<h1 style="text-align:center">Letter of Responses</h1>

Dear Dr Liang,

R1 (original R3) has submitted further comments; could you please respond to this review?

Best regards,
Kate Buckeridge

Response: Dear Dr. Buckeridge, we are grateful for the follow-up comments from the reviewer. We have revised the manuscript based on the new comments. Detailed point-by-point responses to the comments are below. We hope you will find our revision satisfactory.

Report #1
Anonymous Referee #3:
I appreciate the authors addressing my comments and believe the manuscript will be of interest to readers of SOIL. As stated in my previous review, I think this work addresses an important uncertainty in the SOM field.

Response: We appreciate the reviewer's recognition of our work. We are grateful for the reviewer's constructive comments during the peer-review process.

I have a few minor follow-up comments:

In response to my comment 3: I appreciated this response and think it would be helpful to a reader to state that running the model to steady state was a 500-year model run. That was not clear to me and makes the use of "C sequestration" more valid.

Response: We agree that adding information about "500-year model run" would help readers understand the model's steady state and long-term predictions of C sequestration. We have revised as suggested (line 178-179): "*The calibrated models were run for 500 years to steady states to compare the modeled SOC change under different scenarios.*"

In response to my comment 4: I suggest that the authors add the amount of glucose-derived MAOC as a percentage of total MAOC at line 212 so the reader can compare POC and

MAOC. Having that information from the authors was helpful for me to understand that the contribution to both POC and MAOC overall was small. Additionally, I think it is important for the authors to acknowledge that DOC still contributed dominantly to MAOC, and so a complete inconsistency with the two-pathway model is not fully supported (lines 261-264).

Response: We agree that adding more information about MAOC is helpful to readers comparing the POC and MAOC formation. We have revised the text in lines 204-205: "*At the end of incubation, the proportion of glucose-derived C to total POC was 0.16% ~ 0.67% and to total MAOC was 0.26% ~ 1.46%.*"

Additionally, we agree that our results are not completely inconsistent with the two-pathway model. They are more like a complement to the framework. To express more accurately, we have revised the text in line 253: "*The result that labile C can enter the POC pool is partially inconsistent with the two-pathway framework.*"

In response to my comment 12: It would be helpful to say in the methods in the manuscript that the authors calibrated to the sum of heavy and light POC.

Response: In the revision, we have added more descriptions about POC calibration (line 165-166): "*For the POC pool, heavy-POC pool and light-POC pool were summed together for the calibration.*"

In response to my comment 13: To clarify, the 100 parameter sets used in the subsequent predictions were chosen from the posterior distribution of the Metropolis algorithm? That would be helpful to state to readers.

Response: Yes, the 100 parameter sets used in the SOC predictions were chosen from the posterior distribution of the Metropolis algorithm. We have added this statement in the revision (line 174-175): "*After the model calibration and validation, we randomly selected 100 sets of parameters from posterior PDFs of the adaptive Metropolis algorithm for further modeling experiments.*"