

Interactive comment on “Spectral Aerosol Extinction (SpEx): a new instrument for in situ ambient aerosol extinction measurements across the UV/visible wavelength range” by C. E. Jordan et al.

Anonymous Referee #3

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The manuscript “Spectral Aerosol Extinction (SpEx): a new instrument for in situ ambient aerosol extinction measurements across the UV/visible wavelength range” by Jordan et al., describes an instrument to measure the extinction coefficient of aerosols between 300 – 700nm. The authors measure NO₂ and different aerosol types (purely scattering, brown carbon and black carbon proxies) to demonstrate its functionality and give its limitations. The manuscript is well written and clear; however, I think it needs several changes before it is publishable in AMT.

The title should be modified. It is not clear why the instrument is new. The authors are not presenting a new technique and the differences between the instrument presented here and the one by Chartier and Greenslade 2012 don't seem substantial to label the SpEx as a new instrument. The SpEx presents improvements in some aspects to the AE-DOAS; e.g., it has lower detection limits, but it's on the expense of size (the SpEx is double in length). On the other hand, the wavelength range is narrower: 300-700nm instead of 225-700nm. Also the authors do not present ambient measurements; all of the aerosol measurements presented are from laboratory measurements of aerosol proxies.

The statement that SpEx can distinguish BrC from other aerosols is not well supported. The SpEx is measuring total extinction not absorption. In page 6489 lines 5 and 6, the authors state "Clearly, cinnamaldehyde exhibits behaviour typical of BrC, while 2-CB does not." It is incorrect; the cinnamaldehyde extinction curve can very well be of a purely scattering substance or of a substance with constant absorption throughout the measured spectrum.

The discussion about the extinction following a power law should be erased or modified (pages 6489 – 6490). For BrC it is the absorption which has been observed to follow an inverse power law relationship with wavelength, the extinction will not necessarily follow an inverse power relationship. Even with a theoretically perfect power law behaviour of the absorption and scattering, the extinction will not follow an inverse power law (see Figure below where first I forced the absorption and scattering coefficients to have an Ångström exponent of 6.3 and 2, respectively. The extinction coefficient plotted is the sum of the absorption and scattering. Finally, I fitted power laws to all traces and a polynomial to the extinction. It is clear from the residual that the polynomial fit is a better fit to the extinction.)

The authors should add a paragraph on how they handled multiple charge particles in the size selected measurements. With 80 lpm flow they probably had very high concentration solutions in their atomizer, this will cause size distributions with large

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mode diameters and when choosing 200nm many multiple charge particles will go through. In other words, the size selection measurements will not be monodisperse and they might not be comparing measurements of the same size.

Other comments:

Is the Xenon lamp temperature controlled? From Fig. S1 it seems critical to have a temperature control as any temperature change might cause a wavelength drift in the output spectrum and hence a wrong calculation of the extinction coefficient.

Did the authors check for stability of the lamp throughout a long time period? What's the intensity variation in say an 1h, 5h or 24h period?

Why was there a 1nm offset found in the absorption spectrum of NO₂? Did the authors do a calibration with a mercury lamp?

Do the authors have measurements of NO₂ at lower concentrations than 5ppm? Can the authors show that the SpEx measured different concentrations of NO₂ at more relevant atmospheric concentrations and that the measured concentration of NO₂ is similar to that measured to the NO₂ instrument?

For the 600 and 900 nm PSL measurements, the authors mention the best fit was found using slightly larger diameters. I'm guessing these measurements were done using a DMA, so if in the DMA the diameters were chosen to be 600 and 900 nm, why the difference? Regardless of the manufacturer specifications the DMA will transmit the chosen diameter.

The authors should compare there PSL results to values in the literature. Chartier and Greenslade 2012, Miles et al. 2010 and Washenfelter et al. 2013 have good summaries of the values available.

Page 6485 line 25: "in Fig. 5b good agreement is found..." Can the authors give percent difference to give a quantitative number to the 'good agreement'?

