Atmos. Chem. Phys. Discuss., 8, S9886–S9899, 2008
www.atmos-chem-phys-discuss.net/8/S9886/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S9886–S9899, 2008

Interactive Comment

Interactive comment on "The effects of global changes upon regional ozone pollution in the United States" *by* J. Chen et al.

J. Chen et al.

Received and published: 15 December 2008

Authors' reply to comment from anonymous Referee 1

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

Comment from anonymous Referee #1 (1) The authors have conducted an assessment of future climate and emissions effects on meteorology and ozone air quality in the United States. They have been more comprehensive than other studies that have considered similar research questions, in particular by modeling/downscaling effects of global scale changes on land cover, biogenic emissions, and meteorological condi-

tions within the United States. Also they modeled spatial patterns of population growth within the U.S., and effects of those changes on land use/land cover. A criticism of the future scenario is that it is unduly pessimistic and unrealistic by failing to include the effects of technology change and emission control rules on U.S. emissions, which will surely be a significant effect between the 1990s and 2050. The authors note their future scenario provides an upper bound on climate change by using the A2 scenario (rapid growth) and neglecting technology change within the U.S. Why not use the U.S. emission forecasts from the A2 scenario rather than assuming no technology change? The authors' assumption of no technology change for the U.S. is not even consistent with the relatively pessimistic A2 scenario.

== Reply: The future modeling scenario was setup to be conservative without major policy shifts and with conservative technology changes such that US energy use and emissions continue in a business as usual manner. Major emission reduction policies such as the 2005 EPA's Clean Air Interstate Rule (currently vacated by the US Court; http://www.epa.gov/cair) were not incorporated in the emission projections. Future US emission projections did consider effects of technology change; however, we assumed no major breakthroughs that would significantly decrease the use of traditional energy. Future emissions for the US were projected based on factors from the EPA Economic Growth Analysis System (EGAS) software. The emission projection factors were applied to the mobile and area emission categories (Table 1 in manuscript). The factors considered regional changes in population, real personal income, real disposable income, employment and estimated future energy consumptions by sectors. For mobile source emissions, the projections were based on vehicle miles traveled (VMT) estimates from the EPA MOBILE6 model. Future VMT projections considered increases in alternative fuel vehicles, decreases in old vehicle fleets; however, the dominant transportation fuels are still gasoline and diesel.

The description on future US emission projections (Sect. 2.2.3) was modified with more details in the revised manuscript.

We believe that our US emission projection to 2050 is in line with that from IPCC SRES for the A2 scenario family. As shown in Table 1, most of the future US emission changes were from Area and Non-road mobile sources. There were minor changes in mobile sources (-1% to -2%) and no changes in point sources from the 1990 base-case. The overall emission projection for the US was relatively consistent with the population increases from IPCC A2 scenario (IPCC SRES, 2000). For the OECD90 region (countries, including US, that were in the Organization of Economic Cooperative Development as of 1990) the A2 scenario had population projection of +35% in 2050 from 840 million in 1990. Similarly, for anthropogenic NOx, NMVOC and CO emissions, the projected increases were 25%, 5% and 31%, respectively. Globally, the projected changes for A2 scenario were much bigger with population increases by 114% from 5.3 billion in 1990 and anthropogenic NOx/NMVOC/CO emission increases by 37%, 53% and 117%.

The conservative emission estimate for the future scenario were explicitly noted in the manuscript (pg 15179) and that the results presented can thus be taken as upper bound on projected US air quality in the future. This approach provides a very useful comparison to other recent air quality/global change studies that assumed a more optimistic reduction in U.S. emissions (Nolte et al., 2008; Tao et al., 2007; Murazaki and Hess 2006).

Murazaki, K., and Hess, P.: How does climate change contribute to surface ozone change over the United States?, J. Geophys. Res., 111, doi:10.1029/2005JD005873, 2006.

Nolte, C. G., Gilliland, A. B., Hogrefe, C., and Mickley, L. J.: Linking global to regional models to assess future climate impacts on surface ozone levels in the United States, J. Geophys. Res., 113, doi:10.1029/2007JD008497, 2008.

