

Interactive comment on “Temporal variation of elemental carbon in Guangzhou, China, in summer 2006” by R. L. Verma et al.

Anonymous Referee #1

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General Comments: While this study focuses on an area of great interest and concern, the conclusion and analysis contained in this manuscript are largely unremarkable. The stated objective of the study is to use the temporal variations in the relationship between EC and gas-phase combustion markers (CO and CO₂) to validate existing emissions inventories and to characterize emissions from the sources of these species. The authors conclude that traffic patterns and meteorological conditions largely influence variations in EC, CO, and CO₂ concentrations, and while the data presented in the manuscript support these conclusions, such observations are not unique and have been reported extensively in the literature. The data set, however, is extensive and unique, and the authors miss an opportunity to use their observations to estimate the impact of various sources on EC concentrations and thus provide a methodology for estimating health and climate impacts and to more vigorously verify emissions inven-

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tory data. Important questions are left unanswered or poorly articulated, e.g., what are the relative impacts (observed) of diesel engines, industrial facilities, and transported pollutants on EC, CO, and CO₂ concentrations? How do the contributions of sources vary with respect to time (hour-of-the-day and day-of-the-week)? From a quantitative perspective, how valid are current emissions estimates?

In order to make a clear contribution, address relevant scientific questions within the scope of this journal, and reach a more substantial and relevant conclusion, I believe that the authors need to make major revisions to the manuscript in order to address the key questions outlined above and the detailed comments/questions outlined below.

Specific Comments: Page 24630, Line 25: Although EC and BC are often used interchangeably, these terms can also be differentiated with respect to the analytical methods used to measure them. In the interests of clarity and the growing body of literature comparing thermal and optical methods, the authors should either clarify this distinction (i.e., BC = optical, EC = thermal) and briefly discuss how comparable these methods are, particularly in light of the comparison data shown in Table 5.

Page 24631, Line 5: While PM has been shown to act as CCN, this reference may not be appropriate with respect to EC. It might be more appropriate here to reference works demonstrating the adverse health impacts of exposure to EC and diesel particulate matter (DPM) in light of the high concentrations of EC reported in this paper.

Page 24633, Line 13: Does the detection limit refer to EC, OC, or TC? Was an external standard used? How often was the EC/OC filter changed during the study?

Page 24634, Line 16: Where do these emissions data come from? A reference is needed here.

Page 24637, Line 10: Throughout the manuscript, the authors report concentrations and mixing ratios +/- standard deviation. What is the purpose of reporting SD? Are the authors assuming that their measurements are normally distributed and the SD is

an indicator of whether concentrations are statistically different? Would standard error be a better choice? For example, Figures 5, 7, and 8 give the impression that concentrations and ratios are not significantly different. The authors need to demonstrate that these concentrations and mixing ratios are indeed significantly different in order to support their conclusions.

Page 24638, Line 12: What is the range of EC/CO and EC/CO₂ expected from HDVs?

Page 24640, Lines 14 – 15: Can this slope be used to quantitatively estimate the contribution of HDVs to EC concentrations observed at the study site?

Section 8.2: It is unclear why mass concentrations are compared across these different sites. These data are best left out of the manuscript or included in supplemental material.

Page 24642, Line 15 - 16: Seems awkward to make conclusions about Tokyo based on data from China when the focus of the paper is China. This sentence should be reworded.

Section 8.3: It is difficult to see from this discussion and the figures/tables how a comparison of EC/CO, EC/CO₂, and CO/CO₂ ratios validates emissions inventory data. Can the derived ratios be used to estimate the contribution of each source to the observed EC concentrations via multi-linear regression, for example? How would these contributions compare with the emissions data shown in Table 1?

Page 24643, Lines 10 – 11: What is meant by reasonably well? It is unclear how these data validate the emissions inventory. The authors qualify their conclusion by claiming the emissions inventory is validated “to some extent.” In order to be useful, this conclusion needs to be better quantified.

Table 2: Wind data should be relegated to supplemental material.

Table 3: How were the time periods (day/night) decided upon? The caption should indicate what time periods correspond to day and night.

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Table 4: As with Table 3, more information on day/night split should be provided in the caption/text.

Figure 2: It is difficult to accept that traffic patterns/volumes have not changed since 1999. Is there any additional data available that might support this assumption?

Figure 5: See previous comment about using SD in these plots. At first glance, the differences shown here with error bars do not seem to be statistically different.

Figure 6: Again, I am not sure of the appropriateness of comparing data from 1999 and that from 2006. The caption must, at the very least, indicate that the HDV data is not from the same time period, otherwise it is deceptive.

Figure 7: It is difficult to tell from these time-series plots if there is a statistically significant dependence between these variables. The authors should show XY scatter plots complete with R² values in order to validate any dependence.

Figure 8: This graph is confusing and difficult to evaluate. How valid are these diurnal variations if the correlations coefficients are so widely distributed? Do the variations in R² coincide with changes in the relative contribution of various sources to EC concentrations? Again, the error bars appear to indicate few statistically significant differences in these data.

Figure 9: Since the R² values are so poor, this figure adds little value and should be placed in supplemental material or left out entirely.

Technical Corrections:

Table 5: Footnote reads "():r²" but no correlation coefficients are given

Figure 10: Make sure to include the definitions of HDV and HDDT in the caption

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24629, 2009.

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