Biogeosciences Discuss., referee comment RC4
https://doi.org/10.5194/bg-2021-275-RC4, 2021

Comment on bg-2021-275

Anonymous Referee #4

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-RC4, 2021

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.

Other comments

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.

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?

Fig. 1: What are the time units for the fluxes % yr^{-1}?

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

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

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.).

Fig. 8. It may be useful to show R^2 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?

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?

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.

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).

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.

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