Review of "Better constrained climate sensitivity when accounting for dataset dependency on pattern effect estimates" by Modak and Mauritsen.

Response to referee 1

Note- Our responses to the reviewer's comments are in red color text.

The authors have addressed my concerns and I support publication. The new version includes the needed details and justifications for the selected methods, and the results progress our understanding of the pattern effect and its relationship to ECS.

Minor language comments are at the end.

SUMMARY OF RESPONSE In my first review, the comment on Seager et al. was phrased poorly, but the new Sec. 4.1 text fully addresses it.

Reviewer 2's suggestion of regressing local T versus global T is a better approach than the nonlinear suggestion I made. I'm glad the authors adopted it for Figs 3 and 5.

I want to thank the authors in particular for the new appendix figures. They are all useful for judging the analysis assumptions (A1,A4) or making it easy to compare with other research (A2).

Thanks. In the reviesed manuscript, we have made all the suggested changes.

GRAMMAR/TEXT CHANGES Sec. 5 could repeat that the post-1970s results are more negative in the mean, they don't just show smaller spread. Part of the community cares more about the modern period and a reminder could be helpful for them.

Thanks. The net feedback is more negative particularly for AMIPII after 1970s and not much with others. In the revised manuscript in section 5, we write "While the estimates from the 1970s until present are less dataset dependent, the major disparities originates in the early period and is driven by cloud feedback".

Minor grammar should be caught by a final check and copy editing but pure grammar corrections could sometimes change the meaning, such as by picking singular when I believe you intend plural. Please check: L48—50: "Whereas a colder tropical eastern Pacific and Southern Ocean, and a stronger tropical western Pacific warming is observed over the historical period, Atmosphere-Ocean General Circulation Model (AOGCM) simulates a long term climate response" –> "Atmosphere-Ocean General Circulation Models (AOGCMs) simulate. . . "?

Done.

L87: "The seven different observed-reconstructed SST datasets applied as boundary condition are HadISST1" –> “boundary conditions” plural?

Done.

L192 “To further investigate if any of the dataset bias the correlation,” –> “any of the datasets” plural?

Done.

L202: “We find that the feedback from. . . ” –> I think you mean plural “feedbacks” since Fig. 7 shows the different components?

Done.

Some bits are clunky and could perhaps be rephrased:

L106—108: “The temperature anomaly in ERSSTv5 forced observedSST-piForcing simulation is lower compared to others, consistent with Fueglistaler and Silvers (2021), is related to ship SST bias corrections made to the temperature during the 1880-1940s and 1950-1960s (Huang et al., 2017)” –> Consider splitting into two sentences or “,.. this is related to ship SST. . . ”

Done.

L123—124: “we conduct a simulation in fixed-SST configuration, but evolving historical forcings to evaluate the effective radiative forcing (F) from 1851-2014.” –> Consider after comma: “but with evolving historical forcings”

Done.

L178—179: “For instance, over the IPWP region, ERSSTv5 dataset shows this region is warming at the same rate as the globe but is true over the EEP region as well however with larger uncertainty.” –> I think this sentence intends to say “this is also true over the EEP region”? Please check, and also convert to "the ERSSTv5 dataset" (added "the").

We thank the reviewer for asking us to check this figure. We find that there is an oversight. The figure in the manuscript shows the local warming to the warming over 50°S-50°N and not global warming which is what we wanted to show but wrongly stated in the caption and in the text. Now in the revised manuscript, we have corrected them. Now the caption replaces "global" to "50°S-50°N".
We now rewrite the text "We find that the local warming to the warming over 50°S-50°N from 1871-2017 in each of these regions have significant differences in some cases among the datasets (Figure 5). For instance, over the IPWP region, the ERSSTv5 dataset shows significant difference compared to the HadISST, COBE-SST2, had4krig, hadsst4krig or Vaccaro2021."

L231—233: “:Here, we apply their pattern effect based on AMIPII only as the number of models are larger which performed the amip-piForcing simulation.” –> Suggest rephrasing the end bit to something like: “. . . based on AMIPII only as they had amip-piForcing simulations from more models.”

Done.

