

Interactive comment on “Differentiating local and regional sources of Chinese urban air pollution based on effect of Spring Festival” by Chuan Wang et al.

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Referee #2's comments General comments: Large reduction in emissions of anthropogenic pollutants in a mega city during the week-long holiday gives an opportunity to investigate how the urban air quality reacts to reductions in emission. This study conducted a systematical examination of the “Spring Festival effect” over three consecutive winters in Shenzhen with a population of greater than ten million, and the difference in the concentrations of various air pollutants between the Spring Festival (SF) and non-Spring Festival (NSF) periods was exploited to indicate that the origins of pollutants are primarily local or regional.

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Although “holiday effect” has been discussed in a large number of studies in recent decades, the rich and comprehensive dataset provided in this study is informative and helpful for understanding of sources of a large array of species with considerable differences in properties. The authors clearly show their own contribution in the study of Spring Festival effect. In the present form, the authors focus on exploiting the percent change in the concentrations of various air pollutants to differentiate contribution from local and regional sources. The value of this paper could be further enhanced if the authors can make more in-depth discussion on the species (e.g., PM_{0.8–2.5} and O₃), which had a small difference between the SF and NSF periods. It is interesting to know that these species revealed only a small difference when traffic flow dropped by 50% and the industrial plants were almost entirely shut down. It is well known that PM has numerous and complex constituents contributed from diversified sources (primary and secondary, anthropogenic and natural), and O₃ is a secondary photochemical product with nonlinear relation with its precursors NO_x and VOCs (anthropogenic and natural). I encourage the author to strengthen the link between their observation results and the possible causes (composition, property, sources, transport, physical and chemical processing, meteorology, etc.). In general, the manuscript was well written and organized. The subject of the paper is well within the scope of ACP. The paper is suitable for publication provided the general comments and following points are addressed.

Specific comments: 1. Page 7 Lines 137-139: 2016. Are “all fragments” of m/z 44 and m/z 57 measured by AMS are the tracers of oxygenated organic aerosol and primary hydrocarbon organic aerosol, respectively? Or “most of fragments” are?

REPLY: We have corrected in the revised manuscript as below: “Most of fragments of m/z 44 and m/z 57 are the tracer of oxygenated organic aerosol and the tracer of primary hydrocarbon organic aerosol (Zhang et al., 2005), respectively, which are measured by AMS.”

2. Page 11 Lines 212-213: This sentence is confusing to me. In addition to emissions

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and sinks, the concentration of air pollutants is also dominated by meteorological conditions, especially PBL and wind field. In Table 1, meteorological conditions in NSFT and NSFMM are not much different (similar wind speed and no precipitation), and it may be the major reason for the small difference in the concentrations of most air pollutants between the two periods. It is not suitable to conclude that meteorology has only a small impact on their concentrations. Please provide a more appropriate interpretation.

REPLY: To be more rigorous, this sentence is changed to: "The decreasing ratios of various species during SF when compared with the NSFT and NSFMM periods are similar, which suggests that the meteorological variations might not be the dominant reason for the species decreasing during SF."

3. Page 15 Lines 282-283: "emissions are greater on holidays than on non-holidays" and "In addition, emissions were higher during the 2008 Beijing Olympic Games" Do the authors mean "emissions of O₃"? If so, I suggest using the word "concentrations" instead of "emissions".

REPLY: We have corrected in the revised manuscript.

4. Page 15 Lines 287 and 288: VOC/NO_x ratio gives an important idea that it tends towards a NO_x-sensitive or VOC-sensitive environment. However, VOCs are a very complex mixture of compounds with large difference in reactivity with respect to ozone formation. The true impact of VOCs to ozone formation is more relevant to the total reactivities of VOC species rather than to the total amount of VOCs. Furthermore, the VOCs reported in the study (Table S1, measured by PTR-MS) include only a total of 13 masses. TVOC/NO_x ratio in the study should be carefully used to explain its effect on the ozone level due to above-mentioned concerns.

REPLY: We agree that based on limited VOCs measurements, it is not reasonable to get the conclusions that whether the O₃ formation is NO_x-sensitive or VOCs-sensitive. Therefore, in the revised manuscript, we only state as below: "As a result, although the

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reduction in emissions of urban anthropogenic sources leads to a significant decline of NO_x and VOCs, this reduction does not mitigate the average ambient O₃ concentration, which implies that the concentration ratio VOC/NO_x plays an important role in controlling O₃ concentration.”

5. Page 15 Lines 288 and 289: Do the authors mean the chemical regime at noon during the SF period was NO_x-sensitive? If so, the authors should provide a clearer explanation to support the statement.

REPLY: Following the reply to the above question, we are not making the conclusion about whether the O₃ formation is NO_x-sensitive or VOCs-sensitive.

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