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## ***Interactive comment on “Attribution of projected changes in US ozone and PM<sub>2.5</sub> concentrations to global changes” by J. Avise et al.***

**J. Avise et al.**

Received and published: 23 December 2008

We thank the anonymous reviewer for the insightful comments. We have considered the recommendations and made appropriate changes to the manuscript. Our responses to the specific comments are detailed below.

== Comment: 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 PM<sub>2.5</sub> concentrations. Clearly, elevated PM<sub>2.5</sub> concentrations can occur year-round, and the relative impacts of the various drivers determined in the present study may very well be differ-

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ent for conditions other than the July conditions simulated here.

== Reply: We agree that a caveat to these results is that they apply to summertime meteorological conditions only and have added appropriate statements in the abstract and summary to reflect this caveat.

== Comment: The PM<sub>2.5</sub> analysis should be expanded to examine individual species (sulfate, nitrate, etc.) in addition to total mass.

== Reply: We have expanded the PM<sub>2.5</sub> analysis to examine individual PM species.

== Comment: 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 NO<sub>x</sub> emissions from an application of MOBILE6 for 2050, see <http://www.nescaum.org/documents/impact-of-potential-future-climate-change-on-regional-ozone-and-fine-particulate-matter-levels-in-theusa/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?

== Reply: Future anthropogenic emissions were projected based on the region-specific

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emission factors from EGAS and extrapolated to 2050 using assumptions consistent with the A2 business-as-usual scenario. Future emissions accounted for estimated population and economic growth, as well as projected energy use by sector. Future emissions did not include recent emission control regulations or major technology breakthroughs that would affect the use of traditional energy. Future mobile source emissions were generated through EGAS based on estimated MOBILE6 VMT growth rates. Mobile emissions estimates considered increases in alternative fuel vehicles and decreases in old vehicle fleets, but the dominant transportation fuels remain gasoline and diesel. Future anthropogenic emissions were also updated with projected population density distributions from the SERGOM model to reflect urban growth.

Future LULC was derived from CLM plant functional types. The CLM future land use included percent coverage of plant functional type. The MEGAN model includes emission factors based on plant functional type. Therefore, MEGAN emission factors were assigned based on the percent cover of plant functional type for a given grid cell. For the future MM5 land use, a cross-walk was created to map the CLM land cover plant functional types together with the urban land cover from SERGOM to the MM5 land use and land cover categories. In this way, the land use used in MM5 was consistent with the LULC used to drive MEGAN.

Any future emissions scenario is highly uncertain. Consequently, we do not attempt to determine the likelihood of our emissions scenario compared to other possible scenarios. Rather, we expect our results to highlight which air quality drivers will be most important under our specific scenario.

== Comment: 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

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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.

== Reply: The authors agree that the synergistic effect among individual global changes is an important aspect to developing a more complete understanding of the relationship between global changes and air quality. However, the focus of this work was on the relative importance that individual global changes may have on air quality relative to each other, not on the synergistic effects of all potential global change combinations. To this end, we believe that our choice of simulations is adequate. Although individual global change impacts on air quality are not additive quantities, it is possible to elicit information about how combining global changes would impact air quality based on the six simulations conducted in this work. For example, summing the change in ozone from our future boundary condition and future anthropogenic emissions scenarios would not be the same as conducting an additional simulation with both future boundary conditions and future anthropogenic emissions. However, we can infer that such a simulation would result in an even larger increase in ozone than observed in either of the original simulations.

In regards to the impact of a future climate on intercontinental transport pathways and tropospheric chemistry, the authors agree that including future climate impacts along with future emissions changes may have resulted in somewhat different chemical boundary conditions. However, due to the large uncertainty associated with projecting any future global change, we believe the boundary conditions used in this work are a good first order approximation and are adequate to illustrate the importance of boundary conditions on simulated air quality.

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

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== Reply: Title has been changed per suggestion.

== Comment: 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 panels 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?

== Reply: After reviewing the manuscript, the authors agree with the comment and have made updates to the manuscript so that it now reads:

In the western half of the US, changes in DM PBL height are directly related to changes in DM surface temperature, where regions with smaller changes in surface temperature (e.g., Texas, California, Oregon) show decreases in PBL heights, and regions with the largest increase in temperature (southwestern states) correspond to the largest increase in PBL height. In the eastern half of the US, the direct relationship between DM PBL heights and DM surface temperature is generally true for the Midwest (excluding parts of Wisconsin) and northeast (excluding parts of New England), while the opposite is true for much of the southeast (excluding most of Florida). In regions where increases in PBL height correspond to increases in temperature, any reduction in air quality due to increased temperatures may be somewhat offset by increases in PBL heights. However, in regions such as the southeast, where reductions in PBL height occur simultaneously with increases in temperature, changes in both temperature and PBL height will tend to reduce air quality.

