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Interactive comment

Interactive comment on "Strong influence of 2000–2050 climate change on particulate matter in the United States: Results from a new statistical model" *by* Lu Shen et al.

Anonymous Referee #1

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This study describes a new statistical approach to characterizing both local and synoptic meteorological impacts on PM2.5 air quality. The authors develop the statistical relationships based on over a decade of PM2.5 observations over the United States, and then apply these to the ACCMIP models and the GEOS-Chem model to predict the influence of changing climate on PM2.5 concentrations in 2050. They identify the strongest relationship between PM2.5 and temperature and characterize how this is represented by 4 models. They explore the specific response of the GEOS-Chem simulated PM2.5 to temperature in greater detail.

This is a nice study, with a new approach to exploring the meteorological processes controlling air quality. There are a few major points that the authors should address

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prior to publication, the substance of these comments is to expand upon the discussion of the analysis to improve the clarity of the paper. I detail these below, followed by more minor comments.

1. I felt the discussion of the results was a bit superficial. Particularly with regards to the application of the SVD+local statistical relationships to the ACCMIP models. How did the model predictions vary? Were they robust in all regions? The manuscript suggests that the uncertainty in the estimate of the climate impact on PM2.5 can be characterized by using this suite of models (page 12, line 17), but they do not provide estimates of uncertainty or significance. The results in Figure 6 could also use more discussion (page 12, lines 2-3 is a little oversimplistic); the patterns look similar between GFDL and GEOS-Chem, though they are using very different meteorology (whereas GISS is driven by similar meteorology to GEOS-Chem). Perhaps the authors could comment on how the T and PM2.5 patterns compare between the models and obs? If the authors could add a little more discussion of their results, the paper would be much improved.

2. I also found that much (if not all) the supplementary material should be included in the main text. Many of the figures in supplementary are discussed extensively in the main text, and therefore should be more easily accessible.

3. The authors should justify their choice of meteorological variables. Why (only) surface T, RH, precipitation, and E-W & N-S wind speed as predictors?

4. How important is non-stationarity of emissions to the results? There are two aspects here: the changes in anthropogenic emissions (even removing a 5 year moving average of PM2.5 will not eliminate long-term changes in anthropogenic emissions over the 14 year record. Are the statistical relationships similar if the authors use only the early or only the later part of the record?). Secondly: is the 14 year record sufficient for significant T-driven changes in BVOC to impact OA? I assume that this is what the authors are suggesting on page 9 line 13 as the reason for the projected increase in summertime PM2.5 in the eastern US (if not, please clarify in the text), however, it's

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not clear that this relationship would be identifiable in the statistical analysis. Please discuss.

5. The authors did not discuss the impact of covariance on their analysis. The meteorological variables are not all statistically independent. How well correlated are the SVD patterns with the local meteorology? How does this impact the results?

Additional Comments

1. Title: “Strong influence” seems overstated. Strong compared to what? Compared to changes in emissions, these climate-driven responses are not large changes in PM2.5. I suggest that the authors remove the word “Strong”

2. Page 1, Line 9: “we bypass many of the uncertainties inherent in chemistry-climate models”, seems a bit overstated. The authors have developed a statistical approach which is complementary to chemistry-climate model predictions, but not without its own limitations. I suggest that the language be softened.

3. Page 2, Line 4: I suggest that the authors cite the relevant epidemiological literature for these statements rather than the application studies of Lelieveld et al.

4. Page 2, Line 12: “to more robustly quantify” is a very strong claim which is impossible to substantiate. I suggest that the authors soften their language.

5. Page 3, Line 10 & 12: “In contrast” and “inconsistencies” suggests that Day et al. (2015) and Val Martin et al. (2015) disagree, but in fact the results discussed are for different time periods (summer vs annual) and different scenarios. Therefore they are not necessarily in disagreement. Either compare similar results, or modify language.

6. Page 3, Line 24: define T

7. Page 3, Line 26: what does “period T” mean?

8. Page 5, line 10-20: what biomass burning emissions are used in the model. Do they vary year-to-year? If so, how might this impact the analysis? More generally, it would

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be useful to comment on the role of fire emissions (as a possible feedback from climate change) in this analysis.

9. Page 5, line 28-29: This last sentence seems out of place as the suggested analysis does not follow. Please indicate in which section this analysis will be discussed in the paper.

10. Page 6, line 21: “making clear” seems a bit strong. The results are suggestive of a regional climate influence. They may also be indicative of a relatively homogeneous region.

11. Section 3: the time horizon for the analysis is not always clear. It would be helpful if you could clarify the time resolution of the analysis (monthly, as I understand it?), as you present both seasonal and annual averages in the results.

12. Page 7, line 8: identify which dimension corresponds with which variable in matrix A

13. Page 7, line 16: I believe that the authors mean to refer to Figure 1e, not 1d

14. Page 7, line 19: typo? “negative” looks like positive anomalies in the figure? Also these are only seen in Figure 1a (not 1a-1c as indicated in the text).

15. Figure 1 caption indicates that the analysis was for summer. Figure 2 caption does not indicate the time horizon. These should be consistent for the authors to compare them. Please update Figure 2 caption and ensure consistency.

16. Line 11: how were the results from the 17 models combined in Figure 4?

17. Page 9, line 14: “driven by”. Be careful with the language, this is speculation not attribution.

18. Page 10, lines 4-5: May be worth noting that not that many studies have investigated the climate impact on PM2.5 (compared to say O3) and that PM2.5 consists of many different chemical species, so a more complex system to understand the re-

sponse.

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