

***Interactive comment on “Intercomparison Study of the CAPS PM<sub>ex</sub> (Cavity Attenuated Phase Shift Particle Light Extinction Monitor) with the combination of an Integrating Nephelometer and a Particle Soot Absorption Photometer” by A. Petzold et al.***

**Anonymous Referee #1**

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Review of “Intercomparison Study of the CAPS PM<sub>ex</sub> (Cavity Attenuated Phase Shift Particle Light Extinction Monitor) with the combination of an Integrating Nephelometer and a Particle Soot Absorption Photometer” by A. Petzold et al.

General Comments: This paper reports on a short, focused study designed to compare light extinction measurements made using a new CAPS instrument with those made

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by one of the commonly-used scattering plus absorption methods. The experiments were simple and well conceived, the data analysis is robust, and the results are fairly defensible. I believe paper can be published in AMT after the authors' attention to the comments listed below. Specifically, there were a number of assumptions the authors made in this study that need to be documented more thoroughly, or in some cases tests should be performed to support their assumptions. These activities will strengthen the paper and should make this a valuable contribution that can then be referenced by people that use this instrument in the future.

Specific Comments:

### Abstract

I don't think it adds much to have the brand name of the light absorption instrument mentioned in the title of the paper. The specific model of nephelometer is not named here. These names can be in the abstract, since readers often peruse the abstracts, but my feeling is that the comparison methods (not the instruments) should be given in the title. The fact that the MAAP is also used in the comparisons reinforces the idea that the methods (i.e., filter-based light absorption measurement) should be named rather than 1, 2 or even 3 individual instruments in the title. I think it is OK to mention the CAPS by name since this study is focused on getting the CAPS introduced to the scientific community and CAPS results into the literature. A more appropriate title might be "Intercomparison Study of the CAPS PMex (Cavity Attenuated Phase Shift Particle Light Extinction Monitor) with the combination of an Integrating Nephelometer and a Filter-based Light Absorption Photometer".

Pg. 7588, Line 25: replace 'simultaneous' with 'simultaneously'.

Pg. 7589, Line 5: PSAP is not a standard method; rather it is a commonly used method. I recommend replacing the word 'standard' with 'popular' or 'commonly-used'.

Pg. 7590, Line 6: What is meant by '2s'? Is this meant to be 2-sigma?

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Pg. 7590, Line 16: Replace ‘Yu e al.’ with ‘Yu et al.’.

Pg. 7591, Line 4: How were MAAP filter changes handled at high absorption levels? These tend to interrupt the runs and disrupt the flow regime from the mixing chamber. Did you set up the instrument so as not to perform automatic spot advances? If so, how does this affect the MAAP measurement at very low transmittances?

Pg. 7591, Line 23: Replace ‘...where the instruments sampled from.’ with ‘...from which the instruments sampled.’

Pg. 7591, Lines 22-27: ‘When generating external aerosol mixtures, the pure aerosols were fed into a 3 l-mixing volume where the instruments sampled from. Particle-free make-up air was added downstream of the mixing volume to balance the input flow from the atomizers and the overall flow sampled by the instruments. The set-up used for the polydisperse laboratory aerosol studies is shown schematically in Fig. 2.’

Have you measured or performed any calculations to support your assumption that particles are not lost in the sample tubing between mixing chamber and the various instruments? This entire study is based on your assumption that each instrument is sampling the same aerosols. This is not always the case when instruments sample in parallel. Each line appears to have a different flow rate. The loss mechanisms will therefore be different in the tubing leading to each instrument, and size-dependent particle losses could affect the results. This can be measured using particle counters and/or SMPS instruments at the inlet of each instrument, or it can be calculated fairly easily. Unfortunately, instrument measurement artifacts are very difficult to discern from sampling artifacts, and if you don’t eliminate the possibility of sampling artifacts, you may get agreement between methods, but for the wrong reason(s).

Pg. 7591, Line 23: Is a 3 liter mixing chamber large enough given a total flow rate to all instruments in excess of 22 lpm? A larger mixing chamber damps out variability in the ambient or generated source aerosols.

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Pg. 7593, Lines 5-8: 'MAAP and CAPS PMex data were used without further corrections except the adjustment to temperature and pressure measured by the NEPH, i.e. all data refer to same pressure and temperature conditions.'

Unless the PSAP was a modified instrument (with internal T and p sensors), the authors are making the assumption that the pressure and temperature at the PSAP filter was quite similar to that measured inside the nephelometer. This is probably a reasonable assumption for the pressure, but the internal T of the nephelometer can be 4-5C above ambient because of the heating of the internal volume by the lamp.

Pg. 7593, Lines 17-19: 'For the sake of data robustness, PSAP data for  $\sigma_{ap}$  were scaled according to the  $\lambda^{-1}$  scaling law (Bohren and Huffman, 1983) while NEPH data for  $\sigma_{sp}$  were adjusted by applying the measured scattering Angstrom exponent.'