Review of "Better constrained climate sensitivity when accounting for dataset dependency on pattern effect estimates" by Modak and Mauritsen.

Response to referee 2

Note- Our responses to the reviewer's comments are in red color text.

The authors have done a good job addressing reviewer comments. I have some final suggestions for the authors to consider before publication:

Thanks Tim for the appreciation.
The point by point responses are provided below.

1) Figure 5 and 6 and related discussion: I find it interesting that no strong correlation is found between the different SST pattern regions and the pattern effect. However I think the three most convincing SST metrics for explaining pattern effect variations haven't being analysed, and these are based on relative differences rather that single regions. For example Zhou et al. (2016) use the difference in warming between tropical ascending and descending regions, Andrews and Webb (2018) use the difference in south east-Pacific minus west-Pacific SST pattern and Fueglistaler and Silvers (2021) use SST# defined as the warming of the warmest 30% SSTs relative to the tropical mean. I do not demand the authors include these, but I do think it would be a useful addition (at least one of them) as it might be the reason for the poor correlation.

Indeed across all the datasets, the correlation between pattern effect and the regional warming relative to the the 50°S-50°N are of similar magnitude (0.64 over IPWP, 0.31 over EWP, -0.43 over EEP and 0.69 over SO). But as discussed in the manuscript the correlation is very strong over IPWP when ERSSTv5 dataset is not considered while calculating the correlation. In contrast, when Vaccaro21 is not considered, the correlation is only 0.15 over IPWP. In the manuscript, we discuss for other datasets as well "To further investigate if any of the datasets bias the correlation...".

Thanks. We apply the difference in south east-Pacific minus west-Pacific (SEP - WP) SST pattern following Andrews and Webb (2018) and calculated the correlation between the pattern effect and the SEP - WP ΔT. We do not find a strong correlation across the datasets (Figure 1, shown below). We too think this is interesting and worth exploring in our planned multi-dataset and multi-model study.

In the revised manuscript we rewrite the final sentence of 2nd paragraph of section 4.3 "Thus, it is difficult to link the pattern effect variations across datasets only to the IPWP warming, rather we find all regions show a positive correlation with IPWP and SO showing a relatively stronger correlation than EWP and EEP (Figure 6)."

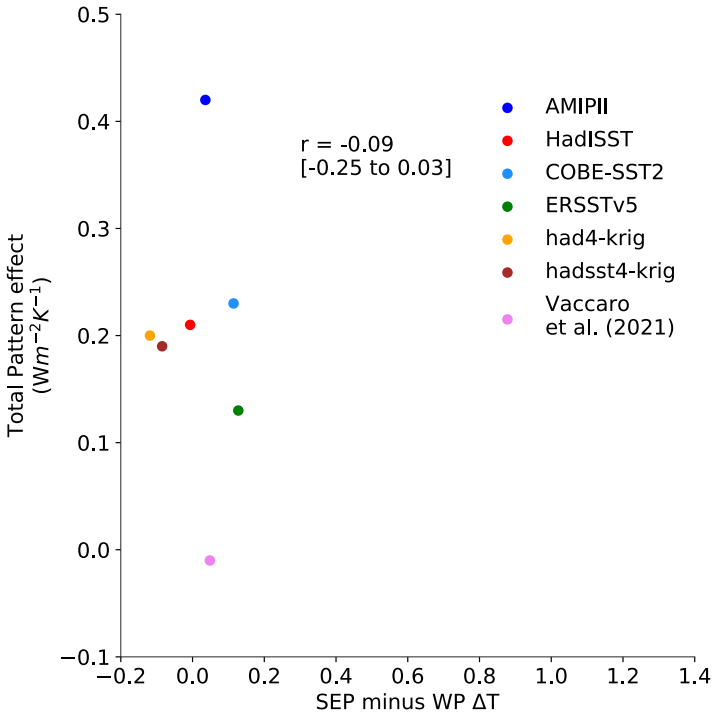

Figure 1

2) Line 154-155 and Fig A2: When comparing the total pattern effect from the CAM5.3 Green's function results (Lewis and Mauritsen, 2021) to the MPI-ESM simulations it is simply concluded that they are "substantially different", but I'm kind of left wondering "and so what"? What exactly is being said with this conclusion? At first I thought it implied an issue for the Green Functions method, but it's a different models Green's function and we know models simulate different pattern effects, so it is not entirely expected that the CAM5.3 green function results would give different estimates to the MPI simulations? So I guess I'm wondering what useful information is being drawn from this comparison? For example are the results substantially different in some interesting or unexpected way? Are the differences within the model-dependent uncertainty allowed for later on in the manuscript? If not, that is interesting and would suggest the uncertainty allowed for is not sufficient. I'm just throwing out ideas, but I think a couple more sentences are needed to explain how the authors interpret the "substantially different" result.

