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Atmospheric Chemistry and Physics Discussions

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

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========== 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

We thank the referee for the precious and constructive comments. Our detailed replies are given below.

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.

Reply: Thank you for your suggestions. For this study, we used the observation from the tower station of the Institute of Atmospheric Physics, Chinese Academy of Sciences (IAP). The station locates at the north of the urban Beijing with meteorological tower height of 325m, and air pollutant measurements setup at 47m, 120m and 280m height which have been specified as vertical Layer-1(47m), Layer-2(120m) and Layer-3(280m) in our manuscript. In our study, the Layer-2 and Layer-3 air pollutant data has been collected for model evaluation. We have added these information in the revised version.

-Model validation at the upper layer

The lack of observed data constitutes an important obstacle of model evaluation at the upper layer (1.1km), however, as you suggested, the vertical profiles of the modeled meteorological results at the upper layer have been validated with the atmospheric soundings data and added in supplement of the revised paper. The observation of the atmospheric soundings data was obtained from http://weather.uwyo.edu/upperair/sounding.html with the Station Number of 54511 lo-

cated at the south of urban Beijing and 9 km away from the IAP tower station. The observation period is at 00 and 12 am GMT, daily. Model evaluation is showed on Fig 1 $\sim$ Fig 4 (Temperature, relative humidity, wind and pressure,). As perceptible on Fig 1, the error of the temperature was less than 2âĎČ in the vertical layer below the 200 hPa over August, 2006 with reasonable modeling results.

Fig 1 The pressure-time plot of the error of the temperature (the error = modeled – observed)

Table 1 The monthly mean of the temperature error in different vertical layers Press .(hPa) 1000 925 850 700 500 400 300 200 100 Err. 0.8 -0.2 -0.5 -0.4 -0.7 -0.4 -0.3 0.2 10.9

Fig 2 shows the modeled pressure layers compared against observed data. The modeled results in different pressure layers match well with the observed. The 1.1km layer discussed in the manuscript is comprised between the 850 hPa and 925 hPa.

Fig 2 The comparison of the height in different pressure layers (700hPa, 850hPa and 925hPa) between the observed and modeled

The error of the wind speed with the pressure-time shown in Fig 3, presents less model performance compared to the results obtained for temperature and pressure. However, in the vertical layers below 700hPa, the error of the wind speed is estimated to less than 5.0m/s, which is reasonable. The wind speed monthly error of 1.2m/s at the 1000hPa is also reasonable.

Fig 3 The pressure-time plot of the error of the wind speed (the error = modeled – observed)

Fig 4 shows modeled relative humidity compared against observed data. As a whole, the high relative humidity in August in the vertical layers below 600hPa is consistent with MM5 results.

Fig 4 The comparison of the relative humidity (Rh) in different pressure layes between

the observed and modeled: upper, the observed Rh; down, the modeled Rh

-Lidar Measurements

The Lidar measurement is a powerful tool to study the vertical profiles of the PM concentration, especially in the upper layer. As Lidar was setup in April 2007 at the IAP tower station (later than this study period), no data are available for qualitative comparison in our study period (August 2006). However, for further pollution trans-boundary analysis in near future, the available Lidar's data should be used. Thanks very much for the suggestion.

-Uncertainties of source contribution for ozone

The uncertainly in the source contribution for ozone was needed more studies. The method used in this study tagged ozone after the photochemical reaction as shown in the animation of the ozone contribution from TJ, which was provided in the supplement. Thus the contribution of precursors in ozone formation, both from the local and surrounding area, was not distinguished in the tagged method. Thus, as point out by the referee, the source contribution estimated by the tagged method has a possibility of underestimation of surrounding area' contribution. However, a substantial analysis has recently performed by the same group member on uncertainties of ozone precursor emissions (Tang et al., 2010) since the most important uncertainties in the simulation of ozone were the uncertainties in ozone precursor emission. Moreover, In comparison with the sensitivity approach often employed in quantifying the contribution of chemical transport (Street et al., 2007, X.An et al., 2007), the on-line tracer-tagged technology used in this study appears to give a more accurate quantification of transport due to the importance of non-linearities in the transport and photochemistry of ozone and its precursors, whereas the sensitivity approach provides a more policy relevant quantification by describing responses to emission controls, but with critical non-linearity of model response to emission changes (Derwent et al., 2004).

