Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-298-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "2013–2019 increases of surface ozone pollution in China: anthropogenic and meteorological influences" *by* Ke Li et al.

Anonymous Referee #2

Received and published: 13 June 2020

This study quantifies the most recent trends in summertime O3 concentrations in China and investigated the possible causes. This is a timely paper which has implications for the improvement of China's ongoing control policies. However, I have the following concerns which need to be addressed before the manuscript can be considered for publication in ACP.

Major comments:

1. The multiple linear regression (MLR) is a key method used in this study to quantify the meteorological contribution. However, this paper lacks a lot of details regarding the data sources and results of the MLR method. In Section 2: "The regression model is first applied to select the key meteorological parameters driving the day-to-day variability of ozone for each grid cell." What meteorological parameters are considered in

the selection? Which parameters show statistically significant contribution based on the regression? What criteria did you use the select the parameters used in the formal analysis? How much did the selected parameters explain the overall variability? In Section 3.1 and Section 3.2, you talked a lot about the dominant meteorological predictors in China and various metropolitan regions. However, no MLR results supporting these conclusions are shown. How much did these parameters contribute? Are the contributions from these parameters statistically significant?

2. After reading the paper, my overall impression is that the author should tune down the statement that they have elucidated the relative contribution of meteorological and anthropogenic factors to the O3 trend. The meteorologically driven trend is quantified by fitting O3 to selected met parameters while the residual is regarded as the anthropogenically driven trend, so the anthropogenically driven trend is largely unconstrained. This attribution method is subject to a large uncertainty, especially for the anthropogenically driven part. I would not recommend the author to conduct a modeling simulation to test the anthropogenic contribution which requires a lot of additional work, but I am deeply concern that the quantitative attribution to the two parts may not be accurate without further constraint. Even for the meteorological part, you only considered a subset of met parameters in the MLR. Can these selected parameters represent the overall contribution of meteorology? This again points to my last comment that showing the results of the MLR analysis is important.

3. Section 3.1: When you talk about the observational trends, you need to point out whether these trends are statistically significant. Fig. 2 shows some significance testing results, but it's also important to incorporate such information in your description.

4. Abstract Line 20-22: Whether the anthropogenically driven O3 trend is caused by decrease in PM2.5 or reduction in NOx is a controversial issue. This study actually did not carefully investigate this issue but just referred to a previous study. Therefore, you may at most infer that this might be a cause rather than state with certainty that this is the actual explanation.

5. In your regression analysis to determine the O3 trend, you included sites with partial records. Since the number of observational sites grow dramatically from 2013 to 2019, the trends can be biased by the differences in observational sites. I suggest that you repeat the analysis using only continuous sites and examine whether this affects your results significantly.

Minor comments:

1. Sometimes you abbreviated “meteorologically driven trends” to “meteorological trends”, which I think is not accurate.

2. The spatial extents of NCP, YRD, PRD, and SCB are not defined in the paper.

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