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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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Comments on "The effect of global changes upon regional ozone pollution in the United States" by Chen et al. (ACP Discussion, 8, 15165-15205)

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 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?

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.

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 emis-

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sion 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.

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.

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.

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 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?

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

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Interactive Comment