Anonymous Referee #3

The manuscript of "Characteristics of concentrations and chemical compositions for PM2.5 in the region of Beijing, Tianjin, and Hebei, China" described the results of the one year observational study on PM2.5 in four key cities in North China Plain, where is suffering from serious air pollution in the past few years. It is valuable to report the seasonal variation of PM2.5 mass concentration, chemical components and the possible sources. I believe publishing this paper will help us to have a deeper understanding on the pollution status and potential formation mechanism on PMs in North China Plain.

Comments:

1) The authors reviewed the some previous researches on particles in China, including mass concentration, chemical-physical characteristics, optical properties and photochemical processes. It will be better if the authors can summarize the references based on the parts in "Results and discussions" of this study.

The devices, analysis methods, or monitoring sites were different in previous studies. Most of results in previous studies were not comparable with data of this study. So I listed some references in introduction and chose only He's study for comparison in 3.2.5.

2) More reference papers should be added, especially those conducted in the North China Plain. (e.g. the impact of chemical components on formation of particles, the trend analysis of PMs)

I will add some representative references in introduction.

3) There is sth wrong about the introduction of the implementation of the newly revised national ambient air quality standard (GB3095-2012), in which the standard is supposed to be implementation step by step from key regions (Beijing-Tianjin-Hebei) to the key cities and then to the national level, from 2012 to 2016.

I have misunderstood the timetable of standard implementation. The statement of the implementation of new AAQS will be revised in ACP.

3) In section 3.2.2, the authors gave a very important information on the formation of NH4NO3 and (NH4)2SO4 (or NH4HSO4) under the condition of lack-in-NH4+. It will be better if a deeper discussion were added on the possible impact (e.g. Pathak et al., 2009).

Ammonium nitrate is often formed in areas with high ammonia and nitric acid concentrations and low sulfate concentrations. According to thermodynamic results from laboratory experiment, NH4NO3 would not be expected to be formed in BTH under summer conditions. But in this study, the highest concentrations of NO3- had formed in acidic and ammonium-poor aerosol in the summer. In study of Ianniello et al. (2011), very similar seasonal trends for secondary ions were reported. In study of Pathak et al. (2009), high concentrations of NO3- were also found in the ammonium-poor PM2.5 samples at Beijing site in the summertime. The discussions in those papers are very helpful to the result discussions in this paper.

4) The authors mentioned the opposite seasonal trend of OC compared with the so called secondary ions. And the growth of particles was taken as the possible reason on high OC in winter. However, it has been well known that the upper limit size range of

growth process is about 100nm to 150nm, in which the particles were not supposed to be responsible for the high mass concentration. Therefore a reasonable discussion will be needed in this part.

Compared with concentrations in the summer, wintertime OC were nearly doubled, and had much more increase percentages than EC. In addition, OC/ECs were higher when atmosphere was relatively stable, especially in the winter. The increases of direct emission from sources in winter could not well explain these results. So I think the secondary formation was very important. The actual processes of secondary formation for OC in the winter needed to be deeply discussed and could not be revealed just through mass concentrations of OC and EC in this paper. I will revise the descriptions of this part and make it more reasonable.