Dear Editor, reviewers, and Dr. Exbrayat

We acknowledge the editors and all the reviewers for sharing their time and providing valuable comments on our study. All reviewers suggested that we should additionally focus on vegetation–soil interactions with respect to SOC changes. We completely agree and have added a new analysis of this aspect in the revised manuscript. Our responses to each reviewer's comments are as follows:

#Referee 1

However, the study would be even more valuable if some of the following issues were considered: There is some mention within the results of to the extent to which changes in soil C stocks are related to changes in C input rates from plants versus the direct impacts of temperature on decomposition rates (p1043 lines 1-14). In my opinion, it would be useful to develop this discussion in more detail to allow readers to evaluate whether or not the different parameterizations of the temperature response functions are really the key issue, especially given the similarities between some of the temperature response functions. Maps plotting residence times of SOC and how these change over time would be useful. As this is a paper about uncertainty in future soil C stocks, it would be highly desirable to indicate how much of the uncertainty and differences between models is related to vegetation feedbacks (inputs), versus how much is related to the different parameterisations of SOC dynamics.

Thank you for your fundamental suggestion. We agree that the map of SOC resilience time provides very useful information for tackling the underlying uncertainties problems in SOC modeling uncertainties. Unfortunately, heterotrophic respiration as a variable is not available in the ISI-MIP database, and thus so we cannot directly estimate directly in soil resilience time in each grid cell. Instead, we have estimated turnover rates using a statistical model (a somewhat tricky technique). Estimating this parameter using this statistical model is time consuming. Hence, we have made our calculations for the global scale only. But, we believe it also to be an important starting point to develop a simpler method to determine global SOC dynamics in the earth system.

Regarding the vegetation input, we add the new analysis as Fig. 5d and discussions in revised manuscript. As you and the other reviewers suggested, we agree that this is the primary driver of the SOC stock dynamics (and terrestrial carbon budget).

In results

The different values of $\Delta SOC_{CO2-fixedCO2}/\Delta VegC_{CO2-fixedCO2}$ (Fig. 5d) indicate a different turnover rate in the vegetation (via litter) than in the SOC increase among the biome models and regions. This is because of the assumption of almost the same states except in VegC dynamics between RCP8.5 and fixed CO2 scenarios. $\Delta SOC_{CO2-fixedCO2}/\Delta VegC_{CO2-fixedCO2}$ were varied among with latitude and among the biome models. In almost all the models, $\Delta SOC_{CO2-fixedCO2}/\Delta VegC_{CO2-fixedCO2}$ was high in the higher latitude regions. In Hybrid model, the $\Delta SOC_{CO2-fixedCO2}/\Delta VegC_{CO2-fixedCO2}$ was relatively low in all regions, compared with other model results.

In discussion

A large variance in $\Delta SOC_{CO2-fixedCO2}/\Delta VegC_{CO2-fixedCO2}$ was observed among the biome models (Fig. 5d), suggesting that the vegetation–soil interactions including the vegetation turnover rate (Friend et al., 2013) and litter decomposition rate also had large uncertainties. This variance might cause an SOC projection difference among the biome models. To reduce these uncertainties, the more observation-based validation is desirable. For the litter decomposition process, for example, global database of the long-term intersite decomposition experiment team (LIDET) is one useful validation case study (Bonan et al., 2013). In addition, the process in SOC formation from alteration of litter via decomposition process (called humification) and in their stabilazation have not yet been implemented well in biome models when compared with actual SOC formation processes (Sollins et al., 1996; Six et al., 2002). This is another major process missing from the vegetation–soil interaction in biome models. To comprehensively address the biome model uncertainties in each successive process, the traceability framework developed by Xia et al. (2013) could be helpful.

The changes in inputs versus decomposition rates also has implications for determining the impact of changes in precipitation. Declining precipitation may reduce C inputs for plants as well as decomposition rates and the fact that both changes operate in the same direction, at least in water limited areas, may explain why the overall effect is smaller than the temperature impacts. Again it would be useful to distinguish whether precipitation changes alter inputs and outputs for the different biomes, but have less effect on stores (see also Schwalm et al. 2010 Global Change Biology 16, 657–670).

