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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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Land carbon models are critical for understanding controllers of atmospheric carbon dioxide under a changing climate. As such, accurately estimating soil respiration sensitivity to temperature and moisture is critical. This manuscript presents a re-analysis of existing data products to propose new biome specific parameterizations focusing on temperature effects. Unfortunately I find the manuscript confusing on several points and their main conclusions flawed.

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Discussion paper

We appreciate the reviewer's valuable comments. We carefully address to the points made by the reviewer and try to clarify them in the below.

This manuscript uses global data products to examine the temperature and moisture of soil heterotrophic(?) respiration. Given that the authors did not provide their analysis scripts, and how they presented the variables used in this study, I'm forced to conclude that the soil respiration they used to drive this analysis was, itself, a model (Hashimoto et al., 2015). This makes this study a reanalysis of an existing land carbon model. While that could be interesting, since the parameterization of the Hashimoto Rs data product environmental sensitivity was global and this study proposes biome specific sensitivities of different forms, this does not support the main claims of the study to develop new parameterization. Instead it makes the case that a biome specific model can accurately describe a globally parameterized model.

Soil respiration (Rs) is one of the critical processes for maintaining a terrestrial ecosystem (L49-50), and also important in closing global carbon cycle (L83-85). The process also affects GPP indirectly through the soil decomposition flux of carbon at the root zone (L174-175) that affect plant assimilation. The conventional ESMs count on the sensitivity of Rs to the soil temperature using a constant value of Q10, but it is globally uniform, regardless of plant function types (PFTs).

The novelty of this study is to develop a new parameterization method for Q10, instead of using a fixed value in the conventional ESMs. There may exist different concepts for "parameterization". To be faithful to the concept of "parameterization" in the earth system modeling, the new parameterization in this study determines the Q10 value dynamically depending on abiotic conditions (e.g., subsurface soil temperature and moisture) as well as depending on PFTs. This approach seems to be feasible particularly for the use of climate change experiments, in which the temperature sensitivity may not be static in a warmer climate. It is also arguable to use a constant Q10 value for a specific biome, having said that there may exist a substantial variation of subsurface temperature and soil moisture even within a same type of PFT, either zonally or meridionally. This is often the case of most ESMs where the biomes are represented by several dominant PFTs in a global domain due to a coarse spatial resolution. These were the main motivations for the new parameterization of Q10.

One may use either theoretical or empirical approach to derive the relationship between Rs and the abiotic conditions. The parameterization of Q10 in this study is based on the empirical relationship between Rs and subsurface temperature and moisture for the given PFT. In doing this, the crucial part is the quality of the reference data and the degree of fitting (L220-221). Regarding the data, as the reviewer criticizes, the practical problem here is there is no real observation data for soil respiration and subsurface temperature and moisture. Although in-situ data of soil respiration are available from the Soil Respiration Database (SRDB, Bond-Lamberty and Thomson, 2010), the data have limited sampling for boreal cold regions (i.e., tundra and northern Siberia) as well as unpopulated regions in the tropics, covering a significant portion of the global biosphere (L159-162). Same problems lie in the subsurface data for temperature and moisture, as there is no comprehensive observation data covering the globe. Recent satellite instruments using microwave channels can retrieve subsurface soil moisture, but this is all limited in spatial and temporal sampling.

As a reference for observations, this study used the re-analysis soil respiration data from Hashimoto et al. (2015). Although the data were derived using an empirical soil respiration model based on Raich et al. (L162-169), they are not entirely modeled data but using the SRDB observation data. This study also conducted an “independent” reanalysis for the subsurface soil temperature and moisture by integrating the land surface model driven by observational forcing for a sufficiently long period (e.g., 1983-2010), which was to better represent the subsurface climatology at the presence of strong interannual variability.

We admit our approach used another modeled data, but the use of reanalysis is the best alternative choice when the exact in-situ data are not available such as in our case. The current parameterization method can be further improved by calibrating the empirical relationship between Rs and soil moisture and temperature, once the exact data from observations are available. The new parameterization for Q10 also demonstrates a good degree of fitting. The biome specific sensitivity for each 17 plant functional types (PFTs) presented in Fig. 1 shows a good skill of Q10 parameterization for the simulation of Rs, even though the soil respiration data of Hashimoto et al. and the off-line land surface model data from this study were produced independently.

For clarification, we attach the Excel file showing data and the regression results between Rs and soil temperature and moisture for each 17 PFTs.

Other points: The authors need to clarify how their GPP analysis ties into their main points about soil respiration (which is unclear whether they are referring to root + heterotrophic respiration or solely heterotrophic respiration).

The soil respiration in this study refers to the sum of root and heterotrophic respiration. The Q10 parameterization in the model changes the decomposition rate of carbon by soil organic matter in the soil layers. Based on the Equations (1) and (2), a higher Q10 value tends to increase carbon decomposition (and nitrogen) into soil layers, which tends to enhance the nitrogen assimilation to plants.

