

2nd Review of "Direct Measurements of Ozone Response to Emissions Perturbations in California" by Shenglun Wu et al.

MS Number: acp-2021-708

Summary:

This paper aims to characterize the overall ozone response to NO_x and VOC emission reductions in California by synthesizing satellite data with the results of experiments that perturbed the ambient photochemistry through in-the-field smog chamber runs. This is an interesting study that provides a useful addition to our understanding of ozone formation in California. The authors have done a commendable job of addressing the two major shortcomings in the description and interpretation of the smog chamber experiments that concerned me in my first review. I am impressed by the sensitivity analysis presented in Section 4.1; that analysis presents a balanced discussion of the usefulness of that approach, as well as its limitations. I recommend that the paper be accepted after the authors address the minor issues discussed below.

It should be noted that my expertise is in laboratory and field measurements, and that I have little expertise in the interpretation of satellite data. The discussion of the TROPOMI measurements are compelling to me, and it seems to be supported by references, which I have not reviewed. I would be more comfortable if the satellite data discussion were reviewed by someone with the necessary experience to thoroughly review the interpretation.

Minor Issues:

1. Line 26 of the text has the phrase "... with baseline chamber O₃ concentrations above 90 ppb ...". I suggest that this sentence and the following sentence be reworded to reflect the discussion of Figure 6 based on ambient MDA8 ozone, rather than on the chamber ozone. I think this would make the information in the abstract more policy relevant, and better reflect the discussion in the paper.
2. Line 88: Should "source" be plural?
3. I suggest adding a sentence to the end of Section 2.4 that mentions the extensive sensitivity tests that were performed to ensure the relevance of the results, and will be discussed in Section 4.1
4. The discussion of Figure 2 in Section 3.1.1 requires improvement. It is noted that good agreement is observed between the time trends of the chamber and TROPOMI satellite remote sensing measurements. It is also suggested that the upward trend in NO₂ concentrations in October–December, 2020 is likely associated with decreased boundary layer heights and increased fuel consumption for heating during the colder fall – winter season. However, the satellite measurement is a column measurement; the in situ chamber NO₂ concentrations would depend upon boundary layer height, but a true column measurement would be independent of the boundary layer height. A more accurate discussion is required here, and that discussion should fully consider the averaging kernel of the satellite column measurement.
5. The discussion in the preceding comment also applies to the comparison between the in situ and satellite column measurements of HCHO and CO*Biogenic; this discussion also should

be improved.

6. The caption of Figure 3 mentions “variance”, a term that has a specific statistical meaning. I suggest replacing it with “variation”.
7. The figures are mis-numbered: two numbered 6 and figs. 8 and 9 incorrectly labeled Figs 7 and 8. The text does refer to the correct numbering.
8. In Figure 10, the cities in the right graph are not all in SoCAB; some are in the Salton Sea AB; I also think that it would be useful to include the Salton Sea AB in the left graph,
9. Line 460: please correct figure number and tense “Figure 10 shows that the chamber model can accurately predicted the measured”
10. Line 464: should refer to Fig. S9.
11. The sentence beginning on Line 492 is easy to misinterpret; I suggest rewording: “Increasing the magnitude of the NO_x perturbation increased the absolute magnitude of the ΔO₃ value but did not shift the chemistry into a different regime.”
12. The last sentence in Section 4.1.4 should be amplified slightly; I suggest rewording: “It should be noted that operation of the mobile smog chamber system in cities with higher ambient NO_x concentrations is expected to give O₃ sensitivity results that are even less dependent on the NO₂ perturbation size.
13. Line 513: I suggest that the sentence begin with “Current California ...”, since California has a history of addressing a great many precursor emission sectors.
14. I would strongly argue that the sentence beginning on line 518 is inaccurate. Ambient measurements indicate that over decades VOCs and NO_x have decreased at average annual rates of about 7.5% (Warneke et al., 2012) and 2.6%, respectively. Over 30-years (1980 to 2010) those rates correspond to decreases of factors of 10 and 2.2, respectively. These are based on measurements in the SoCAB, but are relevant for the entire state. The Cox et al. and Rasmussen et al. references rely on emission inventories for their estimates, which are far inferior to actual ambient measurements. In my view it is important that the tremendous success of emission control efforts is highlighted at every opportunity. At the very least, this paper should discuss both the inventory and ambient measurement estimates of emission decreases.

Reference:

- Pollack, I. B., T. B. Ryerson, M. Trainer, J. A. Neuman, J. M. Roberts, and D. D. Parrish (2013), Trends in ozone, its precursors, and related secondary oxidation products in Los Angeles, California: A synthesis of measurements from 1960 to 2010, *J. Geophys. Res. Atmos.*, 118, 5893–5911, doi:10.1002/jgrd.50472.
- Warneke, C., J. A. de Gouw, J. S. Holloway, J. Peischl, T. B. Ryerson, E. Atlas, D. Blake, M. Trainer, and D. D. Parrish (2012), Multiyear trends in volatile organic compounds in Los Angeles, California: Five decades of decreasing emissions, *J. Geophys. Res.*, 117, D00V17, doi:10.1029/2012JD017899.