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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.

K.-J. Liao et al.

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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).

Reply: We would like to thank the reviewer for providing detailed reviews and comments made. The methodology of constructing extreme climate scenarios along with step-by-step description has been added in the revised manuscript. Three new figures have been added to show the spatial distribution of changes in metrological fields and emissions between the extreme and base case scenarios in 2050. The point-by-point page response details how we have addressed each comment and how the manuscript has been modified. Please see replies below for detailed responses to the comments of the reviewer.

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.

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Reply: Done. In the revised manuscript more detailed description for the IGSM model has been added in Section 2.1: In this study, climate fields from MIT Integrated Global System Model (IGSM) (Prinn et al., 1999;Reilly et al., 1999) simulations, in the form of probabilistic distributions, are used to quantify uncertainties inherent in forecasts of future changes, and their associated effects on regional air quality. The IGSM includes components of: (a) the Emissions Prediction and Policy Analysis (EPPA) model, designed to project emissions of climate-relevant gases and the economic consequences of policies to limit them (Babiker et al., 2000), (b) the climate model, a 2D zonally-averaged land-ocean resolving atmospheric model, coupled to an atmospheric chemistry model, (c) a 2D ocean model consisting of a surface mixed layer with specified meridional heat transport, diffusion of temperature anomalies into the deep ocean, an ocean carbon component, and a thermodynamic sea-ice model (Sokolov and Stone, 1998;Holian et al., 2001;Wang et al., 1998), (d) the Terrestrial Ecosystem Model (TEM 4.1) (Melillo et al., 1993;Tian et al., 1999), designed to simulate carbon and nitrogen dynamics of terrestrial ecosystems, and (e) the Natural Emissions Model (NEM) that calculates natural terrestrial fluxes of CH4 and N2O from soils and wetlands (Prinn et al., 1999). The probabilistic distributions of temperature and humidity changes in 2050 were derived from a set of 1000 ensemble simulations (Webster et al., 2003). In configuring this ensemble of simulations, the model uncertainty is included by using a joint PDF of three climate model parameters, i.e., climate sensitivity, ocean heat uptake, and aerosol radiative forcing along with PDF of predicted anthropogenic emissions of major greenhouse gases which is calculated using the Monte Carlo analysis of the EPPA model.

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.

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Reply: The following paragraph has been added in Section 2.1 of the revised manuscript for discussing the use of 2-D (longitudinal average) climate fields in this study: For uncertainty analyses, extreme cases from probabilistic distributions of climate fields are more important for policy-making. The use of the 2-D model allowed development of wider probabilistic distributions from which a wide variety of proposed policies and extreme cases could be chosen in the future (Prinn et al., 1999).

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

Reply: The concern is understandable. Here, we refer back to a foundational paper (Tagaris et al, JGR, 2007) to discuss interannual variability of meteorology. A discussion has been has been added in Section 2.2 of the revised manuscript as follows: Interannual variability of meteorology is a critical issue since only the year 2050 is chosen as the future episode examined in this study. The analysis for the interannual variability of climate fields has been presented by Tagaris et al. (2007): the results show that cumulative distribution function (CDF) and spatial distribution plots for temperature and absolute humidity are similar for the three consecutive future years (2049-2051). The former paper provides information on interannual variability, and this paper looks at perturbations to the modeled base meteorology.

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?

Reply: First of all, a discussion of using both IGSM 50th percentile and GISS-MM5 in the uncertainty analyses is added in Section 2.1 as follows: The 50th percentile of both the meteorological parameters are adjusted to the GISS-MM5 by minimizing the dis-

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crepancies in temporal and spatial resolutions between the 50th IGSM and GISS-MM5 outputs and used to develop perturbation fields along with the GISS-MM5. Moreover, the differences in temperature and absolute humidity between the 2050 base case and 50% IGSM climate have been added in Table 1. A figure (Fig. S1) is added in Supplementary Material showing spatial distribution of temperatures and absolute humidity for the 2050 base and 50% IGSM climate. The associated discussion for the differences in meteorology forecast between the 2050 base case and 50% IGSM climate has also been incorporated in Section 3.1 of the revised manuscript as follows: Spatial distributions of annualized temperature and absolute humidity between the base case GISS-MM5 and IGSM 50th percentile climate are found to be similar (Fig. S1), although regional average values differ slightly since the IGSM 50th percentile data has been used for re-running MM5 after being adjusted to the base case GISS-MM5 (Table 1).

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.

Reply: We appreciate the concern and suggestions, and the revised text is intended to clarify our approach. The method used in the development of the 2050 uncertainty climate scenarios, using temperature and absolute humidity from the 2-D IGSM, has been moved from Supplementary Material to Section 2.1. Specifically, the processes of development of extreme climate and associated description have been also added in Section 2.1 in order to provide more detailed information regarding to the readers.

6. Are the authors running a version of CMAQ that accounts for secondary organic

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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.

Reply: We include SOA formation, though it is widely recognized that our understanding of SOA formation is incomplete at best. Further the version of CMAQ used does not include SOA from isoprene (but it does include formation from other biogenics). Any mechanism for SOA formation is an approximation, and the amount of uncertainty added is unknown. We have compared the version of CMAQ we use to observations and find that the simulated OC tends to be biased low during the summer, suggesting that the SOA formation is under-simulated. Moreover, the following sentences have been added in Section 3.4 of the revised manuscript: In the CMAQ version 4.3, the SOA gas-particle partitioning model is based on the Secondary Organic Aerosol Model (SORGAM) (Schell et al., 2001), which does not account for SOA formation from isoprene. Some studies show that isoprene significant contributes to SOA formation (Claeys et al., 2004), and SOA levels are predicted to be underestimated without including isoprene as a SOA precursor (Zhang et al., 2007;Morris et al., 2006;Pun and Seigneur, 2007). In this study, the changes in PM2.5 levels attributed to the extreme climate scenarios are dominated by sulfate and nitrate PM2.5 in most of the U.S. regions since SOA formation is not fully captured in current regional air quality models (Table 3)

Analysis:

1. I am 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?

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A figure 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.

