

Interactive
Comment

Interactive comment on “A numerical study of the contribution to the air pollutant in Beijing during CAREBeijing-2006” by Q. Z. Wu et al.

Anonymous Referee #1

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This discussion paper is the development of nested air quality prediction model system with online tagged module and its application to the estimation of the local and regional source contributions on the air pollutants (SO₂, PM₁₀, O₃, and NO₂) in Beijing. The study conducted by Wu and coauthors provides an analytical tool for quantifying the regional source apportionment of urban pollution. They also discuss the importance of both local and regional emissions in the urban air quality in Beijing and indicate the direction of emission control for improvement of urban air quality in the megacity. Within the reviewer's knowledge, this paper is the first one that presents the detailed assessment on the local and regional source contributions in the urban air pollutants in Beijing. Generally, the urban air pollution in the megacities in the world is influenced by not only the local pollution but also the regional pollution in the surrounding area. This fundamental structure is in the same way as Beijing. Hence, it is expected that

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the sophisticated model, the methodology for source contribution estimation, and the some findings on air pollution in Beijing can provide the valuable information in the strategy development of the air quality management in the megacities in the world. The reviewer believes that this paper is of the great interest of the Atmospheric Chemistry and Physics and recommends publishing this paper with minor revisions in response to the following questions and comments.

Specific comments

The authors emphasized that the air pollutants contributions from local and regional sources to the surface layer and the upper layer (about 1.1 km) in Beijing are differentiated and estimated. The authors discuss the differences of the contribution of local and regional emissions at the different layers. Nevertheless, the authors don't show the model evaluation results at the upper layer. This is the most important weakness in this manuscript. Maybe, there is less data on air pollutants measured at the upper layer in Beijing. However, at least the authors should validate the vertical profiles of modeled meteorological parameters (wind speed and direction, temperature etc.) using by sonde soundings data. Additionally, the Lidar measurement may be available for qualitative comparison with the modeled PM concentration in the upper layer. Another weakness in the manuscript is the uncertainty in the source contribution for ozone based on the tagged method. As pointed out by the authors, the transport of ozone precursors from surrounding area of Beijing may have an important role in the ozone production in Beijing. In that case the tagged simulation has a possibility of underestimation of surrounding area's contribution. Thus, the authors should denote the limitations and uncertainties of source contribution estimated by a tagged method in the case of relatively short-range transport.

Technical corrections

1. p. 5274, line 16: The "in (Li et al., 2007)" should be modified appropriately. 2. p. 5275, line 4: It is needed to explain about how to set the side boundary condition for

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the D1 domain. 3. p. 5277, section 2.3: The TRACE-P inventory doesn't include the emission in the Asian part of Russia. Which of emission inventory did the authors use as emission data in the Russian region in the D1 domain? 4. p. 5279, line 17: The "a modeling discrepancy between the urban and rural sites" is relatively unclear. The authors should make a more detailed description. 5. p. 5279, line 29: The "simulated SO₂ at Xinglong station was much lower than the observed" is a mistake. According to the Fig. 4, the modeled SO₂ at Xinglong station is higher than the observed, while the modeled NO₂ at the site is much lower than the observed. The authors need to explain why modeled NO₂ and SO₂ have an opposite sense. As for the NO₂, the model overestimates at Yufa, while it underestimates at Xinglong. The authors need to give the reader any reasons for this difference. 6. p. 5282, lines 21-24: Why do the authors focus on the primary PM₁₀ only? The authors should add any explanation for the reasons and discuss the following points: (1) the primary to secondary ratio for PM₁₀ in Beijing; and (2) model performance for primary PM₁₀ and the implication of the model evaluation for total PM₁₀ (not primary PM₁₀) denoted in section 2.3. 7. p. 5284, line 12: Is Zhang et al. (2009) appropriate as a reference showing the important role of NO_x in tropospheric chemistry? 8. Fig.7: It is better that "at surface layer" is added in the caption. 9. Figs. 8 and 9: The "PM₁₀" should be changed to the "primary PM₁₀". 10. Fig.13: This figure is not clear. The color shaded contour map is better. Additionally, it is better that the maximum value of scale bar is lower.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5271, 2011.

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