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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Attribution of projected changes in US ozone and $PM_{2.5}$ concentrations to global changes" *by* J. Avise et al.

Anonymous Referee #3

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General comments:

The authors present an analysis of summertime ozone and PM2.5 simulations over the continental U.S. under a variety of assumptions about future chances in climate, land use, boundary conditions, and emissions. The authors then compare the relative impacts of these changes to determine the dominant drivers likely impacting future air quality. While other studies have addressed some of the factors described in this study, to my knowledge, this is the first study in which all of these factors are addressed in a comprehensive way. The setup of all components of the modeling system utilized in this study (global and regional climate and air quality modeling, emissions processing and land use modeling) reflects best modeling practices and is described well in the manuscript and a companion paper (Chen et al., ACPD, 2008). The design of the

sensitivity simulations is sound, however, as noted below, I would have liked to see the simulations performed for a longer time period and also would have liked to see additional simulations to address the issue of synergistic effects between various drivers. The analysis presented in the manuscript is straightforward and presented clearly and supports the authors' conclusions.

While the results presented in this study appear sound, I have the following concerns that are detailed further in the specific comments below:

- The authors need to add stronger cautionary statements in the abstract and summary to alert the reader to the fact that simulations were performed only for a one-month period for five current and future summers. The title should be changed accordingly by adding "summertime" before "U.S. ozone". While simulating only July conditions may be sufficient to assess the impact of the various drivers on future ozone concentrations, it is less clear how relevant the results are for future PM2.5 concentrations. Clearly, elevated PM2.5 concentrations can occur year-round, and the relative impacts of the various drivers determined in the present study may very well be different for conditions other than the July conditions simulated here.

- The PM2.5 analysis should be expanded to examine individual species (sulfate, nitrate, etc.) in addition to total mass.

- The approach to emissions processing needs further justification. Why was there a substantial increase in area and nonroad source emissions for the future case but not for mobile sources? Even if the authors only consider the effects of increases in population and do not account for the effects of technology changes, wouldn't mobile source emissions be expected to increase because of increased Vehicle Miles Traveled (VMT)? And what is the rationale for not including the effects of technology changes that are built into models such as MOBILE6? (for example, Woo et al. presented a dramatic reduction in mobile source NOx emissions from an application of MOBILE6 for 2050, see http://www.nescaum.org/documents/impact-of-potential-

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future-climate-change-on-regional-ozone-and-fine-particulate-matter-levels-in-the-usa/praveen-amar-final-arb-jan-8-2007-read-only.pdf/ slides 38-39). Clearly, if the authors incorporated such assumptions about mobile sector technology changes in their simulations, their results would change significantly. In that respect, it might be worth to add a discussion about which, if any, of the simulated factors are more certain than others. In addition, in my understanding the authors changed the spatial distribution along with the magnitude of the biogenic emissions in the future land use case, but the spatial distribution of anthropogenic emissions was not changed to correspond to the changes in land use (increased urbanization). What is the rationale for not addressing this issue?

- The authors do not consider synergistic effects between the various factors. For example, it appears that for the simulation of the effects of future boundary conditions, MOZART-2 was applied with future anthropogenic emission but current climate even though the pathways for intercontinental transport may change in a future climate and global tropospheric chemistry is sensitive to climate change. Since the authors consider four factors (chemical BC, anthropogenic emissions, LU/LC, and climate), a set of 16 simulations would be necessary to quantify all individual and synergistic effects of changes in these factors but the authors performed only a subset of 6 of these 16 possible simulations. At a minimum, the authors need to discuss why this shortcut was chosen and how it may affect the interpretation of the results.

Overall, I recommend publication of the manuscript after addressing the concerns above and my detailed comments below.

Specific comments:

Page 15131, title: Please replace "changes in US ozone and PM2.5 concentrations" with "changes in summertime US ozone and PM2.5 concentrations"

Page 15139, line 6: I do not agree with the statement that changes in DM PBL height are clearly correlated to changes in average DM surface temperature. Looking at pan-

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els a) and b) in Figure 3, the northeastern U.S. and New England show a mix of increased and decreased PBL heights (New Hampshire, Maine) but an increase in temperature. The relationship also doesn't hold true for some areas in the Southeastern U.S. Did the authors compute the correlation coefficient between the two maps? What is the value?

Page 15140, section 2.1.3: For the future A2 simulations, which meteorological fields were used in MOZART-2? Was the effect of increased GHG emissions on climate and air pollution transport pathways accounted for? If not, how does this limit the conclusions of this study?

Pages 15141-15142, section 2.1.4: Does EGAS provide growth factors through 2050? Why was EGAS used to calculate changes in mobile source emissions? Mobile6 would have been the preferable tool. Does EGAS assume changes in Vehicle Miles Traveled associated with population growth? Why were technological changes (i.e. decreases in emission factors) not considered even though the IPCC SRES scenarios incorporate assumptions about technology development? Please provide more details on the updates to future anthropogenic emissions through the SERGOM model to account for increased urbanization and population - did this update only affect the magnitude of anthropogenic emissions or also their spatial allocation? If spatial allocation was unchanged, please provide a rationale. For an example on how one might go about spatially reallocating anthropogenic emissions under future land use and urbanization scenarios, see Civerolo et al., Atmospheric Environment, 2006, pp. 1803-1818. In table 2, please include a column with total emissions and include the future year total emissions for each pollutant below the current year total emissions. For the future LU/LC case, how were the CLM / SERGOM / MM5 categories mapped into the categories required by MEGAN? I assume that the vegetation database required by MEGAN is much more detailed to account for plant specific emission factors than the categories provided by CLM / SERGOM / MM5, so how was this issue addressed?

Page 15143, Section 3.1. Rather than citing CMAQ evaluation studies performed for

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retrospective cases, please refer to CMAQ evaluation studies in the context of climate change applications and discuss the evaluation results of the present study in the context of these earlier studies. For the evaluation of PM2.5, please specify if filter based or continuous instruments were used. If filter based instruments were used, the sample size would be reduced because sampling typically is performed on a 1-in-3 day schedule. Please also provide a rationale for performing the analysis and aggregating the results by EPA regions. A more robust approach to spatial aggregation would be to perform some type of clustering analysis (e.g. PCA) to determine homogeneous regions that lend themselves to grouping.

Pages 15144 - 15147, Section 3.2 and 3.3: In addition to showing the maps of results, please also provide tables showing the changes for each scenario for each EPA region. The discussion often refers to specific absolute or relative changes for individual EPA region, but it is hard for the reader to visually aggregate results from the maps over these regions.

Page 15145, lines 24 - 28: Is this statement based on additional analyses not shown in the manuscript, or is it a hypothesis?

Pages 15146 - 15147, Section 3.3. This section either should to be expanded by including a discussion of the effects of the various drivers on the individual components of PM2.5 or should be removed. For example, is the increase in PM2.5 due to emission changes in the Northeast driven by increase sulfate or primary PM2.5? Is there a decrease in nitrates and OC due to higher temperatures? Furthermore, the authors should add a discussion on how the results from these July simulations might be expected to change for other seasons.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15131, 2008.

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