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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment of $PM_{2.5}$ in Seoul, Korea" *by* J.-B. Heo et al.

Anonymous Referee #3

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The manuscript reports the receptor modeling result for the PM2.5 measurement data at Seoul, Korea over three years. They applied PMF model to identified major factors (or contributors), CPF model to find major local contributors' directions, and PSCF model to find major regional transport patterns or contributing regions. The authors have used a valuable dataset. The models used are developed and tested elsewhere and, thus, considered as reliable ones. The main scope of this research looks like analysis of the modeling results and finding new understanding on the air quality issues in Seoul. However, as shown in following comments, there are some issues that should be addressed, especially, on the validity of the identification and directional transport of major factors. Thus, regrettably, the manuscript is not suitable for publication in ACP at present form. I recommend the manuscript be rewritten. Some of the major points are:

1. I agree with the authors that if reliable source profiles (preferably local ones) are

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not available, it is better to use statistical models like PMF model. Still, when identifying factors the authors used literature results from abroad. Since emission profiles might be different in Seoul as pointed out by the authors, the authors should pay extra attention on the identification of the contributors. For example, based on Ministry of Environment (MOE), Korea, during that sampling period, the majority of the bus fleet in Seoul has changed fuel from diesel to natural gas and control equipments such as DPF and DOC have been installed to a large fraction of heave duty diesel vehicles. Also MOE has set a new emission standard for vehicles effective on 2006 (Homepage of MOE, Korea). Thus, it is highly probable the emission composition of diesel vehicles in Seoul be different from the literature results cited in the manuscript.

2. Related to the problem of the fuel change and control equipment installation on the heavy duty diesel vehicles, is the PMF results still valid with changing emission profiles?

3. As the authors have suggested in section 3.1, it is very hard to separate contributions between gasoline and diesel vehicles. I wonder if these two sources are put into one, whether the performance of the PMF model enhances.

4. Organic carbons (OCs) can be divided into primary and secondary OC. When the authors apply the PMF model, is this considered? One thing that makes the analysis difficult is that the secondary OC might have two contributors, local and regional. Thus, without separating this factor, the factor analysis results based on foreign literature results might be misleading.

5. The authors applied the backward trajectory analysis for five days with the starting heights of 100, 500, 1000, and 1500 m. I do not think for five day trajectories, the results for the height of 100 or 500 m are not that reliable. I think with these low stating heights a large fraction of the trajectories might touch surface, especially on the Yellow Sea. It might explain why major identified sulfate 'source' region is on the Yellow Sea.

6. Regarding to the backward trajectory, I suggest the trajectory analysis result should

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be shown. Also, it is interesting that in north of Beijing, there is a major contributing region for 'secondary nitrate' but no 'secondary sulfate' region is shown. The authors suggested high ammonia concentration might cause it, but that explanation does not explain the phenomenon north of Beijing.

7. In Fig. 7, a large fraction of trajectories have moved over the northeastern region including Beijing which is the heavy coal consumption area with industrial activities. Still, according to the PSCF result, this region is not the major contributing region for 'secondary sulfate.' Please explain.

8. Regarding to the PSCF result, there are several 3-D chemical transport modeling result in the region some of them were dealing with the transport patter in Korean peninsula. Also, there might be several existing receptor modeling results for Seoul. The authors should give a literature survey result in the manuscript.

9. I could not find a description on where and how the authors have measured the meteorological parameters. I presume they measure these at the aerosol sampling site. I am curious about how representative the wind measurement data were for Seoul proper and surrounding area.

10. Related to the wind data, I think the authors have tried too much to get the result from the CPF modeling. For example, they interpreted the southeasterly air flow have transported aged sea salt from the Yellow Sea though the river. However, from Fig.1, the Yellow Sea is on the west of Seoul and the authors stated based on the trajectory analysis that the winds were from west (both southwest and northwest). Thus, without detailed background information, it is hard to envision the relationship between the local wind be different from the major wind direction.

On some specific points:

1. I think the manuscript be more easily readable if English be checked by a native speaker.

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2. The authors use 'source' but it might be misleading. For example, there is no 'secondary nitrate source.' I suggest the authors use either 'factor' or 'contributor.'

3. Also, it might be misleading to the terms like 'secondary sulfate' and 'secondary nitrate.' These terms imply that there are primary sulfate and nitrate sources.

4. In Abstract, section 2.1, and Conclusion, the authors report the sampling started on March 2003, but in the first line of Chapter 3, it was on May 2003. Please correct it.

5. In the last paragraph of Chapter 3, the figure number regarding to soil is not Fig. 10 but Fig. 11.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20427, 2008.

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