Biogeosciences Discuss.,
doi:10.5194/bg-2016-549-AC3, 2017


Biogeosciences Discussions



# *Interactive comment on* "Improvement of Soil Respiration Parameterization in a Dynamic Global Vegetation Model and Its Impact on the Simulation of Terrestrial Carbon Fluxes" *by* Dongmin Kim et al.

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Received and published: 7 May 2017

Anonymous Referee #2 Received and published: 12 March 2017 Q10 is a critical parameter for simulating soil respiration and C cycle and therefore feedback between C cycle and climate. Yet most ESMs adopt constant Q10, which can possibly lead to unrealistic simulations of soil processes and other C cycle processes. Therefore, this paper contributes to improve model performance by implementing a new Q10 parametrization. This has significant implications to modeling studies.

We appreciated the reviewer's thorough and constructive comments. Below is our



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point-by-point response to the specific comments.

I have two major concerns. One is the authors did not explain why improvement of Q10 parameterization and resulting improvement of soil respiration can help improve simulations in GPP. Is it because nitrogen availability resulting from changed soil respiration rates or other mechanisms?

This study implemented the variable Q10 in the parameterization of soil decomposition flux, which directly affects the heterotrophic respiration from soil organic matter (SOM). In addition, the CLM4 model used in this study has the interactive carbon-nitrogen (C-N) cycle, by which it changes the plant assimilation and GPP in the meantime. For example, a higher (lower) Q10 value induces a faster (slower) carbon decomposition rate in the model, and it tends to increase (decrease) nitrogen assimilation from soil to vegetation, thereby increasing (decreasing) GPP by plants.

In CLM4 (Olsen et al., 2013), plant nitrogen uptake from soil mineral nitrogen pool is separated by plant demand for mineral nitrogen from the soil (NFplant_demand_soil) and retranslocated nitrogen (NFretrans) which construct to mobilize senescing tissues. Therefore, total plant nitrogen uptake from soil mineral nitrogen pool is :

ãĂŰNFãĂŮ_(plant_demand_soil)= ãĂŰNFãĂŮ_(plant_demand)-ãĂŰNFãĂŮ_retrans

This total plant nitrogen demand for new growth (NFplant_demand) is calculated by total carbon available for new vegetation growth allocation (CFavail_alloc) from soil as:

ãĂŰNFãĂŮ_(plant_demand) ãĂŰCFãĂŮ_(avail_alloc) N_allom/C_allom

where CFavail_alloc is related with carbon amount in each carbon pools. These processes induce that more carbon decomposition enhanced more nitrogen supplement from soil to plant for new plant growth (increasing GPP).

This aspect is discussed in the introduction section as below: (L130) “Realistic spatial distribution of soil decomposition processes affect not only Rs but also primary production by improving nitrogen assimilation from soil to vegetation”

We further examined the impact of Rs on plants net primary production. The variable Q10 parameterization tends to affect the turnover time of soil carbon, which is defined as the soil carbon amount divided by net primary production (NPP) (i.e., soil carbon/NPP). As shown in Fig. S1 below, the run with variable Q10 (EXP) makes shorter turnover time in northern hemisphere high latitudes and longer in the tropics compared with the control run(CTL). The shorter turnover time in high latitudes suggests the enhancement of nitrogen assimilation to vegetation in EXP, thereby enhancing net primary production by plants. We will address this change in the manuscript as below:

(L336) "The variable Q10 in the parameterization of soil decomposition flux immediately affects the heterotrophic respiration from soil organic matter (SOM) as given by the model formulations in Eq. (1) and (2). Moreover, this modification changes the plant assimilation and GPP in the meantime in this carbon-nitrogen coupled model. A faster (slower) carbon decomposition rate in the model tends to increase (decrease) nitrogen assimilation from soil to vegetation and plants, thereby increasing (decreasing) GPP. This aspect is illustrated well by comparing the turnover time of the soil carbon, which is defined as the ratio of soil carbon amount to the net primary production (NPP), between the CTL and EXP runs (See Fig. S1 below; Fig. 8 in the revised manuscript). As shown in the figure, the run with variable Q10 (EXP) makes shorter turnover time in northern hemisphere high latitudes and longer in the tropics compared with the control run(CTL). The shorter turnover time in high latitudes suggests the enhancement of nitrogen assimilation to vegetation in EXP, thereby enhancing net primary production by plants."

