

Reply to comment by Anonymous Referee #4 on “Disentangling residence time and temperature sensitivity of microbial decomposition in a global soil carbon model”

We provide hereafter some replies to the referee concerns and will fully address them in the revised version of the manuscript. In general, many of the reviewer’s comments result from their first comment – that they are not clear on the problem we are trying to solve. We therefore believe that several of the additional criticisms are not entirely relevant to what we are trying to achieve. We obviously need to be clear and precise about our objectives and will endeavour to do this thoroughly in the revised paper.

This paper is interesting, but it needs to articulate better what problem it is trying to solve. There are many issues introduced by the kinds of simplifications presented here, and without clearly stating which research questions are within or beyond the scope of the analysis it is difficult to understand the extent to which the problems outweigh the advantages of the approach.

We accept this criticism by the referee, and comments by other reviewers, that the scope of our study must be better defined in the introduction of the article. We do not aim to provide new, improved, projections of the response of SOC to global warming. We use this simplified framework, still representative of some more complicated models used in CMIP5 experiments notably, to better understand the sensitivity of the system steady-state and dynamics to some key parameters.

We will seek to improve the framing of our study in the context of our objectives in the revised manuscript.

One issue I have with this paper is that it treats the concept of a single residence time as being meaningful in a transient sense. It is not, and none of the CMIP5 models treat it as such; instead they treat soil C as having a set of residence times, because they are all multi-pool models. This is an important distinction, and will lead to diverging results between the simple 1-pool model here versus the CMIP5 models. So I disagree with the dismissive treatment of this as an issue in section 2.1. Instead, you ought to ask how does this simplification affect your results?

We do not fully agree with the referee’s first statement. In a recent review study by Todd-Brown et al. (2013), 3 of the 11 CMIP5 models do use a single SOC pool, and they have demonstrated that a reduced complexity model, similar in design to ours, performs well to reproduce the broad behaviour of the more complicated structures. While we agree that a multiple pool structure will diverge from a single pool structure, they both use fixed pool residence times, and a considerable change in pool sizes is required for this effect to be important.

We agree however that we need to provide more insights on how we believe that this simplification is valid and some thoughts on the implications of simplifying the SOC turnover to a single pool. We will do so in the revised manuscript.

A second issue is with the idea of a single global Q10 value. A problem with this is that it does not allow for the process of freezing to sharply reduce respiration rates in frozen soils. So by treating temperature dependence in this way you exclude the possibility of freeze/thaw processes from playing a role in the model. As a result, it is difficult to interpret the zonal-mean profiles in figure 7; is temperature sensitivity really less important in the high latitudes than mid latitudes, or is this an artifact of the simplifications you have chosen to make in your model? And why do you go all the way down to -30C in figure 5 while neglecting this obvious point that biological systems work qualitatively differently when they are frozen solid, whether your Q10 is 1.5 or 2.5?

We agree with the issue of using a single global Q10 value, especially when approaching the cold temperatures. However, using a single formulation of  $f_T$  globally is the state-of-the-art approach used in more complicated ecosystem models (see Todd-Brown et al., 2013 and Nishina et al.; 2014). In other words, actual decay rates are adjusted “spatially and temporally as a function of  $T_s$ ” (p. 5001 l. 18).

We nevertheless agree that biological systems behave differently in frozen conditions. However, our study is targeting model sensitivities to some particular parameter values in the current way microbial decomposition processes are represented in CMIP5 models (i.e. without freezing/thaw processes). We are aware that the representation of freezing/thaw processes in terrestrial models is a topical problem (e.g. Koven et al., 2011) as permafrost thawing may remobilize large amounts of SOC in the active cycle. This, however, falls beyond the scope of our sensitivity study of the current parameterization of soil carbon processes.

We will add some comments on this issue and its implications in the model description and discussion parts.

Why does NPP in the driving model increase so abruptly around 1960 to drive the soil C in figure 3? Is that realistic with respect to what we know about the 20th century carbon cycle? What causes the change in sign of the slope in figure 4? Is it also NPP driven? Does the change in slope occur at different times for different parameter values?

