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Interactive comment on "Towards a representation of priming on soil carbon decomposition in the global land biosphere model ORCHIDEE (version 1.9.5.2)" *by* B. Guenet et al.

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Answer to comments from the reviewer.

Comments from the reviewer were left intentionally in this document and written in roman font. Our answers are written in italics.

General comments: The MS by Guenet et al., "Towards a representation of priming on soil carbon decomposition in the global land biosphere model ORCHIDEE (version 1.9.5.2)" describes the attempt to model priming effect at ecosystem scale using the CENTURY-type ORCHIDEE model with new features (and three additional parame-

ters). I admit that SOM matter turnover models should be developed in this direction and I welcome this attempt. This work is also in general trend of microbial-driven model application for global SOM simulation (e.g. Li et al.,2014, Wieder et al., 2014, 2015). The strong side of the work is a thorough model calibration and validation against independent datasets including both laboratory incubation experiments and field observations. All mathematical and statistical procedures as well as experimental data taken from literature are carefully described. My main concern is that authors try to improve the SOM model from previous generation (CENTURY-type, based on first order kinetics) keeping the original cumbersome and complex structure. Including the interactions between pools could help to simplify the model structure, but this was not done.

We choose to keep the original CENTURY-type structure because it is now well known that several pools are necessary to reproduce long-term SOC dynamic. Of course we may change totally the definition of the pools to have more easily measurable pools (physically-protected C, height molecular weight compounds, DOC, etc.) but this would need a more data to parameterize the model. Moreover, the difference between both models would have been more complex to understand, in particular to disentangle the effect of the new decomposition scheme and the effect of the new structure. We considered this action as a second step that we are currently doing.

Therefore, the results are not very impressive: authors show that ORCHIDEE-PRIM hardly improve the prediction of CO2 production in litter amendment experiments and addition of new parameters increase Bayesian Information Criterion in many cases, when original model is compared with modified one, i.e. PRIM model is overparameterized. Authors have to describe better the advantages of their approach, which is based on well-tested and broadly used ORCHIDEE model.

Indeed, as a first guess, the results does not look very impressive but as mention in the discussion section ORCHIDEE and ORCHIDEE-PRIM share the same vegetation module and we were not able to split the observed soil CO2 flux into roots respiration

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and heterotrophic respiration. Nevertheless, when PRIM and the standard soil module are evaluate independently from the vegetation module and using independent data from the ones used to optimize the model the performance are much better with PRIM. We complete the discussion in the new version to better explain why the new approach is interesting.

"Nevertheless, when using independent soil incubations data from the one used to optimize the model the improvement is quite clear with BIC values much lower with PRIM than with the standard soil module (347.4 and 546.2, respectively)."

The capability to describe priming effect is an important improvement, but this can be done explicitly with a help of a simple models and this kind of models already was applied for global scale (see review by Wieder et al, 2015). If direct comparison of new generation model with explicit description of microbial biomass turnover is not possible, you should at least mention alternative approaches and discuss pro and contra of their application in relation to your approach.

We do not totally agree with this statement. Some attempts were done to applied microbial explicit model at global scale but they generally used plants inputs as a boundary condition (Wieder et al., 2014, for instance). In Wieder et al., 2015, the 13 models presented in the table 1 are generally not applied at large scales. Nevertheless, it is interesting to discuss how our approach may facilitate the challenge of integrating microbial explicit models in ESMs.

"Finally, the use of microbial steady state model like ORCHIDEE-PRIM present several advantages compared to explicit microbial models. Wieder et al., (2015) identified several challenges related to the incorporation of explicit microbial models in ESMs. In particular, it may induce unrealistic temporal oscillations in response to small perturbations and it needs much more parameter than the classical approach. With ORCHIDEE-PRIM these two difficulties are resolved since we only add three more parameters and because the model is not subject to short-term oscillations."

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Some further specific and technical comments are below: Discussion section: I find it reasonable to discuss the levels of complexity allowing to present priming effect in the models, similar to way as it was done in paper by Wutzler and Reichstein (2013). PRIM model according to their classification can be described as microbial steady-state model, also according to you description at P9199L23.

We discussed the model complexity in more details in the new version.