Why was this done? The authors are assuming they know that the theoretical small particle limit of  $\lambda^{-1}$  wavelength dependence of absorption applies in these tests. That is a big assumption, even for the laboratory aerosols, and appears to be a 'quick and dirty' approach. The ambient aerosols could have a substantially different wavelength dependence of absorption, especially if significant amounts of organic aerosols were present (likely in that suburban setting). A more robust way of scaling the PSAP data to 630 nm would be to use log interpolation of the 530 and 660 nm absorption coefficients. This is an easy and defensible way to scale the PSAP absorption to 630 nm. At the very least a comparison should be performed on the wavelength-adjusted PSAP data from the log-interpolation and  $\lambda^{-1}$  methods to convince yourselves (and readers) that they are not significantly different.

Pg. 7595, Lines 14-17: 'As a consequence, CAPS PMex data for the instrument evaluation using laboratory generated polydisperse test aerosols and for the intercomparison purposes based on ambient aerosol data were corrected for the new pathlength adjustment by multiplication with a factor of 1.05.'

Is this pathlength adjustment the same for all CAPS PMex instruments, or do you

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have to go through the process of ‘calibrating’ each one using PSL spheres for its own unique pathlength (I am asking this question because of the authors’ association with the commercial production and manufacture of this instrument)? It is also important for readers that reference this paper to know whether they will also have to apply a correction factor of 1.05 to their CAPS extinction data or whether the firmware can take care of that in future models. If this is not made absolutely clear then you will have the problem of some people either not making the correction (by assuming it has been made in the instrument) or making it twice (applying the 1.05 factor on top of an internal pathlength adjustment).

Pg. 7595, Lines 21-23: ‘The analysis of the CAPS PMex data and the combined NEPH-PSAP data was restricted to sequences of stable aerosol concentrations.’

The runs BC1, BC2 and MIX2 in Figure 6 all show decreasing aerosol extinction levels, presumably from decreasing aerosol concentrations. The word ‘stable’ in this sentence should be replaced with ‘relatively stable’. Also, the ambient aerosol measurements show some rather abrupt changes in aerosol amount.

Pg. 7596, Lines 22-23: ‘The small offset is statistically insignificant.’ What statistical test was used? At what confidence level?

Pg. 7597, Lines 21-23: ‘Night-time data for RH as recorded by the NEPH RH sensor were approx. 30 % while peak RH data were > 80 % at the end of Episode 1 before the thunderstorm passage and below 55 % during Episode 2.’ If the NEPH internal RH sensor was reading in excess of 80% at times inside the warm NEPH instrument, it is at least possible that there was condensation in some of the cooler sampling lines. High-RH air entering the PSAP would have likely caused the PSAP filter-based absorption data to be noisy, and possibly inaccurate, during these periods. If there was in fact condensation in the sampling lines, the amount of aerosols getting through these individual lines is unknown. Can the authors discount the possibility of condensation during the high-RH episodes?

Pg. 7598, Lines 1-2: 'An agreement of better than 95 % between these two methods can be rated as excellent.'

Rated as excellent? How, and by whom, is this rating given? This statement should be supported with facts or peer-reviewed references. What are the respective uncertainties associated with each of the measurements? Does the agreement observed in this study fall within the envelope of those uncertainties? If so, then you can say this.

Pg. 7598, Line 25: Check the plural of 'albedo' (albedos vs. albedoes).

Pg. 7599, Line 1: I would not call NEPH-PSAP the 'standard' method to determine ambient aerosol extinction. As before, I would say this is a widely used method, not the standard method.

Pg. 7599, Line 6: 'There are a number of possible explanations for this small level of disagreement.' What about organic aerosols? There are papers in the literature that suggest organic particles may influence the absorption measurement of the PSAP (and other filter-based instruments). See, for example, Lack et al. 2008 and Cappa et al., 2008, both in AS&T. This should at least be mentioned as a possibility for the disagreement on ambient aerosols, although this study (and some others) does not appear to support the Lack et al. and Cappa et al. results of a large positive bias in the PSAP measurement of ambient light absorption.

Fig. 3: Why is '660 nm' listed under the 'NEPH' instrument? The adjustment of the NEPH scattering data is to 630 nm, is it not?

Fig. 3: ' $\lambda^{-1}$ ' should be replaced by 'log-interpolation between 530 nm and 660 nm absorption coefficients', assuming the authors decide to go with this alternate method of wavelength adjustment.

Fig. 6: Are the runs stable enough so that the lag time in nephelometer measurements (i.e., mixing time at 11 lpm) is not a factor that would adversely affect the measurements?

Table 3: The CAPS/NEPH-PSAP ratio for AS aerosols appears to be related to aerosol amount. At the higher extinction levels the ratio is lower, while the lower extinction levels show the largest ratios. What can you say about this? Could this suggest an imperfect aerosol scattering correction for the PSAP, or perhaps a problem with the filter loading correction? If so, the excellent agreement you get between CAPS and NEPH-PSAP may mean that the CAPS agrees very well with a method that gives the wrong answer for extinction.

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Interactive comment on Atmos. Meas. Tech. Discuss., 5, 7587, 2012.

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