, “biases”). However, given that the observations come from many different sites, while the model is only run for one and is not calibrated, I guess they are only included for comparison in terms of order of magnitude.
2. Page 844, line 1: please add an “s” after “move”.
3. Page 846, line 27: I believe the correct spelling is “in silico”.

References

- Fontaine, S. and Barot, S. (2005). Size and functional diversity of microbe populations control plant persistence and long-term soil carbon accumulation. Ecology Letters, 8(10):1075–1087.
- Kindler, R., Siemens, J., Kaiser, K., Walmsley, D. C., Bernhofer, C., Buchmann, N., Cellier, P., Eugster, W., Gleixner, G., Grunwald, T., Heim, A., Ibrom, A., Jones, S. K., Jones, M., Klumpp, K., Kutsch, W., Larsen, K. S., Lehuger, S., Loubet, B., McKenzie, R., Moors, E., Osborne, B., Pilegaard, K., Rebmann, C., Saunders, M., Schmidt, M. W. I., Schrumpf, M., Seyfferth, J., Skiba, U., Soussana, J. F., Sutton, M. A., Tefs, C., Vowinckel, B., Zeeman, M. J., and Kaupenjohann, M. (2011). Dissolved carbon leaching from soil is a crucial component of the net ecosystem carbon balance. Global Change Biology, 17(2):1167–1185.
- Wutzler, T. and Reichstein, M. (2008). Colimitation of decomposition by substrate and decomposers - a comparison of model formulations. Biogeosciences, 5(3):749–759.

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