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In Fig. 5 why is there a difference between the CAPS and the nephelometer at 450nm? Page 6486 line 17-18: “SpEx data were averaged over 5nm...” why do the authors need to average over 5nm? what is the wavelength resolution?

What agreement do the authors get if they do the same analysis done in Fig.6 with only the purely scattering substances and the nephelometer?

Can the authors give single scattering albedo for the absorbing substances they measured at the nephelometer wavelengths? These values can be useful to other authors using them as proxies.

Page 6487 line 25: “for Luberon natural Red, there is surprisingly little difference between the PM1 and PM2.5 spectra.” Maybe it is due that there are not many particles above $1\mu\text{m}$? The authors can check the size distribution from the APS

References

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Miles, R. E. H., Rudic, S., Orr-Ewing, A. J., and Reid, J. P.: Influence of uncertainties in the diameter and refractive index of calibration polystyrene beads on the retrieval of aerosol optical properties using Cavity Ring Down Spectroscopy, *J. Phys. Chem. A*, 114, 7077–7084, 2010.

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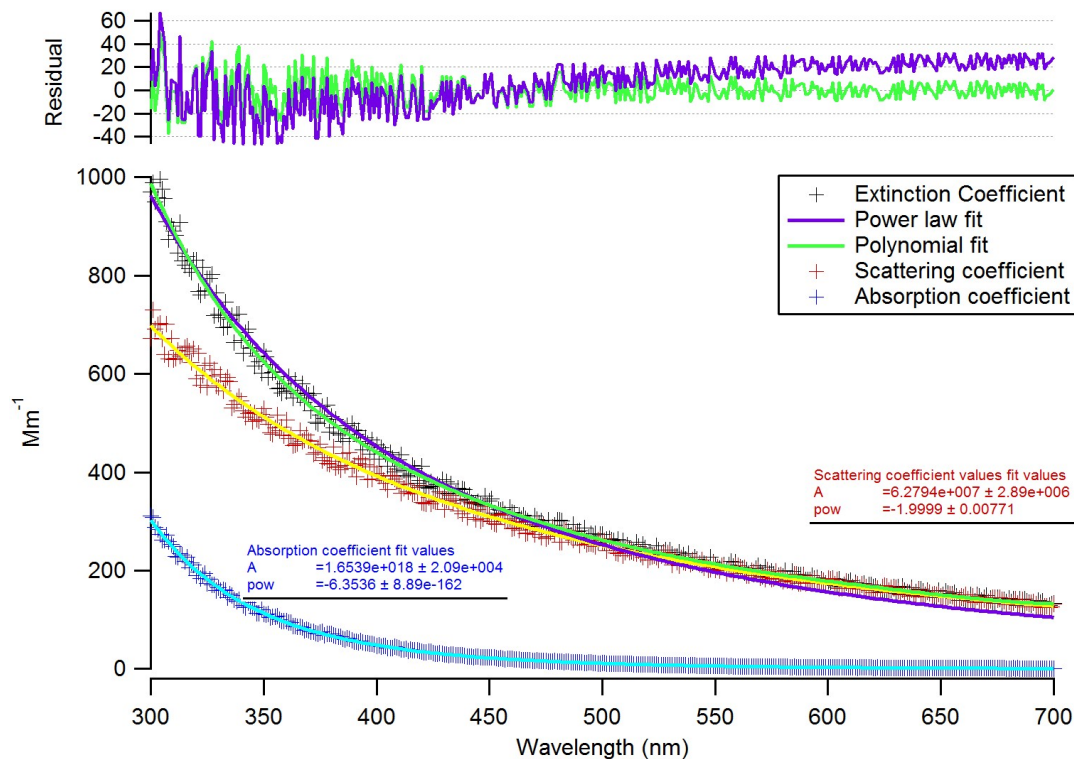


Fig. 1.

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