"The PRIM soil model, which is a microbial steady-state model, might not be able to reproduce short-term response to abrupt change of FOC inputs but with negligible bias over the long term (Wutzler and Reichstein 2013). However, it might have similar performances than more complex models to reproduce long-term trends of FOC inputs (Wutzler and Reichstein 2013)."

P9195 L21 - reference Luo et al is absent in the list.

The reference was added.

P9196 L3-4. Interestingly, that you cite paper by Kemmit et al., 2008 in support of your claim that soil decomposers are the main actors of SOC decomposition. In fact, in the cited paper authors try to prove the idea that microbial biomass size, composition or specific activity do not influence the decomposition - i.e. opinion completely opposite to your statement.

Indeed the sentence we written was a bit awkward. We corrected in the new version.

"The first order kinetics used in most models obviates the role that microbial decomposers are known to play in controlling SOC mineralization (Cleveland et al., 2007; Garcia-Pausas and Paterson 2011), but their activities is controlled by physical and chemical drivers (Kemmit et al., 2008)."

P9197 L16: Expression is not clear "several of parameters" what do you mean with this?

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We modified the sentence.

"Several mechanisms may be involved in controlling priming (Fontaine et al., 2003; Blagodatskaya and Kuzyakov 2008, Guenet et al., 2010b), and conceptual models of priming can have substantial number of parameters making their parameterization quite complex at large scales (Wutzler and Reichstein, 2013)."

P9198 L24 ..the same as..

This was corrected in the new version

P9201 L9-12: What was a basic underlying principle for the selection of data for model calibration? You sometimes take one treatment or incubation or variant among several published datasets.

We clarified this aspect:

"Finally, several treatments might be performed in the studies used to optimize the model (different soils, different types and amount of FOC). When the treatments performed differed on aspects reproducible by the model (amounts of FOC added, different clay content in the soils used, etc.) we considered all the treatments. In the opposite case we averaged the results of the different treatments to perform the optimization except in case where the treatments clearly impact the results without the possibility to reproduce the experimental design with the model (addition of mineral N for instance)."

P9202 L6 correct misprint.

Done in the new version

P9203 L10: Please, describe how these two fraction of respiration flux were separated. Was root respiration the same for litter amendment and litter exclusion variants?

The two fluxes were not separated on the field experiments introducing difficulties to evaluate the full ORCHIDEE and ORCHIDEE-PRIM models since they only differ in their SOC decomposition schemes. We clarified this point in the new version.

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"The data measured at field scale are the soil CO2 efflux including the heterotrophic respiration but also root respiration in the same flux without clear separation of the two components."

Figure 2 and 3: Please indicate (as in text) that Figure 2 present result of model calibration (dataset 2.2.1) and Fig. 3 present the result of model evaluation on independent dataset (2.2.2).

We modified the figure captions.

"Figure 2: Scatter plot between data and the PRIM model outputs for the incubations with FOC amendment (a), without FOC amendment (b) and for priming effect (c). The dataset used here are the similar to those used for optimization (a) or are the control incubations (b) and are described in section (2.2.1). Red lines indicate the 1:1 line. Different symbol indicate different studies.

Figure 3: Scatter plot between independent data from optimization (dataset describes in section 2.2.2) and the soil module of ORCHIDEE outputs (a) or between data and the PRIM model outputs (b). Red lines indicate the 1:1 line."

References: Li J, Wang G, Allison S, Mayes M, Luo Y. 2014. Soil carbon sensitivity to temperature and carbon use efficiency compared across microbial-ecosystem models of varying complexity. Biogeochemistry 119 (1-3):67-84. Wieder WR, Allison SD, Davidson EA, Georgiou K, Hararuk O, He Y, Hopkins F, Luo Y, Smith MJ, Sulman B et al. . 2015. Explicitly representing soil microbial processes in Earth system models. Global Biogeochemical Cycles: DOI: 10.1002/2015GB005188 Wieder WR, Bonan GB, Allison SD. 2013. Global soil carbon projections are improved by modelling microbial processes. Nature Clim. Change 3 (10):909-912. Wutzler T, Reichstein M. 2013. Priming and substrate quality interactions in soil organic matter models. Biogeosciences 10(3):2089-2103.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9193, 2015.