Another is the confusion of sensitivity of soil respiration to temperature (i.e., Q10) and sensitivity of Q10 to parametrization.

From the Eq. (2), the soil respiration in this model is already an exponential function of soil temperature. The Q10 value in the original scheme is a global constant of 1.4, and therefore, the sensitivity of soil respiration to temperature is constant, regardless of plant functional types. This study implemented a state-dependent Q10 parameterization for each 17 plant functional types. As we discussed in the text (L216-219), "the dependence of Rs on soil moisture and temperature can be dependent on PFT", and "this approach is to consider the nonlinear relationship between Rs and the two major soil environmental factors (i.e., soil temperature and moisture [Davidson et al., 1998; Raich et al., 2002]".

Specific comments are listed below.

Lines 84-85. Please cite literature(s) for this statement.

Todd-Brown et al. (2013) emphasized the importance of soil carbon pool in the carbon exchange between atmosphere and land.

(Addition after L85) "(Todd-Brown et al., 2013)".

Lines 89-90. There are many studies on soil respiration under experimental warming that which should address this point better.

We will add more reference in the revised manuscript as: "Many studies investigated the response of soil respiration (Rs) under global warming, and most of them suggested the warming would accelerate the release of $CO_2$ from soil in future (Cox et al., 2000; Dufresne et al., 2002; Friedlingstein et al, 2003; Suseela et al., 2012)."

Lines 93-94. There is a recent review paper talking about this issue (Global Biogeochemical Cycles, 2016, 30: 40-56)

We will add this recent review paper: (After L93)"Moreover, Luo et al., (2016) suggested that optimizing parameters in the current ESMs are needed based on observations for improving soil carbon projection in the models. The reduction of uncertainty in the parameterizations of the biogeochemical process in the soil system remains a challenge for the ESM modeling community."

Lines 102-103. I would not say that because there are many field studies examined

Q10, but it is true, we lack long-term observation data on how Q10 changes overtime. It may be difficult for regular field studies to explore dependency of Q10 on temperature as they derive Q10 through entire seasonal temperature range when Rs is measured. However, it can be tested via lab incubation experiments or field experiments with warming treatments.

Following the reviewer's comment, we will delete this sentence (L102-103).

Lines 104-105. I think they meant Rs tends to decrease with temperature increase. This needs to be clearly stated.

Belay-Tedla et al. (2009) suggested that warming-induced changes in plant growth and community structure can considerably influence the quality and quantity of substrates which in turn regulates the responses of soil respiratory C release to rising temperature.

We changed this sentence (L104-105).

Line 145. Add "time resolution/interval/step" before "is monthly".

We will add the time resolution for the data in this sentence. (L145)

Line 151. "Each tile is 1200 X1200 km" needs to be clearer. Does it refer to original MODIS17A3 GPP and NPP data?

We will remove this sentence.

Lines 155-156. Which year are these data for or are they the average from 2000 to 2006?

It is based on the average of 2000 – 2006.

Line 157. This sentence is not clear. Did they mean that they compared modeled Rs against the data by Hashimoto et al. (2015) for validating their model? What is time period of Rs data by Hashimoto et al. (2015)?

(L157) The sentence will be clarified as:





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“Simulations of soil respiration (Rs) by CLM4 will be verified using the gridded reanalysis dataset from Hashimoto et al. (2015), which has the data period of 1983-2005.”

Line 159. What is “SRDB”? It needs to be fully spelt.

We add the full name for SRDB (L159) : “soil respiration database (SRDB) version 3 (Bond-Lamberty and Thomson, 2010)”

Lines 165-167. What does “assuming” mean?

Hashimoto et al. (2015) developed the semi-empirical model parameterized with many Rs data points using near surface temperature and precipitation. Using this semi-emipirical model, Hashimoto et al. (2015) derive the long-term gridded Rs data.

(L167) “Assuming” will be replaced with “deriving”.

Lines 176-177. Why is the decomposition flux calculated by multiplying carbon amount from dead leaf? Soil respiration include both microbial respiration and root respiration. The substrate for microbial respiration is SOM in the soil, which is originally derived from litter (dead leaf, wood, and root). And root respiration is respiration by live roots, which is related to root biomass. If their model does not separate the two components, it should be carbon content of soil.

