Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1001-RC2, 2021

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 29 January 2021

The authors analyzed South Asian mean precipitation response to BC and SO2 changes in multiple models. Overall this is a good study. I have a handful of comments, most of which are fairly minor. I apologize but I was not able to read the author response to the first review, which I notice was recently posted, so it is possible that some of these have already been addressed. Specific comments:

Line 67: needs citations

Line 68-74. Results (Fig S1 and Fig 1) should be in the results sections, not in the introduction section

Line 78-80. I think Westervelt et al. (2020) should be cited here, which looked at the relative contribution of aerosol vs greenhouse gas to the South Asian monsoon.

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Discussion paper

Westervelt, D. M., You, Y., Li, X., Ting,M., Lee, D. E., & Ming, Y. (2020).Relative importance of greenhouse gases, sulfate, organic carbon, and black carbon aerosol for South Asian monsoon rainfall changes. Geophysical Research Letters, 47, e2020GL0883 63

Line 105-107. A bit of a run on sentence with the multiple “ands” and the i.e. Consider rewriting.

Line 119-120. What is meant by “in more remote regions”? The US, or Europe? If so, a citation is needed to back up the claim that those regions emissions are less likely to change in a major way.

Line 123-124. About China’s emissions likely decreasing and the Rao paper citation. Is this based on a certain SSP scenario? Do they all have Chinese emissions decreasing in the near term?

General introduction: In general the references in the introduction are a bit dated. There has been a lot of work on aerosol-climate in India since e.g. Ramanathan et al. 2005. Suggest a more thorough literature analysis.

Line 175. Conley et al 2018 is an incorrect citation here. That paper is about temperature. I believe the authors are referring to this paper instead:

Westervelt, D. M., A. J. Conley, A. M. Fiore,J.-F. Lamarque, D. Shindell, M. Previdi,G. Faluvegi, G. Correa, and L. W. Horowitz(2017), Multimodel precipitation responses to removal of U.S. sulfur dioxide emissions, J. Geophys. Res.Atmos., 122, 5024–5038, doi:10.1002/2017JD026756

Line 186-187. 40 years is rather short for precipitation responses in a coupled climate model, so this is an important detail that requires some additional explanation.

Line 227-228: What about the different aerosol schemes makes the ensemble spread so large? This is briefly mentioned and then abandoned immediately in favor of a cloud formation explanation.

Line 230-234: citations needed

Line 234: Is the semi-direct effect of BC included in every model?

Fig 2: For BC and sulfate, something odd is going on in the Had-GEM3 model. How much can we trust that model's result?

Line 282: Almost all models just means 2 of 3, right?

Line 284: The use of "regulations" here is odd and brings in an unjustified policy aspect. Suggest changing to "reductions"

Line 336. Work has already been done on comparison of sulfur dioxide vs greenhouse gases on the south Asian monsoon.

Conclusions/general comment: if even a tenfold increase in BC in $\sim$6 models still gives unclear and confusing results on the impact of BC on South Asian monsoon, is there really any hope to better understand the effect?

Line 382: This could also be a disbenefit, especially if aerosols impact extreme precipitation, in causing more flooding etc. There has been some work on aerosols and extreme precip that is probably worth citing.

Fig. 4: Between the colors, country borders, climatology, statistical significance stippling, and wind barbs, it is tough to decipher all of the layers of this figure. Perhaps wind speed and direction could be its own figure.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1001, 2020.