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Comment

Interactive comment on “Characteristics and the origins of the carbonaceous aerosol at a rural site of PRD in summer 2006” by W. W. Hu et al.

Anonymous Referee #3

Received and published: 26 September 2011

General comments

This work reports about one month’s online ECOC measurement data at a rural location in the Pearl River Delta (PRD) region. Such measurements were the first in the PRD region. As many other aerosol properties have been measured in the same campaign, the rich data set provides opportunities for more comprehensive understanding of ambient aerosols in this location. There are a few important data issues (i.e., irreconcilable OM and OC, erroneous assumption of negligible non-combustion primary OC) as detailed below that the authors need to explain before this paper can be published in ACP.

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1. Section 3.1 Comparison between organic aerosols and Fig.3: Here the authors compared OM measurements by AMS and OC measurements by the ECOC analyzer. Fig. 3 indicates that OM/OC ratio is 1.01 ± 0.02 . Such a low ratio signals that either OM or OC had poor measurement accuracy. If OC measurements are accurate (considering the calibration of the ECOC analyzer is fairly straightforward), then OM estimation by AMS is biased low. The interpretation of OM and OC data provided in the first paragraph in section 3.1 is in error. OM simply can not be lower than OC, as OC is part of OM. The authors need to discuss the measurement accuracy of OC by the ECOC analyzer and the uncertainty in the OM estimation derived from AMS data to explain why OM/OC was so low.
2. SOC estimation: The assumption of negligible non-combustion OC is not supported by the measurement data (pp21612, lines 6-7). The large intercept of the OC vs. EC plot for the nights of local emission days (15.1 ugC/m^3 in Fig. 7a) indicates the otherwise. SOC estimates are therefore questionable, at least for the local emission days.
3. Page 21605, Line 14-15: The authors cited the particle size measurements by Yue et al (2010) and stated that the difference between PM_{2.5} and PM₁ is negligible. An inspection of the volume size distribution data shown in Yue et al's paper indicates that PM_{1-2.5} is at the order of $\sim 10\%$ of PM_{2.5}. The authors should give a more specific value for the difference between PM_{2.5} and PM₁, given that this data can be calculated from Fig. 1 in Yue et al's paper.
4. In the discussion of EC and OC measurements, the authors have missed some relevant studies of ECOC in the PRD (e.g., Chow et al., 2005; Hagler et al., 2006; Yu H. et al., 2010). In particular, the study by Yu H. et al (2010) reported size-segregated ECOC measurements during the same field campaign.
5. Table 1: As SOC concentration estimation is one of the main focuses of this work, it will provide readers a better quick summary of the major results if the

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- SOC data can be included in Table 1. There is no mentioning in the text of SOC concentrations and SOC/OC ratios in three different periods. Readers would be interested in knowing whether SOC was enhanced during periods of strong local biomass burning, as some studies have reported gaseous biomass burning emissions could serve effective precursors to SOA formation.
6. Abstract and conclusions sections: “The average SOC concentration in BG site was about 2.0+/-2.3 ugC/m³”. Is this average value for normal days only (the local emissions days excluded)? If so, this needs to be clearly indicated.
 7. Fig. 4 shows that during some hours on 25 July, Cl⁻ concentration (from AMS measurement) was as high as 18.8 ug/m³. There were also high Cl⁻ hours on 31 July. Such high chloride concentrations in PM₁ are very rare. Can the author comment on the measurement accuracy of chloride by AMS and how chloride measurements by AMS and by IC compare.
 8. “precipitation” is mis-spelled as “participation” throughout the manuscript.

References:

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21601, 2011.

ACPD

11, C9305–C9308, 2011

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