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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How Asian aerosols impact regional surface temperatures across the globe" *by* Joonas Merikanto et al.

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Response to Reviewer 2:

We thank the reviewer for the valuable comments on the manuscript. We have revised the manuscript according to the suggestions. Below, we itemize the original review comments and the changes made to the manuscript.

Comment 1: Line 64, Line 243, Line 402 and perhaps somewhere else "removal of South and East Asian aerosols". I think this study removes ANTHROPOGENIC aerosols in S&E Asia, not ALL aerosols in S&E Asia. Please be more precise in the context.

Reply 1: Throughout the text we now refer to "S&E Asian anthropogenic aerosols".

Comment 2: Line 98: What is "indirect instantaneous aerosol radiative"?

Reply 2: There was a word missing from the sentence. It now reads "indirect instantaneous aerosol radiative forcings".

Comment 3: Line 107-109: What does the "natural background aerosol" exactly mean? Sulfate from DMS over ocean? Carbonaceous aerosols from natural sources such as wildfire? I think the species and brief info about sources of "natural background aerosol" or "background aerosol" should be specified at least.

Reply 3: We agree that the description was vague. While the background aerosols in both models are mostly of natural origin, they actually aim to represent the pre-industrial aerosols. This issue is more thoroughly discussed in Fiedler et al. (2019), to which we now also refer to: L101 in revised MS: "The background pre-industrial aerosols (mainly consisting of natural organics and sulfate, sea salt and dust) for ECHAM6.1 are prescribed using the climatology of Kinne et al. (2013), while for NorESM1, they are simulated by the model's bottom-up aerosol microphysics scheme (Kirkevåg et al. 2013) (see also Fig. 2 and Appendix A in Fiedler et al. (2019) describing the pre-industrial aerosols for both of the applied models, and the related discussion within)."

Comment 4: Section 2.1: As the manuscript focuses on surface temperature response to the radiative forcing of anthropogenic aerosols in S&E Asia. I would be curious about what the climate sensitivities of the two models are. Climate sensitivity is essentially related to water vapor feedback, cloud feedback and ice-albedo feedback etc. I think knowing the climate sensitivities of the two models would help the audiences better understand how sensitive the surface temperature is responded to different physical processes (especially the cloud-related process).

Reply 4: We thank the reviewer for this suggestion, as it helps to further contextualize the results. We added the following text and references to Section 2.1: L123 in revised MS: "The reported equilibrium climate sensitivity is 3.5K for NorESM1 (Räisänen et al.,

2017) and also 3.5K for ECHAM6.1 (Mauritsen and Roeckner, 2020)."

Comment 5: Line 183: "such as to changes in atmospheric and surface temperature AND/OR? water vapor"

Reply 5: We thank the reviewer for spotting this typo. The line now reads: L197 in revised MS: "such as to changes in atmospheric temperature, surface temperature, or water vapor under clear-sky and all-sky conditions."

Comment 6: Line 237-241: I do not get the point quite well here. What do you mean the "cancellation of differences in ΔIRF"? Is it referring to Figure A1 that ΔIRFd in ECHAM6.1 is stronger than that in NorESM1 but ΔIRFid in ECHAM6.1 is weaker than that in NorESM1. ΔIRF is obtained by summing up ΔIRFd and ΔIRFid, thus ΔIRF in the two models have more similar distributions and has higher model-to-model correlation coefficient than ΔIRFd and ΔIRFid respectively. Is it correct?

Reply6: This is correct. We agree that the explanation was unclear. We now clarify this "compensation of differences" by adding a sentence: L255 in revised MS: "While the aerosols enhance the cloud albedo, clouds also diminish the direct reflection of sunlight by aerosols with compensating effects on the total radiative response."

Comment 7: Line 323-324: Why does emphasize the similar cc for SWcld+LWcld and for total cloud cover here? I see from Figs. 3A and 2E (Figs. 3B and A2E; Figs. 3C and A3E) that the distribution of total cloud cover is more similar to distribution of LWcld, not the distribution of SWcld+LWcld. Is it correct?

Reply 7: This is correct. However, in the section at question (lines 323-324) we focus on the model-to-model differences, and mention that the total cloud response SWcld+LWcld between the two models correlates as "poorly" (cc=0.37) as the total cloud cover change between the two the models (0.37). The cloud cover change does have a rather high correlation with LWcld (cc=0.77) and a rather high anti-correlation (with signs being opposite) with SWcld (-0.74), as discussed in the section before (line

279). To emphasize that here we discuss the model-to-model differences, we modified the sentence in question to: L342 in revised MS: "The total surface temperature response due to clouds in the two models, SWcld+ LWcld (cc=0.37) has a similarly low correlation as the change in total cloud cover (cc=0.37) between the two models."

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References:

Mauritsen, T., and Roeckner, E.: Tuning the MPI-ESM1.2 global climate model to improve the match with instrumental record warming by lowering its climate sensitivity, Journal of Advances in Modeling Earth Systems, 12, e2019MS002037, https://doi.org/10.1029/2019MS002037, 2020.

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