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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Response of surface ozone to climate change over the Eastern United States" *by* P. N. Racherla and P. J. Adams

Anonymous Referee #2

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General Comments:

The authors use a three-dimensional global chemistry-climate model to examine changes in eastern U.S. ozone episodes from the 1990s to the 2050s under SRES A2 scenario. Annual, spatially averaged ozone mixing ratios change little, although the frequency and severity of ozone episodes increase. Up to 60% of the increase is attributed to an increase in isoprene emissions. In contrast to earlier work, little evidence is found for an increase in ozone episodes due to circulation changes. The study extends these prior studies by examining the seasonality (and changes therein) in ozone episodes. Two key conclusions are that the ozone season lengthens into spring and fall, and that at least 5 years of simulation are needed to separate out interannual variability from the true climate change signal on ozone episodes. The manuscript should

be suitable for publication after the concerns outlined below have been addressed.

Specific comments: 1. Some analysis/discussion is needed as to the ability of the model to simulate ozone episodes (their frequency and intensity) as observed over the eastern United States.

2. The conclusion that at least 5 simulation years are needed to separate out the signal of climate change on ozone episodes from the interannual variability has important implications for future modeling studies. How robust is this conclusion to the specific climate change scenario applied here? In particular, if a less extreme warming scenario were chosen, would additional years be needed to extract the signal?

3. Since the annual mean ozone changes little, and higher isoprene is responsible for much of the increase in the extreme ozone concentrations, is there a discernable increase in mean concentrations during summer (or the ozone season) that is masked in the annual mean by a decrease in mean values during the rest of the year?

4. The term “ozone season” is used in the text and defined in Table 1 as May-September, but ozone episodes occur from March-October (Figure 6). How was the May-Sept period chosen? I recommend including some discussion of the ozone season when the results in Section 3.1 are presented, since the annual mean focus hides a large seasonality as evident from Figure 6.

5. The study nicely quantifies the role of changes in isoprene emissions as causing up to 60% of the increase in future ozone episodes. I’d like to see more discussion of two uncertainties: (1) p. 9881 how the conclusions might be expected to change if isoprene nitrates were allowed to act as a sink for NOx, and (2) the suggestion on p. 9878 that the remainder of the ozone episode increase is driven by temperature (noting that the isoprene emission changes are also driven by temperature) - is this intended to refer to the direct effect of temperature on reaction rates, or the more general relationship between temperature and changes in other meteorological factors (such as stagnation, humidity, PAN decomposition)? The study of Steiner et al. (JGR, 2006) may be useful

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in this discussion; they separated the roles of temperature change on reaction rates vs. biogenic VOC emissions vs. humidity for three urban regions with different NOx levels in northern California.

6. p. 9878 lines 16-20 Could the change in the NOx lifetime be quantified? Is it expected that this result is sensitive to the treatment of the isoprene nitrates?

7. p. 9879 lines 21-22 The lengthening of the ozone season is interesting, and would be exacerbated by future growth in global anthropogenic emissions which are also expected to increase intercontinental transport of ozone to the U.S. in spring and fall.

8. Are all the panels in Figure 4 necessary? I recommend either explaining the significance of the selected model boxes or simplifying to one illustrative panel.

9. For the results in Table 2, is the model boundary layer well mixed such that the results are robust to whether the surface layer or the entire boundary layer are considered?

10. More specific information would help to clarify in a few places where prior literature is referred to:

p. 9870 lines 5-8 Which climate scenario did Hogrefe et al impose?

p. 9870 lines 15-20 Do these results apply only to the ozone season?

p. 9870 lines 24-25 Does this apply only to the eastern United States?

p. 9873 lines 18-19 The Horowitz et al. study states in the abstract that complete recycling (and slow deposition rates) are incompatible with the observations.

p. 9875 lines 5-10 Is ozone (or aerosols) included in the climate forcing scenario?

p. 9877 lines 10-15. How different are the driving SSTs in these studies? The method used here to diagnose changes in summertime cyclone frequency seems quite similar to that used by Murazaki and Hess [2006]; some further analysis/discussion as to what

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might be causing this discrepancy would be useful.

11. Comments on the abstract:

The averaging time period (i.e., decadal mean?) for the “zero change in spatiotemporally averaged ozone mixing ratios” should be given.

The isoprene contribution to increased episodes could be quantified.

It should probably be clarified that the “increased chemical production and shorter average lifetimeĚ” results are specific to the eastern U.S. and which season these results apply to (summer and fall from Table 2? Dry deposition doesn’t seem to be important in spring)

Finally, the authors may wish to point out the relevance of the increases in the 95th percentile / 80-90 ppbv range to attaining the air quality standard for ozone.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9867, 2007.