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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 #2

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I believe the study presents several analyses investigating projections of climate change impacts on PM2.5 pollution that provide valuable insights to the air quality modeling community. The manuscript is well-written and clear. I appreciate the authors' effort to undertake a study that includes several layers of research: developing and describing a new statistical regression model, applying the model to the projections of a multi-model GCM ensemble, using these results to guide an investigation into PM2.5 projections from CCMs, and using a CTM to identify factors contributing to the inconsistencies in simulations of PM2.5 impacts. As it stands, the study presents several useful findings that make it worthy of publication. However, by addressing different research needs and different modeling approaches (including a statistical regression

model, 17 different GCMs, 4 different CCMs, and 1 CTM), the analysis of results for each research question being investigated is at times limited. I encourage the authors' to go deeper in their discussion. I would also persuade the authors to further investigate the major findings of their work individually in follow-up research. Some specific comments are included below.

- I felt there is some disconnection between different aspects of the study as it moves from the regression model to GEOS-Chem. The study could be broken down into separate analyses: (1) a PM2.5/meteorology linear regression model; (2) projection of PM2.5 climate impact from the CMIP5 GCM ensemble; (3) PM2.5/temperature relation in 4 ACCMIP CCMs; (4) GEOS-Chem sensitivity of PM2.5 to temperature. The connection between (1) and (2) is evident, while the connection between subsequent sections is not as clear. In moving from sections 4 to 5, the manuscript goes from statistical inference of PM2.5 changes from 20-yr present/midcentury simulations with 17 GCMs, to atmospheric chemistry simulations from 4 CCMs covering a different 15-yr present period and conditions, to a CTM simulations for a different 9 yr period. Is there truly a clear connection between these different types of models and the nature of these simulations, other than saying that the temp-PM25 relationship is important? The scope of the study limits the depth with which each finding is examined.

- One topic I would encourage the authors to discuss further in their manuscript is the impacts of 2050 climate derived from the CMIP5 ensemble and the regression model. Only ensemble-mean results are presented. I would be very interested in seeing the differences in the projections from individual GCMs. Additionally, I would like to see the interannual variability across individual years in the 20-yr penalty estimates. How robust is the study's finding of an anthropogenic-induced climate change impact by 2050 under a stabilization scenario, given that natural variability has been shown to significantly influence U.S. temperature and precipitation projections on timescales as long as 50 years (Deser et al., 2014; doi: doi:10.1175/JCLI-D-13-00451.1.)? There is a great opportunity to explore climate-related uncertainty in PM2.5 projections at much

greater depth within this data.

- I suggest combining figures 4 and 5 and showing additional details of the penalty projections within the manuscript.

- One analysis that is absent but would greatly benefit the study is a comparison of a climate penalty projection generated by the regression model to that generated with a CTM for the same GCM meteorological fields. Comparing the midcentury climate penalty estimated with the regression model, to a projection generated by driving a CTM with the same weather fields (e.g. a GEOS-Chem simulation driven by the present/future met fields from one of the CMIP5 models) would provide great insight into the potential to replace computationally expensive CTMs with a statistical model, and limitations associated with either approach. I encourage the authors to undertake this analysis in future work.

- The penalty projections generated using the regression model assume that observed relationships between PM2.5 and meteorology remain valid at midcentury under significantly different meteorological conditions and emissions levels. Is this an adequate assumption? An interesting analysis would be to compare the penalty projections of regression models generated under different levels of emissions within the 15-yr observational record.

- The study explores PM2.5 response to surface temp. in 4 CCMs, and then further investigates the dependency in GEOS-Chem. Given the differences between GEOS-Chem (a CTM) and CCMs, what insights from the GEOS-Chem analysis may useful to identify the causes for the discrepancies between observed and simulated sensitivities in CCMs? Would the authors expect to see any similarities?

- PM2.5 concentrations and meteorology could be mapped on a finer resolution grid, and CMIP5 fields interpolated onto that grid. Would there be a benefit or significant change if the statistical regression model were built at higher resolution, rather than the coarse 2.5°x2.5°?

- When listing the range of reported projections for climate change impacts on PM2.5 (e.g. pg. 3, line 7), I recommend using the updated range from the reviews published by Fiore et al. (doi: 10.1080/10962247.2015.1040526.)

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