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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Uncertainty in Aerosol Radiative Forcing Impacts the Simulated Global Monsoon in the 20th Century" *by* Jonathan K. P. Shonk et al.

Anonymous Referee #2

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Review of "Uncertainty in Aerosol Radiative Forcing Impacts the Simulated Global Monsoon in the 20th Century" by Shonk et al., ACPD, 2020

This paper aims to contribute to the question of how persisting uncertainty in past emmissions of anthropogenic aerosols (AA) and their radiative forcing have impacted the climate system during the last ∼100 years. To this end, they analyse the impact of AA magnitude on planetary-scale characteristics ranging from the global-mean surface temperature to monsoon area and intensity in the SMURPHS ensemble of the coupled HadGEM3-CG3.1. model. The topic is well within the scope of ACP, the paper is well-written with good graphics, and it presents a (mostly - see my ITCZ-related comment)

clear documentation of how changing the AA magnitude impacts climate. In my view, a weakness of the paper is that it very much remains at this documentary level, and gives little insight into the mechanisms that underlie the AA impact. It also limits its discussion to time-mean changes between 1950 and today, although it would seem that shorter-term changes would be more powerful to understand whether a low or high AA scaling (and thus AA radiative forcing) is more plausible. This is a long-lasting debate, and it would seem that SMURPHS could contribute here. While this somewhat limits the implications of the work, I am still in support of publication in ACP. Below are a number of smaller comments that the authors might want to consider.

1. P2, L16: Maybe cite the updated radiative forcing estimate provided by Bellouin et al. (2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019RG000660)?

2. P2, L20: Is Fig. 1b referring to Fig. 1b of Samset et al.? I assume so, and I think this should be pointed out by writing "their Fig. 1b" (if not then in L19 China is referred to as East Asia in Fig. 1, and consistent wording would be preferrable ... but since the SSP are for the future and Fig. 1 is for the past century I assume that Fig. 1 is that of Samset et al.).

3. around P2, L25: I agree that a single model can estimate the impact of AA changes in that particular model. But a model ensemble would be needed to know if the results from the single model are plausible. This needs to be acknowledged here.

4. P2, L24: Really for the first time? I think there were other studies that played with varying aerosol emissions. E.g., the work by Dan Westervelt and colleagues (https://doi.org/10.5194/acp-18-12461-2018). This is just a study that came to my mind immediately, there are more.

5. P4, L20: I don't think standard deviation is a good measure of inter-ensemble spread for a 5-member ensemble. Why not give the range instead?

6. The model version is a development version towards the final CMIP6 version of

HadGEM3-GC3.1. Can the authors comment on how important they deem the model differences? I.e., can one combine the SMURPS ensemble with simulations with the final model version, or should the two be considered two models?

7. Fig. A1: The authors state that the figure demonstrates the fidelity of the SMURPHYS simulations (P4, L6). I find that hard to see from Fig. A1. The figure shows that the simulations capture the magnitude of the variability in time, but I am wondering whether they also capture some of the signals that one believes are driven by AA changes, and whether one would expect the simulations to capture such signals or not. The authors should expand on this point.

8. Fig. 2 and Tab. 1: It would be helpful if the figure and table would include the observational estimates. These are included in Fig. A1, so it should be easy to include them here as well.

9. ITCZ definition (p5, L7): I do not understand how the ITCZ is defined, and Shonk et al. (2018) did not help me either because Shonk et al. (2018) indicate a zonal-mean that does not seem to be applied here for the 50% criterion. This should be made more transparent, or, and this would seem preferable to me, the authors should consider using one of the established ITCZ defitions that are based on the precipitation centroid between 20N/S (e.g., Adam et al., 2016, doi: 10.1175/JCLI-D-15-0512.1). I would also prefer to define the ITZC based on the zonal-mean precipitation directly instead of averaging the zonal values of the ITCZ, as I would expect this to be more robust and more closely related to the hemispheric perturbations in the atmospheric energy budget introduced by the AA. This would also explain why the authors diagnose the ITCZ to be at the equator or even in the SH in the zonal-mean, which is at odds with a large body of previous work that has put the ITCZ at several degress north of the equator.

10. Definition of monsoon metrics: These include ocean areas, as can be seen in Fig. 4. I know there is some debate about whether a monsoon should be thought of to only

exist over land or not. It would help if the authors could at least briefly acknowledge this.

11. P8, L3: typo –> although they lie

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