Technical corrections

1. p. 5274, line 16: The “in (Li et al., 2007)” should be modified appropriately. Reply: thank you for your remark! In the revised paper the “in (Li et al., 2007)” has been revised as “in Li et al., (2007)”

2. p.5275, line 4: It is needed to explain about how to set the side boundary condition for the D1 domain. Reply: thank you for your comments. The side boundary condition for the D1 domain of the NAQPMS model was clean boundary condition, but the area of the research interest (D2 and D3) was far away from the side boundary, and the influence of the boundary condition would be minor. On the other hand, the side boundary condition of the meteorological model MM5 was provided from the NCEP FNL data. In the revised paper, the explanation has been added in p.5275. “The side boundary condition for the D1 domain of the NAQPMS model was clean boundary condition”

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? Reply: The emission in the Asian part of Russia was blank in this study, thus, the pollution contribution of the other regions might be slightly underestimated.

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. Reply: Thank you for your comment! A detailed description has been added in the p.5279 of the revised paper. “That means the model has the different performance in the urban and rural sites, the IAP tower station was a urban sites, the Yufa and Xinglong were the rural sites. Although the emissions have been updated, such discrepancy might be due not only to existing local specificities of emissions sources and uncertainties, but also to the fact that the regional emissions used content more detailed upgraded emission data in urban areas than that in rural areas”

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. Reply: Thank you very much! The mistake has been corrected in the revised paper. In p.5279: “the simulated SO2 at Xinglong station was much higher than the observed in the whole concentration distribution, indicating that the SO2 emissions to the east of Beijing might be overestimated.” In p.5286: “And the simulated SO2 at Xinglong station in the east of Beijing was much higher than the observed, likely due to the overestimated source emissions in this region.”

The emission uncertainty might be a possible reason of the opposite sense of modeled SO2 and NO2 in Xinglong station, since the SO2 emission in the east of Beijing was overestimated. Moreover, as mentioned above, the local particularities of emission sources and uncertainties (deficiencies in estimated local emissions) may have different impact on tagged species and model performance. Actually, in sight of the accuracy of updated emissions used for the present study, statistical errors in emissions sources over individual locations are able to increase inventories uncertainty, so that the model could not display high performance at each station for all species. Another reason of this situation could be model resolution; observations are based on a sparse set of point measurements, whereas model simulations are based on a volume average over unit grid cells. These reasons might be also the trigger of the opposite sense of NO2 at Xinglong and Yufa. Xinglong station is located in the forest-grass region with greater uncertainty, shown in Fig.2 as the “green” region, susceptible to significantly influence the modeled concentration of NO2, whereas Yufa station is located in the plain region with surrounding crop filling activities, shown as the “Orange” in Fig.2.

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. Reply: Same as the ozone, the secondary PM10 was tagged after the aerosol chemical reaction; the contribution by its precursors was not distinguished in the tagged method. For example, the PM10 precursors emitted in Beijing and transported to Tianjin, undergo nucleation to form new particles, and then transported back to Beijing. When transported back to Beijing, the contribution is considered as from Tianjin in the tagged method due to the fact that the secondary PM10 is tagged after the aerosol chemical reactions. Therefore, we focus on the primary PM10 only in this work, since analysis of the contribution of secondary PM10 needs further study including the observed ratio of the primary to secondary PM10 in Beijing, the process of forming new particles and etc.

7. p.5284, line 12: Is Zhang et al. (2009) appropriate as a reference showing the important role of NOx in tropospheric chemistry? Reply: yes, since Zhang et al., (2009) detailed the emission sources and variation over East Asia as well as its role as precursor of regional ozone formation.

8. Fig.7: It is better that “at surface layer” is added in the caption. Reply: thank you for your remark! The words “at surface layer” has been added in the caption of Fig.7 in the revised paper.

