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Atmospheric
Chemistry
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Discussions

Interactive comment on “Optical properties of absorbing and non-absorbing aerosols retrieved by cavity ring down (CRD) spectroscopy” *by* A. A. Riziq et al.

Anonymous Referee #4

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General comments:

This article presents an experimental study on the optical characterization of aerosols (absorbing and non-absorbing) using the cavity ring down technique. This technique is largely applied in the field of molecular spectroscopy, but only few works, well reviewed by the authors, use it for aerosols studies. As other groups, their CRD setup determines experimentally the refractive index of aerosols. The originality of this work relies on the measurement of this index for pure and mixture of aerosols. It enables a direct comparison among different mixing rule models (molar refraction/absorption, linear, Maxwell-Garnett, extended effective medium approximation) and the core plus shell Mie model. This is greatly relevant for the calculation of aerosol radiative properties in climate models. As a validation of their experimental setup, they retrieve the known refractive index (real and imaginary part) of polystyrene spheres and ammonium sulfate aerosols in a dimension range of hundreds of nanometers by measuring the absorption coefficient of mondispersed aerosols at 532 nm for different concentrations and for several dimensions, and by adjusting experimental data with the Mie scattering calculation. Using the same procedure, they determine refraction index of aerosol mixtures with variable molar mixing ratio (sodium chloride + glutaric acid for non-absorbing mixtures, and rhodamine 590 (peak absorption at 530 nm) + ammonium sulphate for absorbing mixtures), and the refractive index of each pure aerosols. Accuracy of the results enables to reject the core plus shell model and the molar refraction/absorption model in case of non-absorbing species which is consistent with the aerosol generating system. However, data accuracy of absorbing aerosols is not sufficient to perform any discrimination among models.

The paper is clear and should be published in ACP. However, as it valid or not the experimental results, several points concerning the precision and the accuracy should be discussed more in detail in the published version.

- GC1 : The dispersion of the monodispersed aerosol population is not specified and the possible modification of the Mie extinction curve when it is taken into account should be discussed. - GC2 : The sensitivity is given for the empty cavity. Is it extracted from the fit algorithm or deduced from a statistical analysis on the ring down time value (shot-to-shot measurement)? The second is the right way to proceed in case of molecular absorption measurement. When measuring extinction of an aerosol flux, the ringdown standard deviation also reflects the fluctuation of the aerosol number included in the cavity mode volume. For instance, when measuring the extinction of a concentration of 100 pp/cm3, given your cavity (mirror curvature and cavity length fix the volume of the interactive TEM00 mode around 0.2 cm3), only 20 pp are interacting in average with the cavity mode. Hence, large fluctuations on the ringdown signal should be observed and it should drastically increase the time of averaging before being able to obtain the claimed sensitivity (if this is done (?)). - GC3 : Are the error bars of the extinction values deduced from the linear fit results? For the linear fit algorithm, each data point should be weighted by its standard deviation extracted from the ringdown shot to shot measurement, is it the case? I will expect an error bars decrease when size increase, it seems not to be the case, are there any reasons?

- GC4 : The choice to extract results from a subset of data (starting from 350 nm) is not so clear. It is true that the lower sizes of the polystyrene sphere give higher extinction than expected and this could be well explained by larger multiply-charged particles. However, as lower sizes of ammonium sulphate aerosols (as well as NaCl or mixture with rhodamine) do not give higher extinction value, it does not hold as a general way to proceed.

Addressing those different questions and trying to identify the limiting factors of the experimental approach would strongly increase the article interest.

Specific comments:

- SC1 / p. 12351 (l. 18): “Deacan” replaced by “Deacon”

- SC2 / p. 12354 (end) :

- SC3 / p. 12358 (l.25) : the “retrieved index” is not n=1.53+i.0.0 but is n=1.52+0.i for the subset of size (l.17). Thus, it agrees well with the two first axis and not the third.

- SC4 / p. 12360 (l.2) : why “the fits for the subset Ě are shown” since the next paragraph indicates better agreement when all the size contribute to the fitted curve ?

- SC5 / p. 12362 (l.16) : “Ěincreased the errorĚ” a discussion about the random or systematic nature of this error would be interesting.

- SC6 / p. 12364 (l.4) : “As beforeĚ” this is in contradiction with the second paragraph of p. 12360.

- SC7 / fig. 7 : The scale of the residual axis is missing. Also, the ripple structure should be washed out when comparing the residual at small and large sizes. Doing this, the conclusion should be mitigated since the difference is lower than a factor 2.

- SC8 / fig.9 : The last sentence of the caption is partly repeated.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12347, 2006.

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