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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.

Dongmin Kim et al.

dmkim@unist.ac.kr

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I fear I cannot really write a more positive review for this very manuscript. I have to say that I am quite confused by this paper. In particular, I have two very fundamental concerns:

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

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1. The authors write in the abstract "... (non-uniform spatial distribution of Q10) ... improves the simulation of gross primary production (GPP). It leads to a more realistic spatial distribution of GPP, particularly over high latitudes ... ". This statement suggests that GPP= f(Q10;soil; ...) which makes no sense at all to me. Or do I fundamentally misunderstand the model assumptions here? To me this sentence suggests that either I don't understand the basic dynamics under scrutiny, or that the authors have been very sloppy in putting the manuscript together, or that there is indeed a very fundamental conceptual issue here. I fear that we are talking about the latter.

⇒ 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 CLM4 model tested in this study uses the carbon-nitrogen (C-N) coupling in the parameterization of terrestrial carbon fluxes. Unlike the other ESMs, the interactive C-N cycle implemented in CLM4 acts as a limiting factor for photosynthesis and gross primary production (GPP) in this model.

⇒ The modification of Q10 in CLM4 tends to modify the carbon decomposition flux in the soil layers. The non-uniform spatial distribution of Q10 directly changes the decomposition flux in the soil (Eq. (1) & (2) in the manuscript) and heterotrophic respiration by root and soil organic matters. The parameterized process is such that a higher Q10 produces faster carbon decomposition. This increased carbon decomposition tends to increase nitrogen flux into the soil from debris, and hence increase the nitrogen assimilation from soil to vegetation. These processes tend to improve the simulation of GPP distribution by modified Q10 parameterization of CLM4.

2. The authors write that "the Q10 value derived from soil respiration measurement tends to decrease with temperature because substrate availability decreases as temperature increases". To my mind this is rather reflecting that any regression model that considers abiotic drivers only, would be confounded by co-variations with e.g. substrate

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supply or other biotic drivers. This has been shown e.g. in Reichstein & Beer (2008), Mahecha et al. (2010), Wang et al. (2010), Graf et al. (2011), and we could cite more recent papers. Inferring from varying abiotic controls that Q10 should also vary across geographic locations is misleading. Non-constant parameters in biosphere models may actually reflect missing process detail: For instance, if one tries to subsume in Q10 variation of e.g. substrate supply then one is simply missing a good representation of the supply term. Various papers by e.g. Davidson (e.g. 2012 and more recent ones) explain this in a very didactic manner and should be studied before discussing this aspect further and tweaking models without actually, developing the underlying model structures further.

⇒ First, whether Q10 is a global constant or variable across the abiotic conditions as well as the type of vegetation is controversial. We admit it and will insert this issue 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, Mahecha 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)."

⇒ Secondly, we admit other biotic and abiotic factors may complicate the process and result in spatial variability instead of Q10, as the reviewer criticizes. But, from the parameterization point of view, the concept of Q10 is implemented differently across the state-of-the-art global prediction models, rather indirectly or implicitly in representing the dependence on biotic and abiotic conditions.

⇒ Like the formula in Eq. (2) & (6), the Q10 value in the CLM4 land surface model used in this study has a much simpler form and inclusive in counting on abiotic conditions such as soil temperature and moisture. These dependences can be also plant func-

tional type (PFT) dependent. While the original version of CLM4 uses a constant value of 1.4, this study attempts to use variable Q10 values depending on PFT and abiotic conditions. Due to its much simpler form of parameterizations, those dependences are not to be represented in other means.

⇒ To be a little more detail, the soil decomposition of the plant detritus in CLM4 depends on the climate conditions as well as on substrate nutrients such as carbon and nitrogen ratios and their amounts. CLM4 considers biotic drivers in the carbon decomposition processes indirectly. These soil carbon dynamics are based on the kinetic theory of biological reaction which are related with soil temperature and moisture. As the reviewer commented, Davidson and Janssens (2006) and other studies used more comprehensive formulations with more biotic and abiotic drivers such as soil aggregation and chemical protection of soil organic matter in addition to temperature and moisture. Biotic drivers are not considered at all or too much simplified in the existing ESMs like CLM4.

⇒ 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. To my mind, the authors first need to clarify these two aspects in a very convincing manner before discussing the “minor” issues of the manuscript. However, these other aspects that are in fact not so minor. For instance when the authors write that “the Q10 parametrization tends to enhance the relationship between Rs and soil temperature from CTL” they refer to Fig. 6 which show differences in respiration modelled with different runs and Tsoil. But the scatters are all over the place (positive/negative) and it is unclear about what relationship we are talking here. And I

find more examples of this kind in the text . . .

⇒ Figure 6 shows the scatter plots for Rs change (EXP minus CTL in y-axis), as a function of soil temperature (x-axis). Presumably due to the sub-biome variability in biotic and abiotic conditions, the scatter plots exhibit some nonlinearity in the curvature, but most of the values lie in the positive range for the moderate to warm temperatures (i.e., increased Rs at the given temperature by the variable Q10 formulatoin). Note that this relationship is not uniform in space, as the change of Q10 is not uniform in EXP (as shown in Fig. 2).

So in the overall view, I would like to encourage the authors to carefully rethink what the focus of this study can be and what can be really learned with this experiments.

⇒ We appreciated your crucial comments for overcoming scientific deficiencies in the manuscripts.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-549/bg-2016-549-AC1-supplement.zip

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