

## ***Interactive comment on “Ozone air quality during the 2008 Beijing Olympics – effectiveness of emission restrictions” by Y. Wang et al.***

**Anonymous Referee #2**

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This paper presents an interesting data set. However, the analysis and discussion have major omissions that will require significant revisions prior to acceptance in ACP.

1)The paper describes an O<sub>3</sub> control experiment that has numerous parallels to deliberate multiyear strategies and to weekend/weekday variations observed in many other cities. However, the paper does little to put its results in the context of these studies. From those studies we know quite a bit about the spatial extent of the combined and separate effects of VOC vs. NO<sub>x</sub> controls on O<sub>3</sub>.

2)The authors begin by discussing non-linearities in the chemistry and then proceed in their observational and modeling analysis as if these non-linearities are non-existent and irrelevant. There are a number of papers in the literature suggesting that the spatial scale of the authors' model is too coarse to capture the non-linear effects of chemistry

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on ozone. Murphy et al. 2006 and 2007 ACP and ACPD show an example of the non-linear feedbacks on chemistry of other species. These ideas and prior results should inform the authors' analysis of changes in ambient concentrations of SO<sub>2</sub>, NO<sub>y</sub>, CO and O<sub>3</sub>.

3)The paper needs to more clearly define who/where the controls were designed to benefit and to show that Miyun is indeed representative of the chemistry at the location(s) that were the target. To this end, the paper should include a map of the region with approximately 150-200km dimensions that clearly marks the boundaries of the region referred to as Beijing, indicates the major stationary sources and roads and points out the locations of the Olympic venues. Miyun is 100 km from Beijing according to the text. Based on prior analyses of the spatial extent of the weekend effect and on the NO<sub>x</sub> dependence of the sign of the weekend effect on O<sub>3</sub>, Miyun might reasonably be expected to have experienced lower ozone while the core of the Beijing metropolitan area experienced higher ozone. That scenario occurs in many other cities and has been described thoroughly in the literature.

4)The paper uses the word “effectiveness” to describe the response of ozone to the control strategies implemented, but it does not give a clear definition of the word. Presumably what we want to know is how much ozone was reduced below a dangerous threshold in locations where athletes and tourists were most likely to spend their time during the Olympics?

5)The paper would be much more convincing if it used the same methods to pursue an analysis of a control situation without implantation of control strategies. For example, May, June or October 2008 could be analyzed in parallel with August to show how the accuracy of the approach for months where no controls were implemented

Other issues to be addressed:

CO is a nice tracer of anthropogenic VOC reactivity but not of total VOC reactivity. The paper should describe the fraction of reactivity that is biogenic. A 15 ppb decrease in

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O<sub>3</sub> from meteorological factors alone is unusual. If there is precedent for this please cite. Could the decrease instead be due to biogenic emissions?

Since O<sub>x</sub> is so strongly affected by temperature, it would be useful to compare the correlation of O<sub>3</sub> with temperature in the different years to show that the slope is indeed different with the controls. Perhaps using that as a metric would allow the authors to incorporate ozone data from other sites in the study region.

Explain why RH is relevant to the analysis. Is it because of increases in O<sub>1</sub>D + H<sub>2</sub>O or as an indicator of stagnation or some other physically based reason.

Non-linear chemistry will affect the location within the plume where HNO<sub>3</sub> is formed resulting in changes in the fraction of NO<sub>y</sub> deposited during transit from the source. This may or may not be important in the year to year changes reported.

The analysis does not appear to be accurate enough to prove or disprove sources of variability at the 20% level. I recommend omitting the claim that regional reductions are shown to be responsible for any ozone changes.

The non-linear effect of the emissions reductions should be most strongly seen in SO<sub>2</sub> and may explain the apparent difference between its emissions as inferred from the observations and as reported in the inventory.

If the paper is to argue that ozone is reduced everywhere in the domain, it should at a minimum show using a box model, that O<sub>3</sub> at both Miyun and the center of Beijing is in a NO<sub>x</sub> limited regime or that the VOC reductions are so large as to overwhelm the enhanced ozone production if the photochemistry is in a VOC limited regime

#### References

see for example these and many references therein

J.G. Murphy, D.A. Day, P.A. Cleary, P.J. Wooldridge, D.B. Millet, A.H. Goldstein and R.C. Cohen, The weekend effect within and downwind of Sacramento: Part 2. Observational

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J.G. Murphy, D.A. Day, P.A. Cleary, P.J. Wooldridge, D.B. Millet, A.H. Goldstein and R.C. Cohen, The weekend effect within and downwind of Sacramento: Part 1. Observations of ozone, nitrogen oxides, and VOC reactivity, Atmos. Chem. Phys. 7, 5327-5339, 2007

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9927, 2009.

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