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Reply on RC4

Weilin Huang et al.

Author comment on "Implementation of mycorrhizal mechanisms into soil carbon model improves the prediction of long-term processes of plant litter decomposition" by Weilin Huang et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-275-AC4, 2021

Response to Reviewer 4

R4.0. In the paper "Implementation of mycorrhizal mechanisms into soil carbon model improves the prediction of long-term processes of plant litter decomposition", authors Huang, van Bodegom, Viskari, Liski, and Soudzilovskaia added effects of arbuscular (AM) vs ectomycorrhizal (EM) dominance to the soil carbon model, Yasso15. They selected a model where mycorrhizal dominance differentially affected labile and recalcitrant litter pools, compared predictions of this model to long-term litter decomposition data, and evaluated how AM- versus EM-dominated systems would be expected to differ in litter decomposition dynamics. The mycorrhizal model provided a better fit to the data and it was found that EM systems exhibit slower decay of recalcitrant litter fractions. To me, the most interesting finding was that the mycorrhizal model showed much lower sensitivity to climatic factors (i.e. temperature), suggesting that climatic effects are confounded by vegetation dynamics. Overall, this was a nice paper and a useful development of a model that can be applied at the global scale. But I do have some concerns about the framing and model evaluation.

A great deal of the motivation for this study seems to be a better mechanistic representation of mycorrhizal effects. The authors criticize that current models "treat mycorrhizal impacts…as a black box" and describe different potential pathways of mycorrhizal effects. Yet, the approach taken in this study is to add rate-modifying parameters to a linear first-order decomposition model. I find this to be a very useful and reasonable approach to leveraging mycorrhizal information to improve predictions, but I don't see how this analysis advances our mechanistic understanding of mycorrhizal effects in a meaningful way. The authors should consider re-framing with this point in mind. Especially as I see other reviewers have touched on this same point.

It is unclear how much of an improvement the mycorrhizal model represents. By RMSE, it appears that the original Yasso15 model is better than three of the four tested Myco-Yasso models (depending on the evaluation dataset) despite having 2-8 fewer parameters. And the change in the mean mass remaining for the case study (fig. 5) is very minor (~5% less mass after 10 years). That would be a difference of k = 0.15 y-1 vs 0.18 y-1 in a single exponential decay model. I definitely think there is value in these mycorrhizal models (for example if climate and mycorrhizal dominance are more decoupled in the future), but I think the authors could make a better case for the value of the proposed changes.

Re R4.0: Thank you for your overall positive feedback as well as for the detailed suggestions to improve the manuscript. The points of the mechanistic model and a small improvement in the model predictive power coincide with the R2.10 and R2.6 comments of Reviewer 2. Please see our reply to these points.

We have addressed other inquiries in the replies to comments point to point as indicated below. Our responses are highlighted in bold.

Other comments

R4.1. Line 24: How does an overestimation of climate impacts result from exclusion of mycorrhiza-induced mechanisms? This is elaborated on in the main text, but is missing context here in the abstract.

Re R4.1: We will provide more details in the abstract, changing the sentence in Line 24 to 'A sensitivity analysis of litter decomposition to climate and mycorrhizal factors indicated that ignoring the mycorrhizal impact on decomposition will lead to an overestimation of climate impacts on decomposition dynamics.'

R4.2. 40: Throughout the paper, it is not totally clear what is meant by "controlling the decomposition environment" and how the current study focuses on this. Doesn't the model integrate all potential mycorrhizal effects?

Re R4.2: Mycorrhiza can affect C cycles via three mechanistically distinct pathways of "(1) provisioning dead mycelium as the substrate for decomposition, (2) mediating plant litter quality and amounts, and (3) controlling the environment of plant litter decomposition" (see Lines 337-339). Our work in this paper focused on one pathway only "controlling the environment of plant litter decomposition", which refers to a composite decomposition environment with different types of mycorrhizal vegetation and its impact to the litter decomposition process. We could not specify the other two pathways as our calibration and validation datasets were based on litter bags experiments in which litter amount and initial litter types were controlled. We will further clarify this in the main text, while combining the comments in R2.15 and R4.8: Line 143 will change to "We calibrated our new model using litter decomposition databases (litter bags experiments in which litter amount and initial litter types were controlled, see Appendix B for more details) used in Yasso modelling which included total mass loss and different chemical components variations over time (Tuomi et al., 2009, 2011b, 2011a)".

