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Interactive comment on “Characteristics and source apportionment of atmospheric aerosols at the summit of Mount Tai during summertime” by H. Xu et al.

Anonymous Referee #1

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The manuscript presents and discusses the composition of aerosol samples collected at the summit of Mount Tai (China) in June of 2006. Based on calculated backtrajectories, the authors explain the variations of some PM components in terms of wind transport of the pollutants from regional sources towards the site. In my opinion, the analysis presented contains important omissions and inconsistencies. Therefore, I cannot recommend publication of the manuscript in its current form.

SPECIFIC COMMENTS:

1) In general, the manuscript is very descriptive, but it is not insightful at all and it fails to properly discuss and support the reasoning that led the authors to their con-

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clusions. For example, in Section 3.1, the authors found concentrations of secondary pollutants at Mount Tai that are close to those in Beijing (page 16366, lines 15-16); however, “Mount Tai metal element concentrations (e.g., Zn and Pb), which are mostly from anthropogenic pollution sources, were much lower than those of [Chinese] cities.” (page 16367, line 12-13). The authors explain that such difference “. . . indicates that secondary pollutants are easier than primary pollutants to transport via aerosols in regional areas” (page 16367, lines 17-19). However, the authors failed to recognize, nor discuss, other important processes such as chemical processing and/or aerosol dilution, as possible explanations to the observed differences. In section 3.3 (page 16369, lines 14-19) the authors explain larger concentrations of all ions (except for K⁺) at day than night in terms of differences in wind velocity. However, they do not consider, nor discuss, the boundary layer height as an other plausible explanation.

2) The description of the atmospheric aerosol chemical properties is not thorough. The manuscript describes only some chemical properties of the aerosol samples collected at Mt. Tai, such as size distribution and concentration of some of the aerosol components. It fails, for example, to show or even mention the total PM mass concentration and size distribution, which usually provide very important and useful information on the characteristics, sources and processes of aerosols. Does the total mass reside in PM_{2.5} or PM₁₀? Is there any variation in concentration and/or size distribution on a day to day, or day to night basis? What is the PM composition in mass percent? Also, the authors could have a deeper discussion on the size distribution variability. In section 3.3 the authors mention that the secondary ions show large variations in concentration between day and night. They also describe significant differences from day to day. Are these changes associated with day to day and day to night size distributions variations? Answering this question might further support the conclusions in section 3.3.

3) In general, the description of the experimental methods used to digest, extract and analyze the samples is very poor and does not have any bibliographic references;

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hence it is impossible to determine the quality of the data presented. If the methods have been used and described before, the authors are making a serious omission by not referencing previous published work. On the other hand, if this is the first time that the methods are used, then they should be described and discussed thoroughly. The description should include an analysis of their method of validation, uncertainties, reproducibility, precision and accuracy.

4) In section 3.3 (page 16371, line17-21), the authors mention that “K⁺ had the same concentration variations as for secondary ions prior to June 13; . . .” and later they conclude that the source of K⁺ is biomass combustion in North China. This is not consistent with their previous observation that highest concentrations of secondary ions occur when the site is affected by winds from polluted areas in the South of China. Why, if K⁺ is transported from the North, its concentration shows the same variations as the secondary ions coming from the South?

5) In Section 3.2 (page 16368, lines 4-10) the authors state: “During the observation period, sulfate size distributions exhibited little variation, and the median aerodynamic diameter (MMAD) remained within 0.2 to 0.8 μm The MMAD increased with increasing sulfate concentration when it was less than 10 $\mu\text{g m}^{-3}$; however, when the sulfate concentration was larger than 10 $\mu\text{g m}^{-3}$, the MMAD remained between 0.5 and 0.8 μm . . .” In these statements, the authors first claim that the sulfate size distribution exhibited little variation; but then, they describe a variation that seems to be important. Furthermore, the description is not clear at all; a figure showing how does the size distribution changes with sulfate concentration would be very helpful to understand what the authors mean. Moreover, the size limits stated by the authors in these sentences (0.2, 0.5 and 0.8 μm) do not correspond to the 50% cut off diameters for the eight stage impactor (9.0, 5.8, 4.7, 3.3, 2.1, 1.1, 0.65, and 0.43 μm) described in the experimental section.

6) In Section 3.1 (page 16366, lines 5-7), the manuscript reads: “Figure 2 indicates that water-soluble ion concentrations, especially secondary ions such as SO₄²⁻, NH₄⁺, and

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NO₃⁻, reside mainly within PM₁₀, and K⁺ concentrations reside mainly within PM_{2.5}.” This statement is then contradicted in section 3.2 (page 16368, line 3) when the authors say: “SO₄²⁻ was found in the accumulation mode, with 87% of its mass present as fine particles with aerodynamic diameters less than 2.1 μm.” Further contradiction is found in section 3.2 (page 16368, line 20) when the authors write: “The ions SO₄²⁻, NH₄⁺, and K⁺ were found in the accumulation mode, with more than 70% of their mass present as fine particles with aerodynamic diameters from 0.43 μm to 2.1 μm”.

7) The identity of the elements analyzed by ICP-MS is confusing. In the experimental section, the authors list 19 elements whose mass concentration in the samples were determined. However, they later discuss the size distribution of elements that are not mentioned. For example, in figure 2, they present the size distribution of Se and Sb, and in section 3.2 (lines 20 and 26 of page 16368) they mention Ti, Co, and Mo; none of these 5 elements are listed in the experimental section. The size distribution of Se and Sb, shown in figure 2, is not mentioned in the text; but the authors discuss the size distribution of Cu (page 16368, line 2), which is not shown in any figure.

8) Does the title of figure 2 (“Typical size distribution”) refer to the average size distribution of all the samples collected, or to the size distribution of one of the samples? If the latter, does the sample correspond to a “night” or “day” period?

9) Does the concentration shown in Figure 3 correspond to the total (sum of all stages) PM concentration? It should say “Variability” instead of “Variety”. Time series of Zn, Pb and crustal elements should be shown since they are mentioned in the text (section 3.3, page 16370, lines 27 – 30).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16361, 2009.

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