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Interactive comment on "Effects of model assumptions for soil processes on carbon turnover in the earth system" *by* B. Foereid et al.

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Review of Foereid et al., Effects of model assumptions for soil processes on carbon turnover in the earth system Heterotrophic respiration is one of the least understood terrestrial ecosystem processes. Its representation in terrestrial biosphere models is fairly abstract and possibly unreliable and hence needs improvement. In this manuscript, Foereid et al. aim to assess global impacts of assumptions about two individual mechanisms, temperature sensitivity of decomposition and priming effect. Model assumptions and equations are changed. Then, resulting steady state SOC stocks are compared to an observation-based estimate. Also, transient simulations until 2100 were performed and global trajectories of carbon stocks compared with each other. The topic is of high interest and hence merits a publication. However, I have

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quite a number of issues that should be addressed prior to any publication. Response: Again, we first wish to thank the reviewer.

1) Please, concretize the aim of the study. Do you aim to provide a new model version of CLM that is representing more reliably heterotrophic respiration and SOC decomposition, or do you aim to study model uncertainty related to single effects of temperature sensitivity and priming on steady state SOC stocks estimates? In the first case, that means this model version should be used for further applications in other contexts, I would expect a deeper evaluation of the model, e.g. by using lab incubation experiments, heterotrophic respiration measurements and by evaluating NEE against eddy covariance tower observations or atmospheric CO2 inversion results. In the latter case I would expect a more theoretical study using not only one set of parameters but a range / distribution of parameters in a Monte-Carlo approach and discuss the resulting uncertainty from the parameterization, maybe for a few representative grid cells if CPU time is limiting. Response: This is a sensitivity study to identify the potential effects of including emerging biological understanding of factors affecting SOC turnover in models. We accept that before these processes can be included in model, better calibration etc. is needed. However, the study has demonstrated that this improved representation will have significant effects on model outputs, this being a necessary step, justifying future work to generate next-generation models. We attempt to modify the title and text to show this more clearly (in last sentence of abstract as well as introduction and conclusions).

2) It remains unclear if T-sensitivity and priming experiments were performed individually. If so, why is there no experiment with both together? Response: We aimed at investigating the effect of each change as a sensitivity study to better understand the response. They could also be performed together, however, it was not the purpose of this study.

3) Section 2.1: Which T-sensitivity function has been used with the old model (Lloyd and Taylor, 1994 as in Thornton and Rosenbloom, 2005?) and which data has been

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used for finding the respective parameters? Response: A Q10 function was used in the standard version. This information is now added (p5 l 19). This function is still there in the modified version, but a further modification aimed at only changing the sensitivity so that it is different for different carbon pools.

4) Section 2.2.1: I cannot understand this text and the formulas at all. I think that kmod is the new modified decomposition rate and k is the standard model decomposition rate. The latter is already temperature-dependent and I understand that you further change this dependence using the exponential term of the last equation in this section. - If the standard model already uses a temperature sensitivity following Lloyd and Taylor (1994) which is similar to an Arrhenius-type of equation, how can you multiply another Arrhenius-type of equation to modify this sensitivity? Please, explain the theory behind and write down a full equation for k_mod with an explicit term for k. - It remains unclear to me what is I2 (is that the second litter pool?) and what is then Ea*I2? - Which values of Ea have been used and where do they come from? Please, compare your temperature sensitivity to other values used in the literature, e.g. by fitting a Q10 model to your results and compare these Q10 values. - The aim was to have temperature sensitivity increasing with mean residence time (section 2.2.1). However, Fig. 1 suggests the opposite. Please, explain. Please, provide a table with mean residence time, activation energy and final temperature sensitivity for each pool. Response: This overall point was also raised by the other reviewer (12-14). We have changed the explanation of this quite significantly, hopefully it is now clear. Sorry, Fig. one was wrong (graph labels switched). This has now been corrected.

