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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics and source apportionment of fine haze aerosol in Beijing during the winter of 2013" *by* Xiaona Shang et al.

Anonymous Referee #2

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General Comments:

This paper present aerosol chemical composition, total aerosol mass and major gas phase air pollutants during the winter of 2013-2014 in Beijing. This winter followed the severe haze events that have been reported previously in the preceding year, in January of 2013, and also followed implementation of emissions reductions in between the two years. The authors use an NMF analysis to identify the major source factors that influence aerosol composition, and sort the data according to non-haze events and increasing severity of haze events that are defined by the Chinese national air quality standards.

The analysis provides information about total aerosol mass, its composition and its likely source contributions, both chemically and spatially, with respect to meteorological backward trajectories associated with each category. The information should be of interest to the readership of ACP and should be published following the authors attention to the comments below.

Specific comments:

Page 3, line 53: Does this mean 3 – 16 days per year?

Page 3, line 56: Is there a quantitative estimate for the boundary layer depth?

Page 3, line 62: suggest phrasing: "winter haze episodes are 5 days in duration"

Page 3, line 72: replace "Over the past seven years (2000-2006)" with "Over a seven-year period (2000-2006)". Then on line 75, add ". . . by 85% over this period".

Page 6, line 157: negative rather than negatice

Page 6, line 164: The uncertainty description is not clear. What are the units on "0.3 + the analytical detection limit" ? Is this a relative error, or does it have concentration units ?

Page 7, lines 178-179: What is meant by "secondary standard of GB 3095-2012" ?

Table 1: Should the number of days with PM2.5/PM10 > 0.5 and < 0.5 add up to the total number of days with comparison to PM10? In other words, 47 + 47 does not equal 67. The text implies that it should (e.g., that 70% of the events were developed type, which would be 47/67). Is the correct number for PM2.5/PM10 < 0.5 = 20 ?

Figure 3: The factors are shown on a log scale to illustrate the contributions from all of the components of chemical composition. However, the log scale hides the large contributions of individual components to each, such as sulfate to coal combustion. Can the figure also be shown on a linear scale for comparison to illustrate which components make large contributions to each factor? A linear scale would increase the

contrast.

Page 9, lines 241-243: Traffic is attributed to a factor with high nitrate an ammonium, with the ammonia precursor attributed to the same emission source as NOx, presumably. Should there also be an agricultural factor for the ammonia emissions? Can the authors comment?

Page 9-10, lines 256-264: The authors suggest that secondary production is a relatively unimportant consideration. However, it is well known that sulfur oxidation rates in winter are typically slow, while NOx oxidation rates to NO3- can remain rapid (e.g., Calvert et al., Nature 1985). Can the authors comment on the source of sulfate? Does this likely arise from secondary oxidation of SO2, or does it rather come from a primary emission of more oxidized sulfur that leads to sulfate? An easy metric here would be the ratio of sulfate to SO2 in molar units. A similar comparison could be given for NO3- to NOx.

Page 10, line 271: A large carbonaceous component is shown for blue / no alert days. However, there are only 4 days and 4 samples in this category. Is it possible that the deviation of the carbonaceous aerosol from the trend of decreasing contribution as the haze level increases is simply a result of the small number of samples in the blue / no alert category, leading to a statistically anomalous result? Can the authors comment on this?

Page 10, line 283 – 287: Following from the comment above, how does the sulfate / SO2 ratio vary as the haze alert level increases? Does this ratio increase, decrease, or stay the same? If there is a trend, it may have information about the primary source of sulfate from SO2 emission or the rate of secondary sulfate production from SO2 oxidation.

Page 11, lines 325-326: There is not a clear difference in Figure 4 between the blue / no alert trajectories and the non-haze trajectories. Are the authors sure that the 4 days are meaningful in this category to attribute the large contribution of industrial emissions?

In Figure 5, this category remains different from the trend in most other categories as the haze severity increases.

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