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Atmospheric Chemistry and Physics Discussions

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.

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The authors wish to thank the referees' valuable comments and feedback on our manuscript. We have endeavoured to address each of the referee's suggestion and comments. A detailed response to the comments is shown below. We welcome any further feedback that the editor and/or referees may have.

Response to referee 3#

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

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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.

Response: We have changed the Fig. 3a and Fig. 3b. (Fig. 4a and Fig. 4b in the revised paper, Fig. 1 in this response paper). Comparing the original ones, we separately showed the data points for primary emissions days and normal days. For the primary emission days, the OM/OC ratio was quite low (1.0), which means the plume was quite fresh. The following part of SOC calculation also showed neglegible levels of secondary formation. For normal days, the OM/OC ratio was 1.6, which resulted in the average 47% OC attributed to be the SOC. In the original Fig. 3a, all the data points were linear fitted in the campaign and the slope almost equals to the value in primary emission days. We think that the fit is somewhat erroneous and the linear fit weight too much to the points from the primary emission days due to the extreme high concentrations. So, we corrected the figures and also the relevant text.

Text from paper, line 184-189: "...The OM and OC concentrations showed good correlations both in primary emission days and the other days (normal days, typhoon and precipitation days) (both R2 were 0.83) in Fig. 4a. During the primary emission days, the OM/OC ratio was quite low (1.0) that was much lower than the one (1.6) in normal and typhoon and precipitation days. Previous studies show that the more aged air masses tend to have higher OM/OC ratio (Aiken et al 2008; Turpin and Lim, 2001), implying the OM and OC in the other days were more aged than that in primary emission days. ...".

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 μgC/m3 in Fig. 7a) indicates the

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otherwise. SOC estimates are therefore questionable, at least for the local emission days.

Response: The non-combustion sources was explored by the intecept of direct regression results of OC with EC. The intecept of the OC/EC regression is -0.2, which means OC from non-combustion emission was negligible in normal days. However, the large intercept of the OC/EC regression in primary emission days (Fig. 2) may come from the regional background of OC other than combustion sources. This again proved that the sources of carbonaceous aerosol between normal days and primary emission days is different.

We also made some modification to the original manuscript, line 256-261: "...OCnon is referred to the non-combustion OC from biogenic primary OC or regional OC background. The intercept of the OC/EC regression line is regarded as the OCnon. The OCnon in normal days was around -0.2, which suggested OC from non-combustion emissions was small and it does not significantly affect our results. Thus, OCnon in normal days was not considered here, which is also consistent with previous studies (Lim and Turpin, 2002; Cabada et al., 2004; Polidori et al., 2006). ...".

3. Page 21605, Line 14-15: The authors cited the particle size measurements by Yue et al (2010) and stated that the difference between PM2.5 and PM1 is negligible. An inspection of the volume size distribution data shown in Yue et al's paper indicates that PM1-2.5 is at the order of 10% of PM2.5. The authors should give a more specific value for the difference between PM2.5 and PM1, given that this data can be calculated from Fig. 1 in Yue et al's paper.

Response: Thanks for referee's comments. We added the evidence to the paper. From the research of Yu et al (2010), the author also showed that the EC below PM1 contributed nearly 100% of total EC, which confirms our results.

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

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H. et al., 2010). In particular, the study by Yu H. et al (2010) reported size-segregated ECOC measurements during the same field campaign.

Response: Thanks for referee's reminding. We read the paper for the part that referee mentioned above. Some of the papers metioned above were added to the citation in our paper, including the paper of Yu et al. (2010).

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 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.

Response: In table 1 the SOC data from the selected literatures are incomplete, therefore, the SOC is not included. We agree with referee's comments. Among the three periods, only normal days was used to calculated SOC concentration. Because the removal effciencies of OC and EC were different in rain days, the (OC/EC)pri in typhoon and precipitation days is hard to define. So SOC in this period was not calculated. During the local emission days, the OC strongly correlated with EC. The traditional and the modified EC-tracer method both attributed most of OC concentrations to primary emitted. Although secondary formation from biomass burning influenced emissions is interesting and very important, it is also difficult to seperate primary and secondary biomass burning in the atmosphere. The EC-tracer method cannot fulfil this task at this time.

6. Abstract and conclusions sections: "The average SOC concentration in BG site was about 2.0$\pm$2.3 μgC/m3". Is this average value for normal days only (the local emissions days excluded)? If so, this needs to be clearly indicated.

Response: The value of 2.0$\pm$2.3 μgC m-3 is specially for the normal days. The text in

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the paper has been corrected as "...the average SOC concentration (normal days) at BG site was about 2.0±2.3...".

7. Fig. 4 shows that during some hours on 25 July, Cl- concentration (from AMS measurement) was as high as 18.8 μg/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.