This is our mistake and it is supposed to be “litter”. In CLM4, the root respiration is from live roots as your comments. Therefore, the heterotrophic respiration of soil is from microbial respiration from SOM. (L176-177) “dead leaf” → “litters”

Lines 186-187. It should be “the water potential for soil decomposition”. We modified this sentence.(L186-L187)

Lines 212-213. In a multiple regression analysis, why are the relationships between Rs and T and between Rs and M separately?

In our parameterization, Q10 is changed not only by temperature but also by soil moisture. This is same as in Qi et al. (2002).

Using Eq. (8), multiple regression analyses were conducted for each plant functional types simultaneously with soil temperature and moisture.

Lines 224-235. This paragraph is very confusing. The authors need to give time step of each dataset to avoid confusion. Are GPCP and TRMM data used to rescale precipitation data by Sheffield et al. (2006) to a time step of daily and 3-hour step for forcing CLM? And CLM is forced by 3hr data, then why daily data are needed? Is it for regression analysis? In addition, Sheffield et al. (2006) data have radiation, so what are radiation data by NASA for? If they are for all descriptions of Sheffield et al. (2006), it needs to be clear.

The whole paragraph is revised as: (L224-237) "To obtain these variables, this study conducted the land surface reanalysis for recent 30 years (1981 – 2010), using the off-line land surface model driven by observed meteorological forcing data archived by Sheffield et al. [2006]. The 3-hourly forcing data by Sheffield et al. (2006) consists of the National Centers for Environmental Prediction–National Center for Atmospheric Research reanalysis (Kalnay et al., 1996), which were corrected with independent observations. For precipitation, the daily Global Precipitation Climatology Project (GPCP, Huffman et al., 2001) data were processed into the 3-hourly data using the Tropical Rainfall Measuring Mission (TRMM: Huffman et al., 2003) 3B42RT but constraining daily mean amount from GPCP. Surface temperature was constrained by the observation from the monthly Climatic Research Unit (CRU) 2.0 product (Mitchell et al., 2004). The observed radiation was also used from the monthly NASA Langley surface radiation budget (Stackhouse et al., 2004) data. Remaining meteorological conditions such as surface wind and humidity were from the National Centers for Environmental Prediction–National Center for Atmospheric Research (NCEP-NCAR) atmospheric reanalysis. Interested readers refer to Sheffield et al. (2006) for the detail. Using this 3-hourly forcing data, this study integrated the off-line land surface model with 3-hourly time steps and at a $0.5^\circ \times 0.5^\circ$ spatial resolution."

Line 226. Year for the literature is needed.





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We will modify as “The forcing data archived by Sheffield et al. (2006)....”.

Lines 236-237. How can an audience find a reference that is not published?

We delete this sentence in the revised manuscript.

Line 238. I think they are talking about soil respiration by Hashimoto et al. (2015), but this needs to be specified.

It is correct. We specified the data sources as (Modification from L238∼) : “Figure 1 shows the r-squared values from the multiple regression analysis between the soil respiration data from Hashimoto et al. (2015) and the soil temperature and moisture derived from the off-line land surface model. The result is presented for each PFT type in the figure.

Lines 246-247. The period of forcing data by Sheffield et al. (2006) is reduced to be the same with GSWP2?

Yes, and when we adjust the sampling period from the long-term period (1983-2010) to the short one (1986-1995), the r-squared values become similar as the ones obtained using GSWP2 data for the period of 1986-1995. This suggests the sampling period for regression is important (See Figure S2 below). This study conducted the offline land surface model integration forced by observational forcing for a sufficiently long period (e.g., 1983-2010), which is to better represent the subsurface climatology at the presence of strong interannual variability.

Line 251. Does this mean a global constant and unchanged over time? Fig. 1. Why 28 years? Is this because of time period of data by Hashimoto et al. (2015)? The title is confusing and needs to be revised. It would be useful for audiences to see regression models for all PFT as supplementary information.

The CTL simulation is global constant of Q10 and unchanged over time. In contrast, the EXP changed the Q10 in every time step in the model at the change of soil temperature and moisture. The relationship is taken empirically from the multiple regression analysis. To remove the impacts from interannual variability in meteorological data due to El Nino/La Nina cycles, this study calibrated the regression model for the long period of 1983-2010, which is the longest available period for the Hashimoto et al.'s data.