Tao, Z., Williams, A., Huang, H. C., Caughey, M., and Liang, X. Z.: Sensitivity of U.S. surface ozone to future emissions and climate changes, Geophys. Res. Lett., 34,

ACPD

8, S9886–S9899, 2008

Interactive Comment

2007.

(2) Another concern is the incomplete model evaluation that has been reported. The authors report model performance for summer months only (JJA), but then consider changes in future ozone and emphasize the importance of shifts in ozone during winter and spring. Table 2 and and Figure 6 need to be expanded to evaluate model performance for all 4 seasons (DJF, MAM, JJA, and SON for instance). The current presentation leaves this reader presuming that model performance was not so good during other seasons. Since there is significant emphasis in the paper on ozone changes during non-summer months, model evaluation for the base case is needed at those times as well. The meteorological evaluation (Figure 4) includes all 12 months, so clearly the authors are thinking about changes over the full year.

== Reply: We agree with the referee that it is important to include more complete model evaluation in this work. Additional analyses comparing base-case modeled ozone values with long-term observations were carried out and reported in the revised manuscript. Additional model performance statistics were compiled for Spring (MAM) and Fall (SON) seasons and briefly discussed in the manuscript. Due to lack of routine ozone measurement data for the winter months (DJF) (measurement data from http://www.epa.gov/ttn/airs/airsaqs/detaildata), model performances for winter months were not available. In the revised manuscript, Table 2 and Figure 6 were updated accordingly with the new analyses results.

(3) It is not clear from the text at the top of p. 15175, whether the changes in land use/land cover that the authors estimated were fed in to the fire scenario builder, or were only the changes in future meteorology from MM5 included? Also there will be important history effects of fire suppression policies and accumulated/remaining tree cover on fire frequency and extent in 2050, so I question whether it is appropriate to jump from the 1990s to 2050 without representing what happens in the intervening years.

ACPD

8, S9886–S9899, 2008

== Reply: Future fire emissions from FSB were driven by changes in MM5 meteorology. The changes in the described landuse and land cover could not be used by the Fire Scenario Builder (FSB) because they do not provide biomass of each fuel type or fuel loading, which are critical for determining fire emission factors. In one way this could be seen as a limitation to our scenario, but this approach provided a more direct accounting of the climate change effects on wildfire regimes and their impacts on air quality. More specifically, we could isolate the effects of a changing climate on the factors that influence fire, i.e., the day-to-day fire-spread potential depends on fine-fuel moisture. It is correct that we did not account for the historical effects of previous fires. Fire initiation and fire size in FSB were modeled stochastically, accounting for fuel moisture and fuel types. More importantly, given the scale of the model at 36-km by 36-km grid area and the typical sizes of wildfires, attempting to specify exact fire perimeters for 50 years of simulated fires would introduce not only a huge computational burden but also cumulative error from false precision. Given the coarse resolution of the meteorological and air-quality modeling, we believe that explicitly incorporating fire history in the model would introduce cumulative errors that would surpass the errors of omission.

(4) Table 2: clarify with footnotes what average ozone conditions vs. episodic ozone conditions mean. Indicate that results shown are for JJA only. Add evaluation data for the other three seasons as separate parts of the table.

== Reply: Table 2 caption was revised to clarify the means of average vs. episodic ozone conditions. The table was also updated with model performance statistics for the Spring (MAM) and Fall (SON) seasons.

(5) Figure 9 needs a legend to indicate the two sets of bars correspond to (I suppose) current and future climate scenarios.

== Reply: Figure 9 was revised with the appropriate legends for the two climate scenarios.

(6) Figure 11 caption says similar to Figure 7, but I think the authors mean to refer to

ACPD

8, S9886–S9899, 2008

Interactive Comment

*Figure 6 instead which is more clearly analogous. For clarity, please indicate in Fig 11 caption that both current and future cases are *modeled* ozone distributions.*

== Reply: Figure 11 was revised with caption and legend as suggested.

(7) It would be helpful for a broad readership to relabel the EPA Region numbers appearing in Figures 5, 6, and 11 using more intuitive and readily understood names. This will help readers who aren't familiar with the details of EPA's somewhat obscure regional organization structure. For example, "Northeast" would work better as a label than "R1-3". Figure 5 does give this information indirectly, but it would be better to be able to understand Figures 6 and 11 without having to refer back to Figure 5.

