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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantification of impact of climate uncertainty on regional air quality" *by* K.-J. Liao et al.

Anonymous Referee #2

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General comments:

This paper discusses the impact of future climate variability on air quality. The authors select two climate variables (temperature and relative humidity), use distribution functions from an undescribed simulation, and perturb the meteorology, which is used as an input to drive a chemical transport model. As written, the paper does not comprehensively address uncertainty in air quality as the title suggests. Currently, there is no detail about the distribution functions used by the authors (see comments below), so it is difficult to assess what uncertainty the authors are actually applying to the chemical transport model. By using this methodology, the authors constrain the uncertainty in several ways: 1) they select specific met variables, 2) they select only one year to evaluate, 3) they are specifying only one future climate scenario, 4) they are specifying a set of future anthropogenic emissions, and 5) they are assuming a certain set of parameterizations for both the meteorological and chemical transport model. So the uncertainty that they present seems to be highly dependent on the selected model framework and simulation time period.

The authors conclude that the uncertainties due to a range of distribution functions have a "moderate" effect on VOC emissions and ozone concentrations, and that PM is "less sensitive" to these changes. Overall, I find that the conclusions are based on a limited set of simulations lacking detail in their methodology and discussion.

Additional discussion and figures are needed to improve this paper prior to publication. Specific questions about the methodology and analysis are provided below to assist in these improvements. In particular, a figure of the changes of the two controlling met variables (temperature and humidity) is needed for this paper (a figure similar to Figure 3 for precipitation).

Methods:

1. There is no description of the use of the MIT model and how the distribution functions are generated. The two references in the model description on p. 7784 (Prinn et al., 1999; Reilly et al., 1999) are almost ten years old, and are likely using outdated scenarios. Later in the text, there is reference to Webster et al., 2003 (p. 7787) that mentions some details about the distribution shape. This should be discussed in greater detail so that readers can have an idea of what is driving these distribution functions.

2. Why is a longitudinal average used (Eqn. 1)? Why not retain the full spatial distribution of the probability function? There is often a large difference in the climate response of two regions on the same longitude, and this is especially pertinent in this study are the differences between the Midwest and Southeast region. The authors provide no rationale for this methodology.

3. The authors provide one sentence of rationalization for the use of the year 2050 (p.

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7784). But they have not addressed the uncertainty caused by interannual variability, and this should be added to the discussion of results and highlighted in the overall conclusions.

4. Regarding the "base case" (or 50model): The differences between the GISS-MM5 simulation and the MIT model should be discussed as to account for the differences shown in Table 1. I imagine there a long list of differences, but why are the authors using this distribution mean to provide a base case?

5. The model methodology is not clear as written. My current understanding is the following: 1) The authors use GISS to drive a MM5 run, 2) they then compute a new T and RH for each time step based on the distribution function of the MIT model, and 3) re-run MM5 with these new initial and boundary conditions. "The resulting fields were reanalyzed to assure that similar changes in temperature and humidity remained." Please clarify this statement and also the methodology used for this analysis. It may be helpful to move the supplementary information into the main body of the paper for this reason.

6. Are the authors running a version of CMAQ that accounts for secondary organic aerosol (SOA)? (discussion on p. 7790) Some versions of CMAQ do include this type of aerosol, and the authors should explain what version they are using and if they account for SOA. If not, then they should discuss other results that do include SOA (e.g., Zhang et al. 2007) and explain how much uncertainty they are missing (or introducing) due to the lack of this mechanism.

Analysis:

1. I'm confused about the spatial patterns in Figs 2a and 2b. At first look, the incoherent spatial patterns of ozone increases appear to be noise. The authors claim that these increases are due to "impacts of meteorological changes... on the photochemistry of tropospheric ozone". Yet, if this were the case, then the patterns should follow that of temperature. Do the temperature changes show that much spatial variability? A figure

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of the met changes is needed in this discussion. Also, how do we know that these are not associated with emissions changes? The authors mention "sensitivity studies"(pp. 7788 and 7789), yet it is unclear if they are merely citing other studies or if it is based on their own work. A more complete discussion is necessary here.

2. Are precipitation changes (Figure 3) induced by the temperature and humidity changes statistically significant? What convection scheme is used in MM5, and how might this impact the results of the precipitation changes?

3. Figures 2c and 2d: The authors do not discuss why the PM concentrations increase in some locations of the low-extreme case (Fig 2c).

4. It would be helpful to see the spatial distribution of emissions changes with respect to the discussion in section 4.

Specific comments:

1. Results and discussion: Naming of the "base case" is confusing. I believe that the authors used the 50% term from the MIT model, but calling it the "base case" makes it sound as if it were the unperturbed simulation.

2. Table 1: For the "low-extreme" case in temperature, why are the differences the same across every region?

3. Page 7788, section 3.3: Specify that the "slightly sensitive" changes to ozone are on the regional scale, but that the localized response can be up to 10 ppb. This is unclear as written.

4. P. 7793, Conclusions: please specify that the 1 ppbv change in ozone is when you are performing a regional average.

5. I would suggest a change in title that focuses on the uncertainty introduced by perturbations in temperature and relative humidity, rather than a general "uncertainty."

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