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Interactive comment on "The implications of microbial and substrate limitation for the fates of carbon in different organic soil horizon types: a mechanistically based model analysis" *by* Y. He et al.

Y. He et al.

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We appreciate the constructive comments by reviewer 1, Dr. Will Wieder. We have revised the manuscript based on the discussion and have carefully taken into account of all comments.

Response to specific comments:
(1) Can the presentation of materials introduction be cast somewhat narrowly to support findings that are described in this study?

Reply: We adopt the reviewer's comments and have restructured the introduction to be consistent with the discussion section and the major message of the whole study.
(2) Model projections here are evaluated with observed soil respiration data, this should be clarified.
Reply: We clarified this in the revised manuscript (L89).
(3) Where the evaluation of C stock is presented? Is this change in C pools associated with warming and changing soil moisture?
Reply: We reworded the sentence to avoid mis-interpretation (L476). We evaluated the sensitivity of various C pools under different temperature and moisture scenarios.
(4) Page 2245, Lines26-29, I am not sure this statement is justified with data presented.
Reply: We deleted this statement in the revised manuscript (L422-432).
(5) I wonder what effects assumptions made about "fixed" parameters have in the sensitivity analyses presented in this study? For example what are effects in sensitivity analyses of holding microbial biomass to 2% of SOC and CUE around 0.4?
Reply: These values are not technically "fixed" in the model as the model dynamics determines that CUE and MIC changes over time with temperature and moisture. The CUE and MIC/SOC used here is to set an average state for inverse modeling, so that the parameters vary in a range that produces reasonable output. We also added sentences in section 4.2 to discuss the impact of this on our conclusions (L456-459).
(6) As I understand the approach, sensitivity analyses were largely conducted under standard temperature and moisture conditions.
Reply: We conducted sensitivity analysis under standard and 3 altered scenarios (a total of 4 scenarios). We added figures from elementary effects under altered scenarios in the second paragraph of results section 3.2 to make the organization structurally clearer. A permafrost effect was reflected in the altered moisture scenarios.
(7) The authors imply this model structure is better than standard approaches. How does this non-linear model compare to results generated by a standard soil C model.
Reply: We reworded sentences to focus on the contribution of sensitivity analysis on

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identifying dominating process and guide experimental work (L422-432). Comparison of this model with other standard soil C models was not addressed in this study, but is addressed in another paper that is now under review.
(8) The importance of microbial turnover and the fate of those microbial residues in amorphous soils received little discussion. These results align well with other microbial models (Wieder et al., 2014).
Reply: We expanded the discussion on these issues in revised manuscript (L428-432), and we also cited the relevant literature. See the last paragraph of section 4.1.
(9) Figure legends should be expanded with more descriptive text to help the figures stand alone as display items.
Reply: We revised figure legends accordingly.

Technical corrections:
(10) Use of the word "global" is confusing
Reply: We deleted the word global from the abstract and clarified the meaning in the last paragraph of introduction (L90-91).
(11) page 2237, I think there are 10 parameters in Table 3.
Reply: We corrected this in revised manuscript.
(12) page 2243, line 1-9. This general applicability of our findings statement is kind of empty.
Reply: We deleted this paragraph in revised manuscript.
(13) page 2254, lines 7-8. I don't understand the contradictory statements included this sentence "permafrost soils are likely to have high inherent decomposability" which is prescribed as recalcitrant in the model.
Reply: We rewrote this sentence in the revised manuscript to eliminate this inconsistency (L393-398).
(14) Table 3 is not that helpful for readers who are not intimately familiar with the model. It may be more helpful to just note key parameters with an asterisk in Table 2 – omitting table 3 entirely?

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Reply: We adopted reviewer's suggestion and marked the selected parameters (previously in Table 3) in bold in Table 2 and omitted Table 3.
(15) Figure 3: can error estimate on observed soil respiration be presented? The caption for figure 3 needs to be much more specific, describing where observational data came from and briefly outlining how modeled results were generated.
Reply: We have variance for each observed data point, but because the inverse modeling approach does not take into account observation uncertainty, we choose not to present this information in the figure. The figure caption is revised to include more information to be able to stand alone.
(16) Figs 45 have the same x-axis. Is there any advantage to stacking results from EE analyses into a vertical 4-panel figure so parameter effects on all pools are clearly shown? My only concern with this recommendation is that points and texts would be so small to communicate any information.
Reply: We actually had tried to organize our figures this way, but as the reviewer anticipated, the text and data points were too small.
(17) Could the color bar and associated text be larger in the figure?
Reply: We replotted the figure so that the text and color bar can be more easily read.

Interactive comment on Biogeosciences Discuss., 11, 2227, 2014.