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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future changes in Beijing haze events under different anthropogenic aerosol emission scenarios" *by* Lixia Zhang et al.

Anonymous Referee #2

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Summary: The manuscript presents aerosol forcing sensitivity simulations from 2 GCMs. It focuses on the China, and asks what effects changes in aerosols may have on future air pollution/haze events via their impact on circulation change. It is found that reductions in aerosols promote circulation patterns associated with haze events, however the intensity of such events is decreased due to the reduction of the particulate load.

Recommendation: Air quality in China is of major concern to public health officials, impacted citizens, and environmental scientists. The focus on the influence of aerosols on meteorology is novel and interesting. Generally speaking, projections of future air quality over China have primarily focused on the influence of GHGs on meteorological conditions. In the below I suggest greater engagement on the authors' parts with the

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Discussion paper

subject of internal variability and its potential ramifications for the result presented here and the robustness thereof.

Specific Comments:

- On Line 140 the authors indicate 2 models are used to assess the robustness of results. However, some modeling groups have made available single forcing large ensembles, including simulations in which the single forcing is aerosols. Both CESM and CanESM have made available these data sets. This is a big ask, but the claims of the paper are substantial and require rigorous testing. I would have much more confidence in the claims presented here, if the authors were to test their hypothesis using these data:

– Relevant CESM publication: Deser et al 2020 https://journals.ametsoc.org/jcli/article/33/18/7835/353234/Isolating-the-Evolving-Contributions-of – Relevant CanESM publications: —Swart et al 2019 https://gmd.copernicus.org/articles/12/4823/2019/ —Santer et al 2019 https://www.pnas.org/content/116/40/19821

- I am likewise concerned with the authors' lack of consideration/discussion of model simulated internal variability. Beijing's haze events have received a lot of attention, to include work done by researchers that have articulated the role of internal variability on past (Zhang et al 2020 https://acp.copernicus.org/articles/20/12211/2020/; Callahan et al 2019 https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2018JD029738) and future (Callahan & Mankin 2020 https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL088548) air quality-relevant circulation changes. I would like to see the authors contextualize their results in light of the findings presented in these papers. Do the sample sizes studied in this manuscript approach those required to account for signals rising above noise?

- The presentation of the results gets muddled beginning on Line 210. Up to this point

we've been discussing HWI greater than 0, as defined by Cai, but now we've switched to greater than 1.0. We are also now talking about reanalysis data, but we only know that from the figure caption.

- JRA55 renanlysis is used to assess the ability of the models to simulate key synoptic features relevant to air quality/haze events. However, I am curious if JRA55 is able to simulate historical poor quality conditions over China. Haze data in China goes back a few decades and there are some notable examples or extremely poor air quality. Does the JRA55 capture these events? Can they be identified on Figure 3?

- Figure 3 a&b: The blue CLE data obscures the MTFR data. If showing the data to your readers is important, please do so.

- Figure 3 c & d: This may be personal preference, but I've always had trouble reading discontinuous box plot pdfs. Could simple linear pdfs be used here? I assume the relatively differences in chape is more important to convey than the numbers at each gradation of HWI? I would also appreciate the statistical analysis indicated on each distribution comparison, i.e., at what statistical threshold are these distributions significant?

- I would reiterate the above comment for all of the plots of this style in the manuscript.

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