Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-834-RC1, 2021

Comment on acp-2021-834

Anonymous Referee #1

Referee comment on "Insights into the significant increase in ozone during COVID-19 in a typical urban city of China" by Kun Zhang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-834-RC1, 2021

The manuscript entitled "Insights into the abnormal increase of ozone during COVID-19 in a typical urban city of China" by Kun Zhang et al. explored the drivers of elevated ozone concentration during COVID-19 lockdown. The manuscript provides valuable information for understanding ozone chemistry under rigorous emission reduction measures and efficiently directing ozone mitigation in the future. I would recommend publication if my following concerns are well addressed.

General comments:

(1) VOCs were measured by a PTR-TOF-MS in this study. However, this method cannot measure alkane and most alkene species, which will underestimate the ozone production and could mislead the diagnosis of ozone sensitivity regimes. Therefore, some uncertainty analysis regarding this deficiency is necessary.

(2) Temperature and solar radiation increase rapidly from Pre-lockdown period to Full-lockdown period, which could significantly contribute to the increase in ozone concentration during Full-lockdown period. This influence is not fully considered in the manuscript. Relevant analysis is also suggested to be included.

Specific comments:

Line 28: "the observed O3 "should be changed into "the increase in the observed O3".

Line 34-35: Here, the authors describe that the changes in precursor emissions (or NOx/VOCs ratio) contributed 2.4 ppbv to the O3 increase, which is inconsistent with 5.1 ppb in lines 27-28. Please double check it.

Line 58-59: also include actinic flux in meteorological conditions and cite the papers (*1, 2*).

Line 175-176: The influence of RH on ozone is very complicated. Higher humidity is conducive to OH production and thus likely increase O3 production. I suggest to add some references about RH influence here and simulate the influence of RH on ozone by box model.

Line 177: please explain why RH>70% can be an indicator of adverse weather conditions.

Line 192: What does the r^2 represent? You should state out it in Section 2.4.

Line 195: I think "$O_{3,Obs}$" should be "$O_{3,Normal}$" here.

Figure 3 and Figure 12: In figure 3, $O_{3,Normal}$ during Full-lockdown period is higher than that during Pre-lockdown period by 12 ppb. However, the corresponding value is only 2.4 ppb. Please explain this inconsistency.

Figure 3: My understanding is that the deweathered method normalizes the influence of meteorological factors on the difference between the same periods in different years. Were meteorological factors between different periods also normalized? The authors should clearly explain this in Section 2.4. This is important to figure out the influence of meteorological factors on ozone increase during Full-lockdown period compared to pre-lockdown period.

Line 214-215: I suggest to at least give some evidences that the decrease in VOC is due to the decrease in industrial activities and traffic volume. Besides industrial activities and traffic volume, solvent usage is also an important source of VOC.

Line 266-268: The expression is ambiguous here. Acetaldehyde and formaldehyde don't belong to aromatics.

Line 273-274: “As for alkene, this could be explained by their chemical reactivities, which led to the fast degradation after emission.” I don’t agree with this statement as aromatics tend to have similar chemical reactivity as alkenes.

Line 277-278: This could also be due to enhanced solar radiation and temperature from January to March.

Line 281-282: “This result suggests that the VOCs in Partial-lockdown should produce less O3 than that in Pre-lockdown, and Partial-lockdown period”. This is misleading. First, the former “Partial-lockdown” should be Full-lockdown. Second, the MIR that you calculate here refers to the ability of VOC species composition to produce ozone, is it correct? Thus, I suggest to further explain the concept of MIR and change this sentence into “This result suggests that VOC species composition in Full-lockdown is more conducive to ozone formation……”. In addition, please specify each dot represent 1-hour average or 24-hour average in Figure note.

Line 304: “Feb 14^{th} ” should be “Feb 1^{st}”.

Line 325-327: I suggest to explain the reason why OVOC kept stable among the three cases, which is inconsistent with the remarkable difference in measured OVOC among the three periods as you shown in Figure 4.

Line 330-355: PTR-TOF-MS is unable to measure alkanes and most alkenes, which could influence the diagnosis of ozone sensitivity to precursors. Lower VOCs concentrations lead to more VOC-limited regime. I suggest to provide uncertainty analysis about it.

Figure 2: The legend of different parameters at the top of the Figure should be placed in corresponding sub-panels. Besides, the legend of ozone and TVOC is not given at present.

Section 2.2: How were photolysis frequencies been considered in the model? Were they constrained by photolysis measurements or calculated by a radiative transfer model (e.g., TUV)? If they were calculated, what about the uncertainty compared to the real condition? And what would be the influence on the afterwards data analysis?

Line 375-376: "underestimation of ozone sensitivity to alkanes and alkenes" should be "underestimation of ozone production from alkanes and alkenes".

References:

- Wang *et al.*, The impact of aerosols on photolysis frequencies and ozone production in Beijing during the 4-year period 2012–2015. *Atmos. Chem. Phys.* **19**, 9413-9429 (2019).
- Wang *et al.*, Exploring the drivers of the increased ozone production in Beijing in summertime during 2005–2016. *Atmos. Chem. and Phys.* **20**, 15617-15633 (2020).