Dear Dr. Subke

Please find below our replies to all reviewers' comments on our manuscript "Disentangling residence time and temperature sensitivity of microbial decomposition in a global soil carbon model". You will note that our comments are mostly similar to the ones published in the discussion forum and we have added here the details of the revisions made to the attached manuscript. In the following, reviewers comments were copied in black will our replies are in blue.

We hope that you will find our replies satisfactory to allow publication of this paper in Biogeosciences. Please do not hesitate to contact me if some extra information is required.

Yours sincerely,

On behalf of the authors

J-F Exbrayat

Reply to comment by J. Xia on "Disentangling residence time and temperature sensitivity of microbial decomposition in a global soil carbon model"

This paper includes some very interesting analyses which try to improve our understanding of the huge uncertainty in global soil carbon modeling in current generation of earth system models. The authors used a simplified model to explain the uncertainty in modeled soil carbon stock among 17 global land models, and largely reduced the uncertainty through constraining the simplified model parameters by a global soil carbon database. The paper is well written and the quality is high. The topic is also in the scope of Biogeosciences. The only issue I found in the paper is that the authors used a range of *Q10* from 1.5 to 2.5 together with a combination of baseline turnover rate *k* from 10 to 40 years (Line 142-144). I plotted the f_T in equation (2) and (3) against air temperature with the lower (1.5) and upper (2.5) limits of *Q10* (please the following figure):

The above figure is simplified version of Fig. 5. Then I found many results in this study in a relatively simple way. For example, the Q10 difference is much smaller with air temperature <15oC (which was set as the baseline temperature in this study) than that with >15oC. In the historical simulations, the temperature is relatively low, and then the sensitivity of f_T to Q10 might be relatively small in comparison with the 4-fold variation in *k*.

That means, although the authors used different values of Q10 in the analyses, but f_T didn't vary much in the historical simulations. This may result in the more important role of *k* than *Q10* in Fig. 1.

Similarly, since temperature increased by several degrees in future projections (RCP8.5), *Q10* became important in Fig. 2b (Lines 213-217). As shown in the above figure, when air temperature is <15oC, a lower *Q10* leads to a higher f_T. That's why the authors found 'models with the largest value of Q10 tend to accumulate only 69% of the amount that the lowest Q10 models do' at lines 209-211. Also, the results in Fig. 7b are reasonable because hot regions near the equator will act as C sources since high temperature rapidly increase f_T, while low temperatures in cold regions limit the f_T. The issue I raised above is for the current models which usually use the first-order parameterization of microbial decomposition. It's better if the authors can discuss it in their revised version.

Overall, I would like to recommend the journal to accept this paper after a minor revision.

We would like to thank Dr. Xia for his interest and positive comments about our work.

First, we agree that differences in the absolute values of f_T are smaller when T_s is below 15°C as shown in the reviewer's own Figure 1. However, relative differences are comparable between f_T with different values of Q_{10} for T_s greater or lower than 15°C as shown in our Figure 5. This explains the approximately two-fold range in zonal SOC at equilibrium found in both the warmest and coldest regions (Figure 6a). As SOC input and soil physical states are the same in all model versions, we can attribute it to the response of decomposition to the spin-up procedure: a low (high) value of Q_{10} involves a low (high) decay rate in warm regions, and the build-up of large (small) SOC stocks; and a low (high) value of Q_{10} involves a high (low) decay rate in cold regions, and the build-up of small (large) SOC stocks. Regions with temperatures closer to 15° C are less sensitive to the choice of Q_{10} during spin-up. Overall, there is a zonal compensation that explains the apparent lack of sensitivity of global SOC to Q_{10} in our Figure 1a notably. Of course, we could have increased the sensitivity of equilibrium SOC stocks to Q_{10} by choosing a reference temperature that would have not led different f_T to cross within the spectrum of prescribed T_s.

Second, we also agree that as long as temperatures remain similar to pre-industrial conditions, the sensitivity of the model to the choice of Q_{10} is low. Conversely, when a strong warming is imposed the highest Q_{10} leads to the strongest depletion (or smallest accumulation) of global and regional SOC, while *k* influences the magnitude of the changes. Still, the magnitude of the change is very dependent upon the value of *k*.

