

Reply to comment by S. D. Allison on “Disentangling residence time and temperature sensitivity of microbial decomposition in a global soil carbon model”

This manuscript describes a sensitivity analysis of residence time and Q10 in a global soil carbon model. The analysis examines responses of soil carbon stocks and future soil carbon change. The study is valuable because it allows for direct comparison of changes in residence time versus Q10 on soil carbon processes. The conclusion is that residence time is most important for determining standing stocks and the magnitude of C change, whereas Q10 determines the direction of response to temperature change, locally and globally.

I think the comparison is valuable, but I don't think it should be surprising that residence time controls carbon stocks in a first-order model. I suggest shortening the discussion of this overall result. It is more interesting that the initial stocks, as driven by the residence times, determine the magnitude of response to temperature change. This point is somewhat buried in the discussion, but I think it should be emphasized more clearly.

We thank Dr Allison for his positive view of our work. We agree that we should emphasize the influence of the initial stocks, and hence residence time, on the magnitude of the response. We will make sure to put this result forward more clearly in the revised version of the paper.

In the introduction, I am missing a statement of the key question this work seeks to address. Clearly the goal of the paper is to determine the relative influence of turnover versus Q10 variation on soil carbon stocks. But what is the motivation for doing this? Can this work be placed in a broader context of improving the global models? Is the assumption that if models used accurate turnover times and Q10 values that matched observations, then the model predictions would be valid? I'm not so sure, given that most models, including the one used here, do not replicate spatial patterns in soil carbon very well.

In Exbrayat et al. (2013a) we showed that altering the formulation of f_T and f_W could lead to large differences in SOC turnover and hence to a change in the sign of the net ecosystem exchange. In Exbrayat et al. (2013b) we showed that changing these functions in a global coupled model could induce a two-fold range in historical terrestrial carbon uptake regardless whether nutrient limitations on NPP were used or not. Changing these environmental factors also led to a range in equilibrated SOC comparable to the one exhibited by the ensemble of CMIP5 models.

Todd-Brown et al. (2013) showed that this large range is not representative of available datasets and, similarly to their approach, we explore the likely reasons of its existence prior to transient simulations with a reduced complexity model. We target the time-invariant turnover rate and the locally adjusted temperature function that describe the baseline functioning and dynamics of the system.

In response to this comment, we will revise the introduction to clearly state that the key question we address is to understand the sensitivities and pitfalls of this ubiquitous approach to modelling soil carbon processes.

There are some clear patterns in this analysis that raise questions about the validity of the underlying model (or any similar first-order model). It is good that the authors compared the stocks to the HWSD, but there is no further discussion on the realism of the model outputs. One issue is the overall

size of the historical and current soil carbon sink (see comments below). Another issue is the spatial distribution of soil carbon change. The authors contend that mid-latitudes will determine the sign of soil carbon balance, but that's only true if the model assumptions about zonal drivers of soil carbon storage are correct. For example, even with the highest Q10 values, the models predict large carbon storage in boreal/tundra latitudes. Yet most empirical and biogeochemical evidence suggests that high latitude soil C is highly vulnerable to climate change (see work by Schuur and others). Conversely, soil mineralogy could constrain the temperature response of decomposition in tropical soils. Some of these issues are discussed in another recent paper by Todd-Brown et al. in BG. In short, current biogeochemical models lack important mechanistic details and produce questionable predictions about zonal soil carbon change.

We thank Dr Allison for this interesting point of discussion and provide a clarification hereafter.

We do not discuss the realism of our results because we are aware that our reduced complexity model lacks some processes, especially the representation of land use change in NPP. Our aim here is clearly to investigate the influence of two key parameters, often set based on controlled laboratory experiments, on the definition of steady-state and the dynamics of the system in response to climate change. We find it remarkable that the CMIP5 range can be reproduced by a simple model, and that using the HWSD allows considerable constraint in the uncertainty in projected carbon store. As shown by Todd-Brown et al. (2013), CMIP5 perform clearly poorly in that space and we argue that putting more effort in representing stocks is a way forward to reduce the uncertainty in land-atmosphere fluxes.

We agree that this may not have been clear enough in the previous version of the manuscript and will add these points in our revisions.

