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Interactive comment on "Modeling transient soil moisture limitations on microbial carbon respiration" *by* Yuchen Liu et al.

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Major concern 1: They sampled a relatively deep depth (0-165 cm) in multiple core sections (and observed expected trends in bulk parameters), yet the measurements were not replicated, nor did they conduct analyses that seem critical to their question. For example, how does microbial biomass change under different moisture conditions, or with depth?

First, we thank the reviewer for pointing out the issue of replicates in our incubation data. We completely agree that this dataset is not as extensive as would be desired for a study specifically targeting constraint of the soil moisture – respiration relationship for a specific sample site or environment. We are currently expanding this dataset to a

highly resolved and replicated set of soil moistures and depths. However, the current study is not intended to specifically resolve a reproducible relationship for soil carbon respiration in these samples, but instead offers a simple dataset that produces the same general trend in soil moisture vs. respiration rate as reported in the vast majority of the literature for a huge diversity of environmental conditions. Therefore, we use this dataset as a simple example from which to base the model development, and leave the application of these models to a higher resolution, replicate dataset for this field site to a subsequent study. Thus, this paper is intended as the first in a series, and focuses on model establishment and comparison. In this study, we showed that the FO models (both FO1 and FO2) have a monotonic nature, which is incompatible with the general trends of the data regardless of the parameter values chosen. In contrast, the DM models (especially DM2) have the ability to reproduce the Birch effect, which represents a big advance in performance. In conclusion, the sparse dataset does not detract from the main points of this paper.

Second, we thank the reviewer for their insight regarding the quantification of biomass, particularly with reference to changes under different moisture contents and depths, and agree that the model we have developed points to the significance of this parameter value. Despite not having microbial biomass data for our incubations, we feel our modeling results are insightful and informative. In designing our experiments, we specifically sought to test the influence of variable soil moisture content on otherwise identical samples taken from the same soil core and depth, which supports the assumption that the biomass content of all subsamples is the same. Although the reviewer is correct in that the microbial biomass measurement is straightforward, our lab did not have the capability to make these measurements at the time that we conducted these incubations. Perhaps more importantly, our results point to is not the need for quantification of active vs. dormant vs. dead biomass through time, which is unfortunately more complex than measuring total biomass (as pointed out by anonymous referee #3). To address this concern, we plan to add to section 5.1 stating that a key outcome of the present model is the need for measurement of active vs. dormant vs.

dead biomass as a calibration for the DM model to natural settings. However, in the scope of this paper, model application to specific environmental conditions is not the emphasis. As such, we think the model (especially DM1 and DM2) treatment of the transition between active and dormant biomass under changing soil moisture is more important than the actual number of active and dormant biomass, since the model is capable of achieving any number of active and dormant biomass by modifying the parameters (a, b, and Sehalf). In Fig. 7, we show an illustration of the transition between simulated active and dormant biomass as the model responds to modification of the moisture content. This transient behavior of the biomass is a novel and vital contribution of the current study, since it uniquely produces Birch - type behavior rather than the simple monotonic decrease generated by simpler models.

Major concern 2: Finally, I found that paper to be overly detailed and rambling, especially through the discussion of the models in Section 4. Model Development (a combination of model description and results, oddly), as well as the Discussion. In the case of Section 4, I'm wondering if some of the detail can be included in Supplementary Material. In the case of the Discussion, the authors need to provide more of a framework at the beginning of the section that guides the readers. Also, the authors did not spend sufficient time relating their findings back to insight about process or contributions to improving models, which seems critical for a journal like Biogeosciences. Brief mention of these implications appeared in the last few lines of the Conclusions section, but would be better suited to more attention in the Discussion.

We thank the reviewer for these observations and completely agree that there is too much detail in Section 4, and a framework guiding the readers should be added to the Discussion section. The other two referees also noted the unusual structure of this paper. To address those concerns, we've decided to change the structure of the model in a more traditional way. In particular, we plan to reorganize the manuscript such that the model description will appear in the "Materials and methods" section, and the modeling results will be located in the "Results" section (as suggested by

the other two reviewers). The model description will be shortened, emphasizing the differences between the four models, and some of the currently detailed information will be moved into supplementary. In addition, the subtitles of the "Discussion" section will be modified to directly state the main take-home point of each subsection, focusing more on the big-picture questions. A new paragraph will be added after line 377 as a summary containing the key points of the discussion section. Another paragraph will be added to section "5.4 Future direction" specifically about how we are going to upscale the reactive transport model with the application of the DM2 model, for improved simulation of hillslope to watershed scale carbon cycling, which relates back to model improvements and big-picture applications.

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