== Comment: 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?

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== Reply: The effect of increased GHG emissions on climate and air pollution transport pathways was not accounted for in the MOZART-2 modeling. Please see Horowitz (2006) for a description of the meteorological fields used in the MOZART-2 simulations. The authors acknowledge that including the effects of climate change on air pollution transport pathways may have resulted in differences in the chemical boundary conditions used in this work. However, we do not believe that this in any way limits the conclusions of this study. Projecting a future climate and emissions is a highly uncertain endeavor, which means that any modeling work incorporating these types of projections represents only a single realization of a future atmosphere. What we have presented in this work is one possible realization of a future atmosphere based on one possible set of projected changes in climate, boundary conditions, and emissions. If we had used a different set of boundary conditions that reflected potential changes in air pollution transport pathways, then we would have simulated a different realization of a future atmosphere (but no more correct or wrong than what we have presented in this work).

== Comment: 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 fu-

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ture 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?

== Reply: Please see response to the third comment above.

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

== Reply: We have added the following text discussing CMAQ evaluation studies driven by downscaled climate model output:

CMAQ model evaluations for simulations using downscaled climate model output are limited, but suggest that CMAQ is a suitable tool for use in climate impacts on air quality studies. Hogrefe et al. (2004) found that CMAQ performed best for predicting patterns of average and above average ozone concentrations, as well as the frequency distribution of extreme ozone events. Tagaris et al. (2007) found mean 8-hr ozone concentrations were approximately 15% higher than observations, while PM2.5 concentrations were approximately 30% lower than observed. Furthermore, Tagaris et al. (2007) found that PM2.5 model performance is significantly more region specific than mean 8-hr ozone performance.

PM2.5 observational data was obtained from the EPA AQS database  
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(<http://www.epa.gov/ttn/airs/airsaqs/detaildata/downloadaqsdata.htm>). The AQS database contains PM2.5 data measured every day, every 3rd day, and every 6th day.

Performing our analysis by EPA region is consistent with what has been done in other global change regional air quality simulations. For example, Tagaris et al. (2007) performed their analysis based on five regions: West (EPA regions 9 and 10), Plains (EPA regions 6, 7, and 8), Southeast (EPA region 4), Midwest (EPA region 5), and Northeast (EPA regions 1, 2, and 3). In addition, Huang et al. (2008) focused their analysis on four regions: California (EPA region 9), Texas (EPA region 6), Midwest (EPA region 5), and Northeast (EPA regions 1, 2, and 3).

Hogrefe, C., Biswas, J., Lynn, B., Civerolo, K., Ku, J.-Y., Rosenthal, J., Rosenzweig, C., Goldberg, R., and Kinney, P. L.: Simulating regional-scale ozone climatology over the eastern United States: model evaluation results, *Atmos. Environ.*, 38, 2627-2638, 2004.

Huang, H.-C., et al.: Impacts of long-range transport of global pollutants and precursor gases on U.S. air quality under future climatic conditions, *J. Geophys. Res.*, 113, D19307, doi:10.1029/2007JD009469, 2008.

Tagaris, E., Manomaiphiboon, K., Liao, K.-J., Leung, L. R., Woo, J.-H., He, S., Amar, P., and Russell, A.G.: Impacts of global climate change and emissions on regional ozone and fine particulate matter concentrations over the United States, *J. Geophys. Res.*, 112, D14312, doi:10.1029/2006JD008262, 2007.

== Comment: 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.

== Reply: We have added the suggested tables to the manuscript (Tables 3 and 4).

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

== Reply: This statement is a hypothesis that is supported by additional analysis not shown in the manuscript. We still call it a hypothesis, because without invoking process analysis in CMAQ we can only look at atmospheric mixing ratios of the pollutants of interest, and cannot directly follow the chemical and physical processes that lead to those mixing ratios.

We have updated the manuscript so that it now reads "The large decrease in the south and southeastern regions is most likely the result of" instead of "is primarily due to"

== Comment: Pages 15146 - 15147, Section 3.3. This section either should be expanded by including a discussion of the effects of the various drivers on the individual components of PM<sub>2.5</sub> or should be removed. For example, is the increase in PM<sub>2.5</sub> due to emission changes in the Northeast driven by increase sulfate or primary PM<sub>2.5</sub>? 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.

== Reply: We have expanded the PM<sub>2.5</sub> analysis to examine individual PM species.

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

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