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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Precipitation response to regional radiative forcing" *by* D. T. Shindell et al.

Anonymous Referee #2

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The authors explore the effects of regional radiative forcing on global rainfall, with the aim of producing regional radiative forcing (so-called "Regional Precipitation Potential" (RPP)) relationships. They explore this topic by applying historical forcings on the CMIP3 generation GISS coupled model, individually and furthermore in localized regimes (tropics, NH midlatitudes, Arctic, SH extratropics), and examine the global and regional precipitation response over key regions. Broadly, they find that remote forcing often have as large or larger influence than localized forcing on rainfall; and furthermore, their results support previous claims for a significant role for aerosols in the rainfall response. Methodologically, they demonstrate that the 'RPP' relationships capture the full transient climate change response well, indicating that the relationship may be useful in estimating changes in precipitation given radiative forcing, and before the simulation is done.

This is a worthwhile attempt to characterize the role that regional radiative forcing has

on local and remote rainfall. However, I have a major concern with regards to the methodology. Atmospheric circulations are strongly affected by spatial gradients of forcing - so, slicing up (say) well-mixed CO2 forcing into localized regimes artificially creates large-scale atmospheric circulations (extratopical westerlies or Hadley circulation changes) that would have otherwise not have occurred in the first place. Furthermore, the impact on rainfall would depend on where you decide to cut off the localized forcing, since it is where the forcing gradients are that typically the atmospheric circulations would have the largest effects. The authors make no mention of this potential artifact in the discussion, and how it may affect their results. I comment on this more specifically below.

A related concern is the lack of detailed discussion in the large-scale dynamics related to the precipitation changes, of which the literature fairly substantial for the key regions of interest (Sahel, Western US, SE Asia). In the specific comments, I highlight a few areas where the discussion could be improved.

I support publication of this study, but the authors should address and quantify potential artifacts at the forcing edge boundaries, and explain how they view such effects in their RPP framework. As space permits, they should also work on improving the dynamical interpretation.

Specific comments

p5017 lines 20-26 - can you give a bit more detail on the trend estimation and uncertainties? The description is too brief to understand.

p5018 lines 21-28 - it would be helpful to the reader to show the spatial distribution of the individual radiative forcings (in particular sulfate, BC, and ozone), if space permits.

p5019 lines 15-23 - the danger with this sort of normalization is that if the forcing is relatively small, then you may end up amplifying a noisy signal. Several panels in figure 1 look distinctly 'noisier', in particular the anomalies for tropical SO4, tropical

BC, and NH mildatitude ozone. I fear that you may be unduly putting emphasis on a noisy signal. Can you comment?

p5020 lines 27-28 - the similarity in the precipitation patterns for the NH midlatitude SO4, BC, and CO2 runs may not be surprising - these are all consequences of extratropical thermal forcing on the tropical climates, the dynamics of which have been previously examined (e.g. Chiang and Bitz 2005, Kang et al. 2009). Can you discuss?

p5021 lines 5-14 - what creates the dipole-like response over SW Asia and India/Bangladesh, and how it responds differently to the various forcing configurations? I wonder if this dipole-like response between SW Asia and India/Bangladesh is a specific consequence of 'slicing' the radiative forcing at 30N, and which may produce anomalous zonal winds around this longitude. The interaction of the anomalous zonal winds with the Tibetan highlands may produce stationary waves that then impact the rainfall over this region. A recent study by Park et al. (2012) highlights this potential influence. A quick check would be to examine the response of the upper-level westerly anomalies in these runs - what do they show?

p5021 lines 27-28 and p5022 lines 1-8 - again, I have a similar concern with rainfall over the western US being affected by the alteration of westerlies by the latitudinal restriction to the radiative forcings. Please check the zonal wind changes over the North Pacific, and comment?

Technical comments

p5017 line 17 - instead of the AR4 model, the better term is to use the CMIP3 model. Also, what is the spatial resolution of the model used?

p5022 lines 9-15 - Kawase et al. (2010) I think was the first to comprehensively demonstrate the link between sulfate aerosols and Sahel rainfall.

Figure 4 (and section 3.3) - I don't understand how to interpret this figure, and the caption is not very helpful. Can you be more clear about how to read this plot?

References

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Kang SM, Frierson DMW, Held IM. 2009. The tropical response to extratropical thermal forcing in an idealized GCM: the importance of radiative feedbacks and convective parameterization. J. Atmos. Sci. 66:2812–27

Kawase H, Abe M, Yamada Y, Takemura T, Yokohata T, Nozawa T. 2010. Physical mechanism of long-term drying trend over tropical North Africa. Geophys. Res. Lett. 37:L09706

Park, H.-S, J. C. H. Chiang, and S. Bordoni: "Mechanical impact of the Tibetan Plateau on the seasonal evolution of the South Asian Monsoon". Journal of Climate, 25, 2394-2407

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