== Reply: Due to the space available on graphs, we retained the original labels on Fig. 5, Fig. 6 and Fig. 11. However, a reference legend is now added to indicate the geographical regions to the reference EPA Region numbers. Furthermore, the use of EPA Region throughout the manuscript was revised to their meaningful geographical labels.
EPA Region labels and their corresponding geographic labels are:
R1-3: Northeast
R4: Southeast
R5: Northern Midwest
R6: South Central
R7: Southern Midwest
R8: North Central
R9: Southwest
R10: Northwest

(8) In Figure 12, are the number of ozone episodes (in brackets) total episode days or total number of episodes (counting multi-day episodes as 1 episode)? It would be preferable to report the number of episode days, since the distribution of episode

ACPD
8, S9886–S9899, 2008

duration is shifting as well, a comparison of number of episodes is misleading if they are not the same duration between current and future cases.

== Reply: The numbers in brackets on Figure 12 indicate the total number of ozone episode-day across the 10 year simulation periods, not number of episode (counting multi-day episodes as 1 episode), at each location. This is made clearer in the revised manuscript. There is a shift in episode length distribution because the values were normalized by total number of episode-day from each decade. In the future case, the model estimated a shift in percentage of shorter-duration episodes in exchange for more cases of longer-duration episodes. Since this is a change in percentage of the total, the actual number of episode-day in the future is still higher due to higher total number of ozone episodes.

Authors' reply to comment from anonymous Referee 2

We thank the anonymous reviewer for the thoughtful reviews and for given time to improve this article. We have considered the recommendations and made appropriate changes to the manuscript. Our responses to the specific comments are detailed below:

Comment from anonymous Referee #2 Overall Comments: The manuscript describes and demonstrates a modeling framework developed and applied to investigate the impact of global changes on regional air quality in the United States from 1990s to 2050s, particularly focusing on the ozone pollution issues. The manuscript is generally well organized and the results are scientifically sound, considering the large suite of models employed in the study and the complicated couplings involved in the work itself. This work is by-virtue one of the more well-thought studies in addressing global change impacts on air quality since it considers future LULC changes, implements a relatively robust emission growth scheme, and directly applies model results of a dynamic global chemistry model (MOZART) for future boundary conditions in regional simulations. On the other hands, there are a number of areas that can be further clarified (see specific comments below). The manuscript should be accepted for publication after some minor revisions.

Specific Comments:

1. The work involves extensive downscaling of global model results to the regional scale simulations using different modeling systems. It will be helpful if the authors can provide more description and discussion on the potential downscaling concerns caused by the inconsistent model sciences and/or data used in the global model and regional simulations. For example, are the transport schemes and chemical mechanisms consistent in both models (MOZART-2 and CMAQ)? The two models use different emission inventory data sets in the simulation, are the emission inventories within the US consistent with each other? Is the same emission growth method applied for both sets of

ACPD

8, S9886–S9899, 2008

Interactive Comment

grown emission inventories? Are the same LULC data in the US employed for both global and regional simulations? These questions should be explicitly clarified in the manuscript. If there is any inconsistency, what would be its potential impact?

== Reply: Additional descriptions and references were added in the revised manuscript in Sec 2 and Sec. 2.22.

Model downscaling between global and regional models were not discussed in detail because work similar to this has been performed and discussed elsewhere (Tong and Mauzerall, 2006; Barna and Knippin, 2006; Appel et al., 2007; Huang et al 2008). For example, Barna and Knippin (2006) and Tong and Mauzerall (2006) described the downscaling between GOCART / REMSAD and MOZART-2 / CMAQ models, respectively, and they demonstrated that using global model output as boundary conditions for regional air quality models improved the model performances. More recently, Huang et al. (2008) applied the same model downscale approach to study the effects of long-range pollution transport affecting the US ozone conditions in the future. They showed significant result differences with and without considering global pollution transport within the regional model. Similar to our approach, these studies coupled the global and regional models by having global models provide spatial varying, time-stepping lateral boundary conditions into regional air quality models.