The abrupt response of NPP is due to the response of the driving model to the step change in atmospheric CO<sub>2</sub> concentrations from around 1960 onwards. This was documented in a more detailed way in a previous study (Exbrayat et al., 2013).

The common behaviour between model versions is first an increase in NPP following the rise in atmospheric CO<sub>2</sub> concentrations. The corresponding warming triggers higher  $R_h$  that eventually completely offsets the CO<sub>2</sub>-fertilization effect on vegetation. Therefore, the change of slope in Figure 4 indicates that  $R_h$  has become greater than NPP, hence the depletion of SOC stocks. Following the text in the discussion paper the “timing of the peak, i.e. when soil carbon starts to deplete, varies between 2035 and 2080” (see paragraph from p. 5004 l. 26 to p. 5005 l. 14). If we had represented the time series rather than cumulative changes, this would correspond to when the net SOC balance becomes irreversibly negative. To answer the referee’s comment, while Figure 3 mostly shows a response of the SOC balance to NPP, Figure 4 provides a picture of the interaction between NPP and  $R_h$  through their respective responses to increasing atmospheric CO<sub>2</sub> concentrations and temperatures.

We recognise that we have not investigated the variations of which parameter explains most of the difference in the timing of the slope change in Figure 4 between model versions and we will not fail to

do so in the revised version of the manuscript. We will also improve the description of the driving NPP dataset.

I don't understand what we are supposed to learn from figure 5, if not that  $T_{ref}$  matters as a parameter in this type of analysis, because it defines the relationship between  $k$  and  $Q_{10}$ . So why don't you vary  $T_{ref}$  in figures 6 and 7? Is there not uncertainty on this point?

Figure 5 illustrates the implications of applying a single formulation of  $f_T$  globally. While absolute differences in the value of  $f_T$  with different values of  $Q_{10}$  may seem negligible in cold regions, this can introduce large relative differences and notably lead to the building of very different SOC stores during spin-up (as shown in Figure 6a). Spatially varying  $T_{ref}$  could be an approach but it would require finding corresponding values and redefining residence time  $k$ , both of which are far beyond the scope of the current paper.

We will add a few sentences in part 2.1 about this issue of using a global formulation of  $f_T$  with a single reference temperature (one of our main criticism of the current parameterization of  $R_h$ ) and the issue about freezing/thawing raised in the previous comments as we nevertheless agree with the referee and we believe that it will help better framing the scope of this paper.

Exbrayat, J.-F., Pitman, A. J., Zhang, Q., Abramowitz, G., and Wang, Y.-P.: Examining soil carbon uncertainty in a global model: response of microbial decomposition to temperature, moisture and nutrient limitation, *Biogeosciences*, 10, 7095-7108, doi:10.5194/bg-10-7095-2013, 2013.

Koven, C. D., Ringeval, B., Friedlingstein, P., Ciais, P., Cadule, P., Khvorostyanov, D., Krinner, G. and Tarnocai, C.: Permafrost carbon-climate feedbacks accelerate global warming., *Proc. Natl. Acad. Sci. U. S. A.*, 108(36), 14769–74, doi:10.1073/pnas.1103910108, 2011.

Nishina, K., Ito, A., Beerling, D. J., Cadule, P., Ciais, P., Clark, D. B., Falloon, P., Friend, A. D., Kahana, R., Kato, E., Keribin, R., Lucht, W., Lomas, M., Rademacher, T. T., Pavlick, R., Schaphoff, S., Vuichard, N., Warszawski, L., and Yokohata, T.: Quantifying uncertainties in soil carbon responses to changes in global mean temperature and precipitation, *Earth Syst. Dynam.*, 5, 197-209, doi:10.5194/esd-5-197-2014, 2014.

Todd-Brown, K. E. O., Randerson, J. T., Post, W. M., Hoffman, F. M., Tarnocai, C., Schuur, E. A. G., and Allison, S. D.: Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparison with observations, *Biogeosciences*, 10, 1717-1736, doi:10.5194/bg-10-1717-2013, 2013.