

Interactive
Comment

Interactive comment on “Gaseous pollutants in Beijing urban area during the heating period 2007–2008: variability, sources, meteorological and chemical impacts” by W. Lin et al.

Anonymous Referee #1

Received and published: 25 April 2011

This article presents the measurements of gaseous pollutants in Beijing during the winter of 2007. The variations in the ambient levels of air pollutants were attributed to the changes of synoptic weather systems. The sources of NO_y were resolved using regression analysis, and the ozone production efficiency of NO_x was inferred. The topic of this article is consistent with the scope of ACP. Considering that published data for characterizing the megacity air pollution in the East Asia, particularly in China, are rather limited so far, this paper is valuable to further studies upon the air quality and atmospheric chemistry in that region. However, there are indeed some issues need to be clarified or discussed in more details before the final publication in ACP. 1. It was inferred that the air pollutants were mostly emitted from local sources according

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

to the high NO_x/NO_y ratio. Please note that the oxidation of NO_x should have been declined in wintertime. The NO_x/NO_y ratio could not be decreasing along the transport of air mass like in summertime. I suggest making a comparison of NO_x/NO_y between a stagnant and a long range transport air mass to see if the inference is warranted.

2. The sources of NO_y were resolved using CO and SO₂ as indicators of mobile and stationary sources, respectively. This approach assumes that the CO in Beijing is exclusively from mobile sources, and SO₂ is exclusively from stationary sources. Does this assumption be supported by the emission inventories? For instance, there could be substantial emissions of CO from biomass burning in North China. Thus, what are the uncertainties associated with the assumption?

3. It was shown that the co-linearity between CO and SO₂ was not significant. However, the correlation analysis gave a high correlation coefficient of 0.8. Thus, the correlation was rather obvious. Please clarify this contradiction.

4. The contributions from mobile and point sources to NO_y were estimated as 66+/-30% and 40+/-16%, respectively, and gave a total contribution > 100%. Please make clarification.

5. An averaged OPE_x of 0.76 was estimated by the regression of Ox vs. NO_z. It seems that the analysis was based on the whole dataset of the study. The relationship between ozone production rate and the level of NO_x is known being nonlinear, and is sensitive to the concentration and reactivity of the VOCs. The Ox-NO_z regression is usually applied to a period of several hours. Given that the meteorological conditions, VOCs concentration/composition, and the radicals production and loss could be varied from day to day, the results of the analysis could be very unreliable, as shown in the figure 11 of the manuscript. I'd like to suggest making analysis for daily data and giving the range of the daily OPE_x for the study period.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6919, 2011.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)