R4.3. Fig. 1: What are the time units for the fluxes % yr-1?

Re R4.3: The fluxes of '%' are fixed rates/percentages of carbon transformation per time being modelled, as it comes with the decomposition rate, which means that the fluxes are with a unit of "yr^{-1}". We will add the flux unit to the figure description to make clear it refers to decomposition rates and carbon transfers.

R4.4. 170: What is the justification for these four particular models and what hypotheses do they represent? This could be better motivated.

Re R4.4: As described in Lines 171-177, each model scenario represents a possible conceptualization of mycorrhizal impact, explicitly separated from climate impacts. The Yasso model assumes that different chemical components in the litter decompose with distinct rates of mass flows to other pools and to the atmosphere. We tried different scenarios: Myco-Yasso.v1 is based on the assumption that mycorrhiza impact all these different recalcitrant pools differently, and in Myco-Yasso.v3 we assume the impact is the same for all pools. For Myco-Yasso.v2, we assume that mycorrhiza has a similar magnitude when affecting WEA pools while affecting the N pool differently. This assumption relates to previous findings of Yasso that climate factors have similar impacts on WEA pools, but are different for the N pool. For Myco-Yasso.V4, we assume that mycorrhiza could only affect the most recalcitrant pool of N. We will add these details in the Methods section to describe the assumptions of different model versions.

R4.5. 275: Here and throughout, I recommend just spelling out these different pools (N, W, A, E) or giving a more intuitive abbreviation.

Re R4.5: These abbreviations have been defined in Lines 105-108, and are coherent with all other Yasso papers. The only annoyance would be the N pool, as 'N' is a widely acknowledged abbreviation for nitrogen, while it refers to the Non-hydrolysable pool here. We will remind the reader about this at strategic places in the paper.

R4.6. 295: This is a very interesting finding. I do wonder if other confounding factors may still be folded in with mycorrhizal dominance or climate variables (e.g. biome boundaries, soil orders, etc.).

Re R4.6: Thank you for your support. We were also interested in this finding, and we are working on a global estimation of the mycorrhizal impact which could probably reveal more insights. However, it is not within the scope of this model description paper, and we look forward to sharing more interesting findings with you in the future.

R4.7. Fig. 8. It may be useful to show R2 which is a slightly more intuitive measure of model fit. Also, is there a measure you can use that corrects for the number of model parameters?

Re R4.7: We agree that R^2 is a way to compare model fit, but it is not suitable in our case as our models had a different number of parameters which is not corrected for in R^2. The same applies to Pearson's r, also see reply to R2.7. Instead, both AIC and BIC include a penalty for an increasing number of estimated parameters. You can see both AIC and BIC showing the lowest value in the case of Model.V2 which supported our model selection.

R4.8. 323: In addition to total mass loss, confronting models with actual litter fraction data would be extremely valuable for validation. Are these data available for any of the individual experiments?

Re R4.8: Thank you for checking this issue which has also been raised by Reviewer 2. As we discussed in the rebuttal to R2.2, models were calibrated using the actual measurements of litter decomposition. These data included both the total mass loss information as well as WAEN fractions dynamics over time. Though it has been specified in Appendix B, we feel it is necessary to add more details to describe the database in Line 143 to avoid confusion: "We calibrated our new model using litter decomposition databases (litter bags experiments in which litter amount and initial litter types were controlled, more details see Appendix B) used in Yasso modelling which included total mass loss and different chemical components variations over time (Tuomi et al., 2009, 2011b, 2011a)".

R4.9. 339: "Earlier works did not explicitly differentiate between these pathways". Maybe I am missing something crucial, but I am not sure how the current study differentiates between these pathways.

Re R4.9: In the sentence which you quoted, we aimed to emphasize that mycorrhiza can affect C cycles via three mechanistically distinct pathways of "(1) provisioning dead mycelium as the substrate for decomposition, (2) mediating plant litter quality and amounts, and (3) controlling the environment of plant litter decomposition" (see Lines 337-339). Our work in this paper focused on one pathway, i.e. "controlling the environment of plant litter decomposition", which refers to a composite decomposition environment with different types of mycorrhizal vegetation and its impact to the litter decomposition process. We did not specify the other two pathways because we used litter bag experiments in which litter amount and initial litter types had been controlled. Also, see reply to comments R4.2.