In summary, we agree with Dr. Xia and we have implemented these notions in the discussion part of the revised manuscript ll. 351-355:

> Furthermore, the difference between f_T with different Q_{10} grows with the absolute value of the difference T_s-T_{ref}. Therefore, using a value of T_{ref} that is outside the range of actual temperatures would lead f_T with different Q_{10} to keep the same relative position globally. It would introduce larger relative differences between these functions.

and ll. 377-379

> We however note that the value T_{ref} used in our experiments is well within the range of actual temperatures. Therefore, the historical warming does not induce large changes in the values of f_T with different Q_{10}.

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 have put this result forward more clearly in the dedicated results section of the revised version of the paper ll 239-241

> In other words, while Q_{10} decides of the sign of the change, *k*, and hence the initial stocks of SOC after spin-up, drives the magnitude of the response.

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 have revised Section 2.1 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 ll. 142 – 148:

In summary, we wish to reiterate that this study investigates the sensitivity of the first-order parameterization of microbial decomposition and R_h processes used in current ecosystem models to its uncertain parameters (Todd-Brown et al., 2013; Nishina et al., 2014). We do not intend to provide improved results of the response of soil carbon to climate change but rather illustrate and better understand the implications of the current ubiquitous approach to parameterization and initial value prescription described in Section 2.2.

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 to clearly 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 have added these points in our revisions in response to the point raised above and below this one.

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 have put more emphasis on the aim of our study in the revised manuscript ll. 145 - 148:

> We do not intend to provide improved results of the response of soil carbon to climate change but rather illustrate and better understand the implications of the current ubiquitous approach to parameterization and initial value prescription described in Section 2.2.

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 have therefore replaced the term "parameterization" by alternatively "structure" or "representation" to avoid misconceptions. These changes are to be found lines 13, 51, 56, 63 and 426.

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 has been noted in the revised manuscript: ll. 161 – 163:

> These values are based on the range of results previously obtained by Todd-Brown et al. (2013) with their own reduced complexity model.

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 have clarified the description of the boundary conditions of our reduced complexity experiments in the revised manuscript ll. 167 - 172:

> We note that these drivers do not include the representation of land-use and land cover change and their effect on *NPP*, T_s and θ_s. Therefore, SOC input are likely to be higher than in reality. However, as stated earlier we are using the reduced complexity framework to understand the behaviour of the SOC model in response to variations in its parameters and we do not aim to provide improved estimates of global scale terrestrial carbon sinks.

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

We agree with this comment and have rephrased this sentence l. 242 - 244:

> If we consider only models that fall within the CI_{95} of the HWSD for current total soil carbon (dashed contours on Figure 2a and 2b) the spread in simulated total soil carbon balance is largely reduced.

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, the aim of this manuscript to understand the implications of the baseline and dynamic components of this ubiquitous first-order parameterization of decomposition.

We agree that the land sink is driven by vegetation. Here we investigate the dynamics of the modelled response of SOC to an increase in plant uptake. We have added this point to the discussion of the revised manuscript ll. 367 - 370

> This historical net carbon sink that is driven by the response of vegetation to increasing atmospheric CO_2 (and hence SOC_{in}) is in accordance with previous studies (Friedlingstein et al., 2006; Sarmiento et al., 2010; Zhang et al., 2011; Wania et al., 2012; Anav et al., 2013; Exbrayat et al., 2013b).

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 agree that the same fractional change applied to a larger pool will lead to a bigger absolute change. While the change in decay rate varies between model versions, we wanted to highlight that the size of the pool dominates the absolute response. This is of particular concern because the size of the pool is determined during spin-up. We have rephrased this sentence to clarify our point ll. 400 - 402

> Therefore, the size of pools to which the change is applied seems to dominate the response even when higher values of k imply a smaller relative change in the decay rate $k^{-1} \times f_T \times f_W$ used in equation 2.

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 have rephrased the caption accordingly:

> **Figure 2.** Change in total soil carbon in the reduced complexity model as a function of parameter values for each period as indicated. Dashed contours in panel b indicate model versions that produced soil stocks within the CI_{95} of the HWSD in 2005 (830 – 1550 Pg C). The thick black line represents no change.

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

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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.

