

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 #2

Received and published: 29 March 2018

General:

The paper is very well-written and concerns a topic of considerable interest to air quality planners. However, there are some concerns about the suitability of this particular model configuration to address some of the stated objectives of the paper (lines 76-79), as discussed below. In general, the paper would be improved if there was greater clarity about the potential connections between the findings and possible configuration concerns. The value of the paper would be enhanced if the conclusions section was bolstered with a “next steps” or “considerations” sentence or two that described how such a global model-based sensitivity study could be improved in the future.

[Printer-friendly version](#)

[Discussion paper](#)



In particular, there is concern about the use of a coarse resolution model (2 x 2.5 deg) to investigate contributions of U.S. anthropogenic emissions (O3_USA) given that those contributions originate at scales much smaller than the resolution of the model (i.e., point source emissions, urban area emissions). The paper acknowledges the limitations associated with the coarse modeled resolution in several places (lines 212, 242, 399). The paper may want to revisit these caveats in the conclusion and perhaps provide some thoughts on what alternate global model configurations would be better suited for an analysis of source contributions.

Kudos to the authors for providing sufficient detail regarding the performance evaluation to allow readers to interpret the contribution findings in light of the model bias/error. However, the ozone overestimations (3-14 ppb in JJA MDA8 top 10 days by region, even worse for JJA all-day averages) suggest caution should be exercised in over-interpreting the contributions. Based on Figure 5 and the associated analyses, it appears that the model vastly overestimates ozone on hot days in the late summer, especially in the eastern U.S. (even without consideration of potential additional emissions due to increased power demand on those days). Section 3.3 briefly summarizes potential causes for this overestimation based on similar studies, but it would be valuable if the paper provided more application-specific hypotheses for the underlying cause. FYI, along w/ the possible causes from the Travis research, others have raised concerns about MEGAN biogenic VOC estimates (e.g., Bash et al., 2016; Carlton and Baker, 2011; Kota et al., 2015; Wang et al., 2017).

One of the more noteworthy findings concerns the modeled trends over the 10-year period (e.g., lines 386-389) where the analysis appears to confirm previous findings that improving trends in U.S. air quality from emissions controls have been tempered by increases in background contributions (and increases in temperature). However, one interesting finding here that could use additional explanation is the regional breakout of this “USB vs. USA” tradeoff. Table 5 suggests that the largest increases in high JJA-day O3 USB concentrations between 2004-2006 and 2010-2012 have occurred in the

[Printer-friendly version](#)[Discussion paper](#)

New England and Mid-Atlantic regions, not the western regions where USB concerns are typically greatest. More explanation of the regional differences in modeled USB trends would be beneficial (e.g., is this just an artifact of the meteorology of the two 3-year periods in these regions).

Given model performance findings, would the authors see value in revising the “2-step” contribution analysis (assessing contributions on high-bias days, then assessing contributions on high/all observed days) to a “3-step” contribution where as an intermediate step you also investigated contributions on top-10 modeled days? This could be valuable presuming that the subset of days would differ from top 10 highest bias days.

Rather than lumping the Mount Bachelor observations (and subsequent pairs) with surface sites in Region 10, it would be interesting to see how model contributions varied as functions of model performance and observation concentration as a standalone site.

Specific:

Line 86: “download” should be “downloaded”.

Lines 124-127: Would be easier to read, if a new sentence was started w/ “On the days with ...”.

Line 146: “Avian” should be “Aviation”.

Line 195: Is the word “summer” missing from this sentence ... “The model, however, has a maximum in [summer] and underestimates springtime baseline O₃”?

Line 205: Are Travis et al. (2016) conclusions regarding 2011 NEI relevant to a model configuration based on 2005 NEI w/ annual scalars?

Line 248: Clarify that these monthly averages are MDA8 O₃ (not hourly)?

Line 367: Move mention of lack of daily variation in emissions to early section?

Line 396: Same as above, maybe mention this earlier in modeling methodology sec-

[Printer-friendly version](#)[Discussion paper](#)

tion?

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

ACPD

Interactive
comment

Printer-friendly version

Discussion paper

