

## ***Interactive comment on* “Size distributions of elemental carbon and its contribution to light extinction in urban and rural locations in the Pearl River Delta region, China” by H. Yu et al.**

### **Anonymous Referee #2**

Received and published: 7 January 2010

The authors report the measurement data of the EC size distribution at seven location in the PRD region, China. In addition, they calculate the light extinction coefficient based on the EC and other chemical species data. Most of the measurement data are collection of the measurements reported preciously by the authors. Thus, I think this part can be drastically reduced or shortened. For example, sections 3.1 and 3.2 can be combined into one section with concise description of the major characteristics.

The approach they used in the estimation of the light extinction coefficient is an interesting one. I believe this kind of data analysis is a valuable one. However, there are several approximations or assumptions to calculate the light extinction coefficient.

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One example is that the observed light extinction values at HKUST used were obtained from a site 10 km from HKUST. With this kind of approximation, it is hard to validate the estimated values with the 'so called' observed one since the light extinction coefficients apart from 10 km might be significantly different. Another example is that the concept of the hypothetical particle (in subsection 3.3.1) which needs more validation or confirmation. In addition, it is essential to carry out more detailed sensitivity analyses on the values used in the calculation.

Thus, I think the manuscript is not suitable for publication in the Journal at present form. I recommend that the authors revised the manuscript, especially on the validation of their calculation approach and resubmit the manuscript.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23021, 2009.

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