Reply to comment by K. E. O. Todd-Brown on “Disentangling residence time and temperature sensitivity of microbial decomposition in a global soil carbon model”

The authors used a reduced complexity model (one pool soil decomposition with a temperature and moisture dependency) to study the sensitivity of the carbon stock projections to first order uncertainties. The relative contributions of decomposition (k) and temperature sensitivity (Q10) to the change in soil carbon stocks are relevant and interesting. However I have a major concern with the use of this particular RCM in examining the change in soil carbon stock.
I’m concerned that the proposed RCM would not be a good predictor of the change in ESM soil carbon as implied by the manuscript. I would like to see a comparison with the RCM predicted dC and the full ESM simulation run. Todd-Brown et al 2013 a,b did show that the RCM explained the initial distribution of C well, assuming steady state (ToddBrown et al 2013a), and the distribution in Rh both spatially and temporally (ToddBrown et al 2013b). However this is unlikely to directly translated into well explained dC over the time period since the RCM only capture the first order differences and dC is the result of relatively small differences between inputs and outputs.

We thank Dr. Todd-Brown for her interesting suggestion. However, while we agree that the proposed RCM would not be a good predictor of the change in ESM soil carbon, we must clarify that we use the ESM data as boundary conditions to study the specific behaviour of the RCM. We do not aim to reproduce the ESM with our reduced complexity model as the cited papers by Dr. Todd-Brown et al. already give evidence of the skill of an RCM to do so. As we already answered in response to other reviews, we are using the reduced complexity model to illustrate the global and zonal implications of using the first-order parameterization of Rh with the classical approach of a long spin-up until steady-state followed by changes in boundary conditions over a shorter period of time.

We are fully aware that the scope of this study needs to be better defined as it did not appear clearly to most of the reviewers. We have improved the definition of our objectives in the revised manuscript ll. 132 – 148:

> We are aware of that our reduced complexity model relies on questionable assumptions such as the use of a single soil carbon pool and global values of k, Q_{10} and T_{ref}. However, while we agree that a multiple pool structure would provide diverging results, single pool soil carbon carbon models similar to our design are used in 3 of the 11 CMIP5 models described by Todd-Brown et al. (2013) and 2 of the 7 ISI-MIP models described by Nishina et al. (2014). Further, using global parameter values of k, Q_{10} and T_{ref} is consistent with these state-of-the-art models (Todd-Brown et al., 2013; Nishina et al., 2014). Of course, this does not allow representing processes such as the remobilization of carbon in the active cycle following permafrost thaw (Koven et al., 2011) or the probably different behaviour of biological systems in frozen conditions but these are not routinely implemented in the land component of Earth system models and therefore fall beyond the scope of this paper. In summary, we wish to reiterate that this study investigates the sensitivity of the first-order parameterization of microbial decomposition and R_h processes used in current ecosystem models to its uncertain parameters (Todd-Brown et al., 2013; Nishina et al., 2014). We do not intend to provide improved results of the response of soil carbon to climate change but rather illustrate and better

understand the implications of the current ubiquitous approach to parameterization and initial value prescription described in Section 2.2.

Either the authors need to go into greater detail on this caveat in the discussion and a justification in the methods section or show explicitly that these first order differences do, in fact, govern dC in the ESMs. Alternatively the paper could be refocused on Rh instead of soil carbon to sidestep the problem of second order contributions to dC.

We believe that re-framing the scope of our study with the paragraph cited in reply to the previous comments addresses Dr Todd-Brown's concerns.

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, 2013a.

Todd-Brown, K. E. O., Randerson, J. T., Hopkins, F., Arora, V., Hajima, T., Jones, C., Shevliakova, E., Tjiputra, J., Volodin, E., Wu, T., Zhang, Q. and Allison, S. D.: Changes in soil organic carbon storage predicted by Earth system models during the 21st century, Biogeosciences Discuss., 10(12), 18969–19004, doi:10.5194/bgd-10-18969-2013, 2013b.

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 have fully addressed 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 relevant to what we are trying to achieve. We obviously need to be clear and precise about our objectives and have endeavoured 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 have improved the framing of our study in the context of our objectives in the revised manuscript ll. 142 – 148:

> In summary, we wish to reiterate that this study investigates the sensitivity of the first-order parameterization of microbial decomposition and R_h processes used in current ecosystem models to its uncertain parameters (Todd-Brown et al., 2013; Nishina et al., 2014). We do not intend to provide improved results of the response of soil carbon to climate change but rather illustrate and better understand the implications of the current ubiquitous approach to parameterization and initial value prescription described in Section 2.2.