Due to the model scale difference, global model (MOZART-2) and regional model (CMAQ) have different science algorithms, including chemical mechanisms and transport schemes. In this study, the differences in chemical mechanism were handled by explicitly matching the model chemical species. Non-organic compounds (O3, NOx, CO) were explicit to both models and were matched directly. Volatile organic compounds (VOC) were matched by their atmosphere reactivity (reactivity towards OH radical). Chemical concentrations were read in directly at overlaying grids between the CMAQ lateral boundaries and the MOZART-2 domain. Similar approach was taken for vertical layers. Vertical layers between the two models were matched using atmospheric pressure as reference coordinate. Since the main purpose of this downscaling

ACPD

8, S9886–S9899, 2008

Interactive Comment

was to capture long-distance transport of chemicals into the US from other continents, no information within the US continent were used from MOZART-2 to CMAQ. This also ensures that the differences in model science and input data in MOZART-2 and CMAQ, such as LULC, and emissions would have minimal impact on the regional model results.

Barna, M, and Eladio, Knipping: Insights from the BRAVO study on nesting global models to specify boundary conditions in regional air quality modeling simulations, Atmos. Environ., 40, 574-582, 2006.

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.

K. Wyat Appel, Alice Gilliland, Golam Sarwar, Robert Gilliam,: Evaluation of the Community Multiscale Air Quality (CMAQ) model version 4.5: Sensitivities impacting model performance: Part I-Ozone, Atmos. Environ., 41, 40, 9603-9615, 2006.

Tong, Daniel, and Denise, Mauzerall: Spatial variability of summertime tropospheric ozone over the continental United States: Implications of an evaluation of the CMAQ model, Atmos. Environ., 40, 17, 3041-3056, 2006.

2. For a modeling assessment study performed at such a time scale (>50 years), there should be a dedicated section discussing the causes and magnitude of uncertainty in the model results to better evaluate the conclusion of the study. Obviously, the assessment of the coupled uncertainty by many models employed in this study can be tedious. However, without any indication of model uncertainly, it is difficult to distinguish between signals and noises.

== Reply: One of the main objectives in this model study is to capture the overall global change signals impacting regional air quality and minimize noise from day-to-day variability. Long-term simulations over 10-years were performed across the modeling

ACPD

8, S9886–S9899, 2008

Interactive Comment

system (global-, regional- scale models) and evaluated against long-term observational data. The purpose of the long-term simulation is more explicitly stated in the revised manuscript (Sec. 2).

Due to the large amount of data from the model output, it is difficult and uninformative to present results in any finer temporal resolution than monthly averages. In this study, the frequency of model outputs was hour averages. We try to qualitatively show the signals and noises from the model output by presenting results (signal) together with model variability in terms of monthly record scatter, range of standard deviations as well as range of percentile values (i.e. Fig. 4, Fig. 6, and Fig 7). The scattered plot in Fig 4 from MM5 demonstrated the model noise from year to year. Similar plots was constructed for ozone, but was not shown in the manuscript as more information could be conveyed with whisker plots as in Fig 6. In terms of model sensitivity towards major model inputs, additional model sensitivity study with changing meteorology, emissions, chemical boundary conditions and LULC were carried out and presented in a companion paper.

J. Avise, J. Chen, B. Lamb, C. Wiedinmyer, A. Guenther, E. Salathé, and C. Mass: Attribution of projected changes in US ozone and PM2.5 concentrations to global changes, Atmos. Chem. Phys. Discuss., 8, 15131-15163, 2008.

3. I would like to echo the concerns from the other reviewer that considering the A2 scenario and simply regarding the model results as the worst-case scenario would be somewhat misleading. I suggest that the authors provide some discussion, at least in qualitative terms, regarding how the model results would be different if other emission scenarios or emission control advancement were considered. I also agree with the other reviewer's comments that model evaluation data in the cold months should be presented.

== Reply: The description of future US emission projections was modified with additional detail in the revised manuscript (Sect. 2.2.3). As mentioned in the reply to

ACPD

8, S9886–S9899, 2008

Referee 1, the model scenario was setup to be conservative without major shifts in US energy policy and US energy usage. Please see additional comment response to Referee 1.