Thank you for your insight. We agree that, as pointed out by you, the vegetation biomass changes driven by precipitation decrease could affect the SOC dynamics. However, this effect might mainly occur regionally and not globally. In addition, our analysis to evaluate the global SOC sensitivity to ΔP took vegetation biomass changes (Eq. 3) into account. So, we thought that the lower sensitivity of SOC to ΔP resulted in a misrepresentation in the global context, as mentioned in P1048L1-7. In terms of regional SOC dynamics, we should take care of your indication.

The study is correct to emphasise that information on C stocks in the soils of different biomes are not as good as we would like them to be. However, there is a lot of information available (e.g. Harmonized World Soil database) and it is unlikely that global databases will suddenly improve dramatically in the near future. A more informative sensitivity analysis would have also evaluated these models against our best estimates of the global patterns of soil C storage. I'm not suggesting that this must be done in detail in this paper, but would it be possible to add an additional line on Figure 5a indicating our best current estimate of the distribution of soil C stocks, similar to the line for plant C stocks in Figure S2a?

Thank you for your suggestion. I have added the latitudinal estimation from the Harmonized World Soil Database to Fig. 5a for reference (black broken line). In addition, we have described it in the results section.

L198-199

Latitudinal SOC stock in the Harmonized World Soil Database (5a) showed a double peak in both the northern high latitudes and low latitudes. The highest SOC stock was found around 60° N.

Finally, one major limitation which is not mentioned is the failure to include plant-soil interactions, and linkages between nutrient and carbon cycling in some or all of these models. While it is informative to highlight the differences between the current versions of the models, it is likely that feedbacks will differ strongly if the role of decomposition in supplying nutrients is considered directly. Some consideration should be given to this major limitation when predicting future soil C stocks.

Thank you for your suggestion. In addition to the new analysis (Fig. 5d), we referred to the discussion regarding the nutrient limitations on the decomposition process, as pointed out in 4.3:

L344-349

In addition to the indirect CO_2 effects, other nutrient limitations (e.g., nitrogen and phosphorus) and their sensitivities could be large sources of uncertainty in SOC projection via vegetation production (Goll et al., 2012; Exbrayat et al., 2013). In our study framework, we cannot adequately validate these issues since only a few models consider them in their current versions. Therefore, further interactions must be validated to more comprehensively understand the uncertainty sources in SOC projection.

Specific technical comments:

Page 1043 line 19: The reference should be to Table 2.

Thank you. We have revised it.

Page 1047, lines 20-23: The values from the Raich and Zhou studies do not reflect the direct effects of temperature on decomposition rates, and I would recommend removing this part of the discussion.

We have deleted this sentence in the new manuscript.

Page 1049, lines 1-12: It would be useful to provide a fuller evaluation of how seasonally frozen soils are represented in high latitude locations and how the model deals with warming-induced thaw. This is critical to evaluating which models are most likely to be representing high-latitude feedbacks appropriately. Again, this emphasises the need to distinguish between inputs and outputs. Are the losses from high latitude systems in LPJmL related to the inclusion of permafrost dynamics, or reductions in plant productivity (boreal browning).

Thank you for your suggestion. We have added more explanation about the LPJmL permafrost schemes as follows:

L322-324

Because LPJmL incorporated a freeze-and-thaw thermodynamics explicitly in discrete layers, it can simulate vertical water and carbon distributions in the model. This scheme enables LPJmL to describe the surface soil water deficit due to permafrost melting.

Tables Table 1:

More details are required in the figure legend. For example, what does ’compartment’ refer to?

We have added the detailed information to the Table caption.

Figures:Figure 5:The panels do not seem to be in the same order as indicated in the figure legend.

We have revised the same.

Is there any way of correcting the values for the different latitudes by surface area. At the moment it appears that the impacts of CO_2 fertilisation on SOC will be greatest at mid to high latitudes (Fig 5c), whereas previous studies suggested a positive relationship between temperature and CO_2 fertilisation, at least for net primary productivity (e.g. Hickler et al. (2008) Global Change Biology 14, 1531–1542). Is this analysis suggesting that the response for carbon storage in soils should show a different spatial pattern, and to what extent is the latitudinal pattern controlled by latitudinal differences in land area rather than changes in SOC per unit area?

