

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

Anonymous Referee #1

Received and published: 9 November 2017

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

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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.

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.

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.

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 stflf (land surface fraction)? Please check it.

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.

Specific comments:

Page 4, line 42: “even decrease”

Do you have any idea why?

Page 5, line 22-28

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Can you be more quantitative?

Page 5, line 28-31

I cannot understand this attribution. Could you elaborate this more carefully?

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 Todd- Brown et al. 2013?

Page 9, line 1: "the observed relationship between RH and NPP is not strong"

Where did this come from?

Page 9, line 33

Extra parenthesis.

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?

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