

Interactive comment on “Mixing state of individual submicron carbon-containing particles and their seasonal variation in urban Guangzhou, China” by G. Zhang et al.

Anonymous Referee #1

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<General>

This paper presents characteristics of submicron carbon-containing aerosols in urban area of PRD during late spring and fall based on single particle measurements by a laser ablation mass spectrometer. The authors have classified the observed mass spectra using cluster analysis and found that the fraction of biomass burning particles significantly increased in fall. I think the paper is generally well-written and provides a good reference for the mixing state of submicron particles in this region. My major concern is the representativeness of the data presented here. The measurement periods are too short to draw a general conclusion on seasonal variations. In my opinion, the

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paper would benefit by being more focused.

<Specific comments>

1. Introduction

The importance of carbonaceous aerosols in the climate system is described in the introduction, but this is not directly linked with the major conclusions of this paper. Please focus on the current understanding of sources and processes of carbonaceous aerosols rather than their general importance. Please briefly review what is known and what is unknown regarding the mixing state of carbonaceous aerosols (not only in China but also other regions). Such a review is helpful to highlight new findings of this paper more clearly.

2. Experimental set-up

The measurement periods (30 Apr to 22 May 2010 and 5-20 Nov 2010) are too limited to draw a general conclusion on seasonal variations. The authors mention that the instrument detected 700,000 particles for each period. What is the representativeness of the data? I would expect more particles in pollution episodes than average conditions. If the instrument detects 10 particles per second, for example, it needs only a day to obtain 700,000 particles. Please describe the representativeness of the data in more detail and clarify that this paper presents a case study rather than seasonal variations (the title should be also changed).

3.2.2. Mixing state

The discussion on nitrate aerosols in the last paragraph is too speculative. I recommend it should be removed.

3.2.3. Particle acidity

I wonder how the relative acidity ratio depends on the variability of ionization efficiency due to the mixing state. If this parameter is only qualitative, I do not think it useful to

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show it in this paper. Laboratory experiments may be helpful to interpret this parameter.

4. Conclusions

Again, the conclusions and implications should be more specific. I do not think the results of this paper could improve our understanding of atmospheric chemistry and reduce the uncertainty of climate modeling.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 32707, 2012.

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