The values in Fig. 5 are weighted by each grid surface area for conversion of the unit (Pg/2.5 lat degree). For this question, our new analysis (Fig. 5d) can be useful. As in Hickler et al. (2008), our results in almost all models showed the largest increase in NPP (and VegC in Fig. 2S) in the low to mid latitude region. On the other hand, as pointed out by you, the highest SOC increase was found in the mid to high latitude regions. This is because of the difference in allocation of the photosynthesis production to the internal ecosystem.

Referee 2's suggestions

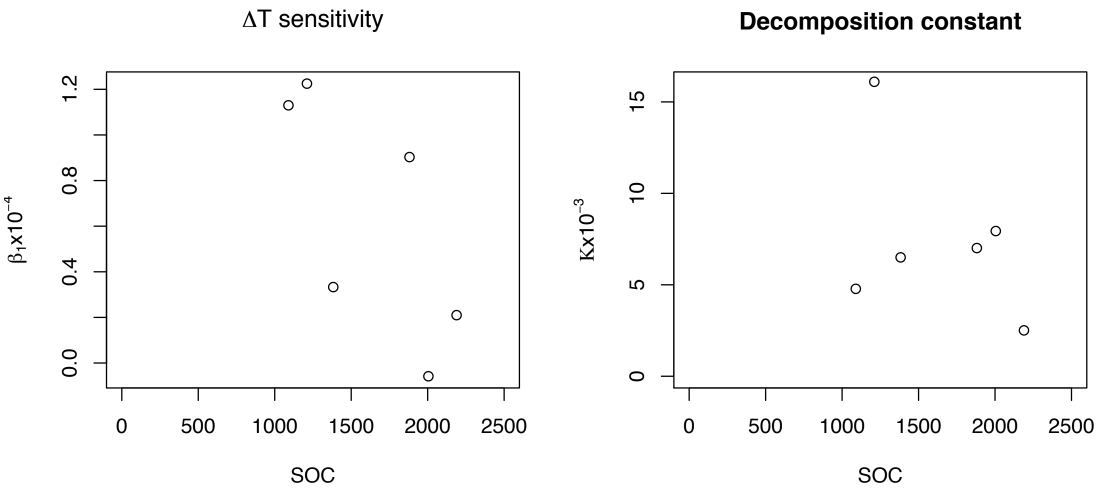
The potential impact and feedback of global warming on soil organic carbon pool is one of the crucial researches to better predict future climate change. Although this topic have made some progresses recently, it still have a lot of uncertainties. In this study, Nishina et al. analyzed soil carbon storage and its change (basing on seven biome models) under the projected climate change, and try to evaluate the sensitivity of temperature and precipitation on global SOC stock. The study made some valuable results. However, there are some aspects need be discussed or clarified.

Specific comments:

1. The differences of modeled SOC storage among 7 biome models are very large (1090 ~ 2650 PgC). As these initial values of carbon pools will necessarily impact the projected SOC (formula 1), the authors need evaluate these initial carbon pools first, which will be helpful for assessing the results of SOC change and temperature sensitivities. In addition, does the magnitude of initial carbon pool have some statistical relationship with the SOC change or sensitivity?

> Thank you for your suggestion. We have added the evaluation of global SOC stocks in this study by comparing it with that in previous studies 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.

We agree on the importance of the relationship between the initial carbon pool and sensitivity (or decomposition constant). However, we assume that seven models are insufficient to check correlations in these relationships (see figures below). Hence, we have not added these figures to the revised manuscript. In the near future, new and different biome models will enter into ISI–MIP phase 2, and then this analysis might be more valuable.



2. The MS mentioned (Page 12 Line 14~17) that this study used a simple substitution (i.e. setting the global SOC stock in each biome models to equal to the referenced value) to test the impact of SOC on projection uncertainty. How does it be conducted? After all, the values of soil organic carbon in each biome model are related with other state variables (e.g. veg pool) and model parameters (e.g. turnover rate). Did they change accordingly?

Thank you for your comments. We did not adequately explain this procedure. First we estimated a steady-state model (Eq. 2–9), which act as an emulator of global SOC dynamics in each biome model. Using this model, we then evaluated the magnitude of global SOC decomposition and ΔT sensitivities.

