

## ***Interactive comment on “Quantitative impacts of meteorology and precursor emission changes on the long-term trend of ambient ozone over the Pearl River Delta, China and implications for ozone control strategy” by Leifeng Yang et al.***

**Anonymous Referee #1**

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General comments: Ozone (O<sub>3</sub>), as a criteria air pollutant, is attracting increasing concerns in China due to the rapid rise in concentrations across the country. This paper studied the O<sub>3</sub> pollution in an economically boomed region of China (PRD in southern China), in terms of the meteorological impacts, local contribution and regional transport. The statistical methods were used, with interesting and meaningful results being reported. Basically, the decadal changes of O<sub>3</sub> in PRD were well explained, except for some aspects where further clarifications or reorganizations are needed. Though some of the findings were already known knowledge, the paper further consolidate our

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understandings and fully demonstrated its value in future O<sub>3</sub> pollution control in this region. Thus, the paper is recommended to be accepted after the following problems are addressed. First, I do not quite agree with the authors' statement of the “conceptual model”. Generally, a conceptual model is established based on some phenomena, and is further verified by the results. In this study, I would like to suggest the authors to replace the “conceptual model” with the “discussions” on the results, because I did not see the verification of the “model”. Second, the discussions on the O<sub>3</sub> episodes were relatively weak and the trend analyses did not seem to be appropriate for O<sub>3</sub> episodes, due to the limited and inconsistent number of episode days in every years. This section needs to be reorganized and some discussions should be clarified or corrected. Third, the discussions on O<sub>3</sub> pollution in 2016 and 2017 look a bit weird, which should be reorganized. Lastly, it will be good if the O<sub>3</sub> distribution, trend and the influencing factors can be discussed separately by seasons. In fact, the increases of springtime O<sub>3</sub> in PRD in recent years were striking, in contrast to the overall unchanged O<sub>3</sub> in summer and autumn. The paper would be more informative with the discussions on O<sub>3</sub> pollution in different seasons and a special focus on the season when O<sub>3</sub> is highest or increasing with the highest rates.

Specific comments: 1. Page 6, line 233-234. What were the reasons of the minor changes in O<sub>3</sub> due to emissions during 2011 - 2015, in contrast to the significant increase before 2011? Throughout the paper, the changes in the meteorological and artificial impacts, especially the turning points of O<sub>3</sub> variations, should be discussed. 2. Page 6, lines 236-238. The strong statement must be evidenced. This statement is actually contradictory to the later finding that “meteorological adjustment does not alter ozone concentration much” (page 9, lines 362 – 363) on episode days. Please clarify. 3. Page 6, lines 259 – 260. Please briefly explain why central and western PRD were the regions that were most sensitive to meteorological conditions in O<sub>3</sub> pollution? 4. Page 7, line 267. “. . .and that most local emissions are concentrated in the central PRD area”. Add references to support the statement. 5. Page 7, lines 303 – 305. The reasons for picking the three sites should be given. It will be better to plot all the sites in

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the supplement. 6. Page 8, lines 349 – 351. Change “strengthening” to “constraining” or “restraining”. Why only VOCs should be controlled? Also, cutting VOCs emissions will not prevent the decrease of NO titration to O<sub>3</sub>. The statements should be more accurate throughout. 7. Page 9, lines 377 – 378. What were the causes of levelling-off and decrease of non-local contributions? As commented above, the changes are worth to be discussed, which may relate to the nationwide emission controls. 8. Page 11, lines 448 – 450. It is most likely that O<sub>3</sub> formation in the northeastern PRD became more limited by NO<sub>x</sub>, however evidences should be provided to prove the shift of O<sub>3</sub> formation regime from VOC-limited to NO<sub>x</sub>-limited in the southwestern. 9. Page 11, lines 455 – 464. The discussions on O<sub>3</sub> episodes need to be deepened. For example, the winds were not always from the east during O<sub>3</sub> episodes, which in fact were from the northeast with low speeds in most cases under continental anticyclones, and from the northwest with the approaching of tropical cyclones. I do not think that the winds during O<sub>3</sub> episodes can be simplified as easterly, so did the other characteristics which were discussed as an integration in this paper. 10. Page 13, lines 529 – 530. The term “optimal effective NO<sub>x</sub>/VOC ratio” needs to be annotated. The optimal ratio in fact means highest O<sub>3</sub> production rates, which is the worst from the angle of O<sub>3</sub> pollution control.

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