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Comment on acp-2022-60

Anonymous Referee #1

Referee comment on "Transport of substantial stratospheric ozone to the surface by a dying typhoon and shallow convection" by Zhixiong Chen et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-60-RC1>, 2022

Chen et al. reported a comprehensive analysis of the abrupt increase of surface ozone observed in the North China Plain on the night of July 31 2021. The authors employed various datasets and tools in their analysis, including (1) the air pollutant observational data from the national monitoring stations, (2) high-frequency ground-based observational data during a campaign in this region, (3) vertical profile observation of ozone, (4) radiosonde meteorological data, (5) reanalysis data, (6) regional meteorological model, and (7) back-trajectory model. They found out that the sudden increase of surface ozone was not due to horizontal transport; instead, the dying typhoon and a local mesoscale convective system brought the high-ozone/low-CO air to the lower troposphere and even the ground surface. The authors did a commendable job by applying various tools to analyze a unique atmospheric phenomenon that has implications for air quality management. I support acceptance of the paper once the following concerns are addressed.

General comment:

1. The authors emphasized that this paper is about the effect of a "dying" typhoon and "shallow" convection in the title, abstract, and many places in the main texts. While this is correct based on the authors' analysis, I wonder would it be better to generalize the mechanism? If I understand correctly, a typhoon (dying or not) likely causes stratospheric ozone that brings the stratospheric ozone to the upper and middle troposphere, and a follow-up mesoscale convective system (shallow or not) would then transport the high-ozone/low-CO air further down to the lower troposphere.
2. Meng et al. (2022) reported a very similar process (an anomaly in surface ozone due to the passing typhoon) in the same region (NCP). I am aware that the authors of the present work started their analysis before this recent paper (Meng et al., 2022) was published, but it would be beneficial to the readers if the authors could add some relevant discussions.
3. The designs of the manuscript and figures require some improvements:
 - (1) Section 2.3, please add a figure (at least in the supplement) showing the domain setting of the WRF simulation. I would like to know whether the inner domain covers the region with strong vertical transport.
 - (2) In The paragraph starting at line 174, a table listing all WRF model parameterizations

and setups will be very clear.

(3) Line 191, why not describe the setup of FLEXPART-WRF here? How many simulations? What is the location (lat, long, and altitude) of particle release? How many days for each simulation?

(4) Line 194-197, these two sentences seem out of scope. Consider removing. I don't think it is necessary to mention the increase in ozone in the past decade in this region.

(5) Line 208-210, this sentence is a bit odd too. Consider removing.

(6) Figure 2, showing the "departure from the 10-day averaged ozone", is not a good choice to demonstrate the sudden increase of surface ozone. Instead, I believe Figure S2 (with the average diurnal pattern of ozone) is a much better option for showing the anomaly of surface ozone in this region. Similarly, I would recommend drawing a similar figure of CO to replace the original Figure 3.

(7) If possible, Figure 4 should also be replaced with one similar to the original Figure S2. In fact, in line 249, the authors stated that "Compared with the normal nighttime ozone concentrations (an average of 36.6 ppbv), the magnitudes of surface ozone surge due to stratospheric intrusions were approximately 40-50 ppbv". If the "normal nighttime ozone concentrations" were already shown in Figure 4, i.e., with the average diurnal pattern, readers would easily see the "departure" of ozone/CO from their "normal nighttime concentration". Also, I suggest only including the "hour" in the X-axis should be informative enough.

(8) Figure 5, what do the positive/negative vertical velocities represent? Positive values (blue) are winds going down to the surface? Or the other way around? Please clarify in the figure caption.

(9) In Figure 7, similarly, I don't understand why an average level of ozone between surface and 700hPa is used as a baseline. Shouldn't the baseline be the 10-day average vertical profile? With the current figure, the readers must be puzzled why the surface ozone concentrations on July 31 and Aug 1 are lower than the average, while the other sections repeatedly show that the surface ozone concentrations in NCP are larger than the average.

(10) Line 360-364, this information should be moved before mentioning Bow-echoes.

(11) Figure 8, why not show the ozone data at all sites at all times and use a colour scale that covers 10 to 100 ppbv? With the current layout, there is no way to tell how much ozone is increased from 2100LST to 0100LST at stations like JN/BZ. It could just be 1 ppbv of increase (if ozone at JN/BZ were 79 ppbv at 2100LST and 80 ppbv at 0100LST) or >80 ppbv of increase (if ozone were <1 ppbv at 2100LST and >80ppbv at 0100LST).

Specific comment:

4. line 37, this line reads like both "water vapour" and "carbon monoxide" are primarily emitted from combustion processes, while only CO is. Consider revising it.

5. Line 75, what problems "require in-depth investigation"?

6. Line 199, is this 36.6 ppbv calculated in this study or from a previous study?

7. Line 228-230, this sentence sounds important. Any figures/data to support it?

8. Line 237, "filed" should be "field". I have spotted a few more typos. Please check through the manuscript.

9. Line 239, it is >30% increase from 45 to 60 ppbv. I would not call it "slightly higher".

10. Line 250-251, an increase in ozone from ~36 ppbv to ~80 ppbv is a large enhancement, but this level of ozone (80 ppbv) should be very common in this region. I suggest toning down the phrase "great threats".

11. Line 262, somehow the authors missed "anthropogenic emission"?

12. Line 266-267, this is probably true, but it will be better if the evidence is presented.

13. Line 271, "indicate" should be "indicated".

14. Line 417, "the analysed at detail" should be "be analysed in detail".

Reference:

Meng, K., Zhao, T., Xu, X., Hu, Y., Zhao, Y., Zhang, L., Pang, Y., Ma, X., Bai, Y., Zhao, Y. and Zhen, S., 2022. Anomalous surface O₃ changes in North China Plain during the northwestward movement of a landing typhoon. *Science of The Total Environment*, p.153196.