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

Anonymous Referee #1

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The authors evaluated four models (two first-order models and two microbial dormancy models) with soil incubation datasets. They concluded that the dormancy model with two microbial communities and two subcategories of organic carbon performed the best in fitting the observations. My first major concern is the lack of measurements and evaluation of microbial biomass that is not difficult to do at all. Microbial biomass is one of the keywords in this study. However, I only saw an example (Fig.7) showing the transition between active and dormant biomass. The authors did not say if the biomass was measured values or just an illustration. By the way, the total biomass (6250 gC/m3) is pretty high according to Cleveland & Liptzin (2007), Wang et al., (2013) and Xu et al. (2013). Another major concern is the unusual organization of the manuscript. Generarly the model description should also be included in the 'Materials and methods' sec-

tion, and the modeling results in the "Results" section. Other comments: (1) Please insert a citation to justify the 'Se dependent inhibition factor' (Line 466-485). The authors stated 'K-selection microbial subcategory . . . utilize both labile and recalcitrant carbon', however, I did not see an equation like Eqs. 10-11 for the use of labile carbon by K-selection microbes. Did you assume there is no inhibition for (Ui=lab, j=K)? (2) Abstract: Though the authors addressed the 'dormancy model' was better than the first-order model, they did not explicitly emphasize the 'DM2' version that was actually the focus of this study. (3) Introduction: please add a brief review of dormancy models in addition to Manzoni et al. (2014). (4) Line 315 & 374: I think, in both places, 'm' & 'n' should be 2 as there are 'two microbial communities along with two subcategories of organic carbon'. (5) Line 358: please insert a citation for 'the Brooks-Corey equation' (6) Fig.1: I didn't see the 'red star'. (7) Table 4: I understand that the authors used 'effective saturation' instead of 'soil water matric potention' in Eqs. 8-9. Please justify the values for parameters 'a' & 'b' in Eqs. 8-9. The authors stated 'the b value employed by Manzoni et al. (2014) is used'; however, Manzoni et al. (2014) used b=4. References: Cleveland CC, Liptzin D (2007) C : N : P stoichiometry in soil: is there a "Redfield ratio" for the microbial biomass? Biogeochemistry, 85, 235-252. Wang G, Post WM, Mayes MA (2013) Development of microbial-enzyme-mediated decomposition model parameters through steady-state and dynamic analyses. Ecological Applications, 23, 255-272. Xu X, Thornton PE, Post WM (2013) A global analysis of soil microbial biomass carbon, nitrogen and phosphorus in terrestrial ecosystems. Global Ecology and Biogeography, 22, 737-749.

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