

## ***Interactive comment on “Air quality during the 2008 Beijing Olympics: secondary pollutants and regional impact” by T. Wang et al.***

### **Anonymous Referee #3**

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The manuscript “Air quality during the 2008 Beijing Olympics: secondary pollutants and regional impact” by Wang et al. describes and interprets measurements obtained from three ground sites near Beijing. These observations provide insight into the efficacy of drastic pollution control measures and are of great interest to the scientific community. Extracting the changes in secondary pollutants that can be attributed to the control measures implemented in Beijing is extremely challenging. The influence of urban emissions must be isolated from meteorology and the regional background. The authors correctly note the importance of meteorology and regional influences upon pollution levels, but I do not think these influences are adequately considered. This paper is very broad and far-reaching, and I think that some of the conclusions require improved quantitative analysis to be justified. Specific recommendations are given below.

Section 2.2: Does the NO<sub>y</sub> detector include particulate nitrate? NO<sub>y</sub> is never defined. This becomes important later. This section ought to at least mention detection limits and time resolution. I assume that all the measurements are above detection limits, but this needs to be stated explicitly.

Section 3: Results and discussions are mixed together, and it is difficult to follow. The results ought to be stated first, and then a separate section should discuss the findings.

Section 3.1: The results section begins with a discussion of API. But the API is for PM<sub>10</sub>, which isn't even measured at these ground sites. Furthermore, the averaging time for the API isn't given, so I'm not sure how the hourly data and daytime data shown should be compared to API. I think this discussion of API ought to be moved to either the introduction or to the end of the paper, if it is used at all. I'm not sure where the measurements shown in Fig 3 come from – this should be stated clearly in text and figure caption. I don't understand the line that states most of NO<sub>y</sub> is oxidation products of NO<sub>x</sub>. It appears to be the other way around in figures 3 and 4. From those figures, NO<sub>x</sub> looks to be about 2/3 NO<sub>y</sub>. But again, NO<sub>y</sub> isn't defined, and I'm not sure if those figures can be compared. The changes in secondary pollutants are stated without uncertainties. It appears that the standard deviations of the mixing ratios are quite large, and would easily encompass these changes. The statistical significance of the changes should be discussed. Additionally, dilution and mixing are never mentioned. Everything is stated in terms of mixing ratios. Are there any ratios to conserved species that could be used to eliminate the possibility that the changes are caused by mixing? Perhaps CO/NO<sub>x</sub> ratios could indicate changes in the automobile emissions?

Section 3.2: The regional contribution is assessed by comparing ozone upwind and downwind of Beijing in Figure 7. But what about ozone precursors? There is still plenty of NO<sub>x</sub> upwind of Beijing, so the upwind contribution is underestimated by ignoring further ozone formation. Also, there may be significant NO<sub>2</sub> levels at all of the sites. Failure to account for this would underestimate ozone contributions from each site.

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Section 3.3: The OPEs are listed without uncertainty, and I am not convinced there is a significant difference between any of the values. Nitrate is a sizable fraction of NO<sub>y</sub>. Does NO<sub>z</sub> account for nitrate? The similarity between the urban and downwind sites is striking, as the authors correctly note. Wind speeds are mentioned only once, where in section 3.4 they are stated to be below 1 m/s. With such low wind speeds, it could be possible that the air simply sloshes around between the urban and downwind site, which explains why they look the same. Further discussion of the wind speeds is necessary to accurately interpret the data.

Section 3.4: By comparing measurements from different years at a site downwind of Beijing, the comparisons include changes in meteorology, urban emissions, and regional emissions. The authors acknowledge that these 3 are tangled together, but still try to separate the regional influence. Wouldn't it be easier to examine data from a site unaffected by Beijing to examine the regional background? I think the conclusions reached in section 3.4 would require an entire paper to justify.

Figures 1: It is very hard to see the symbols on the right panel. They should be made larger and in different colors. Also, could the scale be changed to include XCC and HSZ? Fig 2: c) should be split into c), d), and e). The axes need to be labeled for the last time series. And the averaging time should be stated for panel c. Also, is the wind direction an average over 4 hours, or an instantaneous measurement taken once every 4 hours? Fig 3: Where were these data obtained? Date format on fig should be changed to be consistent with the rest of the paper. Fig 5: Put markers to indicate HSZ, CRAES, and XCC on map. Fig 6: what is red star in b)? Include markers as in Fig 5. Fig 7: Is NO<sub>2</sub> large? Does O<sub>3</sub> titration cause the difference between HSZ and CRAES in the late afternoon? Fig 8: The upwind site is very polluted. The authors note this, but I think the source of the pollution ought to be identified as much as possible. Is it from the previous day's Beijing emissions? Is it all local?

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