Dear Dr. Exbrayat,

First off, apologies for the delay. I have been travelling last week and hadn't had time to send you comments beforehand. I have received feedback from two referees on your revised manuscript. Whilst one is satisfied that their comments have been addressed well, one remains critical of the suitability of your methods and conclusions drawn. I have re-read the manuscript and agree that the applicability of your simplified modelling approach in cold biomes where freezing conditions have significant impacts on carbon cycling does not appear robust. Please carefully address all points raised in the reviewer's statement below. I agree with the referee that the mounting number of caveats for using a simplified model structure limits the real insights we can gain from this exercise.

With best regards,

Jens-Arne Subke

Dear Editor

We must also apologise for our misunderstanding of the email exchange and the delay in revising the paper. We appreciate being given an extension.

We accept the essence of the comments made by the reviewer – we are fully aware of the limitations of our modelling approach in cold biomes – but wish to stress that the purpose of this study is not to make inferences about the true system. We reiterate that this reduced complexity framework is used to illustrate the simple behaviour of current state-of-the-art global terrestrial carbon models used in CMIP5 (for example). We *do not* aim to provide updated results of global SOC dynamics and we state this clearly. Our purpose is to use the reduced complexity model to explain the behaviour seen in CMIP5 simulations, and in particular the critical role of the spin-up procedure in determining the amount of C represented in the active cycle, as well as the response of the system in transient conditions.

The reviewer asks for more complexity in how processes are represented in our simplified model, or omission of regions of cold climates. This would be entirely appropriate if we were attempting to predict the behaviour of these systems. We are not; we are modelling these systems in such a way to illustrate how and why the CMIP5 models perform the way they do. Since our simplified models are as complex as many of the CMIP5 models, the reviewer's criticisms are essentially about how the CMIP5 models work. We agree of course as this is the main point of our paper, but this is obviously not something that we can resolve in the present study.

We provide detailed replies to the Reviewer's comments below.

Yours sincerely

J-F Exbrayat

Referee's comments:

I feel that my main earlier criticisms of the paper aren't fully addressed in the revised manuscript. In particular, I feel strongly that the horizontal axis in figure 5 should be restricted to temperatures above the freezing point; since the authors acknowledge that they are not trying to address freeze/thaw processes it is misleading to include results in the range over which their assumptions do not hold.

While we essentially agree with the Reviewer's assertion that these ranges need to be restricted for this representation to be valid, we feel that we need to reiterate the main aims of this study. We are investigating the behaviour of a reduced complexity model of SOC decomposition because it is representative of current state-of-the-art large scale ecosystem models, including CMIP5 models used to project the impact of climate change on biogenic land-atmosphere carbon fluxes. All these models rely on a simple first-order parameterization of decomposition, which is not consistent with recent observational evidences as we indicate in the revised paragraph ll. 56 -63:

> This simple model structure has recently received some criticism because its lack explicit representation of microbial physiology (Allison et al., 2010; Todd-Brown et al., 2012; Wieder et al., 2013; Xenakis and Williams, 2014). Furthermore, the formulation of the environmental scalar is held constant in time which is not consistent with recently identified enhancing or compensatory responses of microbial communities to changes in boundary conditions (Karhu et al., 2014). Therefore, it can only explain the acclimation of decomposers to warming (Luo et al., 2001) as a result of the quick depletion of labile pools by enhanced microbial biomass (Kirschbaum, 2004; Knorr et al., 2005).

That is, we believe that our very simple approach, however inappropriate it may seem, gives valuable information about the likely behaviour of CMIP5-type models. Essentially we suggest that much of the CMIP5 soil carbon behaviour is pre-determined by a reduced number of model-specific parameters – we do not need coupled climate models to predict the behaviour seen in CMIP5.

We use a reduced complexity model to clearly highlight that the parameterization of microbial decomposition regulates the amount of SOC at equilibrium because of the first-order representation of these processes. This is true among CMIP5 models as well (Exbrayat et al., 2014) but, as we now mentioned in the Methods section, it is beyond our mean to re-perform all these simulations. Our idealized (offline) framework further allows isolating the sole effect of changes in parameters that control microbial decomposition, while land-atmosphere feedback mechanisms would make conclusions more challenging to be drawn. We have summarised these aspects in the Methods section ll. 103-111

> It is not possible to re-run each CMIP5 model or isolate the representation of soil carbon processes from each model. This would be extraordinarily computationally expensive and the associated feedbacks would make the analysis of the results problematic. A far simpler approach is required which led Todd-Brown et al., (2013, 2014) to develop and demonstrate that the CMIP5 SOC dynamics can be successfully reproduced using a simplified model structure. In this paper we develop and then use a reduced complexity model that simulates the monthly evolution of a single soil organic carbon pool, C_s, in

> response to input derived from Net Primary Productivity (*NPP*, g C m^{-2} mth^{-1}) and output by heterotrophic respiration (R_h, g C m^{-2} mth^{-1}).

