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 and we will consider adding this information about the full ESM in the revised manuscript. 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 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 and we will improve the definition of our objectives in the revised manuscript.

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 should address Dr Todd-Brown's concerns but we will also add some information about R_h in the manuscript.

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.