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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The influence of internal variability on Earth's energy balance framework and implications for estimating climate sensitivity" *by* Andrew E. Dessler et al.

Anonymous Referee #2

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Summary: This paper shows that the traditional energy balance framework yields a poor representation of the Earth's energy budget when unforced variability is large, because the relationship between radiative response and global-mean surface temperature is weak. The authors then propose an improved energy balance relationship where the radiative response is assumed to scale with tropical-mean 500 hPa temperature. Using a large ensemble of historical experiments, the authors demonstrate that this relationship yields more accurate estimates of the magnitude of climate feedbacks under CO2 forcing.

Recommendation: Minor revision

Comments: I enjoyed reading this paper, which is clear and concise. The idea is interesting and for the most part I am convinced by the arguments presented by the authors. However, I would like them to show some additional evidence, as described below, and to better discuss some of the potential limitations of the proposed energy balance relationship.

1) It would be helpful to provide a little more physical motivation for the choice of tropical 500 hPa temperature. I see some good reasons why mid-tropospheric temperature should work better (e.g., it should scale better with LR, WV and LW cloud feedbacks), but I don't think this was discussed anywhere. Why use tropical temperature rather than global-mean? Is there a physical rationale, or did this simply work better in MPI-ESM?

Also, although mid-tropospheric temperature clearly works better for the overall feedback, I expect the scaling with Ta might actually be a worse choice for some individual feedback processes (e.g. surface albedo, marine low cloud). This might be worth discussing briefly.

2) A key result is that the revised feedback parameter θ more accurately estimates the "true" feedback strength under CO2 forcing. This is shown to be the case in MPI-ESM (L172-176). However, does this hold for CMIP5 models in general? I.e., do the values of θ estimated in control runs correlate well with those in 4xCO2?

Relatedly, I would also suggest adding the correlation between ΔR and ΔTa in CMIP5 piControl to Fig. 4, as additional bars in a different color.

3) One important issue that isn't discussed in the paper is that the "pattern effect" doesn't simply go away with the improved relationship; rather, it shifts from the feedback parameter to the ΔTs/ΔTa term. This isn't a problem, but the way the paper is currently written, some readers might get that impression.

So if most of the curvature in the relationship between radiative response and temperature goes away with the revised framework (Fig. 6), I expect there must be some curvature in the Ta versus Ts relationship in 4xCO_2 runs. Can the authors confirm this?

4) I expect the ΔTs/ΔTa ratio cannot be reliably estimated from historical runs in the presence of large variability (for the same reason that λ cannot be reliably estimated - because of the pattern effect). So we must rely on models to estimate this ratio under future global warming, meaning that it will be important to understand how future patterns of surface warming will develop. I suggest the authors discuss this briefly, for example in the conclusions.

Other minor comments: - I suggest using colors in Fig. 6, rather than dark grey and black. - L223: Cite Andrews and Webb 2018 - For future reference, it would be useful to mention the value of θ estimated from observations (horizontal dashed bar in Fig. 7a).

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