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Geoscientific Model Development Discussions

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 Recently, there was a debate on Science (van Groenigen et al., 2014) and Global Change Biology (van Groenigen et al., 2015; Georgiou et al., 2015) about how to represent priming effect in large-scale CENTURY type decomposition models. In this regard, Guenet et al. presented a timely and important study, which

offer a simple way to include priming effect in large-scale land surface models. In addition, it's good to see that the proposed PRIM model was evaluated against multiple datasets. However, writing was a little weak in terms of conciseness and smooth, please check my details comments, I listed some issues, author should carefully refine the paper on their own, I believe you will find more. I have two major concerns, the first one may be out of the scope of PRIM, but it is important in terms of modeling priming effect. I am not asking the author to modify PRIM accordingly, but more discussions are definitely needed. Major comments 1. Priming effects is related to nitrogen availability. Why choose ORCHIDEE not O-CN? The priming effect is closely related to soil nutrient availability. Both positive and negative priming have been observed when soil N is limited. Basically, there are two prevailing N-centric priming hypotheses (1) when N availability is low, microbes use fresh carbon to mine old carbon and obtain nitrogen, leading to a positive priming effect; (2) when N availability is strongly limited, adding fresh carbon enhance competition between roots and microbes, consequently reduce microbial activity and depress microbial growth. It is interesting to see how PRIM work with O-CN. In some sense, I would argue that the priming effect modeling must consider nitrogen, because that is the theoretically "correct" way and supported by large amount of priming observations. But having priming effect coded in carbon only model is a good start. Considering nitrogen in PRIM will be a big plus. The author should have more discuss on this issue including existing theories and observations, potential model development.

We fully agree with this remark and we are of course aware the N availability is a major driver of priming effect, nevertheless we decide to start without N to simplify the approach and make the model outputs easiest to understand. Moreover, take into account nitrogen would have probably induced to define the c parameter of eq. (1) to (3) as a function of mineral N and it that case we would need soil incubations detailing the mineral N dynamic to define the equations parameters. Such information is generally not fully available in the priming effect incubations experiments we used to optimize the PRIM parameters reducing drastically the data available to define the model parame-

ter. We add some comments in the conclusion section related to this point. "The role of N in the priming intensity as well as the extra N mineralization induced by priming and its effect on primary production may represent the next addition to the soil representation in a land surface model by adding a control on the c parameter depending on the mineral N availability and on the C:N ratio of the considered pool. Nevertheless, some detailed information on the N dynamic in priming effect experiments would be necessary to do so and very few authors reported the impact of priming effect on N dynamic after FOC additions."

2. About model core assumption The most attractive part of PRIM is that it does not require explicitly microbial dynamics (e.g., microbial biomass), by assuming microbial biomass is always equilibrium with FOC. Such assumption is suitable in terms of simplicity, but is it suitable for model predictability? What's the theoretical basis of this assumption? What's the potential bias by imposing this assumption? Please have more discussion, because this assumption is the backbone of this study.

This assumption is based on the rapid response of soil microorganisms to changes in their environment (Lundquist et al., 1999). The microbial turnover has been observed to be of few days (Schmidt et al., 2007). In particular, in priming effect studies based on soil incubations, the soil microbial biomass is already at equilibrium after few days (Fontaine et al., 2004 for instance). The time step of the soil module is daily, we therefore considered this assumption as acceptable regarding the simplicity it gives to the equations.

The potential bias we may face by imposing this assumption would be to over(under)estimate the priming intensity in particular when FOC inputs reduce (increase) drastically from one day to another (after fires or harvests for instance). Indeed, after harvest a substantial amount of aboveground material may be added to the soil and if microbial biomass is not at equilibrium during the time step considered but need few days to reach its equilibrium value, the priming effect intensity might be overestimated during these few days.

Fontaine, S. et al. Mechanisms of the Priming Effect in a Savannah Soil Amended with Cellulose. Soil Sci. Soc. Am. J. 68, 125 (2004).

Lundquist, E. ., Scow, K. ., Jackson, L. ., Uesugi, S. . Johnson, C. . Rapid response of soil microbial communities from conventional, low input, and organic farming systems to a wet/dry cycle. Soil Biol. Biochem. 31, 1661–1675 (1999).

Schmidt, S. K. et al. Biogeochemical consequences of rapid microbial turnover and seasonal succession in soil. Ecology 88, 1379–1385 (2007).

Specific comments 1. Title: better not use such a detail version number, first of all people who do not work on ORCHIDEE would not care about the version, secondly you can put the details in other section (such as your code availability section), but not in title.

This is imposed by the Journal please see the editor comment.

2. P9198. L18-19. Be concise: "the soil carbon model structure of ORCHIDEE land biosphere model" -> "ORCHIDEE soil decomposition model"

This was corrected in the new version.

3. P9198. L21-23. Sentence doesn't make sense. Rewrite it. How about "SOC decomposition is modulated by soil temperature and moisture functions. Active SOC decomposition is further modulated by a clay function. These functions are the same as in CENTURY."

This was corrected in the new version.

4. P9199. L3. that simulate a priming effect -> to simulate priming effect

This was corrected in the new version.

5. P9199. Eqn 1-3. SOCLabile , please be consistent throughout the paper. Either use labile SOC or active SOC. It's confusing to have both active SOC and labile SOC

meaning the same thing.

This was corrected in the new version.

6. P9199. Eqn. 2-4. Does clay function (gamma) only affect active SOC pool?

Yes, we did a mistake in the equation. We corrected in the new version.

7. P9199. L11. FOC, first time used in materials and methods section, better to have a full name.

This was corrected in the new version.

