

Interactive comment on “A modeling analysis of a heavy air pollution episode occurred in Beijing” by X. An et al.

Anonymous Referee #1

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General comments This paper presents a case modeling study of the air pollution in Beijing and quantifies the effects of regional emission sources (NBS) on the air pollution in the Beijing urban area. This study is interesting because a local government/agency needs such information in their emission control policy making. In addition, since China homes the most urban cities with severe air pollution problem in the world and Beijing will host the 2008 Olympic Games, the worldwide scientific community truly appreciates such kind of research contribution. However, there are several issues needed to be addressed/resolved.

First, The method used to quantify the contribution of the regional sources is not correct in my opinion. The authors used two runs (base run and control run as the authors name them)- one with total emissions (BS and NBS) and the other with regional

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sources (NBS) only- to estimate the contribution of NBS. Many pollutants like ozone and PM are products of nonlinear photochemical processes (at least a portion of them). In a nonlinear system with many factors involved, the total impact of all factors are not linearly additive of the contribution from each factor. The contribution of one factor in presence of other factors can be determined by taking the difference between a model run including all factors and another model run with the factor of interest excluded. Therefore the model runs the authors conducted can be used to quantify the contribution of local sources (BS) to the local pollution, but not the other way around. Their estimate of NBS contribution to SiO_2 is probably fine, since SiO_2 is basically of primary origin, but not for PM, particularly for $\text{PM}_{2.5}$, unless they can demonstrate that the vast majority of PMs are directly emitted in Beijing.

Second, the contribution of regional and local sources to the local pollution is expected to be very sensitive to the accuracy of the emissions of both sources. However, the authors gave no discussions on the emission uncertainty in the local emission data or in the regional emission data (the authors did not even bother to give reference(s) for the local EI and the one for the regional emissions are missing!). Failure to address the EI uncertainty would greatly limit the reliability of their conclusions. For example, results at Table 1 could be significantly different if emissions change.

Third, The effect of regional sources greatly depends on the meteorology. But the authors did not detail the meteorological conditions for the pollution episode selected, which seems to be largely meteorology driven. The authors should provide more descriptions of the meteorological conditions relevant to pollutant long-range transport, since the major merit of this paper is about the contribution of regional sources. This would also help to explain why the specific episode was chosen (see specific comment 1 below).

With the inappropriate method and many important issues listed above not addressed, it is really doubtful about the conclusions in the paper.

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Specific comments (1) Why choose the Apr 3-7 episode? I understand that it was a heavy pollution episode, but there must be other factors behind it for reasoning, such as, is this episode representative of the impact of regional sources on the pollution in Beijing?

(2) P8220 line 15-18, "...transport of Asian dust from ... causing the 4-day high of air pollution ...". I am confused. does this 4-day dust event bracket in the Apr 3-7 episode? From Fig 3 Beijing only witnessed the dust event on late Apr 6.

(3) Table 1. Do the results listed at Table 1 represent the episode-average or just a snapshot like Fig 8? They should be the former for the conclusions to be "general".

(4) Fig 4(b). Is the big discrepancy between the simulation and observation in PM10 during Apr 5-6 due to an underestimate of the primary PM emissions or the inability of the model to capture the dust event, or both?

(5) Fig 11, Are the results averaged over Beijing or at one site? I dont see there should be a definitive relationship between PM local concentration, local flux and NBS contribution, since the concentration is also affected by local emissions and vertical mixing besides advective transport, and the flux is also dependent on wind speed, while the NBS contribution would be more related to inflow flux. My question is what is the point to include Fig 11?

Technical comments (1) Fig. 10, It would be illustrative to present the cross-section along the transport pathway (SW-NE) to demonstrate the NBS effects.

(2) Fig 12, It would be better to include a plot (c) at 14:00Z (as P8224 describes) when the outside emissions intruded Beijing.

(3) Time zone, throughout the context and figure captions, the authors sometimes randomly use local time and UTC, and do not specify the time zone difference. make it consistent.

(4) Episode duration, the episode duration appears inconsistently throughout the pa-

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per: P8216 line 3-7 Apr; P8218 line 11–2-7 Apr; P8222 line 1–2-8Apr; P8226 line 6–3-7 Apr.

More specific comments will follow if the general concerns are resolved.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8215, 2006.

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