R4.10. 408: This study looked at litter decomposition specifically. Extending these results to discuss conservation of soil C generally seems like an over-reach. Especially given ideas that litter decay and soil C formation may be positively correlated (Cotrufo et al. 2013; GCB).

Re R4.10: Although our model addressed litter decomposition and not soil C cycling, "litter decomposition is an important first step in both experiments and models of soil C cycling that has been rigorously documented by litter decomposition studies"(cited from Reviewer 3). And we thank you for suggesting the interesting paper, but in this sentence, the results refer to the fact that the soil accumulation of most recalcitrant component N in the litter is different in AM vs EM-dominated environments. We assume that this most recalcitrant component is an important source/origin of recalcitrant carbon compounds in the soil. However, we will make sure to be specific whenever we mention the soil C cycle.

R4.11. 427: I am unclear how the accumulation of non-hydrolyzable material somehow supports a hypothesis about mineral stabilization of soil C (as this was not addressed in the current study). Also, yes, there is some discussion about different soil C pathways in the literature, but not much evidence that lignin is a main component of mineral-associated soil C. Altogether, the speculation in this paragraph may be beyond the scope of the current paper.

Re R4.11: This comment refers to the sentence "While our work does not address the pathway of formation of minerally stabilized carbon, it provides insights into the important processes preceding C mineral stabilization, as we examine the long term processes of formation of labile C pools potentially available for microbial uptake and the development of recalcitrant plant litter pools potentially forming MAOM by binding to mineral particles". We did not aim to support or falsify the hypothesis about mineral stabilization of soil C. Instead we aim to link to this hypothesis, highlighting that we examined the long term processes of the formation of labile C pools and the development of recalcitrant plant litter pools, and we consider the products of this long-term litter decomposition process as the potential source for mineral associated C formation. We considered it interesting to show the potential links of our work to the emerging concepts of mineral stabilization of soil C, but we agree that it is necessary to highlight the hypothetical nature of this link. Thus, we will rephrase the sentence to "While our work does not address the pathway of formation of minerally stabilized carbon (as hypothesized by Cotrufo et al. 2015, 2019 and Sokol et al., 2019), it provides insights into the important processes preceding C mineral stabilization, as we examine the long term processes of formation of labile C pools potentially available for microbial uptake and the development of recalcitrant plant litter pools potentially forming MAOM by binding to mineral particles".

R4.12. Lastly, the work of Sulman and co-authors is highly relevant to the current study, but I did not see any mention of these works:

Sulman, B. N., Brzostek, E. R., Medici, C., Shevliakova, E., Menge, D. N. L., & Phillips, R. P. (2017). Feedbacks between plant N demand and rhizosphere priming depend on type of mycorrhizal association. Ecology Letters, 20(8), 1043–1053. https://doi.org/10.1111/ele.12802

Sulman, B. N., Shevliakova, E., Brzostek, E. R., Kivlin, S. N., Malyshev, S., Menge, D. N. L., & Zhang, X. (2019). Diverse Mycorrhizal Associations Enhance Terrestrial C Storage in a Global Model. Global Biogeochemical Cycles, 33(4), 501–523. https://doi.org/10.1029/2018GB005973

Re R4.12: Thank you for suggesting these interesting papers. One major reason that we did not mention these two papers is that we are not looking at the nitrogen cycle. When specifying nitrogen in the system, it will need more information to represent microbial activities to constrain the model. However, this kind of detailed information is not always available for global modelling. Our model considers the mycorrhizal impact as an integrated function of the environment on the litter decomposition process, which includes all possible chemical and microbial impacts as induced by mycorrhiza without specifying these characteristics. But, indeed, it is important to mention more related works, and we will include suggested works in the discussion.

References

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Rineau, F., Shah, F., Smits, M. M., Persson, P., Johansson, T., Carleer, R., Troein, C. and Tunlid, A.: Carbon availability triggers the decomposition of plant litter and assimilation of nitrogen by an ectomycorrhizal fungus, ISME J., 7(10), 2010–2022, doi:10.1038/ismej.2013.91, 2013.

Sokol, N. W., Sanderman, J. and Bradford, M. A.: Pathways of mineral-associated soil organic matter formation: Integrating the role of plant carbon source, chemistry, and point of entry, Glob. Chang. Biol., 25(1), 12–24, doi:10.1111/gcb.14482, 2019.