

Atmos. Chem. Phys. Discuss., referee comment RC3
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Comment on acp-2021-834

Anonymous Referee #3

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-RC3>, 2021

The subject manuscript by Zhang et al. presents an analysis of ambient measurements from the Yangtze River Delta assessing the observations of increased ozone (O_3) levels during the COVID-19 lockdown period. Results are presented for three periods: pre lockdown, full lockdown, and partial lockdown and are compared with the same time periods in 2019. The authors seek to understand the relative importance of precursor volatile organic compounds (largely grouped by compound class), nitrogen oxides (NO_x), ambient reactivity/oxidation capacity, and meteorology in determining ozone levels. The authors motivate the study well, and adequately convey the importance of measurements and analysis in this region. However, the methodology is not sufficiently described to support the results and conclusions, and the results and conclusions are somewhat difficult to follow as written. Specific questions, comments and suggestions on these points follow below. It is recommended that this manuscript be reconsidered for publication after major revisions.

Technical Comments:

line 34, lines 173-176: Using "supposed to" in this sentence does not necessarily reflect the complexity of O_3 formation. The prior sentence suggests that during the full lockdown period, the region shifted to a NO_x -limited regime. Thus, it may be expected that a greater decrease in NO_x relative to VOCs would lead to a decrease in O_3 . However, there is also the role of NO_x titration, in which decreasing NO_x can lead to an increase in O_3 . Later in the manuscript, this phrasing is repeated and it suggests that the authors are not necessarily referring to the NO_x regime in the abstract, but the influence of meteorology (specifically T, RH). Again, this is not clear in the abstract, and oversimplified as written. What is the mechanism by which RH affects O_3 formation? How sensitive is O_3 to RH?

Similarly to the use of "supposed to", the authors need to clarify "improper" decline (line 202) and "abnormal" increase of O₃ (line 203).

PTR-TOF-MS measurements (p. 5): Does the Jensen et al. companion paper address losses in the inlet and to the filter? How might these losses affect the results of the analysis presented here? The authors do not need to provide all of the details presented in Jensen et al., but should summarize the main findings, including any limitations, that are relevant to the analysis presented in this manuscript.

Trend analysis: The authors state that the MK non-parametric test is recommended by the WMO. The authors should provide some additional detail here. What does the WMO recommend this test for? Under what conditions? What are the limitations/requirements for applicability in the context of this work? How is serial correlation applicable to the PTR-TOF-MS measurements of individual VOCs? What details of Pathakoti et al. and Alhathloul et al. are relevant here?

Deweathered model: While details of the VOC measurements are somewhat lacking, and more so for the trend analysis, this section is entirely lacking of sufficient detail (and is not, as the authors note on line 191, described in section 2.4). The authors should consider that the information in the manuscript needs to be sufficient such that the results can be reproduced. Further, it is difficult to assess the robustness of the results when sufficient details about the methodology are not provided. What are the uncertainties of the approach? Are data available for all parameters over all time periods? How are missing data handled? How were the model parameters determined (number of tress, minimal node size, and number of samples)? How sensitive are the results to these model parameters?

In general, in the results and discussion, it is often difficult to follow whether the authors

are describing results between the three periods in 2020, or between given periods in 2019 and 2020. It is recommended that the authors try to more clearly differentiate these comparisons.

line 214: TVOC dropped to 22.19 ppb from what mixing ratio?

lines 218 and 234: The authors use "interesting" in these sentences, but it is not clear what is interesting about the observations as presented. The higher mixing ratios due to lower boundary layer heights (line 218) is a common observation, and the lower values of transportation-associated VOCs (line 234) during the lockdown is expected (and has been reported previously).

In line 233, is the decreasing trend based on the Z-score or Q value? Were these metrics consistent? Why or why not? It might be useful to include the Z-scores and Q values for all compounds in the SI.

line 224-226: Can the authors be more quantitative about how many of the measured VOC species shown exhibited this U-shape pattern and then explicitly list those VOCs that didn't? It is a little contradictory to say "most" and then "except for several".

line 269: Is it expected that biogenic emissions would be the dominant source of O₃ in this region?

lines 273-275: This discussion of alkenes is not clear as written. The chemical reactivities of compounds is tied to their oxidation formation potential (and is not independent of).

line 279: How are the MIR values calculated? What are they dependent on? Is it expected that the MIR would be reflective of the different NO_x/VOC regimes? I'm not sure this is the case.

line 286: Was NO_x eliminated (which suggests some chemical/physical removal)? Or were the emissions reduced to a greater extent than VOC emissions?

line 326: What is the relevance of the stable OVOCs across the lockdown periods in the context of emission sources/anthropogenic activity? Is there any offset between emissions and chemistry during this period?

Editorial Comments:

In general, there is somewhat inconsistent introduction and use of abbreviations. It is suggested that the authors check the manuscript to insure all abbreviations have been defined in the abstract and first (and only first) appearance in the manuscript.

line 33: suggest to replace "conjunction" with "boundary"

line 49: I believe "Jan" should be "Feb".

line 104: suggest to replace "inhaled", maybe "air samples were drawn into a 3 m tube" or "air was sampled through a 3 m tube"

line 123: I believe "0-zero" should be "0-D"

Eq 1: The Zhu et al. reference should be replaced by Geyer et al. 2001 (10.1029/2000JD900681), which is where this representation was introduced previously.

line 150: "indicating" should be "indicate"

line 151, line 260: "specie" should be "species"

lines 153-154: Meteorology, emissions, and chemistry (though not entirely independent) can all affect ozone. It is suggested that this be rewritten as: "influenced by meteorological conditions, emissions and/or chemistry". The authors could then indicate that emissions and chemistry are being treated together and separately from meteorology.

line 155: "was" should be "were"