8. P9199. L22-23. Be concise, "assume instead a linear relationship between microbial biomass and FOC. Thus, it implicitly assumes that MB is always in equilibrium with FOC" -> "assumes that MB equilibrates with FOC thus the relationship between MB and FOC is linear"

This was corrected in the new version.

9. P9200. L2. The decomposition model runs at a daily time step. duplicated statement with 9199 Line 1, remove it.

This was corrected in the new version.

10. P9200. L13. "very different situations" What are these situations? elevated CO2? warming? dry/wet?

We rephrase to clarify this statement

11. P9200. L15. "three different models" -> "three different sub-models".

This was corrected in the new version.

12. P9200. L19. "manage the aspects related to" -> "deal with"

This was corrected in the new version.

13. P9200. L24. "describe" -> "classify"

This was corrected in the new version.

14. P9201. L8. priming effect was measured

This was corrected in the new version.

15. P9201. L9. "by comparison with a control without FOC" -> do you mean "by comparing a control study without FOC with a perturbation study with FOC"?

This was corrected in the new version.

16. P9202. L8-10. I don't fully understand. Did you run run ORCHIDEE-PRIM to get equilibrium carbon states? If you run ORCHIDEE till equilibrium and use ORCHIDEE-PRIM to run transient, SOC pools are out of equilibrium at the beginning of the simulations.

We agree that this is a drawback of the study but because we need the fraction of each pool to initiate the parameterization of PRIM we were not able to run ORCHIDEE-PRIM to define the fraction of each pools or with dummy parameters with consequences on the optimization process quite complex to anticipate. 17. P9202. L19-20. how to estimate initial fraction of each pool with location formation?

To run ORCHIDEE for each sites we needed the coordinates to extract the necessary boundaries conditions. Therefore, without it was impossible to run the model. We rephrase to clarify.

18. P9203. L13-14. Be concise: "turnover rate (kSOC) for each of the three pools as well as the priming parameter c of Eqs. (1), (2) and (3) specific of each pool" -> "turnover rate (kSOC) and priming parameters c for each of the three pools"

This was corrected in the new version.

19. P9203. L21. "use all data streams assimilated" -> "assimilating all data streams"

This was corrected in the new version.

20. P9204. L10. How do you calculate J(x) gradient? By finite difference method? adjoint method?

We used the finite difference method. We added this information in the ms.

21. P9204. L24. “only too studies” -> “only two studies”

This was corrected in the new version.

22. P9204. L25. No covariance between c and k? Intuitively, should active C pool have faster turnover time as well as get easily primed (high substrate quality and easily get attacked by extracellular enzyme)?

Actually, it seems that the more recalcitrant pools are most sensitive to priming (Fontaine et al., 2007, Guenet et al., 2012). But the c parameter values depend on the pool considered and have different values.

Fontaine, S. et al. Stability of organic carbon in deep soil layers controlled by fresh carbon supply. Nature 450, 277–280 (2007).

Guenet, B., Juarez, S., Bardoux, G., Luc, A. Claire, C. Evidence that stable C is as vulnerable to priming effect as is more labile C in soil. Soil Biol. Biochem. 43–48 (2012). doi:10.1016/j.soilbio.2012.04.001

23. P9205. L11-14. I’m confusing. (1) Did you use observed total SOC, but simulated fraction. Any motivation? (2) is this fraction important in terms of controlling priming effect, my guess is positive. (3) you can easily do a sensitivity analysis by varying the fractions

Indeed we used observed total SOC but simulated fractions because i) we had not enough information on the inputs, on the soil temperature and moisture to run the model until equilibrium and ii) the pools defined in our model are not measurable so we had no other solution that using the simulated fraction and distribute the total SOC

amount within the simulated fractions.

24. P9206. L5. Same proportion. Why use proportion not absolute amount?

We used proportion because in the papers we used to evaluate the model the treatments are presented as proportion (no above ground inputs, control and doubled above ground inputs). Therefore, we decided to used proportion to follow the same approach and thus be able to compare the observed priming and the calculated priming. Moreover, to use absolute amounts we would had need a high temporal resolution description of the primary production on site (to be able to force the model at each time step) and this information was not available.

25. P9207. L15. why the uncertainty of slow pool is so tiny compared with active and passive pool. Looks like this parameters is perfectly constrained (tiny posterior error)?

The slow pool is always the biggest pool and since the CO2 flux is controlled by the pool size, it is also the main contributor to the flux. Consequently, the optimization procedure mainly act on this parameter to fit the data.

26. P9207. L21-22. If original ORCHIDEE succeeded, then what’s the value of introducing PRIM.

It is not surprising that when we evaluate both models on the dataset used for optimization they performed well but in the following lines we clearly show that when using other datasets (like the control incubations or even more independent data), the incorporation of priming clearly improve the model performances.

27. P9208. L11-13. PRIM works pretty bad. Any comments? Why? How to improve it?

As discussed above, priming is a complex phenomenon resulting from the interactions of different mechanisms (co-metabolism, N mining, competition for a substrate between different microbial groups) that we summarized in a very simple equation. Therefore it is not that surprising that we are not able to fully catch all the variability observed. We

added some text related to this comment in the new version. "Furthermore, PRIM was not able to fully catch the observed variability of priming. As discussed above, priming is a complex phenomenon resulting from the interactions of different mechanisms that we summarized in a very simple equation. Therefore, PRIM is probably good in representing a general trends but not all the complexity of the phenomenon."

References Georgiou, Katerina, et al. "Towards improved model structures for analyzing priming: potential pitfalls of using bulk turnover time." Global change biology(2015). van Groenigen, Kees Jan, et al. "Application of a two pool model to soil carbon dynamics under elevated CO2." Global change biology (2015). van Groenigen, Kees Jan, et al. "Faster decomposition under increased atmospheric CO2 limits soil carbon storage." Science 344.6183 (2014): 508-509

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