

## ***Interactive comment on “Impact of relative humidity and particles size distribution on aerosol light extinction in urban area of Guangzhou” by Z. J. Lin et al.***

**Anonymous Referee #2**

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Review Comments This manuscript proposed a practical method to calculate aerosol light-extinction coefficients by considering aerosol chemical properties and size spectra under various assumptions. More specifically, aerosol chemical properties were utilized to acquire effective volume-averaged optical properties and effective hygroscopic growth factor  $f_{\text{eff}}$  firstly. The measured aerosol size spectra from APS were then converted from aerodynamic diameter to Stokes diameter assuming uniform  $f_{\text{eff}}$  over all sizes. Aerosol wet mass concentration was finally calculated using Stokes diameter, number spectra, and wet density. I would agree that this paper is well presented and has potential to be published in Atmospheric Chemistry and Physics after the following comments have been sufficiently addressed. 1. SCIES,

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the acronym of South China Institute of Environmental Sciences, should be defined in the first appearance as it is shown over the whole manuscript. This rule should also apply to other acronyms. 2. In Abstract and line 5 on page 15644, DIR should be corrected to DRI. Same thing to line 7. 3. The data of instruments not used in this manuscript, for examples, MOUDI, TDMPS and HDMPMS can be eliminated from “Introduction” section to avoid from distracting the focus of this manuscript. 4. The volatilization of aerosol  $\text{NH}_4\text{NO}_3$  is well acknowledged and this effect on the aerosol chemical properties in this study should be addressed. 5. It is generally acknowledged that fragile quartz fiber filter is not good for using in mass weighing. Unfortunately, this manuscript used quartz fiber filter for mass weighing, it is suggested to address possible loss of filter debris and thus underestimation of aerosol mass in the weighing process. 6. Although the electrical charge neutrality was assumed, a validation of this assumption should be made for the data of this study. 7. POM needs to be defined in line 5 on page 15646. 8. It is hard to assess the adequacy of the factor of 1.8 applied to an urban area like Guangzhou for this study as the cited reference not published in a scientific journal. 9. On page 15646, the discussion on potential combined forms of inorganic compounds with assumptions of preferential association of  $\text{NH}_4^+$  with  $\text{SO}_4^{2-}$  or  $\text{NO}_3^-$  and similar inferences for other ion pairs is speculative. For field data in this study, preferential reactions are uncertain and assumptions made may not be applicable. For example, Huang et al. (AR 2011, 99:488) observed aerosol data in Guangzhou and found that higher nitrate involving ammonia and nitric acid were observed for  $[\text{NH}_4^+]/[\text{SO}_4^{2-}] > 1.5$ . Moreover,  $\text{K}^+$  only combined with  $\text{Cl}^-$  at a place very close to biomass burning sites. It is better to apply thermodynamic models such as ISORROPIA II (Fountoukis and Nenes, ACP 2007, 7:4639) and AIM (Ge et al., 2011, Atmos. Environ. 45:561) to this end. 10. Line 2 on page 15647, POM, EC and other unidentified components were assumed not having hygroscopic growth. This might not be true as the salts of minor organic acid such as dicarboxylates are shown to absorb water. An evaluation on the effect of this assumption must be added to the text. 11. The subscript for aerosol component,  $j$ , is the same as the  $j$ -th size range of

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aerosol size spectra. Either one of the notations needs to be changed to avoid from confusing. In addition,  $a$  (in equations 10, 11, and 12) and  $a_j$  (in equation 4) are also confusing. 12. The  $Q$  values for the calculation of  $bsp$  and  $bap$  are suggested listing in a new table in the manuscript. 13. Was the time period for the calculation of  $bap$ ,  $pm_{2.5}$  the same as the cited study (Wu et al., 2009)? If not, provide a discussion for the effect from this deviation as the relationship should be different. 14. Line 15 on page 15651, size should be sizes. 15. Was the mass concentration in Figure 3 based on the controlled RH (40%) in the weighing room or the ambient RH or a more complicated way? In calculating “residual” of Figure 3, one needs to know whether the “water” was estimated from ambient RH or the controlled RH in a weighing room. 16. There are deviations observed from  $bsp$ ,  $pm_{0.5-2.5}$  and  $bsp$ ,  $neph$ . Please provide some discussions on the data pair when the deviations are great. 17. Please provide more discussions and literature supports on why  $PM_{0.5}$  dominated over aerosol mass fractions in Figure 7. 18. Fig. 8 (mentioned in line 6 on page 15655) was missing in the manuscript.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/12/C5651/2012/acpd-12-C5651-2012-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15639, 2012.