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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The positive radiative forcing by the substantial SO_2 emission reductions is counteracted by decreased BC concentrations in China over the recent decade" *by* Mingxu Liu and Hitoshi Matsui

Anonymous Referee #1

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One global model is used to study the impact of changes in anthropogenic aerosol emissions in China on regional radiation budget. The authors found that, compared to 2008, reductions in black carbon and sulfur dioxide emissions in 2016, separately, had an opposite effect on radiative forcing, resulting in a net radiative forcing of -0.04 W/m2 by the changes in black carbon and sulfur dioxide emissions. The main scientific conclusion of this study is by no means novel. A number of studies in the literature have shown similar conclusions over China, East Asia and/or other regions around the world. In addition, I have several major specific concerns regarding the methodology

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and model biases as described below.

Major comments:

1. “the recent decade” in the title and abstract (and a few other places in the main text) should be changed to the exact time period “2008-2016”. Why are these two specific years chosen? Many published studies on aerosol radiative forcing use 2014 as “present day”. On the other hand, the simulations were only performed for two years with one year discarded and the results were mostly presented as the difference between the two time slices. I don’t think the use “trend” in the description of discussion of results is appropriate. Given the CAM experiment design, comparison of model results in 2016, which is based on meteorological conditions in 2008, to observations is not apples-to-apples. Meteorology is probably quite important for interannual variability in aerosol concentrations (e.g., Yang et al., 2018). Please discuss how this setup could affect your conclusions.

2. I also have a concern about the current experimental design and forcing estimates, which cannot be used to support one of the main conclusion (i.e., the net BC and sulfate radiative forcing of -0.04 W m-2). Forcing induced by BC emissions reduction and forcing induced by SO2 reduction are diagnosed in separate simulations with one of the individual aerosol changes included, assuming that the forcings induced by the individual aerosol changes are additive. Results in the current study and some previous studies (e.g., Chen et al., 2017) have shown nonlinearities between emission and forcing changes even for the same species. On the other hand, the CRF in this study is calculated by scaling results of different simulations with the best estimate of effective radiative forcing in IPCC AR5, which needs to be described more clearly. I am not sure how exactly this was done and why this number is comparable to the DRF calculated in this study. As far as I know, the AR5 effective radiative forcing is calculated differently from the method of Ghan (2013). The latter attributes radiation changes induced by above-cloud light absorbing aerosols to DRF.

3. It looks to me that the CAM AOD is so much off comparing to MISR (Figure S2), especially over the continental outflow regions and the high latitudes. A difference plot would be able to show the biases more clearly. There are various studies in the literature that have shown improved aerosol simulations in CAM5 (e.g., Ghan et al. 2016 and references therein). Why aren't those improvements included in this version of the CAM model? Model biases in AOD as well as the vertical distribution of aerosols and how they can impact the forcing estimates in the two regions of interest need to be quantitatively evaluated and more objectively discussed.

Minor comments and technical edits:

Line 57-59: I believe there are quite a few other studies in the literature that are relevant here.

Line 71: CAM5 should be spelled out as "Community Atmosphere Model version 5"

Line 80: Change "simulation" to "simulating"

Line 108: How about global emissions for 2016? No changes in emissions of other regions? The CEDS emissions only went up to 2014.

Line 127-128: Why MISR AOD is used here rather than other remote sensing or hybrid products that are often used by the aerosol modeling community to evaluate models.

Line 164-165: this sentence needs clarification. Do you mean less ammonium sulfate leads to more ammonium nitrate over East China in 2016? Assuming that all else being equal?

Figure 4: It's very difficult to see the AERONET observations in the figure. Is there only one site that can represent East Asia? I don't see much value added here.

Figure 6: How was the total RF calculated? How does this compared to the TOA forcing difference between Exp08 and Exp16?

References:

Chen, Y., Wang, H., Singh, B., Ma, P. L., Rasch, P. J., & Bond, T. C. (2018). Investigating the linear dependence of direct and indirect radiative forcing on emission of carbonaceous aerosols in a global climate model. Journal of Geophysical Research: Atmospheres, 123, 1657– 1672.

Ghan, S. J., Liu, X., Easter, R. C., Zaveri, R., Rasch, P. J., Yoon, J. H., & Eaton, B. (2012). Toward a minimal representation of aerosols in climate models: Comparative decomposition of aerosol direct, semidirect, and indirect radiative forcing. Journal of Climate, 25(19), 6461–6476.

Ghan, S. J., Wang, M., Zhang, S., Ferrachat, S., Gettelman, A., Griesfeller, J., et al. (2016). Challenges in constraining anthropogenic aerosol effects on cloud radiative forcing using presentâĂŘday spatiotemporal variability. Proceedings of the National Academy of Sciences of the United States of America, 113(21), 5804–5811. https://doi.org/10.1073/pnas.1514036113

Yang, Y., Wang, H., Smith, S. J., Zhang, R., Lou, S., Qian, Y., Ma, P.âĂŘL., & Rasch, P. J. (2018). Recent intensification of winter haze in China linked to foreign emissions and meteorology. Scientific Reports, 8, 2107. https://doi.org/10.1038/s41598âĂŘ018âĂŘ20437âĂŘ7

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