We then filter model simulations, and hence parameter values, based on whether they simulate total SOC stocks in agreement with the observational uncertainty of the HWSD. This is a straightforward quality control of the amount of carbon in the active cycle represented by each model. It further helps reducing the uncertainty in *our* simplified simulations by excluding models with unrealistic total C stocks. This forms our main conclusion ll. 460 – 466:

> Applying a constraint on total soil carbon that discriminates between acceptable simulations of total soil carbon leads to a drastic reduction of the range of simulated change. Meanwhile, most of the remaining uncertainty in 21st century projections of total soil carbon can be attributed to zonal differences in the response to change, especially at mid-latitudes. These do not allow us to confidently project soil as either a global source or sink of carbon for the 21st century. However, it is clear that under RCP 8.5 tropical soils are not suited for long-term carbon storage while some more potential exists in high latitudes.

Based on our synthetic experiments, we reiterate our previous recommendations that this quality check should be included in future intercomparison studies (Exbrayat et al., 2014) as SOC simulated in CMIP5 models vary 6-fold (Todd-Brown et al., 2013) ll. 467 - 471:

> Finally, we suggest that future estimates of terrestrial, and especially soil, carbon responses to climate change should be more constrained by available datasets of carbon stocks. This is critical as model structures describe fluxes as a fraction of the substrate pool size. So far, the process of spin-up has too many degrees of freedom that lead to model-specific amounts of active soil carbon.

In other words, we agree with the reviewer in how cold regions should be represented, but they *are not* represented in this way in CMIP5 models. We are trying to explain the CMIP5 model behaviour and *not* improve these models in this paper. We hope that if the reviewer appreciates we are explaining how the CMIP5 models operate they will appreciate that we can explain these models using our simplified approach because they are (at best) very simple in their approach to cold region climate.

And the same goes with the zonal-mean profiles; it is insufficient to just say that one's model doesn't treat freeze/thaw or permafrost processes but that it nonetheless makes predictions about soil C changes at high latitudes. Better to mask out the high latitude regions and say that the simplified model is not appropriate for making predictions over that domain. This issue is of primary importance; what I would have suggested the paper could do if it had made an attempt to get the processes behind the high latitude soil C correct would be to use the current modeled vs. HWSD latitude profiles in soil C as a further constraint on the Q10 (beyond just the global integral, which is clearly a weak constraint) to reduce uncertainty; however this is not possible without some such detail. Lastly, the issue of approximating multi-pool systems as single-pool systems is important as well, and I don't think the authors have thought enough about what the implications are of making this assumption to their results.

We think we have addressed the first part of this comment above. If CMIP5 models do not represent permafrost (and they don't) but do simulate soil C then it is entirely appropriate to see if we can explain the range of simulations by the CMIP5 models with a simplified model that does not represent permafrost. We do, of course, agree with the reviewer that the CMIP5 models *should* represent these processes, but they don't, and that is not something we can resolve. Furthermore, we argue that our model, despite its simplicity, is representative of what is currently used in Earth System Models: a single residence time per pool, a single formulation of the temperature sensitivity of decomposition for all soil carbon pools, and even a single pool in about one third of these models, while permafrost thaw and remobilization of SOC is not implemented in these models. This was clearly indicated in the text ll. 151-167:

> We are aware that our reduced complexity model relies on simplifications such as the use of a single soil carbon pool and global values of k, Q_{10} and T_{ref}. 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 implemented in the land component of CMIP5 Earth system models and therefore fall beyond the scope of this paper. In summary, we fully appreciate that our reduced complexity model is a simplification of the processes that operate in various regions of the Earth System. However, we note that our 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). Our approach is therefore analysing how current models behave and why current models simulate a large range in SOC.

