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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General comments

This paper reports five-year measurements of carbonaceous components in PM10 and PM2.5 made at seven sites in Taiwan, where ambient measurement data are limited. The present work may provide valuable data sets in our understanding on chemical properties of OC and EC in Taiwan. The material itself presented here likely fits with the scientific scope of ACP. However, there are a number of speculations in the discussion part, which has not been scientifically supported. The manuscript lacks scientific rigor without careful examination of the data (see comments below), which makes discussion rather loose throughout the manuscript. Although a large data set presented is really valuable, there are a number of important issues that need to be worked out. For above reasons and specific concerns given below, I do not recommend its publication

in ACP.

Specific comments

One of the most important questions which I could not understand is that "Are SOCs observed in Taiwan dominated by local sources or by effects of long-range transport from China?" The authors stated that the SOC precursors were mainly from local anthropogenic sources, whereas they also concluded that the Asian outflow (from China?) may affect the observed levels of aerosols. If the aerosols were transported from upwind source regions, the estimation of SOC using the EC-tracer method has huge uncertainties and is even invalid. This is because the primary OC/EC ratio used to estimate secondary OC is valid ONLY when carbonaceous particles are emitted from local sources (here, around each sampling site). Also if the emissions of biogenic precursors contributed to SOC (P.7096, L12-13), then the EC-tracer method cannot be used. With respect to the possible contributions of biogenic sources, how do the authors explain the Figures 7 and 8 that show relationships between SOC and anthropogenic tracers? Overall the discussions on SOC sources are not systematic and are difficult to follow.

Estimation of primary OC/EC ratios:

(1)The ratio may change depending on season. If not, the authors should at least show the results of the ratios in each season. Also, are the data with rainfall events (which may contain lower OC/EC ratios) excluded? This may also affect the uncertainties of the ratios.

(2)P.7087,L.20-22: The authors note that significant β values in several sites are due to non-combustion sources. Is there any possibility that the offset is caused by field blanks (e.g., VOC artifacts)? These blanks are different from laboratory blanks and are not mentioned in the manuscript.

Experimental methods: Several basic information are missing. For example, the authors should describe the number of data in each category, and definitions of "spring", "summer", "autumn," and "winter" periods in the text or tables.

P.7087, L.24-25: "The causes of . . ., likely due to uncertainties in the sampling and statistics processes." I could not understand the meaning.

P.7088, L.24: "the SOC concentration in Hualien was comparable with those in the western Taiwan" Is there any possibility that SOC in Hualien is just photochemically aged transported from a long distance?

Figure 6:

(1)Actually, the data are not shown as mass concentrations but as "normalized" concentrations. How did the authors derive them? Why did they use these values?

(2)Also only the values for PM10 are shown, because "the carbonaceous aerosols were mostly confined in the fine mode. (P. 7090, L.23)" However, significant fractions of mass were also found in the coarse mode particularly for OC as seen in Table 2. In fact, the authors have discussed possible contribution of sea-salt and dust (P.7091, L.11) (without showing the authors' data). The authors' statement seems to be inconsistent and not systematic.

(3)Are all the difference discussed in the text statistically significant? In table 3, "the standard error" values are shown in parentheses. Are they standard deviations or 1-sigma values? The values seem to be too small considering that the individual data points show large variability as shown in Figures 2 and 3.

(4)The authors have mentioned that the decreased concentrations of aerosols in fall are due to "meteorological conditions that favored dispersion of air pollutants." However, none of the meteorological parameters (wind speed, wind direction, temperature, RH, etc.) is shown in the manuscript. I cannot understand what the "meteorological conditions" are and how they affect the concentrations of aerosols. This point is true for descriptions in L.22-29, in P. 7091: they are only speculation because the authors

have not shown their original meteorological data.

P.7089,L.25: "For instance, . . . (14% vs. 11% for PM10; 18% vs. 16% for PM2.5)." Are the differences of these numbers statistically significant? The comparison should be statistically strict.

P.7095,L.16-17: ". . . the reduced amount of diesel vehicles in the eastern Taiwan" The authors have not shown any supporting material (e.g., emission inventory, etc.). Again, it lacks scientific rigor.

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