We attach the Excel file for the regression analysis for each 17 PFTs.

Line 252. CESM run needs to be described before "Figure 2".

This CESM data was obtained from CMIP5 ESMs results. We indicate this in the manuscript: (L252) "The offline simulations for GPP and soil respiration are also compared with those from the fully-interactive Community Earth System Model with Biogeochemistry (CESM-BGC) model simulation that used the identical land surface model (i.e., CLM4). The dataset was obtained from Earth System Grid – Center for Enabling Technologies (ESG-CET at http://pcmdi9.llnl.gov/)."

Fig. 3. CTL should be included in the figure title.

We will modify from "CLM4" to "CTL" in Figs. 3c and 3f.

Line 272. The title should be "results and discussions".

We will change the section title as suggested.

Line 330. Sensitivity of Rs to soil temperature?

We will change it to "Sensitivity of Rs and soil temperature" to "sensitivity of Rs to soil temperature (L330)".

Line 334. Which panel in Fig. 6 did they refer to by "enhanced relationship between Rs and temperature" for the northern Eurasian and Chinese regions?

It is possible to misunderstand this sentence. The Eurasian and Chinese regions is mostly covered boreal shrub land and crop field. The regions which are enhanced relationship between Rs and soil temperature are improved to simulate GPP.

Line 273 and the whole paragraph. In method section they never talked about CMIP5, why here a subtitle for CMIP5 GPP? If they use CMIP5 to evaluate or compare CLM EXP, they needs to describe it in method and need to include EXP or CTL in this section and in Fig. 4. They also need to give some details such as how many CMIP5 models, model names and if MME includes CESM-BGC and NorESM. There are few papers with figures that do not include results from their present study. If they want to discuss the issue of underestimation GPP by coupled N cycle, this part should be put into discussion and the figure should be in the supplementary document. Overall, this part is not very relevant to the main purpose of this study.

This study used the GPP data simulated by 10 emission-driven CMIP5 ESMs. We add the list of ESMs in Table 2 in the revised manuscript.

The original manuscript is lack of reasons why this study also examined the changes in GPP. As we answered in the above, we hypothesized that the improvement of soil respiration process by implementing variable Q10 in the model should also improve the representation of GPP in the C-N (carbon-nitrogen) coupled ESMs.

The model intercomparison for GPP simulations by CMIP5 ESMs was to highlight the deficiencies in the GPP simulation by the C-N coupled models. The C-N coupled ESMs (i.e., CESM-BGC, NorESM) significantly overestimated (underestimated) GPP in the tropics (high latitude regions) compared with the rest of ESMs without C-N coupling. The impacts of new parameterization in this study on the GPP simulation is the reduction of systematic biases of GPP spatial distribution.

For a better connection, We reconstructed results part for single section from particulars section. And we revised manuscript as : (L271) “This study further compares the simulation of GPP by various ESMs in CMIP5.”

Figure 4. Figure title needs more information. “MME” needs to be fully spelt. I would use “CMIP5” instead of “MME” in the legend. Are blue bars the average of CESM-BGC and NorESM? Why no results from EXP? Global GPP can also be shown in this figure since it is mentioned in the text. In addition, no y axis (unit) in this figure.

We add the Figure title and the label for y axis in Fig. 4. “MME” is replaced with “CMIP5” as suggested. Also detailed information for blue bars are added in the caption.

As this paragraph is for the overall simulation bias of GPP by CMIP5 ESMs form the interactive climate-carbon feedback simulations, we do not include the offline simulation results from CTL and EXP. Instead, those are given for CTL and EXP in Fig. 8 in the original manuscript.

Line 288. What are they talking about by “these two”? In addition, according to the figure, GPP 60N-80N is not major region.

We delete the sentence for clarification, and modify the preceding sentence as: (L287-290) “These systematic biases in the tropics and the Northern Hemisphere high latitudes are common in the C-N coupled models based on CLM4 (Bonan et al. 2009).”

Lines 293-296. Delete this since it is a repeat of last paragraph.

We delete the sentences in the revised manuscript.

Lines 296-300. How could they conclude that from Fig. 5 since a) is difference between EXP and observation, not the absolute values of observation and EXP. They can show maps of all three data sets (observation, EXP and CTL) in supplementary documents to support this statement.