We have added section (2.3) in the material and methods section to explain this calculation.

3. Why did the authors only analyze the statistical relationship between the global total SOC and global mean temperature? As the output of biome modeles are spatial- explicit, it is possible to get these parameters values (k, beta1, beta2) for different vegetation types (or spatial grids), which should have more spatial information on these parameters and then helpful to assess the sensitivity for different biomes.

> Thank you for your suggestion, and we agree with your opinion. But, as we have already conveyed to reviewer 1, the estimation of parameters in this statistical model is time consuming. So, our calculations were only for the global scale, believing this to be an important starting point for recognizing SOC dynamics in the earth system.

Specific technical comments:

1. Page 9 Line 18~19, posterior distributions listed in Table 2, not Table 1.

> Thank you. We have revised the same.

2. What the meaning of indirect CO2 fertilizer effect (Page 11 Line 5)?

> We defined “indirect CO2 effect ” in the manuscript as follows:
>
> > This suggests that the increases of plant production and biomass by CO_2 fertilizer effects in the increasing CO_2 scenario (RCP8.5) contributed to the SOC stock increases because of the increase of C input to soil (indirect CO_2 effect).

3. The beta 1 of JeDi model is negative value (Table 2), Why?

> Actually, the reason why negative value (almost 0) was estimated in JeDI might be not clear. But, some reasons are presumable. Although JeDi don’t have a soil moisture function in SOC decomposition process, our steady-state model include ΔP. Secondary, the steady-state model was assumed the constant VegC turnover rate (VegC to SOC). In any case, JeDi have relatively low ΔT sensitivity, compared to the other models.

4. The change of Veg C (-527 PgC) seems too high (Page 9 Line 8), comparing with the vegetation pool value (493 PgC). It needs some verifications.

Thank you for pointing out this discrepancy.

The value is not correct, but the change still has a high value (−517 Pg) (maybe a typographical error). This value was obtained from the Hybrid model (from ca. 780 to 260 Pg-C). This large decline is partially due to the high vapor pressure deficit sensitivity of this model (Andrew et al., in press in PNAS).

Referee 3 suggestions

As Reviewer 2 mentioned, one of the crucial uncertainties to simulate future soil organic carbon dynamics is the interactions with the vegetation. Since the models used here have various complexities and parameterizations on plant productivity and litter production, the changes in quantity and quality of litter input to soil organic carbon will have a strong effect to the stock. It would be excellent if the authors could standardize the litter input to elucidate the mechanisms which produce the difference among the models. If this would be too difficult, I would like to read some discussion about this.

Thank you for your suggestion. However, we cannot use a variable for the litter input flux (nor for the amount of litter in all the biome models) in the ISI–MIP. Instead of litter input, we have additionally compared the differences in vegetation biomass C between the RCP8.5 and the CO_2 fixed scenario (ΔVegC) with the SOC difference between the RCP8.5 and the CO_2 fixed scenario (ΔSOC) (Fig. 5d) in the new manuscript. In both scenarios, the same climate variables (HadGEM in RCO8.5) were used; therefore, we can extract the effect of vegetation increase on SOC increase.

In discussion

A large variance in $\Delta SOC_{CO2-fixedCO2}/\Delta VegC_{CO2-fixedCO2}$ was observed among the biome models (Fig. 5d), suggesting that the vegetation–soil interactions including the vegetation turnover rate (Friend et al., 2013) and litter decomposition rate also had large uncertainties. This variance might cause an SOC projection difference among the biome models. To reduce these uncertainties, the more observation-based validation is desirable. For the litter decomposition process, for example, global database of the long-term intersite decomposition experiment team (LIDET) is one useful validation case study (Bonan et al., 2013). In addition, the process in SOC formation from alteration of litter via decomposition process (called humification) and in their stabilazation have not yet been implemented well in biome models when compared with actual SOC formation processes (Sollins et al., 1996; Six et al., 2002). This is another major process missing from the vegetation–soil interaction in biome models. To comprehensively address the biome model uncertainties in each successive process, the traceability framework developed by Xia et al. (2013) could be helpful.

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 CO2 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) × 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.