The fact that we can explain the CMIP5 models as described in the paper helps understand what is driving the results from the CMIP5 models and that directly informs modelling communities regarding appropriate future development, and the necessity to use observational datasets of carbon stocks, and not only fluxes, in a quality control of historical simulations prior to performing multiple projections. We have added these recommendation to the conclusion (see answer to previous comment).

We do not agree that global SOC stocks are a weak constraint for models. Current Earth system models simulate a 6-fold range in global SOC stocks that contributes to model-to-model variations in global SOC change in the future (Todd-Brown et al., 2014), and we previously showed that using global SOC stocks from HWSD to filter model simulations resulted in a non-negligible reduction of the uncertainty of land-atmosphere net C fluxes ll. 206 – 209

> We previously showed that simply using the global amount of SOC in the HWSD dataset to discriminate between acceptable and unacceptable simulations resulted in a non-negligible reduction of the uncertainty in historical net carbon uptake (Exbrayat et al., 2013b).

Therefore, we argue that, because of the 6-fold range of SOC in CMIP5 models, even global HWSD stocks represent a genuine constraint to discriminate between simulations ll. 223 – 226:

> Due to the 6-fold range of SOC simulated by CMIP5 models (Todd-Brown et al., 2013), we believe that global SOC stocks from the HWSD can already represent a strong constraint to discriminate between different simulations.

Ultimately, my reaction to this paper is that the qualitative results are fairly obvious -- that increasing k leads to increasing initial soil C, and that there is a large degree of uncertainty on the response to warming based on k and how sensitive the decomposition is to temperature. But the degree of oversimplification in the single-residence time, single-Q10 model means that the qualifications to any more quantitative results are so high as to render them not useful. And dismissing these issues as side concerns I think risks creating further confusion in the community about what we do and do not know about soil C. So without more insight from the analysis I would not support publishing the manuscript in its current form.

We accept comments by the Reviewer on the quantitative implications of our results. Once again, it is not our aim to provide some improved projections of the response of soil carbon to climate change, but rather to use a simplified framework to better understand the implications of the current approach to soil carbon representation as well as the experimental protocol of spin-up to equilibrium followed by transient simulations. We accept that this was not entirely clear in the manuscript and have more clearly acknowledged this in the introduction ll. 96 -99 :

> We do not aim to provide new estimates of SOC response to climate change with our reduced complexity model. Instead, we suggest that our results will help the CMIP6 community to improve the design of future intercomparisons by highlighting the need and benefits of confronting models with existing data to reduce the uncertainty.

and in the methods ll. 161-170

> In summary, we fully appreciate that our reduced complexity model is a simplification of the processes that operate in various regions of the Earth System. However, we note that our 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). Our approach is therefore analysing how current models behave and why current models simulate a large range in SOC. Our purpose is not to provide improved results of the response of soil carbon to climate change but rather to better understand the implications of existing approaches, using in

> CMIP5, to parameterization and initial value prescription described in Section 2.2.

and ll. 191-194

> 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.

and ll 209-212

> While we do not aim to provide CMIP5-like projections of the soil carbon balance with our reduced complexity model, we investigate the value of using the HWSD to discriminate between plausible and implausible simulations.

We maintain that our modelling framework is representative of current state-of-the art global terrestrial models (see answer to previous comments) and simultaneously accept that its representation of real world processes may be poor. We do agree that the current state of the art models should be more physically based, more complex and represent more important processes. However, they are not, and explaining how these models behave is a useful and we believe important contribution to this area of science.

References

Exbrayat, J.-F., Pitman, A. J., and Abramowitz, G.: Response of microbial decomposition to spin-up explains CMIP5 soil carbon range until 2100, Geosci. Model Dev. Discuss., 7, 3481-3504, doi:10.5194/gmdd-7-3481-2014, 2014. (accepted for publication in Geosci. Model Dev.)

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.

Kahru, K., Auffret, M. D., Dungait, J. A. J., Hopkins, D. W., Prosser, J. I., Singh, B. K., Subke, J.-A., Wookey, P. A., Ågren, G. I., Sebastià, M.-T., Gouriveau, F., Bergkvist, G., Meir, P., Nottingham, A. T., Salinas, N. and Hartley, I. P.: Temperature sensitivity of soil respiration rates enhanced by microbial community response, Nature, 513, 81-84, doi:10.1038/nature13604, 2014.

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, 11, 2341-2356, doi:10.5194/bg-11-2341-2014, 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.