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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An alternative method estimating hygroscopic growth factor of aerosol light scattering coefficient: a case study in an urban area of Guangzhou, South China" *by* Z. J. Lin et al.

Anonymous Referee #2

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This study proposed a modified parameterization for the hygroscopic growth factor of aerosols. It was claimed that the new parameterization formula improved the values of f(RH) in low RH regime. Given that the interaction between aerosol and water vapor is a vital mechanism of haze formation; such an effort is worth encouraging and is helpful to explain/project the degradation of visibility. However, as comparing with the results from a study at the same city (Guangzhou) of this author (Lin et al., ACP, 2013, 1115-1128) I found that the improvement could be within the uncertainties. Moreover, there are some issues need further clarification and/or correction in the manuscript. Thus, I

think it cannot be accepted as an ACP paper in the present form.

Specific comments:

1. Methods: MSP high flow impactor was used to collect aerosol samples in this study. This sampler usually performs well. However, the filter samples are NOT suitable for analysis of OC/EC using thermo-optical instrument. The analysis method used in this work (NIOSH 5040) requires highly uniform distribution of aerosol particles on the filters, whereas the samples from MSP impactor, particularly the first 3 stages, cannot meet the requirement. Thus the measurements of OC and EC in this work could be associated with high uncertainties.

2. Methods: Mie calculation is very sensitive to the size distribution of aerosol species. However, the size distribution data from a high-flow impactor are usually suffering from particle bounce. This is particularly concerned in case where filters, instead of metal foils with grease coating, are used as the substrate. Thus I suggest make an uncertainty evaluation upon the size distribution measurement in this work.

3. Results 3.1: The analysis of charge balance in aerosols (Figure 2a) is wrong. The calculation should be based on “charge equivalence”, not on “molar concentration”.

4. Results 3.1: The relationship between OC and EC is in an unusual pattern. This could be a result of the mis-match between sampling and analysis methods.

5. Results: Na2SO4 showed peak in submicron size range (Figures 3-4). As particulate sodium comes mostly from sea sprays, it is unusual to have the species existing mostly in submicron range. I strongly suspect the occurrence of particle bounce in the impactor.

6. Results 3.3: The parameterization of growth factor, f(RH), is the core of this work. I suggest make a detailed evaluation upon the improvement of your method with the previous ones. Showing the sample table of statistics is not enough to convince a scientist. As shown in Figure 6, it’s hard to make judgment upon the differences between

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the results of this work and the others. Moreover, as shown in your previous relevant paper published in 2013, the old method has been good enough to give a prediction of f(RH) within the uncertainties of measurement. Thus I'm not convinced that the improvement proposed here is significant.

Technical corrections:

1. Throughout the manuscript, "ISOPPORIA" should be corrected as "ISORROPIA".

2. Table 4: "NO3+" should be "NO3-"

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 435, 2014.