Dear Authors
I have read the revised version of your manuscript and your answer to reviewers' comments. Although your answers address well most comments, I still have a major concern about this manuscript.
I strongly agree with reviewer 1 in that apart from one or two points, there is no new major contributions in the analyses you present. A lot of what is presented here has been already discussed in Todd-Brown et al. 2013 (Biogeosciences 10: 1717), 2014 (Biogeosciences 11: 2341), Wieder et al. 2014 (Glob Biog Cycles 28: 211), and Nishina et al. 2014 (Earth Sys Dynam 5: 197). For instance, a sentence in the abstract of Todd-Brown et al. 2014 reads, 'Most of the model-to-model variation in SOC change was explained by initial SOC stocks combined with the relative changes in soil inputs and decomposition rates'. Your analysis is less supportive of the assertion that inputs are a major source of variability among models, but apart from this the analysis presented reinstates a lot of what has been said before.
However, I do acknowledge that analyzing model output from CMIP5 and checking possible reasons for disagreement among models could be a useful exercise that could inform model developers on the performance of their models. So, I will be open to consider a revised version of the manuscript that either 1) more clearly highlights the new findings and explore with more detail the problem of spin-up, or 2) provide elements for model developers to improve their models such as reporting the processed gridded data and/or give additional details in Table 2 that more clearly show differences in parameter values or model structures that lead to larger differences among models.

Regarding 1), I found simplistic your recommendation of improving parameterizations to achieve realistic preindustrial C stocks. I agree that in many places SOC is not in equilibrium during preindustrial times, but to represent this from a modeling point of view, it would be necessary to represent site history (successional/soil age, disturbance history, etc.) during the simulation period. An improvement in model parameters alone cannot achieve the desire result of obtaining C stocks that are not in equilibrium. However, representing site history at the global scale may be unrealistic. I think this is a topic that deserves more attention and you may take the opportunity with this manuscript to highlight this issue further.

I do find interesting Figure 1, specially the bottom panel that shows future C stocks almost on the 1:1 line with pre-industrial C. This implies that the transient simulations, and in particular the functions fT and fW, do not modify future C stocks significantly. In the manuscript you briefly mention the effects of the different fT fW functions, but it would be very informative if you could provide additional details that help to explain why these functions did not modify C stocks in the transient simulations.

I also found confusing your use of the term residence time throughout the manuscript. For example, in line 65 you refer to k from each pool as the baseline residence time, and on lines 78-79 the residence time is the inverse of SOC/Rh. Not only is this confusing, but also an inappropriate use of the term residence time. In the classical literature (Eriksson 1971, Ann Rev Ecol Sys 2:67, Bolin & Rodhe 1973, Tellus 25:58) the residence time is the time an atom spend in the reservoir, and only in single-pool reservoirs is the residence time equals to the ratio flux:stock. The correct term for your calculation of Rh/SOC is turnover time, and k should be called simply decomposition rate.

In summary, the revised version of your manuscript provides only a small contribution compared to similar recent papers on this subject. I would be open to consider a revised version if you are able to better show the new contributions from this analysis and provide elements to move forward in improving SOC simulations in ESM.