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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Emission controls versus meteorological conditions in determining aerosol concentrations in Beijing during the 2008 Olympic Games" *by* Y. Gao et al.

Anonymous Referee #2

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Referee Comments on "Emission controls versus meteorological conditions in determining aerosol concentrations in Beijing during the 2008 Olympic Games" by Y. Gao et al.

This manuscript is an interesting study of the effectiveness of emission controls and its relation to meteorological conditions in Beijing during Olympic 2008. Based on a set of numerical experiments on emission control strategy, this study points out that meteorological conditions are at least as important as emission controls in reducing aerosol concentration during the Olympics. The authors also emphasize the dominant role of regional control in improving air quality in Beijing city. The findings are valu-

able for correctly evaluating the emission control efficiency during the Olympics and for policy-making of control scenario. However, the quantities analysis of the role of meteorological condition during the Olympics is still of lack. And there are some inconsistent between the topic and the experiment designs in this paper. In general, this manuscript is well presented but misses some important details (e.g. the configuration of WRF-Chem), and the simulated meteorological and emission bias need to be further discussed. Before the manuscript is suitable for publication, the listed points below should be clarified.

General Comments: 1) The author gave a statement in abstract "our analysis suggests that meteorological conditions (e.g., wind direction and precipitation) are at least as important as emission controls in producing the low aerosol concentrations appearing during the Olympic period. Can the authors explain how they draw this conclusion quantitatively? 2) The authors perform a set of numerical experiments to investigate the effectiveness of emission controls versus meteorological conditions. To ensure robust result, the well simulated meteorological condition is the pre-requisite. In my opinion, the simulation with a 36 km horizontal resolution may not sufficient to well present the complicated atmospheric circulations over the Beijing and its surrounding areas. The synoptic circulation may interact with land-sea, mountain-valley, and urban heat island circulations. The local circulation plays an important role in determining the transport, dispersion of air pollutants. The authors are advisable to fully evaluate the simulated meteorology. In my opinion, just as what authors found in this paper, precipitation is important in wet deposition and there is underestimate of precipitation after 24 August 2008, it will be useful to use a finer horizontal resolution model (like 4km) in which cumulus parameterization could be closed. I would suggest authors to re-run the model in 4km resolution, at least for a single run with precipitation to provide enough evidence that the uncertainty from courser model resolution won't change the main result. 3) Authors used 2007 as the standard meteorological conditions. The representiveness is questioned. The mean of more years run are suggested to represented as the background of the meteorological condition. If the author want to evaluate the

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effect of meteorological condition and emission control, it is better to use the emission of 2008 in the run with meteorology of 2007 or use the emission of 2007 in the run with meteorology of 2008. There is less use to compare with No-CTL07 and No-CTL in this manuscript. 4) There is no use to compare the results of VTL-BJ0 and VTL-RD0 for the whole July and August 2008, since Beijing area is too small as compare with the whole domain, of cause the regional transport will dominate the air quality of Beijing. This is not new. It will be interested to compare the effect in the 1st, 2nd, 3rd, 4th and 5th day run under different meteorological circulation. 5) The authors start the simulation of WRF-Chem 2 months early than July, just want to have more realistic aerosol background. Can they verify the aerosol background is good enough? 6) The authors present the comparison of WRF model outputs at $\sim$100 m with observed hourly wind speed, wind direction, temperature and relative humidity with observations obtained at the meteorological tower in Beijing for August 2008 (Fig. S1). As far as wind speed is concerned, the model results show frequently very higher wind speed and larger diurnal variations of wind speed compared with observations. In my opinion this seems to be poor model performance due to the coarse resolution, not "In general model simulated meteorological variables agree well with observations" as the authors state. This needs to be discussed more fully. Perhaps more observations at other sites and more meteorological parameters (e.g. precipitation, PBL) are helpful to evaluate the model results. Quantitative analysis of the model bias and the result in July should be included. 7) More details of model configuration needs to be presented. Chemical initial conditions as well as the injection method of the emission inventory should be stated clearly. Apparently these choices will affect the model result. Why authors use default boundary condition in WRF-Chem instead of the output of global model. 8) The model significantly overestimates the PM2.5 concentration during the Olympic and post-Olympic period (Table 3 and Fig. 2). This positive bias may caused by the overestimated emission, which definitely affects the result "modeled concentrations of aerosol species in Beijing were decreased by 30–50% during the Olympic period" as the authors state. So the authors need to further discuss the emission uncertainties

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and its potential impacts on their quantitative results. 9) Since WRF-Chem model is a coupled model, I'd like to know if the emission control would influence the metrological condition, it will be interested to show the precipitation in NO-CTL run also. Specific Comments: 1) Abstract Line 14 – "are" -> "were" 2) Some statement should be more specify for the time scale, it is 1hour, 1 day or a month. like "Transport from the regions surrounding Beijing determines the temporal variation of aerosol concentrations in Beijing" . It the variation is in hour, day, minite or second interval. 3) Sect. 2.2 – Land use data imformation should included the descriptions of the model configuration. Reference should be added for some physical scheme. 4) Sect 2.2 line 9 , it should be (10-55N, 70-150E) 5) P.16670, lines 27-29 – What region (just boundary condition?) are the meteorological variables nudged? The authors need to describe the nudging method. If the nudging is in 6-h interval, is there any jump in meteorological parameter? 6) Sect. 2.3 – The emission inventory is "according to the study by S. Wang et al. (2010) and personal communication with Kebin He of Tsinghua University (2011)" as the authors states. Please briefly address the reason for the changes from the emission of S. Wang et al. 7) Sect. 2.3 – Please briefly state how you horizontally/vertically inject the emissions (Q. Zhang et al., 2009) into WRF-Chem. 8) Sect. 2.3 – The temporal variation of the emission for the simulations should be addressed. For example, how do the WRF-Chem diurnally averaged mass concentrations derived from that emissions inventory? This description needs to be added in Sect. 2.3. 9) P.16674, Line 10 – '(for the period of 2-10 August' replaced by '(for the period 11-19 August)' 10) References - In the references list there is a paper by Q.-H. Zhang et al. (2010) (p.16681, line14) that has not yet been cited in the manuscript. Cite the paper, or delete it from the references list. 11) Fig. 2. – For better comparison, the daily mean value for the simulated PM2.5 could be added in Fig. 2. 12) Fig. S1. – The authors are advisable to present the comparison result for the period of July-August instead of August only.

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