Exploring precipitation pattern scaling methodologies and robustness among CMIP5 models
Kravitz et al., Geoscientific Model Development
Response to reviewers

Reviewer comments in plain text. **Responses in bold.**

General response

We thank the reviewers for their comments on our paper. Both reviewers were critical of the "physically-based" method, and we have carefully considered their points. We agree that it is important to evaluate this method more carefully, including additional checks of the accuracy of our implementation. Exploring this method would also require a discussion of its usefulness as a pattern scaling method and why we obtained the results that we did. Given the large increase in scope this would require, which would distract from our assessments of the performance of the other two methods, we have elected to remove mention of this method from the present manuscript. We will do a better job with it in a future study.

Reviewer #2

The submitted manuscript compares several methods for the pattern scaling of precipitation across time periods and scenarios. They compare a regression based approach, an epoch difference and a 'physically' approach. I cannot recommend this paper for publication because of two significant errors in the methodology, combined with a manuscript which is too long, without a clear structure.

We have substantially shortened the paper and provided outlining and clearer desciptions as to our main findings.

Firstly, the 'physically-based' approach, which is based on the work of Lau (2013), is very likely incorrectly applied. In Figure 4, which is basically a test of whether the methods are able to reconstruct an in-sample pattern of precipitation using the same ensemble and time period as a test response pattern as was used to produce the pattern itself. In this case, the method produces errors an order of magnitude greater than the other approaches - which suggests that there is an error in application. If there is no error, this huge discrepancy requires an explanation.

However, even taking this into account, there is little logic that this approach is 'physically-based' at all. The precipitation rates are binned by different monthly rain rates, averaged over the ensemble and recombined into a single pattern. If a single pattern is being scaled - the ability to treat differently rain rates in different regimes has already been lost. The entire concept is not clearly defensible.

After careful consideration (see general response above), we have removed the physically-based method from this manuscript.

The separation of response patterns into CO_2 and non-CO_2 components could potentially be useful, but the implementation is flawed. The authors assume in Figure 14 that the non-CO_2 response pattern is given by the difference between the RCP8.5 and 1pctCO_2 patterns. This is not correct.

Assume there is a 'pure CO_2' precipitation response which can be measured from the 1pctCO_2 simulation:

$B_{CO2} = \Delta P_{1pctCO2} / \Delta T_{1pctCO2}$ If we assume things are linear, the precipitation response in RCP8.5 is this pure CO_2 response, multiplied by the pure CO_2 warming, plus a non-CO_2 response:

$\Delta P_{RCP\,85} = \Delta T_{RCP\,85,CO2} B_{CO2} + \Delta T_{RCP\,85,nonCO2} B_{nonCO2}$ so - by solving this, we get the B_{nonCO2} pattern and could reconstruct the ΔP_{RCP85} exactly.

We thank the reviewer for this comment. We agree that we were not as careful as we should have been in the previous iteration of this manuscript. We have added Supplemental Section 1, which goes through this derivation and arrives at a more accurate formulation for the non-CO_2 pattern.

However, it's still not clear that CO_2/nonCO_2 is the correct way to break this problem down. The nonCO_2 component is a broadly mix of aerosols, and other greenhouse gases (CH_4, N_2O etc). These two groups can have opposite effects on global mean temperature - potentially making $\Delta T_{RCP85,nonCO2}$ near zero and making the above equation ill-posed.

Furthermore, CH_4 and aerosols have very different precipitation response fingerprints. RCP8.5 and RCP2.6 have very similar aerosol forcings, but very different CH_4 trajectories, so the nonCO_2 pattern appropriate for RCP8.5 would be very different than that for RCP2.6.

A far more logical decomposition would be between GHG and nonGHG forcing. The authors could solve this by treating the 1pctCO_2 response as the GHG response pattern, and then in RCP8.5 calculating the effective CO_2 concentration using the emission factors for each of the non CO_2 gases, and then computing the $\Delta T_{RCP85,GHG}$ as before using effCO_2 rather than CO_2 itself.

We acknowledge the reviewer's excellent point. We have opted to keep the division into CO_2 and non-CO_2 because dividing into GHG and nonGHG components results in nonlinearities that violate the conditions of pattern

scaling. The new Supplemental Section 1 provides more details as to why we made this choice. We have also added a new paragraph of text in Section 4 that describes the above issues that the reviewer raises.

The general formulation of the rest of the paper, and the treatment of the other two pattern scaling approaches, is broadly correct - but the presentation is often frustratingly vague. It is often not made clear what is in sample, and what is being tested. In Figure 8, are the same models being used to make the patterns and the test the errors? In Figure 11, is it 1pctCO2 or RCP85 being reconstructed?
The authors should correct the major errors above and restructure the paper to ensure concise and clear communication before resubmission.

We acknowledge both of the items the reviewer points out. We have clarified our description of Figure 8, also in line with a comment from Reviewer #1. For Figure 11, we have clarified what we are doing in the text.