Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1120-RC2, 2020

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Worsening urban ozone pollution in China from 2013 to 2017 – Part 1: The complex and varying roles of meteorology" *by* Yiming Liu and Tao Wang

Anonymous Referee #2

Received and published: 13 March 2020

This manuscript presented a comprehensive modeling analysis on the surface ozone trends over China during 2013-2017. Significant ozone increases have been observed in China over this period in spite of the strong emission control actions implemented. Better understanding the drivers of these trends is of great scientific importance. The authors have conducted an ensemble of numerical simulations using the WRF-CMAQ air quality model to interpret these surface ozone trends, in particular, quantifying the role of meteorology in this manuscript. The results showed that the model had some success in reproducing the Chinese ozone increase trends, supporting the use of it to assess contributions from changes in anthropogenic emissions vs. changes in meteorology. The results further emphasized the importance of interannual variations

Printer-friendly version

Discussion paper

in meteorology affecting the recent surface ozone trends in China.
This is an important study, representing a great step to understand the drivers of interannual changes in summertime surface ozone pollution in China. The manuscript is well organized and written, and the methodology and results sound solid. I recommend publish after the following comments been addressed.

Specific comments:

1) Page 5, Line 120-125:
Some recent studies have suggested that the ozone increases in China since 2013 were largely driven by the concurrent decreases in PM2.5 levels and the resulting changes in heterogenous HO2 uptake by aerosol surfaces (Li et al., 2019a, 2019b). Since the model applied in this study reproduced the observed ozone increases, did the results support the important role of heterogenous reactions? Although the authors may discuss this issue in the second paper, I suggest put some sentences in this paper in the context of these recent findings.
Reference:
Li, K., et al., Anthropogenic drivers of 2013-2017 trends in summer surface ozone in China, P Natl Acad Sci USA, 116, 422-427, 2019.
Li, K. et al., A two-pollutant strategy for improving ozone and particulate air quality in China, Nature Geoscience, 12, 906-910, 2019.

2) Page 8, Figure 4:
The year 2013 seems to be a special year with particularly low ozone values, for example, as can be seen from Figure 4a) over Beijing (the BTH region). If the 2013 data point was removed from the linear trend calculation, then no trend was observed for Beijing. This is also the case for Guangzhou (Figure 4c) and the long-range transport ozone influences (Figure 7). Can you comment on this?

3) Page 10, first paragraph of section 3.5:
Figure 6 showed that the 2013-2017 changes in wind significantly increased surface ozone over most regions of China, and the authors attributed the ozone increases to enhanced transport from the lower stratosphere. This is not clear to me. It may explain some of the surface ozone increases in the northern and central China as argued by the PV changes, but how about the southern China where I think stratospheric ozone influences would be low at surface? It is not clear that enhanced ozone transport from the lower stratosphere could lead to 6-10 ppbv surface ozone increases in the southern China. I wonder whether changes in horizontal winds still contribute there, e.g., changes in the wind speed and the summer Asian monsoon. Please clarify.

4) Page 10, Line 301-302:
The statement "we found that the impact of temperature via the change in the chemical reaction rate was more significant than that via the change in biogenic emissions from 2013 to 2017" need to be more quantitative. It is difficult to read from Figure 5 and Figure 6 (the color bars are too small). It might depend on regions. I suggest compare their values averaged over the key regions and over China.

5) Page 12, Line 342-345:
The statements here seem to imply that transport of PAN led to the long-range transport of ozone influences. How about transport of ozone itself? Which one is the main pathway? One way to quantify and to separate the influences is to conduct a simulation fixing PAN in the 2013 chemical boundary conditions, yet I do not want to push the authors to do more model simulations. Can you explain the issue with present analyses and results?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1120, 2020.