Biogeosciences Discussions

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Reply on RC1

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-AC1, 2021

Response to Reviewer 1

R1.0. 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.

Re R1.0: Thank you for your overall positive feedback as well as for the detailed suggestions to improve the manuscript. Our work incorporated the mycorrhizal fungi impacts as a driver of decomposition conversion rates, explicitly decoupled from climate. The mycorrhizal impact is based on the decomposition of chemical components of different decomposability (WAEN) and their mass flows in the litter. Like in most other process-based ecological models, in our model, mycorrhiza activities are not modelled directly as enzyme activities, but we represent the overall mycorrhizal impact as a function of the mycorrhizal abundance in the vegetation (reflecting the biomass-ratio hypothesis; Grime 1998) and well-established impacts on different pools of decomposability, as incorporated in many soil carbon models, including original versions of Yasso. The model was calibrated and validated using in-situ measurements of the dynamics of WAEN fractions with decomposing litter material over time. We have addressed the referee comments point to point as indicated below. Our responses are highlighted in bold.

Main concerns

R1.1. 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.

Re R1.1: We agree that mycorrhizal environments are considered here to represent the overall impacts of mycorrhizal activities. As summarized in the reply to R1.0., we accounted for the mycorrhizal fungi impacts on the conversion rate in the model, similar to what other soil C models do. These impacts (for a given abundance of mycorrhizal fungi) are represented by the *m* parameters that are explicitly set being independent from GPP (making decomposition go slower or faster depending on the presence of mycorrhizal fungi). In this way, mycorrhizal presence and identity modify the actual decomposition rate of litter with a particular decomposability.

In addition, decomposability itself is indeed likely to be affected by the composition of the vegetation and of plant species identity in particular (as indicated by the reviewer). To account for this dependency, litter decomposability were taken as inputs of the model (information comes from litter bag experiments). Please note that the decomposition results examined here were also from litter bag experiments. The model shows how the local environment would affect the decomposition taking place in the bag, separately accounting for the impacts of mycorrhizal fungi and the climate. We will clarify this line of reasoning throughout the text.

R1.2. 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.

Re R1.2: Firstly, the validation dataset was selected as 20% of the time series. We will make it clear in the manuscript, rephrasing the current Lines 190-191, "using 80% of data (decomposition time series) randomly drawn from the dataset for calibration and the remaining 20% of the data (decomposition time series) used for validation". Secondly, for Table2, the RMSEs were shown for the validation dataset containing data not used in calibration. However, the AIC and BIC were based on the performance of the calibrated dataset, which used 80% of the dataset. We will specify this in the Methods section to avoid confusion, rephrasing the current text starting from Line 194: "We use root mean square error (RMSE), Akaike information criterion (AIC) and Bayesian information criterion (BIC) as the criteria for comparing relative quality of models based on

the 'cross-validation dataset' (RMSE from the 20% validation dataset, AIC and BIC based on the 80% data used for calibration), thereby selecting the optimal model".

It is true that if we would have used the same data for calibration and validation, we would expect to see the RMSE decrease with the increasing parameter number. However, part of that decrease will be due to over-parameterization which would cause the model to perform worse when tested with data not part of the calibration. This is reflected in the validation dataset results which was not used in the parameterization process.

R1.3. 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.

Re R1.3: As summarized in the reply to R1.0., the model is constrained with measurements of litter composition from the initial stage till the end of the decomposition. Lignin is indeed available in some of the databases, but it is not available in all datasets. Instead, we used data on the WAEN fractions, available in the datasets (next to datasets that indeed only had a total mass loss). See Line 145, "Chemical composition data contains the initial composition of litter in terms of WAEN fractions which were measured for each site, and this data together with other environmental data were used for initializing the model". We will modify this sentence by adding "…chemical composition data (WAEN components in the initial litter, during decomposition process and at the end of the decomposition) were supplemented…". These WAEN fractions also indirectly account for lignin as the majority of the constitutions in N pool consists of lignin.

R1.4. 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.

Re R1.4: This sensitivity analysis reflects the general model prediction variability as a result of the uncertainty in parameters. And indeed, the original and modified models were fitted to the same dataset, but the two models were showing different mean values of prediction. The variation in predicted mean values compared to the original and modified model is a result of the two models' distinct structures. Because their abilities to predict litter decomposition under different ecological environments vary, the modified model will have different predictions compared to the original model under different climate conditions. However, given that we base our analysis on the change in decomposition upon a change in mycorrhizal abundance, these differences in mean values do not affect our sensitivity analysis results.

Other comments

R1.5. L26: the model is limited to litter decomposition, so I would not conclude that results are relevant for soil C modelling

Re R1.5: We agree with this comment, and will re-phrase the statement stressing that we assessed the litter decomposition process in topsoil profiles across 10-years, examining the processes important for initial stages of SOM formation, yet not for long term soil C turnover. Further, we mentioned in the main text that more work needs to be done to include the most recalcitrant compartment of soil defined by the Yasso model as "humus" and address the fate of stable, mineral-associated soil C, the ultimate pool of soil-sequestered C (See discussion section4.4 in Lines 412-434).

