

## Reviewer 4

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### *General Comments*

*This manuscript used a global gridded heterotrophic respiration (RH) obtained from assimilating observed soil respiration into a statistical model to benchmark 25 CMIP5 Earth System Models (ESMs) in simulating RH, globally. The overall RH trends simulated by CMIP5 models are displayed, and possible reasons for the discrepancies between “observations” and modeling results are discussed. The topic of the manuscript is timely, as the RH simulation is not well represented in ESMs. However, there are several parts that are hard to understand. I recommend the authors to clarify the issues included in my comments.*

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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### *Major comments*

*1. P3, L20: The authors used “all available climate models”, and then generated “an ensemble of 25 ESMs”. Then, they mentioned “the first ensemble member from each model”. How many ensemble member of each model? If each model (e.g., CESM1- BGC) has several ensemble members, why the authors did not use the mean of the ensemble members? I think this part needs to be clarified.*

This is a good question, and was also raised by Reviewer 3. Preliminary analyses suggested that there was little ensemble-to-ensemble variation for these variables in CMIP5, and we can include this as supplementary information; if the reviewers feel strongly about this point, however, we are happy to re-do the analysis using the mean of all ensemble members.

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*2. I did not totally understand the “pattern scaling” method even though the equation is shown. Why particularly this method is used in the manuscript? What is the advantage of this method?*

Apologies for the omission. 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).

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*In addition, Table 3 listed single numbers of, for example, RHL-RHG, but Figure 6 displayed the meridional variations of the similar relationships. Was Tables 3 the global mean of Figure 6? If so, what is the physical meaning of calculating the global mean of the RHL-RHG relationship? A*

*better explanation of this method and the related results are needed.*

We agree that this is quite unclear. We think it will be simplest to delete this table entirely, since as you say the physical meaning of such a global average is obscure.

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*3. P5, L16, can the authors show the proofs, saying papers, discussing “the dominant control on RH is temperature in these cold biomes”?*

This sentence, as written, is too definitive, although temperature is certainly an important control (McGuire et al., 2009). We will change its wording, citing relevant literature on this topic (Bond-Lamberty and Thomson, 2010; Davidson and Janssens, 2006; Hursh et al., 2017; Mahecha et al., 2010).

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*4. P5, L40, I suggest the authors to give examples of the models either overestimated or underestimated the  $r$  values.*

We will be more specific here.

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*5. P9, L26, what is the theoretical basis of the “RH-NPP relationship” in different ecosystems?*

We will clarify this. The sentence is intended to point out that at steady state one would expect the magnitude of these two terms to be comparable, whether at a global (Le Quéré et al., 2017) or ecosystem scale, the latter from fundamental ecological principles (Odum, 1969).

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*6. P9, L37, can the authors specify the climate factors here? In other words, besides temperature, what are the factors regulating carbon decomposition rates in a soil water limited environment? In P5, L16, the authors mentioned that “the dominant control on RH is temperature in these cold biomes”. Looking at these two sentences together, does it mean that 1) in reality temperature is the main factor controlling RH in cold regions; 2) the sensitivity of soil carbon decomposition, closely related to RH, to temperature in cold regions is limited by soil moisture in ESMS? If so, can the authors explain the reasons for the difference between reality and models?*

As the reviewer notes, this is closely tied to the comment above about dominant controls on RH at different scales and in different biomes. We will explain this more clearly, pointing out in particular putative reasons for models' divergence from observations, and referencing prior work in this area (e.g. Luo et al., 2016; Shao et al., 2013; Todd-Brown et al., 2013).

*Minor comments*

*1. RH is used as the acronym of “heterotrophic respiration”. In my view, it should be HR, and it is easily to think RH as “relative humidity”, especially for a paper related to different climate factors. It is fine if most of the papers define “heterotrophic respiration” as RH. Otherwise, please correct it.*

Papers vary greatly in using either HR (more intuitive for the reader) or RH (more consistent with RA for autotrophic respiration, Reco for total ecosystem respiration, etc). We will look carefully at this and at recent papers in the field, and adopt whichever seems clearest. We agree that the overlap with relative humidity is unfortunate.

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*2. P5, L14, TAS was named before, and does not need to be re-named. In addition, surface air temperature and surface temperature (not TAS) are two temperature definitions. The authors need to give a clear description here.*

*3. It is not necessary using PR as the acronym name of precipitation. Also, the authors used PR and precipitation randomly. If an acronym name is defined, it can be used afterward.*

*4. P5, L21, should it be “few” or “A few”? The authors used a colon here, and it looks to me that CCSM/CESM, to some extend, can capture the patterns.*

Good points—thank you. These will be fixed.

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