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Interactive comment on "Seasonal variations and spatial distribution of carbonaceous aerosols in Taiwan" *by* C. C.-K. Chou et al.

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Based on an observation network of 7 stations in Taiwan, a new data set for carbonaceous components (EC, POC, SOC) of both PM10 and PM2.5 samples collected in the 5 years (2003-2007) became available. This is a valuable extension of the existing data bank. Analysis and evaluation results of this data set were presented in this manuscript. A strong urban-rural contrast was observed for EC and POC, and a north-south contrast for SOC. The author pointed out that the local sources had dominant contributions as the spatial distribution of EC was consistent with CO and NOx across the network stations, and correlation was found for SOC and particulate nitrate. Sea-

sonality was also discussed for EC, POC, and SOC. The tables and figures were nicely formulated that facilitate discussions and explanations in text. Particularly the Table 2 and Table 3 presented annual means for 5 consecutive years and seasonal means for 2003-2007. The English description is sound and easy to follow. The manuscript is well structured and clear.

Specific comments are as follows. (P7086, line 8-25 and P7087, line 1-8) The EC tracer method is simple and easy to use. Therefore it enjoys widespread applications. However, it is important to be aware of the fact that the real situation may be fairly complicated. As Yuan et al., 2006 pointed out 'In comparison, the method that uses EC as a tracer for primary carbonaceous aerosol sources to derive SOC overestimated SOC by 70–212% for the summer samples and by 4–43% for the winter samples. The overestimation by the EC tracer method resulted from the inability of obtaining a single OC/EC ratio that represented a mixture of primary sources varying in time and space.' The author has discussed that α is a source-specific parameter and varies from one source to another. It is also worthwhile to note that it is season-specific as well. According to Figure 2 in Yuan et al., 2006 this parameter had a factor of two variations (summer 0.41; winter 0.88; spring 0.73; autumn 0.70). It seems more reasonable to extract a parameter (OC/EC)primary from a certain season and use it for that season at the site, rather than to take the annual mean (OC/EC)primary for the year as a whole. It may be advisable for the author to work out a new table similar to Table 1, listing linear regression results (slope and intercept) for the relationship between primary organic carbon and elemental carbon for each of four seasons at seven stations. It would be more scientifically sound to calculate SOC for each season using a season-specific parameter (OC/EC)primary .

(P7083, line 10-12) Here it is said that 'In urban areas, the POA are believed to be mostly from the exhaust of vehicles, whereas biomass burning was suggested as the predominant POA source on global scale (Hallquistetal., 2009). ' However, biomass burning was not mentioned elsewhere in the text. There are some previous studies

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concerning biomass burning in this part of the world. Even in urban and coastal sites of Taichung, Chio et al., 2004 pointed out 'Vehicle emissions was the most important source of PM10 at the urban site, followed by crustal materials, secondary aerosols, biomass burning, industrial emissions and marine spray. There was a similar pattern of sources at the coastal site, . . . Although biomass burning and secondary aerosols were not main sources during clean air quality periods, they were the influential sources causing the increase of PM10 to "episodic" levels at both sites.' There are several more previous studies presented details on biomass burning contributions from either rice straw burning in the agricultural area in Taiwan (Yang et al., 2006; Lee et al., 2008) or long-range transported biomass burning emission from Indochina (Lin et al., 2009). I would like to comment that as a paper on carbonaceous aerosols in Taiwan the biomass burning contributions from either local emission or long-range transported need to be taken into account. Why not leave it open and new insights may come soon from this rich data set when the data evaluation continues, as this manuscript is based on general statistical analysis and little attention has been paid to episodic cases, let alone the application of air mass back trajectory analysis.

(P7095, line 21-22) Here it is said 'Instead, the spatial distribution of SOC was characterized by a north-south contrast.' This 'north-south contrast' shows up suddenly in 'Conclusion', without a proper discussion in 'Results and Discussion' sections.

Five Papers cited. 1. Yuan, Z. B., Yu, J. Z., Lau, A. K. H., Louie, P. K. K., and Fung, J. C. H.: Application of positive matrix factorization in estimating aerosol secondary organic carbon in Hong Kong and its relationship with secondary sulfate, Atmos. Chem. Phys., 6, 25-34, 2006. 2. Chia-Pin Chio, Man-Ting Cheng, Chu-Fang Wang; Source apportionment to PM10 in different air quality conditions for Taichung urban and coastal areas, Taiwan, Atmospheric Environment, 38, P 6893-6905, 2004 3. Hsi-Hsien Yang, Cheng-Hsien Tsai, Mu-Rong Chao, Yi-Ling Su, Shu-Mei Chien; Source identification and size distribution of atmospheric polycyclic aromatic hydrocarbons during rice straw burning period, Atmospheric Environment, 40, P1266-1274,2006 4. James J. Lee,

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Technical corrections 1. Figure 6 could be improved by using different colors for four seasons. 2. P7081, line 6; 'in the aerosol field', what does it mean? 3. It would be nice to give an exact number of the samples on which this manuscript is based.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7079, 2010.