Thanks. In the revised manuscript we now add "We inferred the total pattern effect from Lewis and Mauritsen (2021) for the in-common SST datasets which they calculated based on CAM5.3 Green's function (Figure A2). We find that their estimates of total pattern effect are substantially different from our estimates for some of the SST datasets. The differences could be either because of the Green's function that is applied in Lewis and Mauritsen (2021) is derived from a different model (CAM5.3 Green's function applied to ECHAM6.3) and different models produce different pattern effects, or could be due to its inherent limitations (Zhou et al., 2017). However, we find that the uncertainty in the pattern effect estimates across the in-common SST datasets are of similar magnitudes between the studies. We plan to further address the comparison with the estimates derived from Green's function in a future study."

3) Figure 3: I got confused here. The pattern in dT(lat,lon) per dT ought to have a global-mean of unity, hence all the difference panels should have a global-mean of zero, right? But it doesn't

look that way. To eye some of them are strongly blue (negative). Is this just a visual thing and the global-mean really is zero? Or, is it a land effect, which is masked out here? Or something else?

In Figure 3, we are plotting the slope of the regression of temperature change evolution at each grid (lat,lon) against the global mean temperature change evolution. The global mean of the slope of regression need not be 1. So, the global mean of difference maps are non-zeros. We checked this. Thanks. If it was dt(lat,lon) divided by global mean dT, then global mean of this ratio has to be 1 and the mean of the difference maps has to be zero.

Minor comments:

4) Line 1 Abstract: I think this opening sentence requires a qualifier that it is referring to the best estimate ECS, since the range/uncertainty in ECS is still bigger than that deduced from other lines of evidence (e.g. see Sherwood et al., 2020).

Thanks. We now write "The best estimate of Equilibrium climate sensitivity (ECS) constrained based on the instrumental record of the..."

5) Line 245-247: "Figure A4 shows. . . ." – I did not follow this sentence. Please try again.

In the revised manuscript, we rewrite "While accounting for the weaker pattern effect the assumption is that ECHAM6.3/MPI-ESM1.2LR is different from all the other models in all datasets as in AMIPII. However, one can argue this assumption. We infer from Andrews et al. (2022) and check this. We show in Figure A4 that not only ECHAM6.3/MPI-ESM1.2LR but also other models though produce stronger pattern effect has a similar difference in pattern effect estimates based on AMIPII and HadISST datasets as in ECHAM6.3/MPI-ESM1.2LR."

6) Line 280: append ". . . to address this outstanding concern"?

Appended.

I have signed the review. Tim Andrews.

References

Andrews, T., Gregory, J. M., Dong, Y., Armour, K., Paynter, D., Lin, P., Modak, A., Mauritsen, T., Cole, J., Medeiros, B., and et al. (2022). On the effect of historical sst patterns on radiative feedback. *Earth and Space Science Open Archive*, page 48.

Andrews, T. and Webb, M. J. (2018). The Dependence of Global Cloud and Lapse Rate Feedbacks on the Spatial Structure of Tropical Pacific Warming. *Journal of Climate*, 31(2):641–654.

Lewis, N. and Mauritsen, T. (2021). Negligible unforced historical pattern effect on climate feedback strength found in HadISST-based AMIP simulations. *Journal of Climate*, 34(1):39–55.

Zhou, C., Zelinka, M. D., and Klein, S. A. (2017). Analyzing the dependence of global cloud feedback on the spatial pattern of sea surface temperature change with a Green's function approach. *Journal of Advances in Modeling Earth Systems*, 9(5):2174–2189.