As we already show the spatial pattern of Rs from Hashimoto et al. and the offline simulation (CTL) in Fig. 3d and 3f, respectively, we only show the difference pattern of EXP minus observation in Fig. 5a. We add the supplementary figure in the manuscript below for the discussion.

Fig. 5. I would suggest to add another panel for the difference between CTL and observation. Unit is missing for both panels.

This is already given in Fig. 3f. We add the unit and Y axis in the figure and caption. Also, we changed caption in figure 3 for avoiding confusing.





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Lines 293-331. It would be easier to give panel number such as “Fig 5a)”. Thank you for your comment. We will add the specific figure title in this paragraph.

Lines 312-313. Fig. 6 does not support this point because the y axis is the difference between EXP and CTL. It is not the absolute Rs of EXP or CTL. More changes do not necessarily result in higher absolute Rs. The point may be supported if they draw scatter plots for both EXP and CTL in each panel and show better correlation between Rs and temperature in EXP than CTL. Line 314. “The difference between EXP and CTL increases with temperature” is not supported by Fig. 6 since they are only the cases in a few panels

The relationship between absolute Rs and soil temperature is not much different between CTL and EXP simulation. In figure S4, we provided the r-squared value between log Rs and soil temperature. All vegetation types are positive relationship between log Rs and soil temperature in both simulation.

Temperate, tropical forest and Grass regions show relative higher positive relationship between Rs and temperature in CTL simulation comparing with EXP simulation. However, EXP simulation has higher positive r-squared value in cold temperature regions (e. g., Boreal forest and Boreal shrub). Therefore, non-uniform Q10 value affects more in cold regions than warm regions. Interestingly, PFTs of higher value of climatology averaged Q10 value (Table 1 in manuscript) comparing with standard value (1.5) enhanced relationship between Rs and soil temperature such as boreal forest and boreal shrubs except for crop land.

We added figure S4(Table 3 in manuscript) and modified manuscript as (L312-322): “The sensitivity of Q10 parametrization depend on the surface vegetation types. For instance, boreal forest and shrub regions which has cold climate shows significant enhanced relationship between Rs and soil temperature. In contrast, temperate, tropical forest and Grass regions show relative higher positive relationship between Rs and temperature in CTL simulation comparing with EXP simulation (Table 3). Some re-





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gions in shrub and crop regions is unclear to show this relationship. Interestingly, PFTs of higher value of climatology averaged Q10 value (Table 1) comparing with standard value (1.5) enhanced relationship between Rs and soil temperature such as boreal forest and boreal shrubs except for crop land."

Lines 319-322. This explanation is not convincing because tropical is the opposite and it cannot explain shrub, grass and crop.

We added the supplement figure and discussion in manuscript (L312-322) : "In boreal forest and shrub regions which has higher Q10 value comparing with global constant, the relationship between Rs and temperature in EXP are enhanced than CTL simulation. However, the tropical, temperate forests and grass regions (lower Q10 value than 1.5 in EXP simulation) is unclear for impacts of Q10 parameterization. One of possibility is that these regions are strong sensitivity of soil respiration to soil moisture. Figure S5 supported that this mechanism. In high temperature region, the sensitivity of Rs on the moisture is stronger than temperature. It reflected the unclear change of temperature sensitivity of Rs to soil temperature over tropical forest region."

Fig. 7. Unit should be given.

We add the unit in Fig. 7.

Lines 336-334. Please explain the mechanism for this.

Our parameterization modified the decomposition rate in the soil layers. As we responded in the above (in the Major Points), the variable Q10 values in space and time affects the heterotrophic respiration from soil organic matter (SOM). From the Eq. (1) and (2), in Sect. 2.2, a higher Q10 value tends to increase carbon decomposition into soil layers. Enough nutrients in soil layers induces more carbon assimilation to vegetation and plants. This impact on GPP is reflected on the simulated turnover time difference between CTL and EXP (Please check Fig. S1 above). The results suggest that Q10 variation influences on gross primary production (GPP), which response depends on region and different in the tropics and the high latitudes.

Lines 337-340. The global GPP in FLUXNET should also be given here. “SH” should be fully spelt.

(L340) “SH” is replaced with “Southern Hemisphere”.

Line 344. Are the numbers in Fig. 8 zonal mean or zonal sum? I think they are sums.

