

Interactive comment on “The variation characteristics and possible sources of atmospheric water-soluble ions in Beijing” by P. F. Liu et al.

#### Anonymous Referee

The manuscript presents results from ion chromatography analysis of samples of PM<sub>2.5</sub> collected in Beijing through the year of 2014 aimed at deriving the variation characteristics of water-soluble ions (WSIs) in the PM<sub>2.5</sub>. Since only a small part of studies focused on the variation characteristics of WSIs in the four seasons by now, I think the intentions of the authors are very good and substantial data about the WSIs in the PM<sub>2.5</sub> are provided which can make an incremental gain in the knowledge of the haze occurred in Beijing. The science is sound and the results are meaningful. In addition, the authors are very familiar with the North China plain (NCP) and the agriculture activities and living activities of farmers in NCP. There are interesting findings that the emissions from farmers' activities in the NCP was one possible emission sources and the influence of fertilization events and crop straw have influence on the regional air quality during the harvest seasons periods which have been neglected by most previous studies. Maybe more attention would be paid to the agriculture activities in NCP and that some field observations would be carried out in rural area after this paper. The detailed data of the daily variations of WSIs in this paper showed an obvious seasonal variation characteristic, which may helpful for further exploring how meteorological factor affect the accumulation and dispersion of atmospheric pollutants. And it was found that the atmospheric concentrations of SO<sub>2</sub> and NO<sub>2</sub> in autumn are much smaller than that in winter and spring, whereas the mean concentration of WSIs in autumn was almost the same as that in winter and nearly twice as that in spring. This result indicates that unknown mechanisms of atmospheric heterogeneous reactions and transformation of atmospheric pollutants from gas phase to particulate phase should be investigated. Moreover, it was an interesting observation that the increasing rates of SO<sub>4</sub><sup>2-</sup> during some serious pollution events were much slower than those of NO<sub>3</sub><sup>-</sup>, especially with the elevation of Ca<sup>2+</sup>. The heterogeneous reactions of SO<sub>2</sub> and NO<sub>2</sub> with mineral dust may be an important

pathway for the formation of sulfate and nitrate in the urban cities of East Asia because of the frequent occurrence of dust storms. Most previous studies focused on the heterogeneous uptake of  $\text{SO}_2$  or  $\text{NO}_2$  on mineral aerosol surfaces without considering coexistent gases in atmospheric condition. Only a few studies reported that  $\text{SO}_2$  and  $\text{NO}_2$  likely exert synergistic effect on the surface of mineral dust. To my knowledge there is still a lack of knowledge to explain why the increase of nitrate proportion with increasing pollution levels much faster than the increase of sulfate. I'm interested in the new ideas and inspiring points in this paper.