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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropical atmospheric circulation response to the G1 sunshade geoengineering radiative forcing experiment" *by* Anboyu Guo et al.

Anonymous Referee #2

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The authors examine the response of the combined Hadley-Walker circulation to idealized greenhouse gas forcing and solar geoengineering experiments. Solar geoengineering counters most of the changes in the Walker circulation in response to quadrupled carbon dioxide. Weakening sea surface temperature gradients are associated with a weakening Walker circulation across experiments and models. In contrast, changes in surface temperature and zonal temperature gradients do not effectively predict changes in Hadley circulation intensity, some behavior differs in the two hemispheres, and Hadley circulation intensity remains reduced under geoengineering. The authors hypothesize this may be due to changes in meridional temperature gradients, which project more strongly onto the Hadley circulation than onto the Walker circula-

tion.

This manuscript builds upon previous work and presents new insights into Walker-Hadley circulation change under geoengineering and greenhouse gas forcings. I think the manuscript is well-structured, the conclusions follow from the results, and the authors have done sufficient work to warrant publication. I have some suggestions for the text that would improve its clarity, suggestions for relevant literature that the authors might want to consider, and some suggestions on the figures. I think addressing these would elevate the manuscript and help it reach a broader audience.

=====General Comments=====

While the introduction has sufficient breadth, there are areas where it lacks a discussion of more recent work. For example, regarding the statement "...climate model simulations...indicate a poleward expansion of the Hadley circulation, though weaker than that observed", there numerous studies suggesting that this may not be the case. Choi et al. 2014 and Quan et al. 2014 both suggest that reanalysis trends in the HC edges may be overstated, especially compared to independent observations. And it appears that the model trends may not be so different from the reanalysis trends (Garfinkel et al. 2015, Davis and Birner 2017). The choice of metric also matters (Solomon et al. 2016). My understand is that it's actually unclear whether models, reanalyses, and observations disagree on the response of the Hadley circulation; but that itself is a valid motivation. The authors may also consider connecting their work to studies like Schmidt and Grise (2016), who have investigated some of the longitudinal characteristics of Hadley cell variability. Full references can be found at the end.

I think the GISS-E2-R model should be excluded from the composite figures, while the composites with GISS should be shown in the supplementary information (opposite to what is currently done). The authors have made a good choice to show composites with and without GISS, as its behavior deviates so much from the other models, but I think swapping these figures would better support their conclusions and interpretations

while still maintaining full disclosure.

It would be helpful if the authors were more explicit and definitive throughout the manuscript. For example, on lines 97-100, it would be helpful to readers if the sign of the changes were stated, e.g., ". . .report that decreases in Hadley cell intensity drive the reduction in tropical precipitation under solar geoengineering. . .". Or, on lines 144-146, state whether the abrupt4xCO2 experiment is close to RCP8.5 in terms of CO2 ppm or in terms of radiative forcing. What follows is a list of some but not all of these instances: -Line 74 -Line 80 -Lines 103-104 -Lines 223-224 -Lines 251-252 -Lines 293-294 -Lines 337-340 (are intensity changes signed, or in terms of absolute value?) -Lines 480-482

I have difficulty discerning information from the anomaly contour figures, like Fig. 2. Standard practice is to show control values overlaid as contours on the shading, so that shifts/expansions/contractions can be more easily discerned. This is especially critical for the Walker circulation - its mean structure and response have substantial spatial variability.

The questions posed at the end of the introduction are a great way to orient the reader. It may be worth specifically restating these questions in the discussion as a way of summarizing the results.

The figure production quality is high, but the image quality is low. Per ACP guidelines, PDF or EPS is preferred so that the figures are crisp when zoomed in (save with vector graphics enabled; in MATLAB, it's the "painters" renderer, not sure how this works in IDL or other languages). Otherwise, I think the DPI needs to be increased for the figures.

=====Specific Comments=====

Line 65: Define "SRM" here rather than on line 73.

Line 99: What are the seasonal changes?

Line 135: ENSO was already previously defined.

Lines 191-194: What is the order of the variability in the first 3 years - 1 sigma, 2 sigma? This may help convince readers it's nothing to worry about.