The experiment with the variable Q10 parameterization tends to increase GPP in the northern hemisphere high latitudes (Fig. 7 & 8) where Q10 increases (Fig. 2), and decrease GPP in the tropics and midlatitudes where Q10 decreases. This suggests the change in soil respiration affects GPP through the plant assimilation process.

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). Shorter turnover time in high latitudes suggests the enhancement of nitrogen assimilation to vegetation in EXP, thereby enhancing net primary production by plants.

There is considerable controversy in the field over whether Q10 is a global parameter (Karhu et al., 2014; Mahecha et al., 2010), spatially heterogeneous (as cited by the authors) or chemically heterogeneous . The authors need to review this in the introduction, another good reference for the introduction may be (Davidson et al., 2006; Davidson and Janssens, 2006). While I have no problem with a study to examine the implications of a spatially explicit Q10 sensitivity, to frame this as a broad community consensus is incorrect.

We totally agree with the reviewer, and we will reflect the reviewer's comment in the revised manuscript as below:

(From L102∼) "Whether this value is a global constant or variable in space is still under debate and the conclusions from the previous studies are diverse, which reflect our limited understanding to the soil respiration process. For example, Mahech et al. (2010) suggested that the Q10 value is independent of mean annual temperature and biomes. Karhu et al. (2014) also mentioned that the Q10 is approximately a global constant about 1.4 in the high latitude regions in the northern hemisphere. Another studies, on the other hand, suggested that Q10 may vary in space (Zhou et al., 2009; Xu and Qi, 2001; Qi et al., 2002)."

The authors lost me on Eq 4 (though the subsequent Eq 4 to 8 progression is well presented). What is q in Eq 4 and how does it relate to the traditional presentation of: Rs = k*C*f(T)*g(M)? This is critical to the study and needs to be painfully clear. How is the current approach different from fitting log(Rs) = log(k)+log(C) + log(f(T)) + log(g(M)) which is what I expected when I hear a linear regression estimate of temperature and moisture sensitivities. While linear regressions are common in the field I'm not clear on what exactly was being regressed where.

"q" in Eq. (4) represents the "fractional" change of Rs due to temperature, which can be decomposed further into the sensitivity to the soil moisture and temperature as in Eq.

(8). The reason why we used the fractional change is in that Rs is assumed to be an exponential function of temperature and moisture. This enables one to take logarithm to Rs and expand it to the sum of individual sensitivities, as in the one that the reviewer commented.

Using Eq. (8), we constructed the multiple regression equation for Rs with respect to the changes in T and M. The sensitivity of Rs to temperature has been known to be exponential in the previous studies. The traditional equation of soil respiration is defined by van't Hoff (1898) :

Resp=αeˆβT, (a)

where α, β are fitted parameters for regression. This Van't Hoff equation is modified by Davidson et al. (2006) as:

Resp=ãĂŰR_base Q_10ãĂŮˆ(((T-T_base)/10)), (b)

where T and Tbase are measured temperatures. The subscripts "base" indicates the base state at the specific time. Q10 is defined as the factor of respiration variation by 10 kelvin degree temperature increasing.

Code would help in addition to more details on the exact form of the regression in the methods section. Please make the code available for this study. While it is not appropriate to reproduce the already available public datasets, it is best practice to make the analysis scripts and software available to increase reproducibility. This will also address the question of the exact structure of the regression model used in this study.

We provided the excel file which was used to obtain the regression results between soil respiration and temperature and moisture in each plant functional types (PFTs). Please check attached excel files.

Line by line comments:

Conventionally Q10 is written Q_{10} (with subscripts) the authors may wish to consider reformatting to match convention.

We will modify as Q10 (subscripts) in numerous places in the manuscript. .

Abstract: Is this a paper about Q10, Rs, or GPP? It's ok to consider all of them but that's not how the paper is initially sold in the title and beginning of the abstract. Right now it reads as three separate ideas and very choppy. Consider integrating the abstract by linking the two in the early sentences and then going onto the detailed results for each and then linking them up again in the conclusion.

We will revise the abstract carefully following the reviewer's comment. The first two sentences will be modified as:

(L49-53) "Soil decomposition is one of the critical processes in maintaining terrestrial ecosystem and global carbon cycle. Soil respiration (Rs) sensitivity to temperature so called the Q10 value required for parameterizing soil decomposition process is assumed to be a constant in conventional numerical models, while it is not so in the realistic case with spatiotemporal heterogeneity." Also we revise the introduction part the linkage between Rs and GPP in the manuscript: (L130) "Realistic spatial distribution of soil decomposition processes affect not only Rs but also primary production by improving nitrogen assimilation from soil to vegetation."

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

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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).