Response: The Cl- concentration was measured by AMS and calculated using the method introduced by Allan et al.(2004). A "partial" mass spectrum was extracted corresponding to a particular chemical species (here is Cl-) from the ensemble mass spectrum using known relationships between the peaks (Cl- relates m/z 35 and m/z 36 here) of all of the species involved. The detection limit for Cl- (AMS) is 0.03 μg m-3. The comparison of Cl- concentrations between AMS and IC can be obtained from the papers (Takegawa et al., 2009; Takegawa et al., 2005).

8. "precipitation" is mis-spelled as "participation" throughout the manuscript.

response: Thanks for pointing out the mistake. The wrong spelling has been corrected.

Reference:

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Hagler, G. S., Bergin, M. H., Salmon, L. G., Yu, J. Z., Wan, E. C. H., Zheng, M., Zeng, L. M., Kiang, C. S., Zhang, Y. H., Lau, A. K. H., and Schauer, J. J.: Source areas and chemical composition of fine particulate matter in the Pearl River Delta region of China, Atmos Environ, 40, 3802-3815, DOI 10.1016/j.atmosenv.2006.02.032, 2006.

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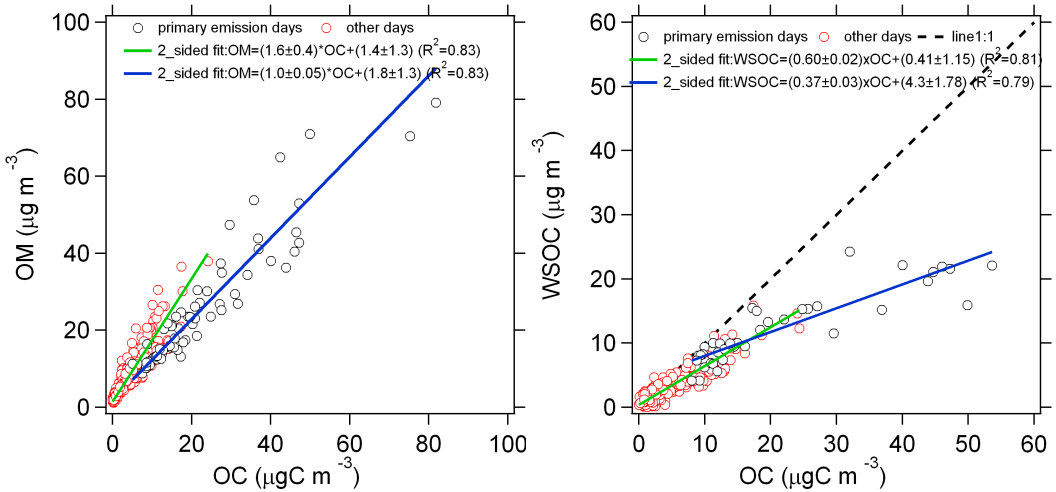

Fig. 1. The correlations of measured data between (a) OM and OC; (b) WSOC and OC.

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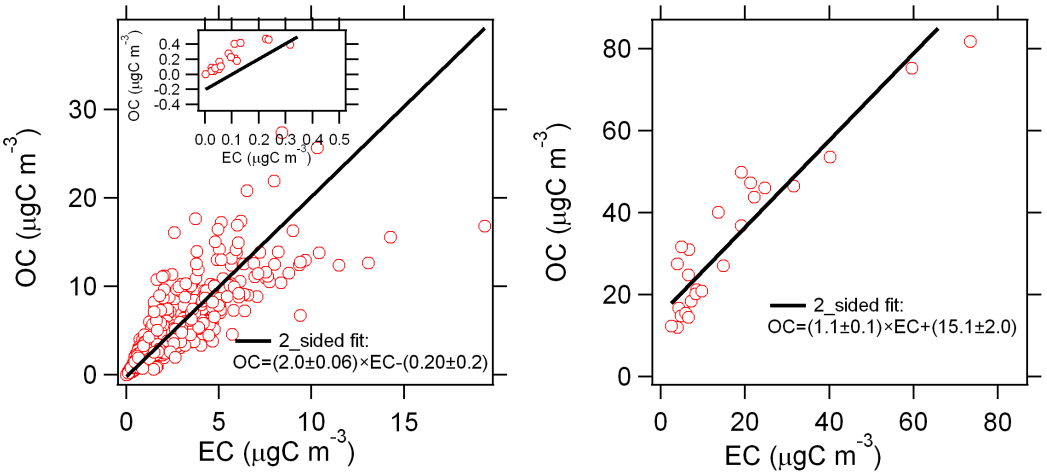

Fig. 2. The scatter plot of OC and EC in normal days and primary emission influence days.

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