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Atmospheric Chemistry and Physics Discussions

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

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We thank the reviewer for their constructive suggestions on the paper. Please note that in our specific replies below, figure numbers refer to those in the ACPD submission.

Specific comments:

1. The reviewer is correct that we did not adequately describe to the reader the motivation for the location of forcing in the experiments. We have added the following to the fourth paragraph of section 2 that describes the setup of the experiments with forcing in particular bands: As there has been very little forcing from tropospheric aerosols or ozone in the Southern extratropics, only forcing from CO2 was imposed in that band. When we imposed forcing in multiple bands to test the linearity of the response in comparison with the sum of responses to the individual band forcings, we also followed historical patterns. Hence we examined aerosol forcing from 30S–90N while for CO2

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we examined the response to forcing at all latitudes. Note also that in response to the comments of reviewer 2, we have added a new figure showing the spatial pattern of the imposed forcings and the latitude band demarcations.

2. We have added further description of how the forcings were imposed, as we agree with the reviewer that this is useful information and the reader should not have to look it up in our earlier paper. As the scale factors varied across runs as needed, we provide the basic principle used to select the factors as well as noting that forcing is solely within the bands. We have added the following text to the fourth paragraph of section 2: Scaling was performed uniformly within the bands, with values chosen to give forcings large enough in amplitude to provide a clear response (scaling factors ranged from 0.2 to 2.0, yielding at least 0.2 W m-2 local forcing within the band in all cases).

3. The reviewer's point is a good one, and we have added some text to the discussion of Figure 1 in section 3.1 although we leave most of the comparison with historical simulations to section 3.3. While the patterns are indeed anticorrelated, Figure 1 shows the precipitation response per unit positive forcing (as pointed out in the caption), so that negative sulfate forcing at NH mid-latitudes could still be responsible for some of the historical changes. Hence it's difficult to tell from Figure 1 alone how much of the historical changes come from NH mid-latitude vs tropical forcings. We hope our new text conveys some of this, while this topic is explored in detail in section 3.3. The added text for 3.1 is: The tropics generally show high sensitivity to forcing located either in the tropics or at NH mid-latitudes, though for CO2 and aerosols sensitivity is greater to local forcing. We note that the historical response shows large shifts in the tropics, e.g. in the location of the Inter Tropical Convergence Zone, that seem most consistent with the responses to positive CO2 and/or negative sulfate forcing (comparison with historical trends is discussed in more detail in section 3.3).

4. We have added a new section 4 on the Physical Mechanisms. Though clearly this could be investigated in far more detail, we now describe the changes in large-scale circulation induced by the various forcings and show their connection with sea-level

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pressure. We have added a figure in association with this section, and discussed how our results fit in context to others in the literature.

Minor corrections:

1. We have added that "each simulation differs only in its initial conditions".

2. We have added a reference to Appendix A here for Oct-Apr results. As we have not yet described the experiment at this stage, we believe it would be confusing to summarize the results themselves here.

3. All now given as 30 degrees.

4. True. The NH mid-latitude CO2 has little effect on precipitation in the Southern extratropics, and we now point this out.

5. Agreed. Revised to reflect this.

6. We agree that May-Sept for Figures 1 and 2 would be consistent with the later analyses. We selected May-Sept as more useful for assessing impacts as this is commonly used in studies related to changing precipitation affecting water supplies and agriculture. We used June-July-August in the initial figures to allow for comparison with the physical science community's literature, which largely uses the traditional four seasons. Using JJA allows comparison of the GISS model results with those assessed in the IPCC AR4, for example, which would not be practical with May-Sept. As we believe this context is important to our study, we reluctantly feel that the different time periods need to remain as they are despite the inconsistency.

7. Revised to note this.

8. We believe that the variation between the months presented in Figure 3 provides useful information along the analysis of the May-Sept average presented in FIgure 4, though we agree that this was not called out in the paper. We have added some discussion and comment on this when these two figures are discussed.

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9. Corrected.

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