Lines 227-233: I am somewhat unclear on the metrics for Hadley cell intensity. It seems like the authors are using the average of the 900-100 hPa stream function; are they using a point maximum or an area average?

Line 239, 283-285: "Intuitively": "effectively" or "naturally" might be more appropriate?

Line 243: Mentioning a specific number is good, as is describing how it is derived.

Lines 263-266: The scatter here is very large among the models, even neglecting GISS, which I think is worth noting.

Lines 289-290: It might be more effective to say that there is enhanced overturning aloft and weakened overturning at lower levels; as written, it almost sounds like the anomalies don't conserve mass (reduced equatorward + enhanced poleward).

Line 309: How is the ITCZ metric defined?

Lines 321-325: I think this was essentially said previously on lines 288-294.

Line 333: Could write simply "more pronounced in the southern hemisphere", as the "than. . ." is implied.

Line 341: I think this is too colloquial, maybe state the p-value or % significance it reaches - stating something like "99.9% significant" is more convincing than "hugely".

Lines 399-400: What is meant by "monthly temperature"?

Line 409: Which experiments? Citation?

Lines 413-415: Suggest stating model names, or at least specifying "two models" instead of "three. . .except BNU".

Lines 420-421: But isn't it the case that there are still some weak, positive correlations in regions like the SPCZ?

Section 6.2/Table 3: I suggest mentioning the highest and lowest model value for each category, or really anything to help illustrate the model spread. With an N of 4, the average doesn't mean as much, but I think this section is still worth including. For the critical relationships, it may help to show them in scatter plots. I'm curious what output fields are needed that are lacking in most of the models?

Lines 513-514: Why is this expected? Does it follow from the vertical expansion, or from the Held and Soden static stability/Clausius-Clapeyron scaling?

Lines 520-524: Grise and Polvani (2016) would be a good reference for the dynamical response in abrupt4xCO2 outpacing the thermodynamic response. Doesn't this call into question the importance of static stability and meridional gradients in driving the changes in the circulation, if the circulation responds faster? Is it possible that the thermodynamic responses the authors examine might follow from some of the circulation changes, as these circulations transport heat?

Line 548: What is "Rx5day"? I would rewrite this more generally, and avoid acronyms unless they will be useful later.

Lines 553-554: "Intense" - subsidence?

Figure 1 caption: Suggest rewriting so description doesn't only apply to the third subplot, i.e., "Walker circulation in the ERA-Interim reanalysis (top), . . . , model ensemble mean under piControl (third row), . . .".

Figures 7, 8, 11, 13: I think the authors have crafted the color scheme to avoid some of the major color-blindness combinations (i.e., no green/red), but a further helpful step is to not rely solely on color when trying to distinguish data points. I encourage the authors to use different symbols/shapes, in addition to their current color scheme, if they want to communicate values for each model individually rather than the behavior

of the models as a whole.

Table 2: How is the percent change in position defined?

Table 3: The definitions should not be in the caption, but should instead be in the text.

=====References=====

Choi, J., S.-W. Son, J. Lu, and S.-K. Min (2014), Further observational evidence of Hadley cell widening in the Southern Hemisphere, Geophys. Res. Lett., 41, 2590-2597.

Davis, N. and T. Birner (2017), On the Discrepancies in Tropical Belt Expansion between Reanalyses and Climate Models and among Tropical Belt Width Metrics, J. Climate, 30, 1211–1231

GarïňĄnkel, C. I., D. W. Waugh, and L. M. Polvani (2015), Recent Hadley cell expansion: The role of internal atmospheric variability in reconciling modeled and observed trends, Geophys. Res. Lett., 42, 10,824–10,831.

Grise, K. M., and L. M. Polvani (2016), Is climate sensitivity related to dynamical sensitivity?, J. Geophys. Res. Atmos., 121, 5159–5176. Quan, X., M.P. Hoerling, J. Perlwitz, H.F. Diaz, and T. Xu (2014), How Fast Are the Tropics Expanding?, J. Climate, 27, 1999–2013.

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