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Interactive Comment

Interactive comment on "Size distributions of elemental carbon and its contribution to light extinction in urban and rural locations in the Pearl River Delta region, China" *by* H. Yu et al.

Anonymous Referee #1

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This paper reports different size distributions of EC at several sites (namely, urban and outskirt locations) in the Pearl River Delta, China, where ambient measurements are limited and huge anthropogenic sources of EC are expected. Mixing state of EC has certainly important implications for the earth's radiative balance, as those presented here, as well as human health. On the basis of the size-segregated EC data and other chemical composition, the authors calculated the light extinction contribution by the EC-containing particles to show a significant contribution of EC to the observed light extinction in these regions. The present work provides nice information in our understanding on chemical and physical properties of EC in Asia, and the material presented here likely fits with the scientific scope of ACP. However, in many parts, the discussion lacks scientific rigor and careful evaluation of the data (see comments below), which makes discussion rather loose. The authors make a lot of assumption in the calculation of chemical and physical properties of EC, most of which are not supported. I think there are several important issues that need to be worked out before its publication in ACP.

Specific comments In addition to the size distributions of EC, one of the most important factors in the authors' discussion is that chemical compositions of non-EC material (i.e., sulfate or OM) that coated EC. Also their relative abundance to EC is certainly important. In many parts of the paper, however, the authors discuss size and chemical compositions of non-EC material without showing any measurement results. I think the authors should show the size distributions and mass concentrations of sulfate, OC, etc. and their relation with EC to support the authors' discussion. Without these information, the assumptions made for the calculations and discussions are weak.

2.5 Modal characteristics of EC at the suburban and rural locations The authors attribute the difference in the size distributions of EC to aerosol aging, which are related with locations of each sites. If so, the authors should show trajectories or local wind fields to clarify the "upwind" and "downwind" relationships among three sites: BG, GZ, and HKUST. On the other hand, the authors say that local biomass burning is a significant source at the BG site (p.23036, l.16-22) without showing any evidence, which indicates that the influence of urban sources in GZ was insignificant in BG. These descriptions are very confusing and seem to be inconsistent.

I cannot understand why the authors show (maybe averaged?) results from five "scattered" sites in GZ. Do they intend to say that the GZ results are spatially representative for urban GZ? Were the samples obtained simultaneously at five sites in each period? From Table 1, the answer may be "No." The authors should provide more descriptions on this point, because they only say that "they are monitoring stations scattered around the city." This information can be related with the conclusion: how representative the contribution of EC-containing particles to light extinction is in time and space?

P.23025, l.12-16: With regard to the way of splitting TC into OC and EC, the authors just mention the PMF and only refer to Yu and Yu [2009]. Because the determination of the EC mass may affect the results, the authors should show more details about the determination of the EC mass instead of using the laser correction (reproducibility in PMF, uncertainties, etc.).

P. 23030, l.18-28: "Influence of organic materials . . . ." I think these descriptions are wrong. Chemical and physical characteristics of EC and other aerosols in "urban," or "suburban/rural" in this work are not necessarily the same as those reported by Saxena et al. Influence of organics on water uptake of particles depends on many factors (e.g., water-solubility of organics, properties of particle surfaces, etc.). The authors have not shown even RH from their measurements. Without these detailed descriptions, the authors can say almost nothing here.

In figure 3, only one case for MAE, MSE, and MEE in HKUST (from only two-days sampling) and this is not mentioned in the figure caption. Are the dependences on size "general" characteristics? How about the GZ and BG samples and their differences compared to the HKUST samples (slopes etc.)?

The authors mostly use single values for any results without showing any uncertainties. As long as they are averages, they have variability (e.g., $\pm$1 sigma). The authors should show these uncertainties or variability. e.g., contribution of water content to particles 10$\pm$?% for GZ samples (p.23030, l.16-17), contribution of EC-containing particles to the observed light extinction (76$\pm$?%) (p.23037, l.6-7), etc.

Technical Comments: Figures 4 and 5: X-axis is not in the right order of time: x-axis in HKUST runs from 2008 to 2007. I suggest the authors to plot each category in different frames.

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