This is zonal mean of GPP.

Line 345. The word “budget” is not suitable here. Use GPP.

GPP is more suitable in that sentence.

Line 348. Are they talking about Fig. 3? Please indicate.

It indicates Figure 4 (zonal averaged GPP in CMIP5-ESMs and C-N coupled model). We will add the figure number at the end of sentence (L348).

Fig. 8. Adding global data to this figure would help. This figure should merged with Fig. 7 (i.e., three panels). Y axis is missing.

We add the global average of GPP in Fig. 8 in the revised manuscript. We also add the label and unit for y axis.

Lines 349-350.What did the authors mean here? How can carbon pool in the soil system affect plant assimilation? Plants do not absorb carbon in soil.

Nitrogen decomposition is closely related with carbon decomposition in CLM4. We modify it as: “The modification to the soil process parameterization can affect the rest of the terrestrial carbon cycle by changing the carbon pools and nitrogen pools in the soil system needed for plant nitrogen assimilation”

Fig. 9. No y axis.

We will add the Y axis.

Lines 398-400. Please cite literature here.

We add the literatures (L400, Bonan et al. 2010 and Bonan et al., 2011).

Lines 404-405. This sentence is not clear.

We will modify this sentence. (L404) : “In fact, the parameterization of photosynthesis in the state-of-the-art ESMs is implemented in a similar fashion with small differences, based on the formulations from Farquhar et al. (1980). ”

References: Bonan, G. B., Lawrence, P. J., Oleson, K. W., Levis, S., Jung, M., Reichstein, M., Lawrence, D. M., and Swenson, S. C.: Improving canopy processes in the Community Land Model version 4 (CLM4) using global flux fields empirically inferred from FLUXNET data, J. Geophys. Res., 116, G02014, doi:10.1029/2010JG001593, 2011. Bonan, G. B., and Levis S.: Quantifying carbonâĂŘnitrogen feedbacks in the Community Land Model (CLM4), Geophys. Res. Lett., 37, L07401, doi:10.1029/2010GL042430, 2010. Cox, P. M., Betts, R. A., Jones, C. D., Spall, S. A., Totterdell, I. J., :Acceleration of global warming due to carbon-cycle feedbacks in a coupled climate model, Nature, 408, 184-187, 2000. Dufresne J. L., Friedlingstein, P., Berthelot, M., Bopp, L., Ciais, P., Fairhead, L., Le Treut, H., Monfray, P.: On the magnitude of positive feedback between future climate change and the carbon cycle, Geophys. Res. Lett., 29, 1405, 43 , 2002. Farquhar G. D., Caemmerer, S., Berry, J. A..:A biochemical model of photosynthetic CO2 assimilation in leaves of C3 species, Planta, 149, 78–90, 1980. Friedlingstein P., Dufresne, J. L., Cox, P. M., Rayner, P.: How positive is the feedback between climate change and the carbon cycle?, Tellus-B, 55, 692–700,2003. Hashimoto, S., Carvalhais, N., Ito, A., Migliavacca, M., Nishina, K. and Reichstein, M.: Global spatiotemporal distribution of soil respiration modeled using a global database, Biogeosciences, 12(13), 4121–4132, doi:10.5194/bg-12-4121-2015, 2015. Luo, Y., Wan, S., Hui, D., and Wallace, L. L.: Acclimatization of soil respiration to warming in a tall grass prairie, Nature, 413, 622-625, doi:10.1038/35098065, 2001. Qi, Y., Xu, M., and Wu, J.: Temperature sensitivity of soil respiration and its effects on ecosystem carbon budget: nonlinearity begets surprises, Ecolog. Model., 153, 131–142, 2002. 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(3), 1717–1736, doi:10.5194/bg-10-1717-2013, 2013. Suseela, V., Conant, R. T., Wallenstein, M. D., and Dukes, J. S.: Effects of soil moisture on the temperature sensitivity of heterotrophic respiration vary seasonally in an old-field climate change experiment, Global Change Biol., 18, 336–348, 2012. Sheffield, J., Goteti, G., and Wood, E. F.: Development of a 50-Year High-Resolution Global Dataset of Meteorological Forcings for Land Surface Modeling, J. Clim., 19, 3088-3111 doi: http://dx.doi.org/10.1175/JCLI3790.1, 2006. Xu, M., and Qi, Y.: Spatial and seasonal variations of Q10 determined by soil respiration measures at a Sierra Nevadan forest, Global Biogeochem. Cy., 15, 687 – 696, 2001. Zhou, T., Shi, P., Hui, D., and Luo, Y.: Global pattern of temperature sensitivity of soil heterotrophic respiration (Q10) and its implications for carbon-climate feedback, J. Geophys. Res., 114, doi:10.1029/2008JG000850, 2009.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-549/bg-2016-549-AC3-supplement.zip

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-549, 2017.



