Biogeosciences Discussions

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Comment on bg-2021-327

Anonymous Referee #1

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-RC1, 2022

This manuscript describes new stable carbon isotope capabilities added to the Yasso model. The new model capabilities are described clearly. The model updates were parameterized and evaluated using measured datasets in a way that was well described and justified. Overall, I though the manuscript was a clear and concise description of a valuable new model capability. 13C measurements are a common metric for understanding soil organic matter decomposition processes and adding this capability to a SOM model is a valuable advance.

I did think that in some areas the introduction and conclusions went beyond the scope of the actual results. Specifically, the model developments and testing were entirely focused on ^{13}C fractionation and did not include changes to or evaluation of overall soil C decomposition rates. Therefore, the hypothesis in the introduction about "significant improvements in SOM decomposition predictions" seems broader than is justified. The study does yield improvements in predictions of ^{13}C dynamics, but this was not used to improve overall SOM predictions.

The first two paragraphs of the introduction (lines 10-20) provides a good justification for improving SOM models. However, the focus in these paragraphs on agricultural soils and carbon monitoring is not well related to the actual model structure and evaluation which only includes litter decomposition and peat systems. Carbon sequestration in mineral soils is sensitive to mineral-organic interactions and mineral-associated organic matter accounts for a large fraction of SOM (e.g., Lugato et al., 2021). However, Yasso does not include mineral interactions and treats humus as a passive pool and was only evaluated using litter and peat decomposition. Therefore, it does not seem justified to introduce the model in the context of agriculture soils. Since the model seems intended to simulate peat systems, I think it would be more reasonable to introduce it in the context of better understanding and predicting carbon dynamics in peatland or organic soils.

Reference: Lugato, E., Lavallee, J. M., Haddix, M. L., Panagos, P., & Cotrufo, M. F. (2021).

Different climate sensitivity of particulate and mineral-associated soil organic matter. Nature Geoscience, 14(5), 295–300. https://doi.org/10.1038/s41561-021-00744-x

Other comments:

Section 2.1: The peat depth profile measurements that were used to validate the model should also be described in this section.

Figure 1: It would be helpful if the figure axes used the *L* notation that is used in the text so it is clearer what is being plotted. Is marginal likelihood in these plots the same as *L*?

Figure 2: Consider using different symbols for the branch and needle data to accommodate red-green colorblindness (which is common) or in the case of printing the paper in grayscale.

Line 138: It was not immediately clear to me how relative ^{13}C content can change over time in the default model without any fractionation included. I think this occurs because the initial pools have different isotope ratios and are mixing over time which causes the isotope ratios to change. But a more specific explanation of this would be helpful. It might also be helpful to show a diagram (perhaps in the appendix) of transfers among the different pools so it is more clear what kind of mixing over time can occur.

Line 145: The actual depths should be included. And I suggest including a more detailed explanation of why the depth sampling was consistent with the 10 year age assumption. Was there evidence from that site that the age difference was actually close to 10 years across depths?

Figure 3: I suggest splitting this figure into separate panels as in Figure 2. The large number of lines and colors makes the figure difficult to interpret. Also, can bulk ^{13}C in the model be calculated to compare with the bulk ^{13}C measurement from peat?

Line 162: The negative parameter values are consistent with the theoretical expectation of slower ^{13}C decomposition rate (as described in the introduction) which is a good result for the model and would be valuable to point out more explicitly.

Line 167: “This situation is not ideal” – why not? Is it inconsistent with measurements or theoretical expectations? It doesn’t seem particularly unreasonable to me.

Line 179-180: It's not clear to me how the results demonstrate improvement to SOM model accuracy and predictability since they were not used to inform any changes to the overall C decomposition rate or structure. Improvements were limited to ^{13}C dynamics.

Line 189: Similarly, it's not clear that the study made improvements to SOM decomposition in general outside the direct comparisons to ^{13}C content of organic matter pools.