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Interactive comment on "Wintertime aerosol chemistry and haze evolution in an extremely polluted city of North China Plain: significant contribution from coal and biomass combustions" *by* Haiyan Li et al.

Anonymous Referee #1

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This paper presents ACSM results from a heavily polluted city in China during wintertime. The PM1 concentrations averaged at 187.6 ug/m3, in urgent need to elcuciate the characteristics of the PM pollution. The paper is overall well written and the figures are informative, i recommend its publication after addressing a few issues listed below

(1) This reviewer finds that some necessary discussions are lacking in the manuscript. The PM1 contains a significant fraction of chloride - 9%. This fraction is in fact higher than the chloride level typically observed in other AMS studies. Considering the PM1 concentration is high, chloride concentration is also significant. I think its sources,

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formation and other charateristics should be discussed. (2)Similar as the comment 1, characteristics of BC should be discussed in more details as well.

(3)Introduction: The authors state that regional transport is a major factor for the heavy haze formation in Beijing. Any comments on the role of local new particle formation and growth?

(4) The authors sometimes use r and sometime r2, it is better to be consistent throughout the manuscript.

(5)The mass fraction of PM1 to TEOM-determined PM2.5 is about 88%, which appeared to be higher than those values in other studies. This probably can be discussed in more details.

(6) The PMF-ME2 algorithm didn't resolve a cooking OA factor. Did the authors try to use any sort of reference COA profile in the PMF analyses, and were any reference BBOA, CCOA profile used as input?(as it was only mentioned HOA profile was used as input).

(7)The weekdays didn't include monday and the weekends didn't include saturday. Was this treatment consistent with and were often used in previous studies?

(8)In Figure 9, this reviewer feels the nitrate fraction increased and then decreased with the increase of PM1 loading rather than only a general decreaing trend.

(9)Figure 11 is for the entire study period, correct? But it was placed in the case study, it probably should be made clear in the figure caption to avoid confusion.

(10)in line 170-175, the authors stated that combustion emissions were significant as it had a good correlation with CO, but the traffic HOA in factor is only a minor contributor to the total OA. I understand the combustion does not only refer to traffic combustion emissions. But this probably should be stated clearly.