Response to comments

Paper #: *GMD-2023-114*
Title: *Modeling biochar effects on soil organic carbon on croplands in a microbial decomposition model (MIMICS-BC_v1.0)*
Journal: *Geoscientific Model Development*

Editor:

Comment #1

Thank you for your revisions to your manuscript, and apologies for the delay in its processing. While your revisions were helpful, they didn't address the biggest concerns of the reviewers and me.

Response #1

We thank the Editor and Reviewer 2 for the additional comments. Following your suggestions, we have reorganized the text to separate the results from the discussion and make the Result section follow the same structure as the Methods section. Please see the detailed point-by-point response below.

Comment #2

As you can see from the latest report from Reviewer 2, the organization of the paper needs to be improved:

1) Methods for all analyses undertaken in the paper should be explained in the main text.

Response #2

As suggested, the methods for all analyses conducted in the paper were introduced in the Section of Method.

Comment #3

2) The Results section should generally follow the same structure as the Methods section—just as you lead the readers through the planned experiments and analyses, so should you lead them through the results.

Response #3

As suggested, we reorganized the text to make the Results section following the same content structure as the Methods section.

Comment #4

3) The Discussion must be its own section; avoid mixing in new results. This would allow more space in the Discussion for interpretation of the many, many complex results that you present in the paper. I appreciate your desire to keep Section 3 focused on model calibration results, which can be achieved by adding a new section specifically related to Sensitivity Test results.

Response #4

As suggested, we added a separate section of "**4. Discussion**". The sensitivity test results were moved to the corresponding newly added Section of "**3.1.2 Sensitivity analysis for MIMICS versions without biochar**" and "**3.2.3 Sensitivity analysis for MIMICS-BC versions**".

Comment #5

Part of the reason it took Reviewer 2 so long to get their comments back is because the paper was so hard to follow. Improving its organization as outlined above is critical for reader understanding.

The Discussion section generally needs to be expanded, especially with more discussion of what the results mean for subsequent work. There are a lot of model versions presented here, and often no clear "winner." Which model setup(s) should future researchers/developers choose and why? Are there reasons that one might choose to use a particular model version even if it doesn't have, say, the lowest AIC?

In addition, please note Reviewer 2's comment about how much this calibration actually improved things. It's still publication-worthy if not much improvement happened—in fact, it's really interesting! But this result and its possible causes should be explored more fully in the Discussion.

Response #5

We thank the editor for the constructive suggestions. We added sentences on **Line 510-536** in **Section 4.1.1** to discuss these points: "We presented a framework to quantify the impact of microbial density-dependent turnover, sorption, and soil moisture effects on cropland SOC dynamics. Regulatory mechanisms (e.g., competition) may limit microbial population sizes, and neglecting this process could lead to indefinite microbial biomass growth with increasing litter inputs (Georgiou et al., 2017). Our analysis demonstrates that restricting microbial biomass size through density-dependent microbial turnover (MIMICS-T) slightly improves the model performance (Fig. 4), but further including sorption and soil moisture effects (MIMICS-TS and MIMICS-TSM) has negligible contribution to the model performance. One possible reason is that the inclusion of these new processes greatly increases model complexity, but in the parameter calibrations, SOC is still the only observational variable to constrain all the newly added processes. Therefore, the model parameters may not be fully constrained due to the lack of direct observations on these new processes.

Another reason is that some other possibly important processes are missing from the model. For example, the MIMICS-TS version considers the impacts of soil clay on the adsorption capacity of SOC_a, but soil pH, ionic strength and mineral content are also found important to the sorption-desorption of SOC (Kothawala et al., 2009; Mayes et al., 2012). The metal ion Ca^{2+} can form bonds between negatively charged clay minerals and available SOC via cation bridging, enhancing the adsorption of available SOC by soil clay minerals (Roychand and Marschner, 2014; Setia et al., 2013). Soil pH can also impact SOC sorption by altering the ionization degree and the surface change of SOC molecules (Shen., 1999). Moreover, the iron minerals can preferentially bind to lignin components through sorption and coprecipitation, protecting it from microbial degradation and consequently increasing SOC (Liao et al., 2022). In addition to the sorption-desorption process that is associated with microbial accessibility to SOC, other factors that influence microbial activity are also underrepresented in MIMICS, such as the soil nutrient availability (e.g., nitrogen), which greatly impacts microbial use efficiency and growth rate (Manzoni et al., 2017). These processes improve our understanding of the mechanisms of SOC dynamics and should be incorporated in the model to represent the microbial-mineral processes realistically and mechanistically.

However, as shown in our results, without further observational constraints on each process separately, the model performance only relying on calibrations against total SOC contents may not improve. Therefore, various versions of MIMICS, representing different levels of our understanding on microbial-mineral processes, are retained in our study for further calibrations when sufficient observations emerge."

