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Comment

Interactive comment on “Light extinction by Secondary Organic Aerosol: an intercomparison of three broadband cavity spectrometers” by R. M. Varma et al.

Anonymous Referee #1

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Overview: The technology behind these measurements is innovative and very useful to help understand radiative transport in earth’s atmosphere. Simultaneous, broadband measurements really offer exciting opportunities to probe the chemistry of the gas-phase while gaining the added bonus of understanding aerosol optics.

The paper also presents important data for SOA formed from pinene + NO₃ - presumably such aerosol may form at night in areas heavily influenced by biogenic emissions, so estimating the refractive index is a worthwhile goal. I think the paper can be accepted for publication in AMT provided some concerns are addressed. These concerns focus on providing the best estimate of refractive index possible for the SOA studied.

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Specific Comments:

One concern is the experiment that is presented as a “validation” of sorts in Figure 1. In this experiment ammonium sulphate aerosol is probed and Mie theory used to provide validation of IBBCEAS measurements. While any NO₃ absorption seems to successfully be “deconvoluted” from the remaining signal, the aerosol extinction coefficients reported are very, very low. Given the tendency for the instruments to exhibit baseline drift, I would encourage the authors to attempt this experiment at higher aerosol loading so it can be determined whether the 20-25% discrepancy between Mie model and measurements is conserved. Also, why no CE-DOAS or BB-CRDS data? The result of this experiment is obviously very important to ascertain the uncertainty in subsequent measurements, and ultimately, the refractive index reported for the SOA proxies.

Figure 2 – it is a pity no particle count concentration data exists for this experiment. It would provide a great opportunity to study how the refractive index evolves in time. Also, if AMS data could be added - that would be a nice improvement.

One improvement that could be offered would be the addition of a description of how aerosol extinction was extracted from the raw data. Would be valuable to this reader from a tutorial perspective.

Bottom of page 6703- authors assume uncertainty in measurements of 10%, but data in Figure 1 deviated from Mie theory by more than this. So authors may wish to expand uncertainty range or simply try measurements at higher levels of extinction for the ammonium sulfate so the performance can be better assessed.

End of page 6706 – the authors write a section about “extinction minus scattering” approaches. It is very unclear why this discussion occurs to me here. I would suggest removing this since this was not attempted in the work, and it “feels” a bit distracting to the reader.

Page 6707 – description of broadband UV retrievals. It seems to me that use of such a

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method in the UV may be considerably more difficult because if any absorbing gases are present that are not accounted for in the model, aerosol extinction would be over-estimated. Despite the vast knowledge that exists regarding absorbing atmospheric gases, some trace components may be unaccounted for – and these absorbing components would mask as aerosol extinction (particularly if broadband absorption). This would cause the worst type of uncertainty in aerosol extinction – one of unknown, potentially variable quantity. In general, this section of text could easily be omitted without much value loss to the literature.

On page 6709 the authors conclude that organic nitrates may be responsible for the high refractive index determined. It seems to me this is perhaps a logical conclusion. Since it appears that AMS experiments were conducted simultaneously, can the authors provide any chemical speciation data to provide insights into composition? Did the aerosol composition change in time?

Also, nitrated aromatics are known to absorb light in the visible portion of the spectrum (see Jacobson, M. Z. (1999), Isolating nitrated and aromatic aerosols and nitrated aromatic gases as sources of ultraviolet light absorption, *J. Geophys. Res.*, 104(D3), 3527–3542). So in this case, is the assumption of non-absorbing aerosol discussed on page 6703 truly valid?

Can the authors provide additional details of the particle sizing / particle counting procedures and extraction of refractive index? A possible source of the discrepancy in refractive index (large value reported here) is an underestimation of particle concentrations. Is it possible aerosol was lost in transfer lines?

Interactive comment on *Atmos. Meas. Tech. Discuss.*, 6, 6685, 2013.

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