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Comment on bg-2021-275

Anonymous Referee #1

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-RC1, 2021

Huang and co-workers propose an improved version of the C cycling model Yasso that includes the role of mycorrhizal fungi in litter decomposition. It is recognized that mycorrhizal fungi play a major role in decomposition, and that distinguishing EM and AM fungi can increase the level of mechanistic detail in C cycling models, so the topic is timely and suitable for the readership of Biogeosciences. The manuscript is mostly clear and the figure and tables provide a good summary of the findings. I have, however, some conceptual and technical concerns, in addition to comments on the text and presentation.

Main concerns

- The main result (in my view) is that EM fungi slow down decomposition compared to AM fungi, based on the sign of the m coefficients in Eq. (2). This equation is not mechanistic, meaning that it does not model fungi per se, but rather it accounts for the effect of GPP on decomposition rates, assuming that such an effect is mediated by fungi. Fungal effects come into play because some species are associated with EM and others to AM, so the GPP effect varies from negative to positive. However, it is not possible with this formulation to attribute the altered decomposition rates to fungi. It is possible that decomposition is just faster or slower—for given litter type—depending on vegetation type. For example, needles of pine trees (often associated with EM) have high C:N and decompose relatively slowly, so that they can negatively affect the decomposition of the incubated litter by immobilizing nutrients or capturing labile C and nutrients percolating through the forest floor. In other words, I wonder if the interesting results found here are actually an indication of site effects mediated by plant community composition in general, rather than mycorrhizal fungi in particular. Without a clearer mechanistic link between occurrence of AM or EM fungi and decomposition, it is difficult to attribute these effects to fungal activity.

- Calibration/validation. L190: it is not clear how the validation data was selected—20% of data points within one decomposition time series, or 20% of the time series? If the validation was done on data points within the same decomposition time series on which also calibration was performed, it would not represent a very strict test, as points within a time series are well correlated. In Table 2, it is shown that RMSE actually increase in some of the improved versions of the model, which have more parameters. With a higher degree of freedom, I would expect a reduction in RMSE, unless the model is constrained in such a way that the 'improvement' is actually counterproductive and decreases model performance.
- Yasso is based on pools defined according to chemical characteristics, so it should be possible to test the model against lignin data, which are often available together with mass loss data in litter decomposition datasets. I would suggest using also lignin data, as the model is currently poorly constrained using only total mass loss for calibration.
- Section 3.3: if I understand correctly, this figure is drawn by assuming the same baseline model parameters and then adding mycorrhizae in Myco-Yasso. But this can increase or decrease decomposition rates, depending on whether the m coefficients in Eq. (2) are positive or negative and on the proportion of AM vs. EM fungi. So the shift in the mean mass remaining can be guessed by looking at the sign of the AM or EM effect. The reduction in variance could be linked to the change in mean, and might not be an intrinsic property of the Myco-Yasso model. My impression is that a more meaningful comparison could be done by setting parameters after fitting the original and modified models to the same dataset, so the mean mass loss is constrained. Then the change in variance can be attributed to the model modifications, not the overall different decomposition rates.

Other comments

L26: the model is limited to litter decomposition, so I would not conclude that results are relevant for soil C modelling

L62: selective uptake of N does not necessarily increase recalcitrance—it just leaves more C behind. What is the mechanism for increased recalcitrance?

L91: please define "best representation"—according to what criteria?

L94: model errors, but also robustness

L162: for consistency, “WAEN” not “AWEN”

L171: “a model where the magnitude…”

Eq. (2) and Table 1: units of the m coefficients should be the inverse of the units of GPP, so units in Table 1 are not correct

L212: wouldn’t it be more interesting to let these proportions vary to see the effect of EM vs. AM fungi?

L213: please use consistently either annum or year as time unit

L253: large positive residuals would equally show low predictive power, such as in the low EM% case

L296: this is an interesting result!

L353: I would rather say that litter decomposition is one of the most studied and understood aspects of C cycling… much less is known about C stabilization in the mineral soil for example

L399: then the mechanism for increased recalcitrance mentioned in the Introduction is the production of N poor and chemically recalcitrant necromass?

L470-471: “i1”, “i2”, “i” should be subscripts

Table C1: are these parameters resulting from calibration of the whole dataset? In L194 it is explained that models are evaluated for different datasets separately, so I was expecting a parameter table for each dataset

Fig. C2, legend: “parameters of the Myco-Yasso…”