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. Further, 2 of the 7 models used in the ISI-MIP experiments (Nishina et al., 2014) also adopt a parameterization based on a single soil carbon pool. 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 do 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 do so in the revised manuscript ll. 132 - 148:

> We are aware of that our reduced complexity model relies on questionable assumptions such as the use of a single soil carbon pool and global values of k, Q_{10} and T_{ref}. However, while we agree that a multiple pool structure would provide diverging results, single pool soil carbon carbon models similar to our design are used in 3 of the 11 CMIP5 models described by Todd-Brown et al. (2013) and 2 of the 7 ISI-MIP models described by Nishina et al. (2014). Further, using global parameter values of k, Q_{10} and T_{ref} is consistent with these state-of-the-art models (Todd-Brown et al., 2013; Nishina et al., 2014). Of course, this does not allow representing processes such as the remobilization of carbon in the active cycle following permafrost thaw (Koven et al., 2011) or the probably different behaviour of biological systems in frozen conditions but these are not routinely implemented in the land component of Earth system models and therefore fall beyond the scope of this paper. In summary, we wish to reiterate that this study investigates the sensitivity of the first-order parameterization of microbial decomposition and R_h processes used in current ecosystem models to its uncertain parameters (Todd-Brown et al., 2013; Nishina et al., 2014). We do not intend to provide improved results of the response of soil carbon to climate change but rather illustrate and better understand the implications of the current ubiquitous approach to parameterization and initial value prescription described in Section 2.2.

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 Q_{10} value, especially when approaching the cold temperatures. However, using a single formulation of fT 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 Ts” (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 ubiquitous parameterization of soil carbon processes.

We have added a comment on this issue and its implications in the model description part ll. 137 - 142:

> Further, using global parameter values of k, Q_{10} and T_{ref} is consistent with these state-of-the-art models (Todd-Brown et al., 2013; Nishina et al., 2014). Of course, this does not allow representing processes such as the remobilization of carbon in the active cycle following permafrost thaw (Koven et al., 2011) or the probably different behaviour of biological systems in frozen conditions but these are not routinely implemented in the land component of Earth system models and therefore fall beyond the scope of this paper.

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 CO2 concentrations from around 1960 onwards. This was documented in a more detailed way in a previous study (Exbrayat et al., 2013). We have highlighted that this information is available in our previous article ll 258 - 260

> After ~1960, we observe a step-change in cumulative ΔC_s that follows a strong response in NPP to the rapid increase in atmospheric CO_2 (please refer to Exbrayat et al., 2013b for a more detailed account of this behaviour).

The common behaviour between model versions is first an increase in NPP following the rise in atmospheric CO_2 concentrations. The corresponding warming triggers higher R_h that eventually completely offsets the CO_2-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. 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_2 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 have done so in the revised version of the manuscript ll. 271 - 273.

> The timing of the peak, i.e. when soil carbon starts to deplete, varies between ~2035 and 2075 and is explained by the value of Q_{10} ($R^2 = 0.74$, data not shown) with higher values leading to an earlier peak.

This result actually corroborates the effect of Q10 on the zonal carbon balance and we have refined a paragraph in the discussion ll. 381 - 386

> Figure 2b clearly shows that the capacity of soils to become carbon sources or remain sinks depends almost entirely on the Q_{10} parameter, and both states can be achieved for any value of k used while remaining within range of previous studies (Friedlingstein et al., 2014; Nishina et al., 2014). Figure 7b indicates that this is clearly a result of differences in the local response of model versions in the mid-latitudes as a function of Q_{10}

We have also improved the description of the driving NPP dataset ll. 167 - 172:

> We note that these drivers do not include the representation of land-use and land cover change and their effect on *NPP*, T_s and θ_s. Therefore, SOC input are likely to be higher than in reality. However, as stated earlier we are using the reduced complexity framework to understand the behaviour of the SOC model in response to variations in its parameters and we do not aim to provide improved estimates of global scale terrestrial carbon sinks.

I don't understand what we are supposed to learn from figure 5, if not that Tref matters as a parameter in this type of analysis, because it defines the relationship between k and Q10. So why don't you vary Tref 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 have added 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 ll. 137 - 142:

> Further, using global parameter values of k, Q_{10} and T_{ref} is consistent with these state-of-the-art models (Todd-Brown et al., 2013; Nishina et al., 2014). Of course, this does not allow representing processes such as the remobilization of carbon in the active cycle following permafrost thaw (Koven et al., 2011) or the probably different behaviour of biological systems in frozen conditions but these are not routinely implemented in the land component of Earth system models and therefore fall beyond the scope of this paper.

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