Comment #6

Various comments re: Response #20 part 3:

- L344-9: "4) with two new biochar-related parameters (i.e., fd and fv) optimized only in the fluxes from SOCa to MIC pools (MIMICS$_{TSMb}$-BC$_{DV-SOCa}$)"—does this mean the parameters are only included in those fluxes, or that they're only *optimized* for those fluxes?

Response #6

It means the two parameters (f_d and f_v) were optimized, and f_v is only included in those fluxes from SOC_a to MIC pools. We modified sentences on **Line 355-356** in **Section 2.3.3** to make it clear: "4)

with two new biochar-related parameters (i.e., f_d and f_v) optimized and f_v only included in fluxes from SOC_a to MIC pools ($MIMICS_{TSMb}$-$BC_{DV-SOCa}$).".

Comment #7

- L344-9 mention the SOCa experiment as $MIMICS_{TSMb}$-$BC_{DV-SOCa}$, but L409-15 mention it as MIMICSTb-BCDV-SOCa (i.e., T instead of TSM). Table 1 makes it seem like you added both. And indeed, the results include both, but one for the short-term and the other for the long-term. Why?

Response #7

Both MIMICS-T and MIMICS-TSM$_b$ versions were used for the biochar addition. The corresponding model versions incorporating biochar, i.e., $MIMICS_T$-BC and $MIMICS_{TSMb}$-BC, were calibrated and validated using two datasets, one for short-term and the other for long-term. We have explained this point on **Line 361-362** in **Section 2.3.3**: "As an alternative model version, we also tested implementation of biochar processes in MIMICS-T that have a highest R^2 and lowest RMSE and AIC in model validation (Fig. S5b)", and on **Line 370-372** in **Section 2.3.3**: "Specifically, the 8-yr SOC data with biochar addition is the sum of field control SOC observations (short-term) and the residual biochar carbon in soil after 8 years. These extended long-term data were also used for model calibration and model evaluation.".

Comment #8

- L344-9: Text should be added here explaining why these were chosen.

Response #8

As suggested, we added a sentence on **Line 349-350** in **Section 2.3.3** to explain it: "One control simulation without any biochar process and three experimental simulations with different biochar processes (**Section 2.2**) were set to test the possible mechanisms of biochar impacting SOC dynamics.".

Comment #9

- Discussion should be added—what does it mean that the $BC_{DV-SOCa}$ setup performed about better than the BC_{DV} setup?

Response #9

As suggested, we added sentences on **Line 568-575** in **Section 4.2.1** to discuss it: "Compared to $MIMICS_T$-BC_{DV}, $MIMICS_T$-$BC_{DV-SOCa}$ performed better in the short-term SOC response to biochar addition but worse in the long-term response (Fig. 6). When biochar is applied, the labile carbon fraction may be immediately utilized by microbes, and thus adding biochar effects on the SOC_a process is important for the short-term response. In the long term, the SOC mineralization may be gradually suppressed via physical protection (Zimmerman et al., 2011), but both $MIMICS_T$-BC_{DV} and $MIMICS_T$-$BC_{DV-SOCa}$ do not include the adsorption process. In the $MIMICS_{TSMb}$-BC versions that include the adsorption process, the available SOC may be partly adsorbed by soil minerals and become physically protected. This could lead to the positive priming effect of biochar on SOC being less evident (Fig. 7).".

Comment #10

Finally, some miscellaneous comments:

- Line 72: Clarify: "an increase in *microbial* biomass turnover", to avoid ambiguity with litter biomass.

Response #10

Revised accordingly.

Comment #11

- Table 1: MIMICS-T and MIMICS-TSM$_b$ appear twice.

Response #11

In **Table 1**, we modified the "MIMICS-T" to "MIMICS$_T$-BC$_{def}$" in MIMICST-BC versions, and "MIMICS-TSM$_b$" to "MIMICS$_{TSMb}$-BC$_{def}$ in MIMICS$_{TSMb}$-BC versions".

Comment #12

- Why did you use different climate data in the calibration for non-BC models vs. the BC models?

Response #12

It is due to the difference in temporal resolution of the forcing climate data between steady runs (**ANNUAL**, WorldClim, MIMICS model, **Section 2.3.2**) and transient runs restarted from the steady state (**HOURLY**, CRU-JRA, MIMICS-BC model, **Section 2.3.3**). As hourly data is unavailable in WorldClim, CRU-JRA was used for the transient hourly simulations. We modified sentences on **Line 336-340** in **Section 2.3.3** to clarify it: "In order to meet the daily time step of transient runs required by MIMICS, the two model runs are forced by 6-hour surface temperature from Climatic Research Unit and Japanese reanalysis data (CRU-JRA, Kobayashi et al., 2015; Harris et al., 2014). The climate forcing data is thus different from the one from WorldClim used for the steady runs (**Section 2.3.2**) because hourly climate data is not available in WorldClim.".

