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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of pollution controls in Beijing on atmospheric oxygenated volatile organic compounds (OVOCs) during the 2008 Olympic Games: observation and modeling implications" *by* Y. Liu et al.

Anonymous Referee #2

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OVOC observations during the Olympics Games in 2008 and summer 2005 were analyzed using statistics methods and box-model simulations. The analysis treatment is comprehensive. The emission reductions during the Olympics Games allowed the authors to analyze the mechanisms that control OVOCs in Beijing and hence mega cities in China in general. While I support the eventual publication of this paper, there are several places where the paper can be improved.

(1) Through the analysis of sections 4.2, 4.3, and 4.4, some general conclusions can

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be given on the relative importance of photochemical production, primary emission, dry deposition (and other losses), and transport to major OVOCs (such as HCHO, acetaldehyde, and acetone). Statements like "This discrepancy is mainly attributed to missing sinks, such as vertical dilution, transport, and heterogeneous uptake on aerosols. " (P. 26147) and "However, the model was not able to predict acetone because of transport effect or local emissions." (in the conclusion section) are too general.

(2) The comparison of Table 2 and Figures 2 and 3 should focus on daytime only for OVOCs since the focus of this paper is on secondary production of OVOCs, which occurs mainly in daytime. Separating day and night in Table 5 is nicely done.

(3) The dry deposition velocities used in M2 are more reasonable than M0 and M1. Why not use those in M0 and M1 too?

(4) I think the interpretation of the simulation error for acetone is problematic. A more reasonable explanation for the observed small change is that in situ secondary production of acetone is relative unimportant relative to transport in Beijing. The box model set up is such that the effect of secondary production is overestimated (probably due to the specified dilution effect). Could the same be said of the other OVOC simulations? I wonder.

(5) I do not think that the statement ". . .during the full control period, the emission ratios of reactive hydrocarbons attributed to vehicular emissions did not present obvious difference." (in the abstract) is supported by the analysis results. The uncertainties of the emission ratio estimates are too large to state that the emission ratios did not change. It's more proper to state that the emission ratios did not change within 50% or so.

More detailed comments

(1) P. 26132, Line 5, change "dramatically" to dramatic.

(2) P. 26138, Line 2-4. Specified dry deposition rates and boundary layer heights for all model simulations should be stated here. As I indicated earlier, dry deposition rates

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in M2 and the observed diurnal varying boundary layer height should be used in all simulations.

(3) Figure 1, please add hourly standard deviations on the figure, so we can see if the difference from emission control is significant.

(4) Figure 1, the diurnal cycle of observed isoprene does not suggest that it is all from biogenic sources (which is assumed in the paper). The decrease after sunset to mid night followed by a constant level from midnight to sunrise and a large increase during morning traffic hours would suggest anthropogenic emissions. Is there strong evidence that isoprene in Beijing is all anthropogenic?

(5) P. 26141, Line 15, change “provide” to provides.

(6) P. 26142, why is the effect of dilution neglected in equations (1) and (2)? What are the uncertainties associated with this assumption.

(7) P. 26142, Line 25-27, this statement is too strong when the estimate uncertainty is 30%. I do not know how the uncertainty of 30% is estimated. In addition to the assumptions that went into equations (1) and (2), the fitting errors should also be included. In Figure 4b, for example, the fitting error (of the intercept) looks quite large, much larger than 5-10% due to the assumption of a constant CO background discussed in line 9 of P. 26143. These errors need to be discussed somewhere in the paper.

(8) P. 26144, Line 22-23, where is the equation? It’s not in Table 2.

(9) P. 26145, Line 4. Is the nighttime OH from the empirical OH-J(D1D) calculation essentially the intercept of a fitting? The statement is unclear.

(10) P. 26145, Line 26-28, a 50% sink of aldehydes to aerosols seems very large. Was there any measurement of the sticking coefficient? Is it really possible with the observed aerosol surface area in Beijing? I would guess that there is enough ammonia in Beijing that the average acidity of aerosols is close to neutral. Are there measurements for high aerosol acidity in Beijing during the Olympics Games?

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(11) Section 4.2, Figure 6. Please explain in the paper why the simulated peak of acetaldehyde leads those of HCHO and MVK+MACR by 3 hours in the model.

(12) P. 26146, Line 8. What fraction of MVK+MACR is due to nocturnal production? Is it significant?

(13) P. 26147, Line 21-23. The acetaldehyde production is mostly from alkene oxidation. Please look at literature to verify that it has been seen in Beijing or other major cities in China before.

(14) P. 26141, Line 17-18. Change the word “stable”. Is this result from another study?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26127, 2014.