

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

Anonymous Referee #1

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The paper investigates future changes in precipitation with respect to different forcing agents (CO₂, sulfate, BC and ozone) and different locations of the forcing agents (only tropics, only NH mid-latitude, etc). It is found that, depending on the location of the forcing agent, different precipitation responses may result. Also, a forcing agent acting only in the tropics can impact the mid-latitudes and vice versa. All in all, the global response appears generally linear and can be reconstructed by summing over the forcing agents and their location. Compared to historical trends, observed trends in precipitation can only be obtained if aerosol forcing is taken into account.

The manuscript follows the same methodology as was previously applied by the authors to study temperature changes. In this sense, it represents a logical next step. Also it is of relevance to understand the relation between forcing agent and precipitation changes. The main challenge of the manuscript is to retain the overall picture. On one hand, I think this is due to the fact that the paper is mainly descriptive. On the

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other hand, the figures seem to show both more than what is explained in the text and less than what might be expected from the chosen analysis methodology, what might have distracted me. Please find below my more detailed comments on these issues.

Specific comments

1. I would have found more straightforward if, especially on Figs. 3 and 4 (and possibly on Fig. 1), results for the experiments CO₂, BC, sulfate and ozone would have always been shown and discussed for global and for the latitudinal bands tropics, NH mid-latitudes, the Arctic and southern hemisphere extratropics. It is e.g. somewhat distracting that the band 30-90S only has the CO₂ experiment, whereas 30S-90N only has BC and sulfate (and these bands are generally not discussed in the text). I think this would have better illustrated the linearity of the response and given a clearer picture of the precipitation response.

2. Because it is mentioned that it is hoped that the method could be used by others, a more detailed description of the method would be useful. For instance: what is the scaling factor for the forcing distributions? When they are localized in a latitudinal band, are they sharply cut at the limit? Maybe a Fig. could help? Note that part of this information can be found in the supplement of Shindell and Faluvegi (2009).

3. A point from Fig. 1 is that precipitation changes in the tropics resulting from a change in the extratropics are somewhat similar between Figs. 1a,c,e,g,i but seem partly anticorrelated to the changes in Fig. 1b (see for instance ITCZ or Africa). This might be worth discussing.

4. It is mentioned on p. 5025 (lines 15-20) that analysis of the underlying mechanisms governing precipitation responses is beyond the scope of this study. I can partly understand this but still I think that some more interpretation would have actually helped the reader (the paper also doesn't seem too long and thus wouldn't suffer from some addition). One possibility might be to better emphasize some coherent spatial pattern in the precipitation response that could be more easily related to large-scale circulation

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change (in the line of the mentioned shift of precipitation from Asia to India). Better exploitation of the comparison of the precipitation response over a given region between remote and local forcing might provide further help in the sense that remote forcing might primarily act through large-scale circulation change while local agent can also involve local feedback processes. One might for instance argue that the wetting of the Sahel from NH mid-latitude BC is obtained because of a northward shift of the large-scale circulation/ITCZ/monsoon, whereas its drying under tropical BC might express a local warming and drying of Africa (the Sahel being notably sensitive to such local processes) and/or a southward shift of the ITCZ. Although, without a look at the corresponding maps of temperature and circulation changes, this is highly speculative.

Minor corrections

1. Page 5017 line 22: how are the five members defined.
2. Page 5018 lines 1-2: I would have welcomed one-two sentences summarizing the findings for the other months and a reference to the appendix.
3. Page 5018 line 12: the band limits are here 28, whereas later in the text and on the Figures they are by 30.
4. Page 5020 lines 17-20: there are also differences between 1a,d,c in the southern hemisphere (30S-90S).
5. Page 5020 lines 21-22: yes and no, the response is actually quite different in the southern hemisphere and in the region of the Atlantic ITCZ.
6. Figs. 1 and 2: averaging over May-September would have been more consistent.
7. Page 5022 line 1: the significance is low except for BC.
8. Figs. 3 and 4 are not discussed separately in the text and it seems to me that most of what is written in the text can be concluded from Fig. 4 only. I would advocate for removing Fig. 3.

9. Page 5023 line 1: "the type"

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