Response to comments

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Reviewer #2:

Comment #1

I want to apologies to the authors and editor for my delay in returning this review. I appreciate the revisions authors made to the organization of this paper, part of the it are more clear to follow.

That said, aspects of the organization are still confusing to me. For example, the methods describe model calibration steps followed by sensitivity tests for non-biochar and biochar experiments (section 2.3.2 and 2.3.3). I'd expect results would follow a similar structure in sections 3.1 and 3.2. (e.g. Fig 4 would be followed by results from the non-biochar sensitivity test, Fig S16). Instead, results from the sensitivity test are included in the discussion (section 4), most of which are only referred to in the SI material (especially in section 4.1). This strikes me as an odd way to tell the story for your paper that somewhat obfuscates results, but I'm not an author on the paper and will let the editor and authors decide what makes sense for this manuscript and for the journal.

Response #1

As suggested, we reorganized the text to make the Result section following the same structure as the Methods section.

Comment #2

At high C densities it seems that even calibrated versions of MIMICS underestimate soil C stocks (Fig 4). You did a ton of work to improve the realism of the model and calibrate parameters, but one take away could be that none of this did much to improve the model from its default state. Is this true? Can the authors offer any insights into processed that may be absent in the model, or other characteristics that provide these higher soil C stocks in the real world? There is limited discussion of this persistent bias in the model (line 482), but the text is rather vague, unless is rice paddies that have the high soil C stocks?

Response #2

Yes, the calibrated MIMICS versions without biochar do not exhibit significant improvement compared to the default model (**Fig. 4**). We added discussions to explain these results and possible missing processes in the model in **Discussion** (please see **Response #5** to **Editor**).

We also added sentences to elucidate the potential factors that may contribute to the higher soil carbon stocks in cropland on **Line 549-556** in **Section 4.1.2**: "In addition, managements such as fertilizer application and possible residue retention can increase SOC stock. Previous evidence indicates that the SOC is increased by 11.3% with residue return (Wang et al., 2020b) and by 13.3% with straw return and balanced NPK fertilizer (Islam et al., 2023) compared to residue removal. However, these management processes of fertilization and residue retention are not represented in the MIMICS model due to the absence of quantitative management data and the poor understanding of the mechanisms. It may explain the underestimation of SOC at sites with a high carbon density by the calibrated MIMICS models (Fig. 4). Therefore, field measurements on the effects of agricultural practices on SOC dynamic are urgently needed to further improve the model processes (Campbell et al., 2007; Congreves et al., 2015).".

Comment #3

Sensitivity test and discussion. I don't really like how a bunch of side experiments that included:

- calibrating a reverse M-M version of the model,

- conducting an idealized warming experiment, and

- adding an ad-hoc tillage modification.

These were all just thrown into a few paragraphs of the discussion and covered in a whole bunch of SI material. I appreciate that this presents a more complete picture of the work these authors undertook, without distracting from the main focus of the paper. At the same time, it seems like a somewhat disingenuous expectation to place on readers and reviewers of the paper to try and wade through a bunch of results that are only hastily described. I wonder if all of these experiments should either describe this fully in the methods and results or remove these asides from the text?

Note: this was an issue I raised before. See comments 28-31 in the author's response to Reviewer #2, which basically amounts to we changed section titles, moved a few figures to the main text, and still present 21 SI figures. Again, I might defer to the editors on how to handle this in a way that keeping with expectations for manuscripts published in GMD.

Response #3

We removed these parts as suggested. For the issue of manuscript structure, we have reorganized the text to separate the Results section from the Discussion section.

Comment #4

Minor and technical comments

Line 297: should this be Newton–Raphson?

Response #4

Yes, the Newton-Raphson was used to obtain the steady-state SOC. We added a sentence on **Line 302-303** in **Section 2.3.2** to make it clear: "The solver of "*mnewt*" is used to solve equations by iteratively calculating the values of model function "*modelx*" and its Jacobian matrix "*modeljacx*" (see codes for details in **Code availability**)".

Comment #5

Line 300: are these first order equations (they don't seem to be, based eq 1)?

Response #5

We modified the sentence on **Line 300-302** in **Section 2.3.2** to clarify it: "A set of ordinary differential equations were built to express the dynamics of carbon flows in soil over time, and it can be solved numerically to obtain the steady carbon pool sizes.".

Comment #6

In Fig 4 are blue points that were used for model calibration and red dots those used for validation? If so, maybe clarify this in the figure caption?

Response #6

Yes, it had been explained on the caption of **Fig. 4** in the original manuscript as: "Blue and red dots in (a-d) represent observation sites for model calibration (80% sites) and validation (20% sites), respectively.".

Comment #7

Line 497, what emerging technologies are going to help calibrate the model better? I'd try to be specific here, or remove the vague sentence

Response #7

Removed as suggested.