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Comment on bg-2021-275

Anonymous Referee #2

Referee 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-RC2, 2021

This study investigates variation in litter decomposition across ecosystems of varying plant-mycorrhizal associations by adding a mycorrhizal association effect to the Yasso15 soil carbon decomposition model. The model was parameterized using a Markov chain Monte Carlo approach from a large set of litter decomposition measurements. The model changes are a simple but reasonable approach to incorporating site-level effects of mycorrhizal associations. Interactions between mycorrhizal associations and decomposition are an important area of study in biogeochemistry and these model developments represent a valuable step toward incorporating these processes into models. However, I think care needs to be taken not to over-interpret the results. The model formulation is a simple linear function of mycorrhizal association on decomposition rates, and does not address the mechanisms of mycorrhizal-decomposition interactions. The parameters and their effects on decomposition are determined by statistical parameter fitting and are validated only using statistical measures of fit to total decomposition over time, so interpretations of the resulting model in terms of changes in litter composition over the course of decomposition are not very strongly supported. The actual improvement in RMSE and related measures is very small, which undermines the stated value of the model changes. I think the interpretation of the results and the support for the value of the model developments would be more robust if model predictions were compared to specific observed trajectories of decomposition over time from sites with different mycorrhizal associations and similar climates (rather than statistical measures over the whole dataset that could hide other covariates or effects). The interpretations would be greatly strengthened if they could be compared with actual measurements of litter composition or lignin content over time. As it is, much of the interpretation of the results relating to different modes of decomposition and changes in the relative amount of labile and recalcitrant litter fractions over time for different mycorrhizal associations is based purely on a model that was not constrained using measurements of litter chemical composition over time.

Specific comments:

Figure 1: It would be helpful to label the blue CO2 arrows with the percentage that is converted to CO2. This can probably be inferred from the labeled green arrows but it's not immediately straightforward what the respired fraction is because there are several green arrows that need to be added up.

Line 145: What chemical composition data were available? Does this mean that the initial composition of litter (in terms of model pools) was measured for each site and used in model initialization?

Line 149: If plant community composition was available for each site, then why was the map of mycorrhizal associations needed? Wouldn't local measurements of plant community composition be more accurate?

Table 1: Are the "a" parameters here the same as the "alpha" parameter in Equation A3?

Line 230: It's not clear what was different about the inputs in Appendix D. Does this mean that the main simulations used measured chemical fractions for each site while the Appendix D simulations used an average chemical fraction? Is there a table or graph somewhere of the chemical fractions from the sites that were used?

Line 237-238 and Table 2: The differences in RMSE seem very small, and in many cases RMSE was higher in the mycorrhizal model than in the original model. Overall it seems like weak evidence supporting new model developments. It's also hard to understand why AIC and BIC decreased for models that had higher RMSE and more parameters than the original model (like V4, which had higher RMSE for all three datasets but a lower AIC). How can the updated model be a better fit than the original if it had higher error? Maybe there is a better way to show model improvement than these statistics which don't present a very strong case.

It would also be helpful to include Pearson's r in Table 2.

Figure 8: It's not clear which axis scale (right or left) is used for the bars and which is used for the line

Figure 9: Showing some measurements from sites of contrasting mycorrhizal associations to compare with the model here would make a much clearer case for whether this model behavior is realistic and whether it represents an improvement compared to the original model. It would also be useful to show the prediction of the original Yasso15 model on these plots to show how the mycorrhizal model compares to the original.

Line 339: It seems like "environment of plant litter decomposition" could refer to any number of processes, from microbial community to litter quality to physical and hydrological effects of the litter layer… It is useful to assess the combined effect of factors that are integrated by different mycorrhizal associations, but it doesn't provide much insight about the underlying processes. I think this makes it somewhat inaccurate to call this a "mechanistic" approach.

Line 356: It is difficult to measure changes in composition and breakdown of litter components over time, but that does not mean modeling these processes is easy either! In fact, it's often more difficult to model processes that are poorly understood from the observational side because it means there is a weaker theoretical basis for developing a model.

Line 357: I would say litter decomposition, not soil C

Line 379-381: If mycorrhizal associations are tightly correlated with temperature, wouldn't this also affect the calibrated model? How can we know that the model's results in terms of temperature and mycorrhizae effects are not also driven by large-scale covariation between climate and mycorrhizal association? One way to investigate this would be to show observed patterns of decomposition from sites with similar climates and contrasting mycorrhizal associations compared with the model as in Figure 9.

Line 407-408: The model does not provide mechanistic insights since it just relates decomposition rates to overall site mycorrhizal associations, not to specific underlying processes. And the model addresses litter decomposition, not soil C.

Line 409:-411 The accumulation of recalcitrant compounds and impacts on labile compounds were model results and were not validated with any measurements of compounds such as lignin or soluble C over time, so I would be wary about interpreting this too confidently.

Line 413: The differences in RMSE from Table 1 and Figure 8 were quite small, so it seems like a stretch to say that it "greatly improves the accuracy." The difference was from RMSE of 19.9 to 19.3, or 10.55 to 10.5, which seems barely significant. Or, according to Figure 8, just a couple of percent of RMSE. If there are alternative metrics that show a clearer improvement, it would be helpful to highlight those. And the model predicted to litter decomposition, not SOM dynamics.

Line 443-444: The differences in recalcitrant compounds are purely a model result, not constrained by any measurements of litter composition over time so I would be wary of drawing this conclusion too strongly.

Figure C1: It's hard to tell much difference between the two model versions from this figure. Would color coding the dots by mycorrhizal association of each site help to highlight any improvements from adding mycorrhizal effects to the model?

Fig. D2: I think this caption should say leaf material, not root material