5) Section 2.2.2: - How are parameters a and b estimated? Are they directly taken from Garcia-Pausas and Paterson (2011)? In this case, it is unclear to me how you translate lab incubation experiment results using glucose into increasing litter fluxes? - Please explain further the underlying lab incubation experiment: Which soil from which ecosystem has been used there and are the results comparable with other priming effect experiments? Is this experiment anyhow representative on global scale? – How

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is the equation applied? Do you calculate the standard model SOM and fluxes in parallel and estimate the correction factor each time step? Or are you using steady state results and mean fluxes (from which period) from the standard model for each grid cell? Are litter pools also affected or only SOM? Are litter pools also included into SOM in the equation? Response: The parameters were fitted directly from the experiment. We have also added an explanation that this is only a first attempt, and for a full implementation of priming into global models, more extensive data sources are needed (p7 l 7-10). Litter pools are not included, and the modifications did not change them much. This information has now been added (p9 l 11-14).

6) Section 2.3: Which [CO2] data has been used for spinup? I also do not understand what you did exactly with the future climate anomalies. Please, explain this part more in detail, e.g. time step. What / how did you cycle through four times 2000-2100? Response: These aspects of the model setup have now been emphasized in the main text and the description of the climate anomalies has been clarified (p7 l 11 – p8 l 7).

7) Section 2.4: I agree with this section. You calculate the SOC content that is in general comparable with the model results which is valid. However, please, explain in addition which SOC is represented, e.g. wetlands or Yedoma deposits should be excluded. Section 3: Please, discuss your SOC content from section 2.4 with other recently published estimates. Why are your numbers in Tab 1 much lower than published values? Response: Wetlands should probably be excluded, however, that is not done usually in global models, and one of the conclusions of our study is that better representation of these areas would improve the model. However, in this study we wanted to compare the standard way of running it, which is not taking out any areas. Also previous authors have found that the model predicts low global soil carbon (Thornton and Rosenblom, 2005). Overall, this shows that the soil part of global models have been given little attention as discussed in introduction (P2 l 30 – P3 l 6).

8) Section 3, general. Only results for SOM are reported while litter pools will be affected by the model changes, too. Please, show these changes in addition. Please,

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show in Fig 4 difference maps as in Fig 3 (data-model) in addition. Please, show spatial details for 2071-2100 differences to e.g. 1980-2010 of results shown in Fig 6 (plus litter pools). Response: Some more results are reported as also requested by the other reviewer. Some information about litter pools has also been added (p9 l 11-14).

9) Section 3, discussion on temperature sensitivity. I) The results of this paper give the impression that the parameterization of temperature dependence of decomposition do not show large effects on SOM content. This is in contrast to Jones et al. (2003). Please, discuss the applied modifications of temperature sensitivity using other modifications in the literature and in comparison to published observations, e.g. from lab incubation experiments. Please, also discuss your resulting effects in comparison to effects found by other's in accordance. II) Why are trajectories of carbon pools into the future similar to the ones from the standard model (Fig 6)? Are projected temperature changes too small to give an effect on carbon turnover? Response: The Jones et al paper looks at the effect of temperature response as such, in this paper we investigate the effect of changing the sensitivity of individual pools, without changing overall sensitivity as much as possible. As for priming, our primary aim was to assess effects of altered sensitivity of SOC pools on global distribution, rather than global stocks. It has proven problematic to quantify the temperature sensitivity of individual SOC pools experimentally (this is briefly discussed in introduction p3 l 23-32).

10) Section 3, discussion on priming effect. Discuss that the underestimation of SOM in many boreal and temperate regions seem to increase when including your representation of the priming effect. This can be better seen when plotting data-model for this experiment. Then, it could also be that the tropical overestimation turns into a tropical underestimation (hard to see from these plots). Response: We added panels to Figure 4 that show the areas where the model modifications improve (and do not improve) the predictions of SOC relative to the observations. This was also requested by the other reviewer.

11) Why is vegetation carbon density lower in the priming experiment (Fig 6)? I would

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assume more N availability for plants with higher decomposition rates? Response: This is addressed, see response to other reviewer. We believe this effect was already exhausted in the equilibrium run. A plausible explanation for the effect of priming during the future simulation is now added. (p11 l 13-17)

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