Reply: Figs 2a and 2b have been re-numbered as Figure 6a and 6b in the revised manuscript. The figures show the differences in summertime fourth-highest daily maximum 8hr ozone between the 2050 base case and extreme climate scenarios. For different, adjacent grid cells, the day with summertime fourth-highest daily maximum 8hr ozone may not be the same. Therefore, the spatial distribution is not continuous. The patterns of changes in 4th MDA 8-hr ozone do not follow that of temperature since ozone formation is affected also by local precursor emissions as well as temperature. In Section 3.3, the following sentence is added in the revised manuscript for more explanation: The spatial distribution of changes in 4th MDA8hr ozone between the three 2050 climate scenarios does not follow that of temperature, absolute humidity and precipitation since ground-level ozone formation is affected by combined effects of different climate fields and local emissions as well.

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?

Reply: Figure 3 has been re-numbered to Figure 4. The differences in annualized and summer-averaged precipitation between the three 2050 climate scenarios are predicted to be small. Precipitation is one of the most uncertain parts in climate forecasts and expected to be affected by perturbations in temperature and absolute humidity from meteorological fields. However, this study does not focus the effects of single climate variable on air quality since air quality is affected by the combined effects of all meteorological fields. The Grell cumulus parameterization scheme with shallow convection was used for MM5 simulations. The Grell scheme is based on rate of destabilization or quasi-equilibrium, simple single-cloud scheme with updraft and downdraft

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fluxes and compensating motion determining heating/moistening profile. Shear effects on precipitation efficiency are considered. The Grell cumulus parameterization scheme tends to allow a balance between resolved scale rainfall and convective rainfall and it is useful for smaller grid sizes 10-30 km which is the reason that we use it in our MM5 simulations.

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).

Reply: Figures 2c and 2d have been re-numbered as Figures 6c and 6d. The following sentence has been added in the revised manuscript to provide more detailed discussion for the changes in PM concentration (Section 3.4): The 1.0 μg m-3 decrease in annualized PM2.5 levels in the Midwest for the high-extreme scenario is mainly due to lower sulfate and nitrate in the winter (January) compared with the base case (Table 3), while the 0.5 μg m-3 increase in annualized PM2.5 levels in the Southeast for the low-extreme scenario is due to higher nitrate in the winter (January) than the base case (Table 3).

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

Reply: Done. Spatial distribution plots of biogenic VOC emissions between the extreme climate scenarios have been added in the paper as Figure 5. The following description was also added in Section 4: Biogenic VOC emissions are predicted to increase for the high-extreme scenario, especially in the Southeast region and west coast of the continental U.S., compared with the base case in the summer of 2050 (Fig. 5). Higher biogenic VOC emissions for the high-extreme attribute to a more NOx-limit environment for ozone formation and increase S_4thMDA8hO3, ANOx.

Specific comments:

1. Results and discussion: Naming of the "base case" is confusing. I believe that the

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authors used the 50% distribution from the MIT model, but calling it the base case makes it sound as if it was the unperturbed simulation.

Reply: In this study, as mentioned in Section 2.1, the base case scenario is the unperturbed GISS-MM5 climate. The IGSM 50% climate was only used to construct the two extreme climate (high- and low-extreme) and determine the difference between the extreme and base case climate scenarios. In Section 2.1 we have added: We keep the GISS-MM5 field as the base case scenario in order to compare with current pollutant levels (note that new fields of the IGSM 50th percentile climate are not identical to the GISS-MM5 fields). The high- and low-extreme fields are calculated as equations 2.1 & 2.2. Therefore, the differences between the extreme and base case scenarios do not depend on the base case emission scenario used (i.e., the IPCC A1B in this study) but the probabilistic distributions from the IGSM outputs.

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

Reply: Table 1 presents the regional average changes. Although regional average changes are similar (digits are rounded off), the spatial distribution plot (Figure 2) is different.

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.

Reply: Done. In Section 3.3, the sentences have been rephrased to: Regional average summertime ozone and daily maximum 8-hr ozone concentrations are found to be slightly sensitive to the extreme climate scenarios for the five regions in 2050. Differences in summer-average ozone and daily maximum 8-hr average ozone concentrations are about 1-2 ppbV between the extreme and base case climate scenarios on a regional basis (Table 2). For the peak ozone levels, summertime (JJA) 4th MDA8hr O3 (4th MDA8hr O3 in the summer of 2050) concentrations for the high-extreme scenario

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are predicted to increase up to 10 ppbV as compared with the 2050 base case in urban areas of the Northeast, Midwest and Texas in the continental U.S. (Fig. 6).

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

Reply: In Conclusions section (Section 5), the sentence has been changed to: Differences in concentrations of fourth-highest daily maximum 8-hr average ozone between the extreme climate scenarios and base case are found up to 10 ppbV (about one-eighth of the current ozone standards) in some polluted urban areas due to higher temperature, absolute humidity and VOC emissions, though the change in summer-average ozone is minimal on a regional basis (˜1 ppbV).

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."

Reply: We believe that keeping the original title is more representative for our work. Here, we estimate the impact of climate uncertainty driven by the uncertainties in temperature and humidity. As a result all the meteorological parameters are changed, based on the framework that we have followed (i.e., rerun MM5). If the title is modified as suggested this work will be considered as a work examines the air quality uncertainty or sensitivity of each meteorological parameter separately.

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