

## Reviewer 1

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*This study analyzed heterotrophic respiration (RH) from Earth System Models (ESM) using “pattern scaling”, and sought the causes of variations in ESMs’ RH output. The terrestrial carbon cycle from ESMs should be tested and constrained by multiple data or multiple carbon flows and stocks; therefore, I agree to the importance of RH in the terrestrial carbon cycle and its simulation. In addition, the application of pattern scaling to RH is new, as far as I know.*

*Overall, what the authors did is simple: they analyzed RH outputs from ESM using pattern scaling, and discuss the similarity/differences among ESMs. However, even after several readings of this manuscript, I could not follow the details of this manuscript, and could not capture “the causes of uncertainty in observed and projected RH”. Rather, the present manuscript appear to simply demonstrate the uncertainty/variations in RH from ESMs. I hope Results and Discussion should be reorganized. In particular, Discussion is too long (please shorten Discussion and more focus on what you really want to say), and the figures are not of publishable quality.*

Thanks for the careful reading and useful feedback. We agree that this manuscript needs significant revisions in many areas, but are hopeful that doing so will greatly improve its clarity, methodological rigor, and ultimately impact.

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### *General comments*

*Pattern scaling is not widely known outside of climate research field. The authors have to describe the methodology, advantage/disadvantage, and what the results mean more.*

This is a good point. In our revision we will provide more background on pattern scaling, as well as the state of CMIP5 models’ carbon cycle performance more generally (Anav et al., 2013; Luo et al., 2016), and using aspects of model behavior to draw inferences about climate- and carbon-cycle response to anthropogenic forcing (e.g. Gillett et al., 2013). In addition, we think that a better discussion of how RH pattern scaling can be treated as a type of emergent constraint (Hoffman et al., 2014; Luo et al., 2015) would be useful.

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*Quality of the Figures. The figures are not clear and are not easy to follow. One reason is that the color is the only identifier of each model. The color, however, is not clear. The authors often pointed a specific model name, but for me very difficult to follow. In addition, suddenly, the model number was used.*

*Figure 5, 7, 8, 9. I am not sure if these figures are necessary. These may be moved to supplement, and/or you can make tables to show the results more clearly.*

We agree. The existing figures will be clarified and streamlined for clarity and to focus on the main points of the manuscript.

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*One serious concern, which I hope is simply my misunderstanding, is that the calculated global RH values seems higher than those I calculated earlier. Did you multiply both areacella (cell area) and sftlf (land surface fraction)? Please check it.*

We will absolutely do so.

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*There are several studies about RH in CMIP5 (for example, by Exbrayat et al.), maybe relating your study with those previous studies would be important.*

Yes, we agree—thank you. The manuscript clearly needs to do a better job of citing and discussing previous work such as Exbrayat et al. (2013a, 2013b), as well as more recent work (Guenet et al., 2018; Luo et al., 2016).

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*Specific comments:*

*Page 4, line 42: “even decrease” Do you have any idea why?*

This is an interesting question that we will discuss, potentially exploring links to changes in climate variability more broadly (Rehfeld et al., 2018).

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*Page 5, line 22-28 Can you be more quantitative?*

Yes, we will rework this paragraph to be both clearer and more quantitative.

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*Page 5, line 28-31 I cannot understand this attribution. Could you elaborate this more carefully?*

Our apologies. The revised manuscript will more clearly explain the potential links between inconsistent model performance in the ‘global South’ and lack of observations in those regions. We will also note that this problem has been addressed in other contexts, e.g. upscaling of FLUXNET data (Jung et al., 2011).

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*Page 8, line 3-21*

*Please compare your results with those by Todd-Brown et al. 2013 more carefully. For example, the MIROC model did not overestimate NPP, why can you say HadGEM2 and IPSL models*

*performed well in capturing observed RH characteristics based on Table 3 in ToddBrown et al. 2013?*

We will carefully review any discrepancies in this section.

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*Page 9, line 1: “the observed relationship between RH and NPP is not strong” Where did this come from?*

This referred to Figure 4c and Supplementary Figure 2c, but we agree that it wasn't clear. We will improve the context surrounding this paragraph.

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*Page 9, line 33  
Extra parenthesis.*

This will be fixed.

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*Figure 2:*

*The colors are indistinguishable. The x axis should be improved (190000?) Figure 3:*

*The number means what (probably model number)?*

*Figure 4:*

*The colors are indistinguishable. The legend should be placed as well. Figure 5, 7, 8, 9*

*Why don't you put each model name on each panel?*

As noted above, we will rework and streamline the figures. We apologize for their current lack of clarity.

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