Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-53-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Worsening urban ozone pollution in China from 2013 to 2017 – Part 2: The effects of emission changes and implications for multi-pollutant control" *by* Yiming Liu and Tao Wang

Anonymous Referee #1

Received and published: 10 March 2020

This is a very well written paper, which provides a thorough analysis of the impacts of gas and PM emission controls on ozone formation across China. The paper is appropriate for ACP and it works well with the companion paper that is also under review with ACPD. As described below, there are a few items that need to be addressed, after which the paper would be suitable for publication in ACP.

Major Comments:

1) Lines 230-232 Here some context needs to be provided for these trends, and some

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evaluation against observations is warranted. Figure S1 shows that observed ozone increased by 18% across all urban areas with ozone monitors. However, the model indicates that urban ozone increased from 55 to 57 ppbv, which is just a 3.6 % increase, a rate that is five times less than the observed rate. Why are the modeled trends so low compared to the observed trends, and which processes are being missed by the models? To help the readier understand the discrepancy between the model and observations the authors need to directly compare the model to observations. For example they can compare the modeled trend in the grid cell (or cells) above Beijing to all of the monitors with data from 2013-2017. They can make similar plots for the other urban areas of YRD, PRD and SCB

Across all of China the model predicts a very small ozone decrease of 0.6 ppbv, or just 1%. It's difficult to believe that this tiny decrease has any real meaning. How is the p-value (0.006) so low? What kind of statistical test was used? To have such a tiny decrease with such a low p-value indicates that the signal-to-noise ratio is very high, which implies that there is very little interannual variability. But Part I of this study shows that meteorology creates substantial interannual variability.

2) This science paper strays into the realm of policy recommendations, as follows: Line 308-310 "The inter-city variations in the dominant causes of increases in O3 concentrations mean that the government should adopt additional, localized emission-reduction measures as part of policies aimed to alleviate urban O3 pollution (see section 3.5)."

Line 343 "3.5 The need for concurrent reduction of anthropogenic VOCs emissions"

Line 370 "Therefore, VOCs emission controls should be implemented together with the PM-targeted measures."

"Line 377-379 We thus conclude that VOCs controls should be implemented in current and future emission-reduction measures to improve the overall air quality."

I understand that the authors want their paper to be beneficial for improving air quality in China, and their results will certainly be useful. However, the recommendations will have to be re-phrased so that this science paper does not sound like a policy document. Fortunately, this is a straightforward editorial process. Instead of saying what the government "should" do, the authors can say something like: "Recent emission controls across China have not reduced ozone and have actually increased ozone in urban areas. If the government wishes to adopt new emissions control policies that will reduce ozone in urban and rural areas we propose the following recommendations for VOC controls...." By phrasing it like this, your paper offers very useful options to the government without sounding like a policy paper.

3) This study focuses on summer, but did the authors also look at ozone changes during the winter months? TOAR-Climate (Gaudel et al., 2018) compares surface ozone trends at non-urban sites across North America, during 2000-2014, a period of deceasing NOx emissions. Ozone decreases across much of the continent in summer, but increases in winter (see their Figures 13, 14 and 15). I wonder if a similar pattern has occurred across China in winter.

Gaudel, A., et al. (2018), Tropospheric Ozone Assessment Report: Present-day distribution and trends of tropospheric ozone relevant to climate and global atmospheric chemistry model evaluation, Elem Sci Anth, 6(1):39, DOI: https://doi.org/10.1525/elementa.291

Minor Comments

Line 286-288 Here the authors state that, in general, BC has a major impact on photolysis rates. But the overall conclusion from this study is that the impact of PM reductions on ozone production is mainly through the changes in heterogeneous chemistry, with the impact on photolysis rates being secondary. Given the conclusions of the study it would be a good idea to provide some additional context for the impact of BC on photolysis rates and ozone production.

Line 104 Here and elsewhere, there is no such word as "uptakes". To make it plural

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you can use “uptake rates”

Line 143 This sentence would sound better as: “The companion paper (Part 1; (Liu and Wang, 2020)) presented validation results. . .”

Line 208 Change “observation” to “observations”

Line 209 Here and throughout the paper, when mentioning a trace gas value in units of ppbv, then the quantity must be referred to as a mixing ratio, and not a concentration, which has units of mass per volume.

Line 331 has should be was “. . .where the PM2.5 concentration was high and WAS subject. . .“

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