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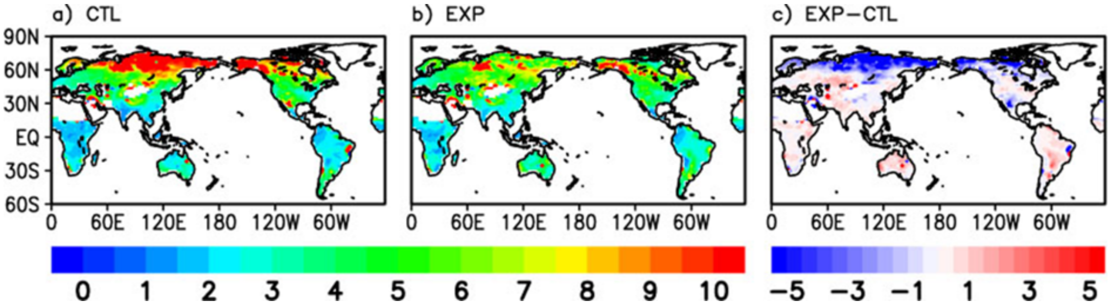


**Fig. 1.** Figure S1. Spatial distribution of turnover time (year) of soil carbon in (a) CTL and (b) EXP. (c) indicates the difference between EXP and CTL simulation. The turnover time is defined as the ratio



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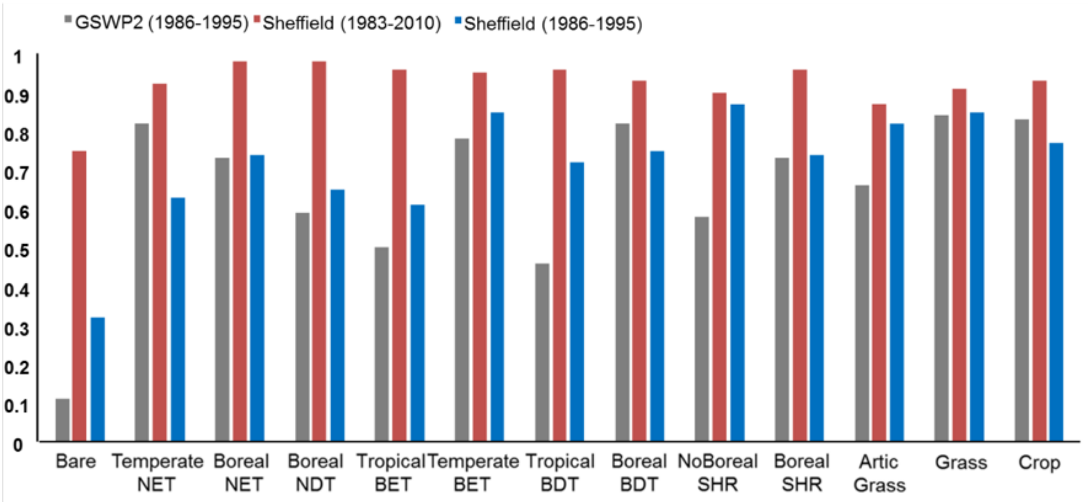


**Fig. 2.** Figure S2. R-squared values for each PFT from the multiple regression analysis between Hashimoto et al.'s soil respiration data and the three different meteorological datasets for soil temperature and



