

# ***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.***

## **Anonymous Referee #1**

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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.

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

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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.

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).

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.

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:  $R_s = k \cdot C \cdot f(T) \cdot g(M)$ ? This is critical to the study and needs to be painfully clear. How is the current approach different from fitting  $\log(R_s) = \log(k) + \log(C) + \log(f(T)) + \log(g(M))$  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. Code would help in addition to more details on the exact form of the regression in the methods section.

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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.

Line by line comments:

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

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.

P3 L83-85 Most ESMs are decoupled, driven by CO<sub>2</sub> 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 CO<sub>2</sub> 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).

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.

P5 L119-121 You need a citation to back up this statement. I suggest (Todd-Brown et

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al., 2013) for a review of CMIP5 soil carbon models or directly citing the CMIP5 ESM manuscripts themselves.

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.

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.

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.

P12 Please avoid the use of acronyms where possible. CTL and EXT break the flow of the manuscript.

P20 L490 Malformatted citation (bad first author name)

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