

Interactive comment on “Characterization of individual aerosol particles collected during a haze episode in Incheon, Korea using the quantitative ED-EPMA technique” by H. Geng et al.

Anonymous Referee #2

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This paper presents valuable results in the field of atmospheric aerosol study. It establishes chemical characterization of individual aerosol particles during urban haze episodes and investigates possible contributing factors to the haze formation in East Asia. The energy dispersive electron probe microanalysis techniques they used in this study have been demonstrated to be a powerful method of analyzing atmospheric aerosol. The paper is overall well-structured and easy to follow. Literature data are thoroughly surveyed and compared. However some redundancy and simplification in language makes the paper somewhat less clear. The paper is certainly publishable. There are a few points that the authors should address before the paper is accepted.

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1. Page 26646, the classification information was briefly mentioned, which is not very clear to me how the authors assign different types of particles, especially regarding the differentiation between carbonaceous particles and $(\text{NH}_4)_2\text{SO}_4/\text{NH}_4\text{HSO}_4$ containing particles. It seems that both particles may contain more than 95% atomic fraction of C+O+S as discussed on page 26648. If the authors also used particle morphology information to distinguish the particles, which seems to be the case, a little more clarification is necessary. An additional table about the classification rules used in the work would be helpful.
2. The statement about the two types of organic carbon on page 26648 seems speculative. I wonder if there are more evidences or literature data to support it.
3. On page 26649, the authors state that holes observed on the particle from secondary electron images. X-ray analysis showed that N signals were very low due to apparent beam damage and vast majority of signals were actually from C+O+S. The authors assigned those particles as $(\text{NH}_4)_2\text{SO}_4/\text{NH}_4\text{HSO}_4$ containing ones seemingly based on morphologic information only. Is that conclusive? What is typical electron beam scanning time? Have the author tried to lower the scanning time or the electron voltage to reduce beam damage so that N signals may be acquired.
4. On page 26649, the atomic concentration ratio of [Na]:[Cl] close to 1:1. Did the authors make any ZAF corrections for the quantification. The concentration ratios of [Na]:[Cl] in NaCl based on energy dispersive X-ray microanalysis have been known to be dependent on particle size. For typical micron size particles as shown in the figure 3, the [Cl]:[Na] is usually close to 1.10-1:15. The ratio reported in the paper close to stoichiometry is sort of surprising.
5. On page 26650, for particles containing Al, how the authors distinguish its signal from the Al foil background contribution?
6. The discussion on page 26654 seems very generic and applicable to many cases. More concise and specific explanation is recommended.

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My remaining comments are minor: 1. In line 24 on page 26642, the period was improperly used.

2. The labels in the figure 3 seem a little crowded. It may be helpful to only keep the particle numbers and have particles information tabulated.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 26641, 2010.

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