Referee Comment ACP-2013-912

General Comment:
While the manuscript does present some useful and hard won data, the analysis and interpretation is quite weak and far from convincing. As noted below, this is the case throughout the paper, but most evident in the section on the estimation of (OC/EC)vehicle. There is little or no attempt to compare and contrast the results presented with the many similar studies done by numerous researchers (including some referred to in the manuscript). Given these serious deficiencies, I cannot recommend full publication in ACP.

Major Concerns:
Section 3.2: The authors describe and use an empirically based method for estimating the contributions of primary and secondary carbon to their measurement data. The authors make reference to earlier work by Harrison's group (Castro et al., 1999) and Turpin's group (Lim and Turpin, 2002). However, their analysis produces results that is not at all convincing and leads me to believe that the assumptions required to yield reasonable estimates for this method may not hold in this case. The Castro et al. work showed urban OC/BC ratios ranging from 1.1. to 1.3, while the Lim and Turpin work showed OC/EC ratios ranging from 1.75 to 2.09 for the data grouped OC/EC ratio (note that all intercepts in this work were positive, on average). In contrast, the authors present estimates for the (OC/EC)pri ratio in Tables 1, 2, and 3 that range from 0.41 to 1.49 (Table 1), from .53 to 1.41 (Table 2) and from 0.44 to 8.56 (!) (Table 3). Another problem is the large number of negative intercepts in their regressions, some as large in magnitude as -2.16. What does this mean physically? There is no acknowledgement of this being a problem, and no explanation. After reading this section, I have no choice but to conclude that this analysis method is either inappropriate for this data set (most likely), or improperly applied. The results simply don't make sense!

Compounding the problem, the authors do not attempt to put their results in the wider context of the many similar measurements made by other groups for many years. As noted above, they reference the Castro et al. and Lim and Turpin papers to introduce the method, but do not show how the results compare. I did not do an exhaustive review, but I am confident there have been some, and maybe many, studies doing similar work in Asian cities in recent years. At least a significant subset of this literature should be referred to, and used to compare and contrast any findings.

Specific Comments:
p. 62, line 18: I believe the average should be 0.35 ugC/m3.
p.65, lines 24-28: While the different sampling periods could increase the scatter, it is not clear how or why it could cause bias!

p. 67, lines 6-19: OC observed at a roadside location is pretty much always dominated by local sources, vehicles in particular. If the OC and EC were not constantly being produced by local sources, there would be a very clear decrease in the middle of the day as the boundary layer reaches its maximum depth.
p. 68, lines 3-11: Figures 4 and 5 show NOx levels on the order of 100-150 ppb and ozone levels on the order of 10-15 ppb. The oxidants in this environment are totally controlled by NO2, and comparing OC to ozone alone is not all that informative.
p. 68, lines 12-17: The authors do not show this data. If the data is plotted with uncertainty error bars, are the "peaks" robust? The text is descriptive, but gives no physical interpretation.