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Comment

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

### **Anonymous Referee #2**

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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,

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I have quite a number of issues that should be addressed prior to any publication.

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 CO<sub>2</sub> 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.

2) It remains unclear if T-sensitivity and priming experiments were performed individually. If so, why is there no experiment with both together?

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 used for finding the respective parameters?

4) Section 2.2.1: I cannot understand this text and the formulas at all. I think that  $k_{mod}$  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  $I_2$  (is that the second litter pool?) and what is then  $E_a \cdot I_2$ ? - Which values

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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.

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 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?

6) Section 2.3: Which [CO<sub>2</sub>] 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?

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?

8) Section 3, general. Only results for SOM are reported while litter pools will be

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affected by the model changes, too. Please, show these changes in addition. Please, 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).

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?

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).

11) Why is vegetation carbon density lower in the priming experiment (Fig 6)? I would assume more N availability for plants with higher decomposition rates?

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Interactive comment on Earth Syst. Dynam. Discuss., 4, 1091, 2013.

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