I think that discussing the plausibility of some of the results in terms of other empirical data (in addition to the HWSD) would strengthen the paper. Still, I like the analysis because it represents a controlled analytical approach for examining two important drivers in detail.

While this would be interesting, we reiterate that we are not trying to establish the validity of this simple model. We rather show that its behaviour and dynamics are comparable to more complicated models and highlight the potential value of using observational data as a tool to reduce the uncertainty in simulations. Following our previous replies, we will put more emphasis on the aim of our study in the revised manuscript.

Specific comments:

4997:21: It's not so much the model parameterization that's criticized, but the model structure and specifically the first-order, substrate-driven nature of decomposition losses. Same for line 28.

We are not sure of the difference between "model parameterization" and "model structure". For us, both refer to the process of translating observations and knowledge of natural processes into a set of equations that will form the mathematical model. However, we agree that the notion of "structure" may be more directly related to this and will therefore rephrase to avoid misconceptions.

5001:8-10: What is the basis for the choice of these parameter ranges? They seem reasonable, but perhaps some citations can be included.

We have chosen parameter limits to be broadly representative of those achieved in the optimisation of Todd-Brown et al.'s (2013) own reduced complexity model. This will be noted in the revised manuscript.

5003:20-21: This result seems unlikely. Is there any evidence that soils have accumulated C at this rate over the historical period? The highest estimates would require rates of ~ 2 Pg/yr, which is nearly the size of the entire current land sink. This result seems to question the validity of the underlying model processes, at least for the longest residence times.

We agree that results seem unlikely in a real-world context. However, our reduced complexity model is driven by a NPP dataset that, for simplicity as explained in Exbrayat et al. (2013b), does not include the effect of land-use change. Therefore, the net uptake is most likely too high as compared to actual estimates, leading to this apparently large growth rates of SOC.

We will precise this information about the boundary conditions in the revised manuscript.

5004:5: Use of the word "significant" implies statistical significance; better to choose a different word here.

We agree with this comment and will be careful to choose a better word upon revising the manuscript.

5008:15-16: I am skeptical of the size the soil C sink in the current analysis. Many of the studies cited here are other modeling analyses, and all the models are quite similar in their response of NPP to CO₂ and their response of soil C to NPP. I don't think there is compelling empirical evidence yet that the land sink has that much of a soil component. Can we really rule out that the land sink is all driven by vegetation?

The current land sink results from an enhanced plant C uptake that outweighs release by heterotrophic respiration. We agree that other modelling analyses are based on similar models and, once again, this is the aim of this manuscript to understand the implications of the baseline and dynamic components of the first-order parameterization of decomposition.

We agree that the land sink is driven by vegetation and displayed here is the response of SOC to an increase in plant uptake and we will add this point to the discussion of the revised manuscript.

5009:18: I'm not sure I think that this is counterintuitive. It's clear that turnover controls the equilibrium pool size and Q₁₀ controls the temperature response. The temperature response is a fractional value, so it makes sense that you get a bigger absolute change if you apply the same fractional change to a larger pool size.

We will rephrase this sentence to clarify our point.

Fig. 2: The caption needs to clarify that the dashed lines are the model runs that produced soil stocks within the 95% CI of the HWSD. The way it's written now makes it seem like the HWSD has soil C change in it.

We will rephrase the caption accordingly.

References:

Belshe, E. F., Schuur, E. A. G. & Bolker, B. M. Tundra ecosystems observed to be CO₂ sources due to differential amplification of the carbon cycle. *Ecol. Lett.* 16, 1307–1315 (2013).

Schuur, E. A. G. et al. Vulnerability of permafrost carbon to climate change: Implications for the global carbon cycle. *Bioscience* 58, 701–714 (2008).

Todd-Brown et al. 2014: <http://www.biogeosciences-discuss.net/10/18969/2013/bgd-10-18969-2013.html>

Exbrayat, J.-F., Pitman, A. J., Abramowitz, G., and Wang, Y.-P.: Sensitivity of net ecosystem exchange and heterotrophic respiration to parameterization uncertainty, *J. Geophys. Res. Atmos.*, 118, 1640–1651, doi:10.1029/2012JD018122, 2013a.

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, 2013b.

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.