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Interactive comment on "Causes of uncertainty in observed and projected heterotrophic respiration from Earth System Models" *by* Cary Lynch et al.

Anonymous Referee #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 CMPI5 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.

Recommendation

Printer-friendly version

Discussion paper

Major revision.

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.

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? 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.

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

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

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

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 tem-

perature 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?

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.

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.

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Discussion paper