Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-50-RC1, 2022

Comment on acp-2022-50

Anonymous Referee #1

Referee comment on "Observation-based analysis of ozone production sensitivity for two persistent ozone episodes in Guangdong, China" by Kaixiang Song et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-50-RC1, 2022

The authors developed an observation-based method to investigate the ozone production efficiency and ozone production sensitivity to precursors for two persistent ozone episodes in Guangdong, China, based on the hourly surface O_3, $PM_{2.5}$, CO and NO_2 data at 77 stations in Guangdong during the period 2018-2019. They also performed a box model constrained by ambient conditions observed during the two episodes for comparison. They find 67% of the station-days exhibit ozone formation sensitivity to NOx, which differs from other previous studies which suggested that limiting VOC emission rather than NOx would be more effective in reducing ozone in Guangdong, and these results are in semi-quantitative agreement with the results calculated by box model. The authors make some arbitrary assumptions and simplifications in derivations of VOC and OH concentrations, which is a major weakness of the current work. I had a number of specific comments for the authors to consider and address before publication.

Specific comments:

- Specific information on the ratios of VOC/CO that is used in this study for the derivation of VOC is better added in the SI. Also, the uncertainties due to this treatment of VOC on OH concentration should be discussed.
- Line 80-81, the author uses the same way to evaluate the leftover VOC as to evaluate the leftover CO in the following day. However, VOCs can continue to be oxidized by OH and NO_3 in the afternoon and at night. How great are the effects of this neglect of the depletion of leftover VOC on the derived VOC and OH concentrations in the following day?
- Line 81-82, the authors state that the oxidized VOC are estimated from the observed ratios of HCHO, CH_3CHO, and ketone to CO in Wang et al., 2016, and other OVOCs are not included. What is the basis for this treatment? Besides, HCHO, CH_3CHO, and ketone are not only photochemical products of VOC oxidation but also from direct

anthropogenic emission. How do the authors deal with the difference in emission-related origin of OVOC among different locations? Given that OVOCs typically make large portion of OH reactivity, the estimations of OVOCs are crucial for the simulation of OH concentrations. How large are the uncertainties of these assumption on the predicted OH concentrations?

- Line 85-112, the derivation of OH concentrations and calculation of oxidized VOC and NOx in this study are all based on the Lagrangian condition assumption, which rarely exists in the real atmosphere, so the authors make a selection criterion to filter out days satisfy the quasi-Lagrangian condition. What is the basis of this selection criterion?
- Line 122-126, why the product of the average OH at noontime and the mean NOx in 13:00-16:00 can be used as the hourly NOx emission rate between 08:00 and 13:00?
- Line 311-315, these sentences are totally a copy of the sentences in line 78-83, and do not provide any useful information, and I would like to suggest the authors to delete these sentences, and provide more useful information about the uncertainty analysis.
- I would like to suggest the authors can review other literatures reporting the ozone production efficiency in PRD areas to strengthen the discussion.
- Considering that all the VOCs data are estimated based on the ratios of VOC/CO in the emission inventory, I think it is not appropriate to name it an observation-based method.