P3 L83-85 Most ESMs are decoupled, driven by CO2 concentrations instead of a full feedback carbon cycle. Thus the variation in traditional climate parameters (surface temperature and precipitation) is not due to carbon cycle representations as is implied in these lines. Variations in emissions targets are backed out post-hoc generally via carbon budgeting from the associated carbon cycle and CO2 concentration scenario. Thus it's the emissions targets that tend to reflect the land carbon cycle uncertainty not the overall climate. Please make this clear in the paragraph or specify that you are restricting your discussion to emissions driven ESMs (which will give you a slightly different set of references you need to cite).

Following the reviewer's comment, we will modify the sentences as: (L83-84) : "Future climate change projection by various ESMs driven by identical anthropogenic emissions is diverse and highly uncertain in the prediction of atmospheric CO2 concentration (Friedlingstein et al., 2006, 2014; Hoffman et al., 2013). Many previous studies (Friedlingstein et al., 2006; Hoffman et al., 2013; Anav et al., 2013; Aroa et al., 2013; Friedlingstein et al., 2014) suggested that the uncertainty of CO2 concentrations simulated by the emission-driven ESMs should be attributed to the carbon cycle over land rather than over ocean. In particular, one of the main. . .."

P3 L90-92 Make it clear you are talking about direct field characterization of global budgets for soil heterotrophic as opposed to indirect carbon budgeting estimates. Right now it reads like no one has ever looked at measuring soil respiration at all which is completely false as the authors go into detail later on.

We agree and modify the sentence as: (L90-92) "However, the amplitude of soil decomposition process has not been quantified through direct field measurements in the global domain, and highly uncertain, mostly due to the lack of observation data and poor estimates of it indirectly from soil temperature (Sussela et al., 2012)."

P5 L119-121 You need a citation to back up this statement. I suggest (Todd-Brown et al., 2013) for a review of CMIP5 soil carbon models or directly citing the CMIP5 ESM manuscripts themselves.

As your comments, we include the relevant papers in the sentence: (L119-121) "However, most advanced ESMs partcipated in Coupled Model Intercomparison Project Phase 5 (CMIP5) still use a globally-constant Q10 value in the dynamic global vegetation models (Anav et al. 2013; Todd-Brown et al. 2013)."

P6 L157 Please make it clear if you are using the global soil map or underlying data set from (Hashimoto et al., 2015). Given the reference to regridding I'm assuming this is the soil map product (if this is not the case please clarify and disregard the following comments). This is a fatal flaw in this study. While the model used to generate this data product is not explicitly a Q10 model it is also clearly not in situ observations which makes this study a reanalysis of an existing model not a new interpretation of observations as the authors have framed this manuscript.

No, this study did not use the "global soil map" data but the gridded global map of soil respiration data from Hashimoto et al. (2015).

As this comment is same as in the above, please check our responses there (the first response in the major points).

P11 Sect 3.1 Why are we looking at GPP here? (Anav et al., 2013) Already looked at GPP in the context of FLUXNET, how is this different? This section still seems disconnected from the rest of the results as was mentioned in above comments on the abstract.

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."

P12 L293-296 This seems to belong in the GPP section. Unless you are also applying the Q10 sensitivity analysis to the GPP product in which case you need to be clearer how that ties into the methods section Eq 4-8 soil referred to Rs.

(L293-296) These sentences are redundant to the previous sections and will be removed in the revised manuscript.

P12 Please avoid the use of acronyms where possible. CTL and EXT break the flow of the manuscript. We use these acronyms for brevity. We will carefully revise the manuscript and improve flowing.

P20 L490 Malformated citation (bad first author name) We correct it as below:(L490).

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Please also note the supplement to this comment:
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supplement.zip

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

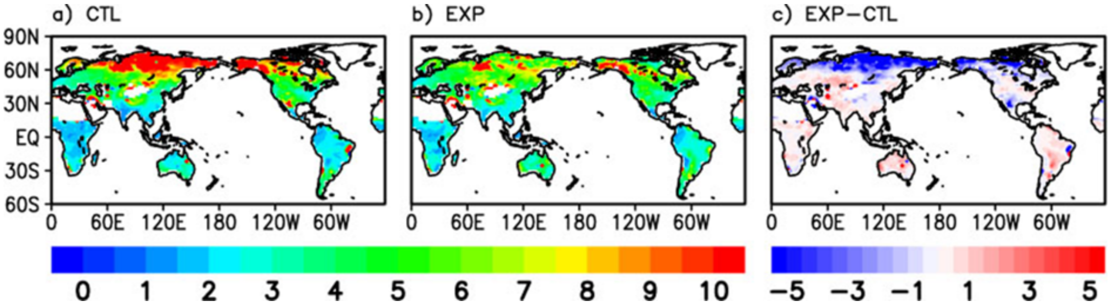

Fig. 1. Figure S1. Spatial distribution of turnover time (year) of soil carbon in (a) CTL and (b) EXP. (c) indicates the difference between EXP and CTL simulation. The turnover time is defined as the ratio o