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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An analysis of long-term regional-scale ozone simulations over the Northeastern United States: variability and trends" *by* C. Hogrefe et al.

C. Hogrefe et al.

chogrefe@asrc.cestm.albany.edu

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We would like to thank the reviewer for the careful review of our manuscript. While we do not share the reviewer's main point of criticism that our manuscript is outside the scope of ACP, we agree that the model simulations described and analyzed in the manuscript could form the starting point for a future study focused on the issue of increasing background ozone concentrations. As noted by the reviewer, such a study would lead to a manuscript very different from the current one, and we may consider such a manuscript as part of our future work. We also appreciate the reviewer's comment that the work presented in our manuscript does constitute a novel contribution to

the field of model evaluation because of the length of the simulation. Our responses to the points raised by the reviewer are shown below.

Reviewer Comment: This paper provides an ambitious 18-year model simulation of ozone trends in the northeastern United States. The main goal as stated in the Introduction and the Conclusions is to provide illustrative examples of how model performance can be evaluated against available observations and to identify key inputs and processes that need to be considered when performing and improving such long-term simulations. Overall I think the authors have succeeded in providing a thorough evaluation which stands out from other studies due to the length of the simulation. My main concern with the paper is that I don't think that ACP is the appropriate journal for this study. The stated aim of ACP is: An international scientific journal dedicated to the publication and public discussion of high quality studies investigating the Earth's atmosphere and the underlying chemical and physical processes. . . . In its present form the paper is much more focused on evaluating model performance rather than answering a specific scientific question. . .. if the authors wish to publish in ACP then they need to shift the focus of the paper away from model evaluation to answering a scientific question. To me it seems that the paper could be well suited to answering the following important science question: "What is causing the increase in the ozone 5th percentile over the northeastern USA?" . . .. A paper such as this would be an important step forward in our attempts to understand the influence of changing baseline ozone on surface air quality, and would fit well with the aim and scope of ACP. This recommendation would of course require a major revision and the paper would be quite different than the one now under review. For this reason I recommend that the paper be rejected from ACP so that it can be re-worked into a paper that addresses a science question

Authors' Response: We disagree with the reviewer that studies focused on model evaluation are outside the scope of ACP. Atmospheric modeling is one of the main subject areas of ACP, and model evaluation is a core component of atmospheric modeling. A search of ACP articles published in the last few years yields a number of articles on model evaluation (e.g. Herron-Thorpe et al., 2010; Mao et al., 2010; Yu et al., 2010; de Foy et al., 2009; Lamquin et al., 2009; Matthias, 2008). Therefore, we do not believe that the manuscript should be rejected from ACP because of its focus on model evaluation. However, we do appreciate the reviewer's suggestions for future work aimed at investigating the role of increasing background ozone on summertime ozone distributions in the Northeastern U.S., and we are providing a separate response to these suggestions below.

Reviewer Comment: An interesting experiment would be to allow the baseline ozone in the present study to increase at the same rate as the observations in the western USA. Would this then produce an increase in the modeled 5th percentile? I also recommend comparing the model to a different set of surface observations. Most of the EPA ozone monitors are in urban locations where local NOx titration could complicate the author's ability to examine the influence of baseline ozone. Rural ozone monitors at elevated sites such as Whiteface Mountain, or the National Park monitors on mountain tops in Shenandoah National Park or Great Smokey Mountains would provide regional background ozone measurements well suited for comparison to the regional scale model and better suited to explore trends of the ozone 5ht percentile (at least in terms of how the changing baseline ozone affects the metric)

Authors' Response: These questions and suggestions provide a good starting point for potential future work. The reviewer suggests testing whether an additional 18-year model simulation using chemical boundary conditions based on time-varying ozone concentrations observed in the western U.S. would improve model performance at the 5th percentile. While it is outside the scope of the present study to perform such additional simulations and analyze them, Figure 10b presented in the current manuscript illustrates that the chemical boundary conditions do have a significant impact on modeled trend estimates at all percentiles. Therefore, we would expect that the simulations suggested by the reviewer, with ozone boundary conditions based on actual observed data, would show an improvement of model performance at the 5th percentile. The

reviewer is also correct in pointing out that some of the increase in the observed 5th percentile of summertime ozone may be due to localized effects, notably a decrease in the amount of NOx titration over time. Such localized effects caused by the location of the AQS monitor appear not to have much effect on the model simulations. Figure 10b suggests that, at least for the model predictions, trends at the 5th percentile are strongly impacted by large-scale features specified through the chemical boundary conditions. Therefore, the reviewer suggests to expand the analysis of observed and simulated ozone trends at additional locations more reflective of rural and background conditions. We have followed this suggestion and performed ozone trend analysis at nine CASTNet monitors located within the 12 km CMAQ domain. The results are qualitatively similar to the information shown in Figure 10b, i.e. the observed 5th percentile shows an upward trend while the CMAQ/STATIC simulations show a slight downward trend and the CMAQ/ECHAM simulations show a strong downward trend. A brief discussion of these preliminary results will be added to the revised manuscript.

Reviewer Comment: Figure 1, UAH is in Alabama, not Tennessee as shown on the map.

Authors' Response: We thank the reviewer for alerting us to this error. We revisited the station metadata file and discovered that the latitude/longitude information stored in the file was incorrect. We will correct the location of the UAH site in Figure 1 of the revised manuscript, and we have re-extracted the model data for the comparisons shown in Figures 13 and 14 to reflect the correct location of the launch site. The impact on the comparisons was negligible, and we will include the updated Figures in the revised manuscript.

Reviewer Comment: Table 4. I was under the impression that the EPA CO monitors were fairly imprecise and only report in units of ppm, or tenths of a ppm. Your table shows CO values with 4 significant digits. Are the instruments really this precise, or is this just an averaging artifact?

Authors' Response: The reviewer is correct in pointing out that EPA CO monitors only report in units of ppm or tenths of a ppm. The additional digits shown for the observed concentrations and trends in Table 4 are introduced by calculating the long-term averages. We will add this information to the revised manuscript. This table will also be revised to address comments raised by Prof. D. Cohan.

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