The US emission projection scenario is consistent with the global IPCC A2, business-as-usual storyline. Future US projections considered regional changes in population, economics, and estimated future energy consumptions by sectors. With regards to technology changes, the study considered the effect of future emission controls for mobile vehicles; however future emission scenario assumed no major technology breakthroughs, and possible emission control policies. Future VMT projections considered increases in alternative fuel vehicles, decreases in old vehicle fleets. The dominant transportation fuels in the future scenario are still gasoline and diesel.

We agree that it is importance to include model evaluation data for the cold months. Additional model comparisons with observational data were performed and discussed in the revised manuscript (Sec. 3.2, Table 2, and Fig. 6).

4. On page 15179, Line 21-23. It would be nice if more quantitative information regarding the difference in ozone concentration change if LULC were to remain unchanged.

== Reply: The focus of this paper is on collective global change impacts on future regional ozone condition. Sensitivity cases such as LULC change impacts to future projected ozone conditions were thus not emphasized. In a companion paper by Avise et al. (2008), the issue of LULC impacts on projected future ozone conditions was analyzed further. In the study, when LULC was varied with constant future meteorology, present-day anthropogenic emissions and present-day chemical boundary conditions, future change to July-averaged DM8H ozone across US was doubled. Changing future LULC enhanced the projected decreases in average July DM8H ozone under present-day emissions condition. The readers are referred to the companion paper for additional discussion on this topic.

J. Avise, J. Chen, B. Lamb, C. Wiedinmyer, A. Guenther, E. Salathé, and C. Mass: Attri-

ACPD

8, S9886–S9899, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

bution of projected changes in US ozone and PM2.5 concentrations to global changes, Atmos. Chem. Phys. Discuss., 8, 15131-15163, 2008.

5. On page 15179, last paragraph. The paragraph states that the frequency of ozone concentration exceeding the 8-hour ozone standard will be increasing. What's the air quality implication from this? It will be useful if the authors can also discuss how the ozone non-attainment situation would be changed based on the model results and the locations that would be classified as non-attainment because of the increased ozone concentrations.

== Reply: Since the base case (1990-1999) model results were not evaluated with respect to ozone attainment designations for the present-day, future changes in ozone attainment were not presented in detail in terms of specific locations that would be in non-attainment.

We tried to put the model results in the context of the newly established 75 ppbv ozone standard by providing simple quantitative comparisons on the possible increases in ozone attainment violations. The temporal comparison in terms of frequency of ozone exceeding the standard was presented here. Increase the frequency of ozone exceeding 75 ppbv increases the chance that a region would be classified as non-attainment since attainment is determined by the 4th highest annual DM8H ozone averaged over 3 years. This relation is noted more clearly in the revised manuscript. In terms of non-attainment areas, or the spatial impacts, the model estimated a 38% increase in regions where ozone would exceed the 75 ppbv standard at least once per year, and 79% increase in areas that would exceed the standard at least four times a year (Sec. 3.5 - 3rd paragraph). Under current model scenario, there is high possibility that more areas in the future would be designated as non-attainment with the federal air quality standard.

6. Page 15180, last paragraph. The information presented here need to be more specific. For example, it is stated that the DM8H ozone would be increased by 10 to

ACPD

8, S9886–S9899, 2008

20 ppbv in most urban areas and by 2 to 10 ppbv in rural areas. Does the difference represent the average maximum increase in 2045-2055 in summer season? Or does it represent the maximum of the increases in the 10 years? What are the primary causes for the changed ozone concentrations in different location?

== Reply: The information is made clearer in the revised manuscript. The changes in DM8H ozone were to specifically address the results presented in Fig. 10, which showed a difference plot of average DM8H ozone mixing ratio for the summer months (future minus base case). The DM8H ozone increases of 10-20 ppbv in urban areas and 2-10 ppbv in rural areas were thus, averages for the summer season (averaged over 10 summers) and not yearly difference.

Since the ozone differences were resulted the collective global change impacts considered in the study, the causes cannot be isolated and addressed. The multiple impacts that caused in the ozone changes were noted explicitly in the manuscript: " The combined effects of higher global and US emissions, expansion of urban areas, and warmer summer temperatures will potentially cause much of the US to experience higher ozone condition in the future; The differences in ozone change are due to combinations of local emissions, environment conditions as well as collective global changes. "

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15165, 2008.