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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of modeled Indian Monsoon to Chinese and Indian aerosol emissions" *by* Peter Sherman et al.

Matthew Kasoar (Referee)

m.kasoar12@imperial.ac.uk

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This paper presents a detailed analysis of the Indian monsoon precipitation response to both idealised and more realistic regional aerosol perturbations, conducted with several different current-generation composition-climate models. The paper is notable for bringing together simulations from several different models in its analysis. The multi-model aspect of this study allows identification of robust features of the response which represents a significant update on previous literature which have studies this problem typically with single models at a time.

As a result, I would recommend that this paper be accepted for publication in ACP, subject to satisfactorily addressing a handful of minor comments/corrections I have

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below, which I hope should all be straightforward.

Minor comments:

- Figure S1: I'm a little curious about the stippling in the multi-model mean Fig S1(l) - the caption says it indicates where >70% of the models agree on the sign. However, all except one of the individual models seem to show +ve responses in the south-west corner, and yet this is the only bit of panel (l) which isn't stippled... Conversely, several of the models seem to disagree about the response in the north-east corner, but panel (l) shows stippling here. Could the authors just double-check that the stippling has been applied correctly, to set my mind as ease.

- Also on Figure S1, and the associated discussion in L65-68: I'm surprised by the magnitude of the response by up to 30-40% in some models. The caption suggests that this is after 30 years of 1% per year CO2 increase on pre-industrial concentrations, which would take the simulations up to ∼380ppm, slightly below present-day levels, at which you would expect a little under 1K warming. Consequently, this precipitation increase would seem to be far greater than what can be explained purely from the Clausius-Clapeyron relationship. Are there additional factors contributing to this?

- Figure S2: What is the source for this emissions dataset? The caption says that it is a 2000-2015 average of the input emissions to the PDRMIP and RAEI experiments, but the methods section indicates that both these sets of simulations used timeslice 2000 or 2005 emissions.

- Table 1: The info for the indirect effects for HadGEM3 and IPSL seems to contradict the equivalent Table 1 of Liu et al., J. Clim. (2018), which describes the PDRMIP regional experiments. According to Liu et al., HadGEM3 includes both 1st and 2nd indirect effects for HadGEM3 sulfate, whilst IPSL includes 1st indirect effect only, which differ from the descriptions given here. Please double check the info.

- The present study uses 6 PDRMIP models (L142 and Table 1), but Liu et al. (2018)

describes 7 PDRMIP models that ran the regional aerosol experiments. Why was CESM1-CAM4 not included in the present study, given that this model had apparently also run the PDRMIP regional experiments?

- Methods and Results sections: On that point, the Liu et al. (2018) is not properly credited in this manuscript. The Liu et al. (2018) paper describes and presents initial analysis of the precipitation response to the PDRMIP regional experiments, including a brief discussion of the Asian monsoon response. This by no means detracts from the present study, which provides a much more in depth analysis of the Indian monsoon response in these PDRMIP experiments, however Liu et al. needs to be appropriately cited. Currently the only place I can find it referenced is in L223 where it is incorrectly referenced with regard to the global PDRMIP experiments, when in actual fact this paper analysed the regional experiments, and was the first to do so.

- L246-247: I think this should say Figure S6 not S7

- L282: "Almost all models" - there's only three models, so maybe just say "2 out of 3", otherwise it sounds more confident than it really is

- Section 3.3/3.4: The authors could consider also referencing Shawki et al., JGRA (2018) in the discussion here, which found the same response of increased Indian monsoon precip in response to reducing Chinese SO2 emissions, using HadGEM3 (precursor to UKESM1), and attributed this to the increased land-sea temperature contrast. This supports your results here, and it could be good to note the consistency with this previous study

- L298-299: "For all reduced BC scenarios, the changes in India's precipitation are generally small ($\sim$5% locally) and not statistically significant at a 90% confidence level" - looking at Figure S8, this statement doesn't seem to be true for UKESM1

- L361-362: Again Liu et al. (2018) should also be referenced here, as it previously showed this for the regional Asian BC PDRMIP experiment as well

References: Liu, L., and Coauthors, 2018: A PDRMIP Multimodel Study on the Impacts of Regional Aerosol Forcings on Global and Regional Precipitation. J. Climate, 31, 4429–4447, https://doi.org/10.1175/JCLI-D-17-0439.1.

Shawki, D., Voulgarakis, A., Chakraborty, A., Kasoar, M., & Srinivasan, J. (2018). The South Asian monsoon response to remote aerosols: Global and regional mechanisms. Journal of Geophysical Research: Atmospheres, 123. https://doi.org/10.1029/2018JD028623

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