

## Response to review comments

1. Some further clarifications are needed (e.g. how is OPE<sub>x</sub> exactly defined and measured, etc.).

R: Thanks for the comment. The definition and calculation of OPE<sub>x</sub> will be added into the revised manuscript. Besides, in response to the reviewers' comments, more clarifications are also made as the followings.

2. In section 1, page 3, the sentence "Furthermore, given that the O<sub>3</sub>-precursors. . .chamber studies" is not clear and needs to be rephrased and further explained.

R: Thanks again for the comment. This is indeed an error in English. It will be corrected in the revised manuscript as the following:

"Furthermore, while the O<sub>3</sub>-precursors relationship was established mostly on theories and/or chamber studies, the air quality control program for the Beijing Olympics provided a unique opportunity to examine the response of ambient O<sub>3</sub> level to a drastic reduction in the emissions of precursors."

3. The authors should comment on the short measurement period used for the case study and the implications that this may have on the results and the conclusions of the paper.

R: The implications of the case studies were discussed in Sec. 3.3.3.

4. In section 3.2, page 6, the sentence "This hypothesis. . .of this study" is not clear and needs to be further explained.

R: The sentence and its context will be revised as the following:

"Chou et al. (2009) indicated efficient conversion of freshly emitted NO<sub>x</sub> to NO<sub>z</sub> in the morning during CAREBeijing-2006. The results of this study further suggest that the diminution of the morning rush-hour emissions of NO<sub>x</sub> can contribute most effectively to the reduction of NO<sub>z</sub>."

5. In section 3.2, page 6, the sentence "Thus the persistent levels. . .photochemical dynamics" should be related to the meteorological data (e.g. radiation) which is important in this respect.

R: We agree with this comment. Actually, the meteorological and photochemical data were analyzed as suggested in Sec 3.3.

6. The last sentence of the conclusions needs to be further clarified.

R: The following paragraph will be added to the end of the conclusion:

"According to the results of this study, it was concluded that the O<sub>3</sub> production in Beijing should have been mitigated significantly during August 2008. The overall reduction in the mixing ratio of total oxidant was achieved for the successful control of the emissions of ozone precursors. In particular, the reduction in NMHCs

emissions was suggested to be the major cause of the mitigated O<sub>3</sub> production during midday. On the other hand, as comparing with the summer of 2006, apparently higher mixing ratios of O<sub>3</sub> were observed in August 2008. In addition to the declined O<sub>3</sub>-NO titration, changes in the composition of oxidants were observed and suggested to be responsible for the contradictions between the O<sub>3</sub> and O<sub>x</sub> mixing ratios.”

7. The quality of some figures (such as figure 3a, b, 5, 7a, b) needs improvement, if to be printed in black and white.

R: Those figures will be improved in the revised manuscript.

8. The contribution of this study to the already extensive literature regarding Beijing Olympics air quality is modest, since it considers only a single site.

R: Regarding the general air quality during the Beijing Olympics, the contribution of this study is indeed modest. Nevertheless, the major arguments of this study are upon the changes in the photochemical production of ozone and/or oxidants. In addition to showing that the ambient levels of total oxidant had been reduced consistently with the precursors, this is also the first study showing the changes in the composition of oxidants due to changes in the emission of precursors. We shall emphasize these in the revision.

9. The meteorological conditions from 2006 and 2008 are insufficiently compared, to gauge whether these may have contributed to the differences in concentrations between years.

R: Averaged meteorological parameters (wind speed, T, RH) for 2006 and 2008 campaigns will be added to Table 2. In summary, the ambient temperature and humidity in August 2008 were slightly higher than during the CAREBeijing-2006 campaign, whereas the monthly averaged wind speed decreased by ~30%. Obviously, the reduction in the mixing ratios of air pollutants can not be explained by the changes in the meteorological conditions. This is in agreement with the results of Wang et al. (2009a).

10 The phrase “O<sub>3</sub>/O<sub>x</sub> production rates” is unclear; I assume you mean one of these terms, not the ratio (p. 1)

R: Thanks for the comments. The phrase will be revised as “O<sub>3</sub> and/or O<sub>x</sub> production rates...”

10. Cite your claim of 15 megacities, or state how megacity was defined (p. 2)

R: The definition of a megacity, i.e. with a total population over 10 million, will be added into the revised MS.

11. Unclear what is meant by “monthly averaged mixing ratio.” The phrase implies averaging over all hours, but results in the Wang paper were for daytime only. (p. 2)

R: That will be corrected to be “monthly averaged daytime mixing ratio” in the

revision.

12. Not clear how you conclude that there were “coupling influences of transport and in-situ photochemical production” based on a single monitor (p. 5)

R: Besides the observation at the PKU station, the inference was made also with the support from the trajectory analysis of air mass. That part was addressed in Sec 3.3.

13. What is the basis for claiming “enhanced wet deposition” may be responsible (p. 6)? Have you compared precipitation amounts in these months?

R: This is indeed a general description based on our experiences during the periods of the respective experiments. So far we don't have reliable precipitation data for Beijing. That's why we used the wording “...could also be a result of...” in the article.

14. It is unclear what is meant by “photolysis of NO<sub>2</sub> and production of OH are expected. . .”(p. 9)

R: That sentence is comparing the photochemical conditions of 15 August 2008 with the previous case, i.e. 3 August 2008. Because the skies were very clear on both days (as shown in the figures), we expected that both the photolysis rate of NO<sub>2</sub> and the production rate of OH radicals were comparable on the two days. To make clarification, that sentence will be modified as the following.

“Given the strong solar flux, the photolysis rate of NO<sub>2</sub> and production rate of OH on 15 August 2008 should be comparable to those on 3 August, respectively.”

15. A shift from VOC- to NO<sub>x</sub>-limited ozone formation over the course of the day has been widely observed in numerous locations and is not a new finding of this study. (p. 11)

R: We agree that the statement is not appropriate. In the revised manuscript, we shall just notice that “such a back-and-forth shift of the photochemical regime of O<sub>3</sub> production necessitates further studies with sophisticated models”.

16. In Table 2, it would be informative to compare ozone on a peak or afternoon basis, rather than 24-hours.

R: Thanks for this comment. We shall revise the manuscript accordingly.