Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-970-RC1, 2018

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Causes of a continuous summertime O_3 pollution event in Ji'nan, a central city in the North China Plain" *by* Xiaopu Lyu et al.

Anonymous Referee #1

Received and published: 27 November 2018

The Lyu et al. manuscript reports on a combined measurement-modeling analysis of a sustained ozone (O3) pollution event over the North China Plain. Continuous online measurements of O3, NO, and NO2 were made in the city of Ji'nan, from the Shandon University campus. For a subset of the measurement period, samples were collected for offline analysis of oxygenated/volatile organic compounds (O/VOCs). Additional chemical and meteorological data were obtained from nearby monitoring stations. Positive matrix factorization (PMF) was used to identify sources of O3 precursors, using the chemical data as input parameters (VOCs, CO, NO, and NO2). In addition, the WRF-CMAQ chemical transport model was used to evaluate processes contributing to O3 formation and depletion, and an MCM-based box model was used to evaluate localized O3 chemistry. HYSPLIT back trajectory analysis was also performed to identify

Printer-friendly version

Discussion paper

origins of air masses. Collectively, the research presented represents a significant effort to identify the primary drivers of the sustained O3 event. No major weaknesses are identified in the approach, the quality of the data, or the simulation results. The major weaknesses are in the presentation of the results and the synthesis of the findings. The introduction starts with a list of publications that have addressed O3 formation over the North China Plain (NCP). As written, it isn't clear whether there are major discrepancies between studies and/or whether there are gaps in understanding/model representation that are unaddressed by existing studies. Further, it isn't clear (based on the abstract or implications) how the current work advances the current state of the science (understanding, prediction capabilities, etc.). As written, the implications section highlights that this work confirms O3 levels are high in the NCP and the NCP may serve as a source region, which do not represent a substantial contribution. However, elucidation of the shifts in regime (from VOC-limited to transition) during the O3 episodes appears to be a new finding, and therefore should be highlighted and expanded upon. Further, though significant effort was clearly made and the quality of the work is high, the results are relatively unorganized and presented as speculative. Regarding the latter, the word "might" is used 30 times in the paper; in many places it seems the authors have sufficient information to make more conclusive statements and the contribution of the work is minimized by presenting it as speculative. Regarding organization, in several places within the results and discussion, individual paragraphs are more than one page long (lines 335-378, 733-762, 795-832). Additionally, there is a lot of repetition in the results and the modeling doesn't clearly build on the measurements (or vice versa). Each section is almost presented as a separate study of processes. Because of these weaknesses, it is difficult to assess the overall importance of the paper and the likely contribution to the field. It is recommended that the paper undergo significant revision before publication in ACP. Specific comments are provided below.

Technical and Editorial: Abstract, line 23: It would be useful to see the fractional contribution to O3, as well as the given production rates.

Abstract, lines 34-37: On line 34, the use of “great” implies something that is positive; suggestions to replace with “major” or “large” or something similar. On line 23, a local photochemical production rate of 14 ppbv/hr is reported for Aug. 9-10 (I believe that is the associated time period) and on line 32, a simulated local photochemical production rate (maximum) of 21.3 ppbv/hr is reported. With these large production rates, the ~1 ppbv/hr decrease in O3 formation with a hypothetical 10% decrease in diesel and gasoline exhaust seems insignificant. Even during non-episode periods, a simulated local maximum production rate of 16.9 ppbv/hr is reported. Thus the suggestion that constraining vehicle emissions is the most effective strategy to control O3 production is not well supported by the numbers presented, and needs further explanation and/or clarification.

Line 45: Suggestion to remove “of researchers”.

Line 52: May to August of which year?

Line 80: Can the authors please clarify what is meant by “air profiles”? Chemical composition?

Lines 92-95: The authors state that sources with a large fraction of alkenes, aromatics and carbonyls are significant contributors to photochemical O3 production. As an example, they cite a paper by Ling and Guo that shows O3 was most sensitive to xylenes from solvent usage, but this alone does not require a major contribution of xylenes from solvents.

Lines 122-123: This sentence starting with “contradictory” is confusing as written. What is contradictory?

Line 183: Where is the “widely used” weather station in relation to the measurement site?

Lines 344-346: The authors discuss the potential interferences and overestimation of NO2, particularly on episode days. Do the authors mean that the OH reactivity during episodes might be overestimated? Or lower than during non-episodes as stated? Is there a way to approximate or bound the potential overestimation?

Line 350: “More importantly” than what? High pressures?

Section 3.3: The authors spend a significant time discussing the quality of the O3 modeling. Since matching observations is not the primary goal of the modeling component, much of that discussion could be moved to the supplement.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-970, 2018.