

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.

Anonymous Referee #2

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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

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and public discussion of high quality studies investigating the Earth's atmosphere and the underlying chemical and physical processes.

The stated aim of ACP's sister journal, Geoscientific Model Development (GMD) is: An international scientific journal dedicated to the publication and public discussion of the description, development and evaluation of numerical models of the Earth System and its components. Manuscript types considered for peer-reviewed publication are: * Geoscientific model descriptions, from box models to GCMs; * Development and Technical papers, describing development such as new parameterisations or technical aspects of running models such as the reproducibility of results; * Papers describing new standard experiments for assessing model performance, or novel ways of comparing model results with observational data; * Model intercomparison descriptions, including experimental details and project protocols.

In its present form the paper is much more focused on evaluating model performance rather than answering a specific scientific question, and therefore is much more suited for publication in GMD rather than ACP. However, 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?”

Several recent papers have shown that the baseline ozone flowing into western North America is increasing at both the surface and in the free troposphere [Jaffe and Ray, 2007; Parrish et al., 2009; Cooper et al., 2010]. Interestingly a global modeling study has trouble reproducing the observed rate of increase [Lamarque et al., 2010]. Given that the western USA is immediately upwind of the northeastern USA, an increase in baseline ozone over the western USA would mean an increase in the baseline air flowing into the northeastern USA. While decreasing ozone precursor emissions in the eastern USA seem to explain the decrease in the daily 8-hr maximum, could the increase in baseline ozone be the cause of the increase in the 5th percentile? An

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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 NO_x 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 5th percentile (at least in terms of how the changing baseline ozone affects the metric). 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, or it should be sent for consideration by the journal GMD where I think the current version would be well received.

Minor comments Figure 1, UAH is in Alabama, not Tennessee as shown on the map.

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?

References

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