9. Figs. 8 and 9: The “PM10” should be changed to the “primary PM10”. Reply: The “PM10” has been changed to the “primary PM10” in figure caption of Fig.8, 9 and 10 in the revised paper.

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. Reply: Thank you for your comments. This figure had been compressed that was not clear in the manuscripts. It has been made clear in the revised paper.

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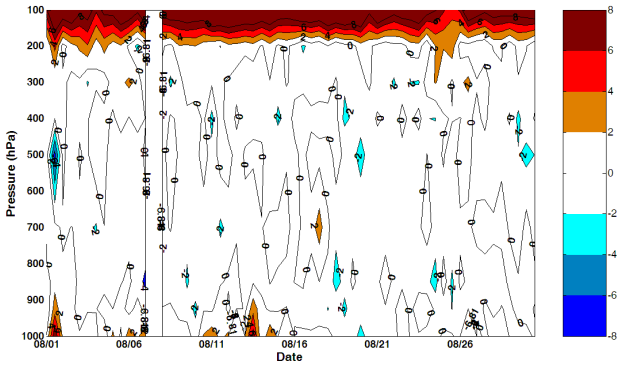

Fig 1 The pressure-time plot of the error of the temperature (the error = modeled – observed)

Fig. 1.

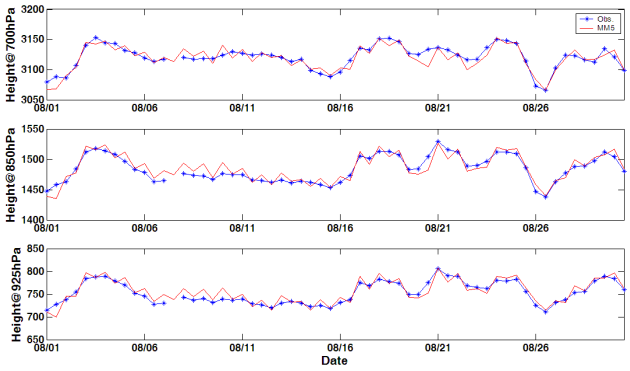

Fig 2 The comparison of the height in different pressure layers (700hPa, 850hPa and 925hPa) between the observed and modeled

Fig. 2.

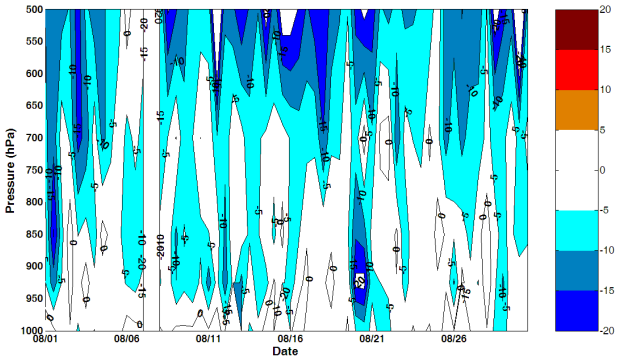

Fig 3 The pressure-time plot of the error of the wind speed (the error = modeled – observed)

Fig. 3.

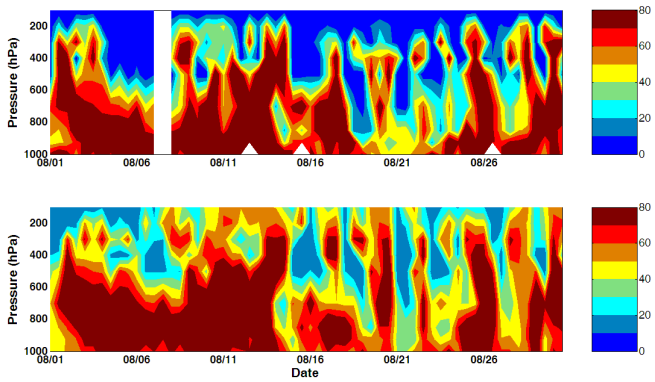

Fig 4The comparison of the relative humidity (Rh) in different pressure layes between the observed and modeled: upper, the observed Rh; down, the modeled Rh

Fig. 4.