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Comment on bg-2021-327

Anonymous Referee #2

Referee comment on "Implementation and initial calibration of carbon-13 soil organic matter decomposition in the Yasso model" by Jarmo Mäkelä et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-327-RC2, 2022

The preprint manuscript "Implementation and initial calibration of carbon-13 soil organic matter decomposition in Yasso model" describes calibration of the Yasso model to 13C data collected from a litterbag decomposition experiment. The model was calibrated using 13C values measured on sequential extracts of pine litter and branch samples from a 4-year litterbag experiment. The decomposition parameter matrix of the Yasso model was modified to account for 13C using simple scalars. After optimization, three out of 4 scalars were negative, which was consistent with the hypothesis that 13C is preferentially retained in decomposing organic matter. The optimized model was applied to data from a peat core and produced more realistic predictions than the default model.

This manuscript is clear and concise. However, I think this manuscript should be framed differently to better showcase the results. The manuscript is framed narrowly in terms of soil carbon sequestration as a climate mitigation tool. However, the analyses and results are not directly relevant to soil carbon sequestration efforts. Specifically:

- The study system is unmanaged and focused on C cycling in litter and organic soils, and has no obvious connection to the agricultural soil carbon management strategies listed in the introduction.
- The 13C calibrated model performs no better at predicting changes in bulk C, hence its relevance to soil carbon measurement and verification efforts are unclear or at the very least indirect.

Later in the manuscript the significance of the 13C calibrated Yasso model is described differently, in terms of integration with 13C enabled ESMs. This seems like a much clearer justification for the calibration effort. Taken at face value, the results presented here are nearly trivial: calibrated the Yasso model to 13C data results in a better fit to 13C data. As a technical result, this is to be expected. What is the concrete significance of this incremental advance for our understanding of soil carbon cycling? What can the calibrated model eventually tell us about the cycling of the bulk C pool or the broader functioning of soil beyond fractionation of 13C?

If the 13C modifiers are generalizable to other systems (which may or may not be the case), I can see how they might enable the Yasso model so that it could be calibrated based on tracer experiments or in cases where the d13C of vegetation has shifted, or how it might be useful for interpreting time series of 13CO2 data to attribute fluxes to different soil C pools. These sorts of application are alluded to, but perhaps the manuscript would stand on its own more clearly if it was framed more clearly as an intermediate step towards these larger scientific goals.

Detailed comments:

Abstract: Details of the calibration dataset are not given in the abstract – consider including them.

Line 1; Line 10: I agree that strategies for increasing soil carbon as a climate mitigation strategy have received increasing attention over the years. However, I think this initial framing is an innapropriate place to start this manuscript (see broader comments above). Carbon cycling in soil is a fundamental aspect of terrestrial ecosystem function. Soil carbon influences the climate system and a whole range of global biogeochemical cycles regardless of how we try to manage it.

Line 7: I suggest deleting "despite of their simplicity", as it implies that we expect that simple modifications will not generate improvements.

Lines 21-32: This paragraph begins by addressing the challenge of deciding which processes to include in models, but the application for 13C seems to mostly relate to parametrization. Is 13C useful for both determining model structure and fitting parameters? Are these distinct challenges?

Line 28: Writing edit -- delete "By" before "estimating".

Lines 114-115: In other words, the precipitation and temperature dependence was the same for both isotopes? These factors are included in the original "alpha" term?

Line 126: how were the parameter "grid" and increment refocused? Was this done in a systematic way?

Figure 1: What do the color gradients represent? Likelihoods, presumably? In the panels situated along the diagonal, does the vertical axis on each panel show the likelihood? What do the vertical lines represent – parameter values at maximum likelihood? This caption needs to be expanded to clarify.

Figures 2-3: Why does d13C change over time in the default case? The default parameters are identical for 12C and 13C, correct? In this case, shouldn't the 12C:13C ratio be preserved in all transformations, and the d13C value remain the same over time?

Methods section: Please include details about the computing methods. How were these procedures implemented? What computing environment was used (e.g., Python, R, Matlab)? Were any R packages used to assist with fitting?

Lines 131-132: I believe there are formal methods for evaluating collinearity between parameters. Computing a "collinearity index" might be useful for determining whether the parameters are identifiable (although such indices still reduce to qualitative rules of thumb). There are methods in R for this sort of analysis (package "FME" might be useful).

Lines 151 – 152: Here the emphasis is on incorporation into ESMs, not MRV for soil carbon sequestration.

Lines 145-146: So depth and time have been exchanged? Is this based on an assumption that the peat is accreting linearly? How was the conversion between depth and time parametrized? Why 10 year intervals, why not 20 or 50 years? More justification/expanation is needed here.

Lines 167-169: I do not follow this reasoning. Is the non-ideal finding that the parameter for the N pool is positive? How does the lack of depth resolution explain this?

Lines 179 – 180: The results presented here indicate that calibration of 13C parameters to 13C data improves accuracy and predictive power for 13C. However, they do not show how this improves the skill of the model with respect to bulk C pools or fluxes. What can these results tell us beyond 13C fractionation?