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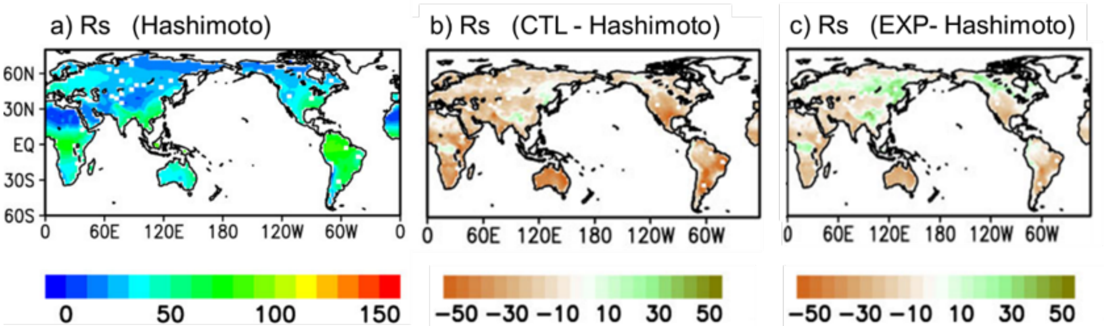


**Fig. 3.** Figure S3. (a) The spatial distribution of Rs from Hashimoto et al. (2015) and bias pattern of Rs in (b) CTL simulation and (c) EXP simulation.



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| | Temperate | Boreal | Tropical | Shrub | B. Shrub | Grass | Crop |
|---|---|---|---|---|---|---|---|
| CTL | 0.40 | 0.06 | 0.34 | 0.06 | 0.04 | 0.38 | 0.27 |
| EXP | 0.36 | 0.25 | 0.31 | 0.05 | 0.20 | 0.28 | 0.31 |

**Fig. 4.** Figure S4. R-squared values between log Rs and soil temperature by PFTs in CTL and EXP



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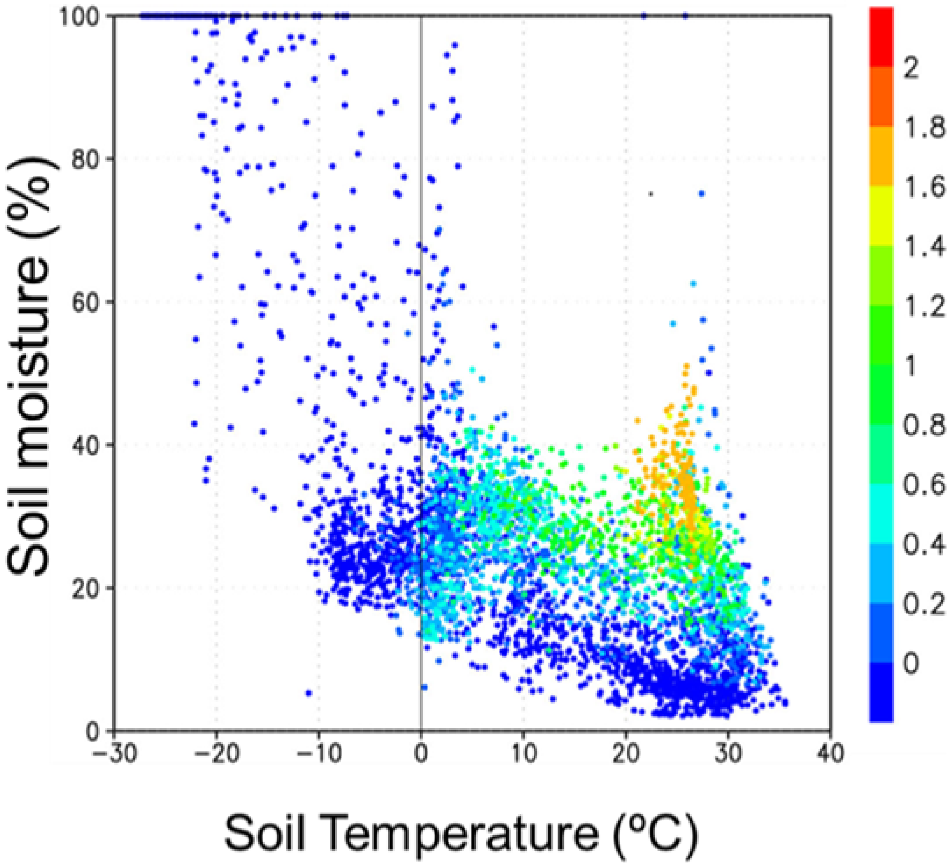


**Fig. 5.** Figure S5. Spatial Matrix between soil moisture (Y-axis) and soil temperature (X-axis) with Rs (color dots) in the CTL simulation.