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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.

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General comments:

He and co-authors present a description and evaluation of a microbial-explicit modeling framework for boreal forest soils, emphasizing results from a series of parameter sensitivity analyses. The authors correctly highlight the broader applicability of their approach to the evaluation of microbial-explicit models, but as written this argument is somewhat obscured. The paper jumps from the premise that microbial models are

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needed to results from the sensitivity analysis. Revisions of the paper could include considerations of how / why explicit consideration of microbial dynamics are “better” than a traditional approaches (based on first-order kinetics and implicit representation of microbial activity) that simulate permafrost dynamics. Throughout, a modest restructuring and reframing of the manuscript will ultimately increase the impact of the work presented. Suggestions towards this end are listed in specific comments below.

I really like the ideas brought up at the end of the discussion (Page 2247, Lines 9-27), and feel like expanding on these ideas could help frame the whole manuscript. Microbial explicit models are poorly constrained, and determining parameter sensitivity and uncertainty through analyses like the ones presented here would help generate guide critical experimental work to inform this class of models. Could this motivation be used throughout the paper, starting with the introduction?

Specific comments:

Ideas brought in the introduction seem somewhat conflated. For example the paragraph beginning on pg 2230 lines 14 implies that empirically modeling soil C dynamics from site-specific data are part of the problem with traditional soil biogeochemistry models; but the authors present a model that uses site-specific data to generate parameter estimates that may not really have any biophysical meaning. I don’t argue that the model structures presented here represents an important development in our broader consideration of modeling soil C processes; but it’s not clear how the examples introduced here, specifically response to environmental change (e.g., acclimatization) are better informed with a microbial explicit approach. Can the presentation of materials introduction be cast somewhat more narrowly to support findings that are described in this study?

Authors should take care in accurately describing the work presented. For example: 1. Model projections here are evaluated with observed soil respiration data, this should be clarified (pg. 2232 line 14-15). 2. Where the evaluation of C stocks is presented (pg.

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2248, line 2)? Is this the change in C pools associated with warming and changing soil moisture (section 2.3)? If so, a sensitivity analysis showing the importance of different parameters is presented (Fig 4-7), but not an evaluation of soil C stocks. 3. Page 2245, Lines 26-29. I'm not sure this statement is justified with data presented. I'm on the microbial model bandwagon, but why are microbial dynamics critical to simulate in boreal forest soils? What's insufficient with standard temperature and moisture relationships in first order models? These questions are not really evaluated with the data presented here.

I wonder what effects assumptions made about "fixed" parameters have in the sensitivity analyses presented in this study and the conclusions the authors draw from them? For example, what are effects in sensitivity analyses of holding microbial biomass to 2% of SOC pools and CUE around 0.4 (which may be high for sites with low quality organic matter Sinsabaugh et al. 2013)? I'm not sure new analyses are needed, but limitations of the approach and assumptions warrant discussion.

As written, it's not clear where or how sections 2.3 and 2.4 are related? Where are results from 5-year permafrost degradation simulations shown (section 2.3)? How do C stocks change with warming and changes in soil moisture using the model parameterization that generated results in Fig. 3? Were permafrost change simulations run before, after, or concurrently with exploration of parameter space with the EE analysis (section 2.4.1 and 3.1)? As I understand the approach, sensitivity analyses were largely conducted under standard temperature and moisture conditions. Altered environmental conditions are not presented until Fig 7. Perhaps matching the organization of methodological subsections (section 2), with corresponding results subsections (section 3), and display item presentation would help provide some structural clarity to help readers better understand the approach outlined here.

The sensitivity analysis that forms the core of this manuscript is well presented aside from the methodological concerns (above), but part of this paper documents a new model structure and its application in a boreal forest. This accomplishment is brushed over in the discussion manuscript (see page 2236 lines 6-9). How does this non-linear model compare to results generated by a standard soil C model (Yi et al. 2009, cited in this paper)? The authors imply this model structure is better than standard approaches (especially in global change scenarios) because it better represents our mechanistic understanding of soil microbial processes. Another interpretation of these results, however, could be that it's unnecessarily complicated and introduces too many poorly constrained parameters to be useful for projecting boreal soil C dynamics in a changing world.

One finding that caught my attention, but which receives little discussion, is the importance of microbial turnover (r_death) and the fate of those microbial residues (MIC-toSOC) in amorphous soils (Figs. 6 & 7). These results align well with other microbial models (e.g. Wieder et al. 2014), but have received relatively little attention from observational and modeling communities. How do we quantify the rate of microbial biomass production and it's partitioning to different C pools? What about the model assumptions or ecological reality makes these turnover terms more/less important in different soil environments?

Figure legends should be expanded with more descriptive text to help the figures stand alone as display items.

Technical corrections:

Use of the word "global" is confusing, especially since this paper describes a "soil decomposition model framework for boreal forest ecosystems" (e.g. page 2229, line 7-8).

I'm also not sure how to simplify the results, but the laundry list of abbreviated parameter names is cumbersome for readers to wade through and obscures some of the nice results presented in the paper (e.g. section 3.2.1 & 3.2.2).

Page 2237, line 9. I think there are 10 parameters in Table 3.

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Page 2243, lines 1-9. This general applicability of our findings statement is kind of empty. I like the broad approach presented in the sensitivity analyses (pairing EE screening tests with more detailed Sobol's test), but the generalizability of the findings that are presented aren't really that novel (i.e., parameter estimates, temperature, and soil moisture are important to modeled soil C response).

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)". How is biochemical recalcitrance prescribed in this model? Why is to prescribed if it contradicts expectations about permafrost soil C characteristics?

Table 3 is not that helpful for readers who are not intimately familiar with the model. How do these parameters relate to Fig 2? Why are there two processes called "SOC enzymatic decay to soluble C" and two processes called "ENZ turnover"? It may be more helpful to just note key parameters with an asterisk in Table 2- omitting table 3 entirely?

Figure 3. Can error estimate on observed soil respiration data 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.

Figs 4 & 5 have the same x-axis, don't they? 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 text would be so small to communicate any information?

Fig 7. Could the color bar and associated text be larger in this figure?

Additional references to consider: Wieder, W. R., Grandy, A. S., Kallenbach, C. M., and Bonan, G. B.: Integrating microbial physiology and physiochemical principles in soils with the MIcrobial-MIneral Carbon Stabilization (MIMICS) model, Biogeosciences

Discuss., 11, 1147-1185, doi:10.5194/bgd-11-1147-2014, 2014.

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