R1.6. L62: selective uptake of N does not necessarily increase recalcitrance—it just leaves more C behind. What is the mechanism for increased recalcitrance?

Re R1.6: With less N, C is bound to more recalcitrant molecules, see for instance the paper 'Carbon availability triggers the decomposition of plant litter and assimilation of nitrogen by an ectomycorrhizal fungus (Rineau et al., 2013)'.

R1.7. L91: please define "best representation"—according to what criteria?

Re R1.7: Please find the description of the model selection criteria in the methods section "We use root mean square error (RMSE), Akaike information criterion (AIC) and Bayesian information criterion (BIC) as the criteria for comparing relative quality of models and thereby selecting the optimal model. The conceptualization with the lowest RMSE, AIC and BIC was selected as the optimal model with the best performance", Lines 194-199.

R1.8. L94: model errors, but also robustness

Re R1.8: Thanks for pointing this out, we adopted your suggestion and will change the sentence in the current Line 94 to "…in terms of model errors, robustness and temporal dynamics".

R1.9. L162: for consistency, "WAEN" not "AWEN"

Re R1.9: Thank you, we will correct it to "WAEN" and make sure the whole paper use the same order of "WAEN" for consistency.

R1.10. L171: "a model where the magnitude…"

Re R1.10: Thank you, we will make the sentence clear by changing it to " Myco-Yasso.v1 – a model where the magnitude of mycorrhizal impact on carbon loss from each of the W, A, E, and N pools differs among the pools".

R1.11. 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

Re R1.11: Thank you, the unit of GPP per unit area is "g m^{-2} yr^{-1}", thus the unit of *m* coefficients should be "g^{-1} m^{2} yr".

R1.12. L212: wouldn't it be more interesting to let these proportions vary to see the effect of EM vs. AM fungi?

Re R1.12: We indeed performed the sensitivity analysis by varying AM and EM proportions, but we did not include it in the appendix as we also found that the magnitude of this effect from mycorrhizal proportions on litter decomposition was dependent on the climate conditions (while the sensitivity analysis was performed at global mean climate conditions). We do not have enough space for more discussions on this aspect, but we plan to address this issue in future papers. However, we included an estimation of litter decomposition by varying AM or EM proportions in the supplementary material (Fig.S1&Fig.S2) which might be interesting for some of the readers.

R1.13. L213: please use consistently either annum or year as time unit

Re R1.13: We will modify the time unit to "yr^{-1}" and make sure the units in this paper are consistent.

R1.14. L253: large positive residuals would equally show low predictive power, such as in the low EM% case

Re R1.14: We would like to emphasise that the residuals are inclined to be negative in these cases. Indeed, the low EM% cases with large positive residuals also show low predictive power. We have added this fact to the sentence by modifying the sentence to "The model had relatively large negative residuals at low values of the AM fractions (AM<10%) and high values of EM fractions (EM>85%), but relatively large positive residuals at low values of EM fractions (EM<10%), which suggest a lower predictive power in these data groups".

R1.15. L296: this is an interesting result!

Re R1.15: Thank you for your support.

R1.16. 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

Re R1.16: We agree that there are other less-known aspects of soil C cycling.

Still, there are a lot of uncertainties and unknowns about litter decomposition when we consider microbial interactions and global climate changes. Therefore, we will rephrase the sentence as follows: "The temporal dynamics of organic matter decomposition is essential for a comprehensive understanding of soil C cycling. However, there are still many uncertainties and unknowns in litter decomposition dynamics." Line 354.

R1.17 L399: then the mechanism for increased recalcitrance mentioned in the Introduction is the production of N poor and chemically recalcitrant necromass?

Re R1.17: This relates to comment R1.6: with less N, C is bound to more recalcitrant molecules. Please see our reply to R1.6.

R1.18 L470-471: "i1", "i2", "i" should be subscripts

Re R1.18: Thanks for pointing this out, we will pay attention to these symbols and make sure they are in the correct format, indeed subscripts in this case.

R1.19 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

Re R1.19: It seems that there are some misunderstandings. The parameters in TableC1 are the result of the calibration of the whole dataset including CIDET, LIDET and ED. In the text starting at Line 194 (current manuscript version), we explained that the model performance of RMSE and R^2 were evaluated separately "to account for the fact that the data in the different datasets varied in measurement uncertainty and the number of observations". Thus following the calibration for the dataset as a whole, the resulting parameterization set was used to assess the prediction accuracy of each dataset separately. We will modify our descriptions to avoid confusion.

R1.20. Fig. C2, legend: "parameters of the Myco-Yasso…"

Re R1.20: We agree, this is a confusing sentence, and we will change the legend to "Correlation between parameters of Myco-Yasso C model…." as suggested.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-275/bg-2021-275-AC1-supplement.pdf