

Interactive comment on “Average versus high surface ozone levels over the continental U.S.A.: Model bias, background influences, and interannual variability” by Jean J. Guo et al.

Anonymous Referee #3

Received and published: 11 April 2018

This paper presents a comprehensive modeling analysis of surface ozone and the various factors that contribute to its variability over the United States. By conducting multiple sensitivity simulation removing various sources for the 2004–2012 period, the authors estimate the influence of different background sources and of U.S. anthropogenic sources on mean surface O₃ and high O₃ events as a function of region, season, and year.

Two aspects of the paper that I'd like to see more discussion on are listed below:

1) The paper is very detailed with many figures and tables and is one more study on top of a rich set of published work, including by some of the co-authors. The authors

[Printer-friendly version](#)

[Discussion paper](#)



often cite previous work, saying it is consistent with their results, but it would be useful to highlight what are the new key contributions from their specific analysis. What new information did the detailed modeling analysis bring to this prolific field?

2) There isn't much discussion on the causes of the large summer bias over the Eastern US and how this bias affects the interpretation of the results. Discussing this in more detail would strengthen the paper. The authors have one sentence addressing this by referring to the work of Travis et al. (2016) using a more recent version of the GEOS-Chem model. They mention potential errors in anthropogenic NO_x emission in the NEI inventory, but Travis et al. use the NEI 2011 inventory while the authors use the NEI 2005 inventory. How different are they? If the NEI NO_x inventory is indeed too high, how would that affect the calculation of O₃_USA? They mention meteorological factors associated with boundary layer mixing and cloud cover which would affect the vertical distribution of O₃, but Travis et al. used different meteorological fields (GEOS-FP) compared to the MERRA fields used by the authors. It is unclear whether these potential explanations apply in this case. If MERRA meteorology is indeed biased, then that would certainly affect the validity of the relative influence of various sources on the "most-biased" days analysis and on the average MDA8 O₃ levels. A discussion of this would be valuable.

Minor comments:

Line 154. "Anthropogenic emissions. . . are scaled each year on the basis of economic data". It would be useful to have a bit more discussion on how anthropogenic emissions are scaled over the continental U.S. which uses 2005 as the baseline. By how much do NO_x emissions change over the time period of the simulation 2004-2012. Are these scaling factors taken from the NEI trends report (<https://www.epa.gov/air-emissions-inventories/air-pollutant-emissions-trends-data>) itself or was independent estimate done?

Line 195. "a maximum in and underestimate springtime. . ." is "summer" missing after

Printer-friendly version

Discussion paper



maximum?

Line 196. While the authors talk about potential causes for the springtime underestimate (stratospheric intrusions), they do not talk about the summertime overestimate, which is quite large.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-115>, 2018.

ACPD

Interactive
comment

Printer-friendly version

Discussion paper

