Dr. Exbrayat suggestions

First, we greatly appreciate your joining in the open discussion for this manuscript. Your study (Exbrayat et al., 2013) was very interesting and helpful for our paper. According to your comments and suggestions, we have added the following discussion and details in the new manuscript.

However, I must agree with Reviewer #2 that the spread in initial SOC stock is of concern. Basically, it accounts for about half of the range in CMIP5 models that was highlighted by Todd-Brown et al. (2013). I therefore think that it should be given more importance in the results or discussion.

Thank you for your suggestion. From your and RC#2's comments, we have added this point in the discussion as follows:

L243-259

There were some estimations available for global SOC stock, ranging from 700Pg C (Bolin, 1970) to 3000 Pg C (Bohn, 1976). The most widely cited studies (Post et al., 255 1982; Batjes, 1996) estimated global SOC stock to be about 1500 Pg C (0–100 cm depth). On the other hand, in the CMIP 5 experiment, the simulated global SOC stock by ESMs varied from 510 to 3040 Pg C (Todd-Brown et al., 2013). Even though the global SOC stocks for the year 2000 in this study were within range of those in Todd-Brown et al. (2013), this SOC stock uncertainty could still invoke future projection uncertainty in SOC dynamics.

First, more explanations on why this range exists and the initialisation procedure are needed. In particular, quantifying the respective contribution of differences in NPP and differences in residence time (and/or decomposition) at equilibrium would highlight where models disagree the most.

Thank you for your suggestion. This is a very important point for SOC projection. We have added an explanation of initialization in the material and methods section and the discussion as follows.

L86-88

For the spin-up of each model, we used de-trending forcing data for the years 1951–1980 repeatedly until reaching equilibration of VegC and SOC. For CO_2, we used the CO_2 concentration for 1950 while running the 30-year spin-up data set.

L366-370

In fact, SOC was formed in slow turnover fractions over thousands of years (Trumbore, 2000). Therefore, when getting an initial SOC by the spinn-up phase in biome models, there may not be enough information on the historical climate conditions and vegetation dynamics to duplicate in the entire SOC formation history. This is one of the biggest issues for accurate estimation of SOC stock in biome models.

Second, as substrate availability controls heterotrophic respiration (e.g. your equation 1), initial conditions must play a role in the response of SOC stocks and decomposition to climate change. In other words, is the steady-state of the pool driving its dynamics? This would provide insights on how important it is to initialise models to match existing SOC stocks. A more philosophical point is whether simulated SOC is comparable to actual SOC, or whether it should be considered a model-specific state variable (see work on soil moisture by Koster et al., 2009).

Thank you for this information. The viewpoints of Koster et al. (2013) are very insightful (e.g., “properly interpreted (model outputs?)”). However, in my personal opinion, there were some big differences between SOC and soil moisture. First, the time series SOC stock data is still seriously lacking even in plot scale (except soil CO_2 flux, because it also include not only SOC decomposition but also heterotrophic respiration by litter decomposition and autotrophic respiration), and thus the biome models could not be well validated for time-dependent variances even on plot (regional) scales. In this regard, the SOC might keep the memory over the millennium (Trumbore, 2000). This makes SOC dynamics difficult to predict because of amplification. Finally, I (not we) am not sure whether the "steady-state condition" is a good assumption.

For your information, we have recently touched on these aspects in a sensitivity analysis targeting the formulation of the environmental scalar $f(T) \times f(M)$ in a model driven by similar NPP (Exbrayat et al., 2013).

Thank you for your information. As we stated in the discussion and as shown in your study, it is more important to recognize the structural uncertainties in the projection. I have cited this paper in the revised manuscript. Thank you.