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Comment

## ***Interactive comment on “An algorithm for retrieving black carbon optical parameters from thermal-optical (OC/EC) instruments” by A. Andersson et al.***

### **Anonymous Referee #2**

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Reviewer’s comment for MS No#: amtd-4-1233-2011

General comment:

Although technology has advanced to very high level, the measurement of BC mass and related optical properties has been a matter of debate in the literature over a long period (see Muller et al, amt, 2011). Recent studies have utilized the laser transmittance signal of Sunset Lab’s EC-OC analyzer to derive absorption coefficient and mass absorption efficiency of EC in aerosols (e.g. Ram and Sarin, 2009). As the authors rightly said, the methodology used in Sunset Lab’s EC-OC analyzer has many advantages as it does not require two separate instruments to measure EC mass and

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Discussion Paper



absorption coefficient separately, and all the three parameters (EC mass, absorption coefficient and mass absorption efficiency of EC) can be measured/ derived on the same aliquot of the filter sample. In the present MS, authors report on the measurement of the two key aerosol optical parameters from Asian and European sites. Overall, the paper is well written and authors have extended the methodology of Ram and Sarin, (2009) to explain how the changes in the transmittance signal can affect the estimation of optical properties of aerosols. I recommend publication of the MS, provided the authors revise the MS carefully and address their approach in details and uncertainty in the measurement of optical properties of aerosols.

Major comment:

My major concern is on the use of terminology in the MS. There is a lack of consistency on the use of notation and definition of various terminologies (as also realized by the authors) and those used in the literature. As a reviewer, I find very difficult to grasp and have to look back many times on the terminology used by the authors. I strongly recommend and urge the authors' to stick with the standard definition used in literature (see suggested nomenclature given in Table) or suggest authors to add a table on the nomenclature of various terminologies. This will also provide a good and fluent reading of the paper. Furthermore, methodology section of the MS is weaker and authors should provide fine details of their methodology as they claim development of a new algorithm.

Authors are suggested to provide details of the uncertainty in their measurement of optical properties. Specific comments:

P1236, L21-22 and other places in the text: The term ATN is widely defined in literature as  $\ln(I_0/I)$  or  $100 * \ln(I_0/I)$  which is a unit less parameter. The term attenuation coefficient has a unit of  $m^{-1}$  (see equation 3 of the MS). I strongly recommend and urge the authors' to stick with the standard definition of Bond et al, 1999, Bond and Bergstrom 2006, Weingartner et al, 2003; Ram and Sarin, 2009 (all of them cited in the paper).

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Otherwise, authors are suggested to add a table on the nomenclature of various terminologies used in the MS I am providing the following table for the authors which can be useful in defining various parameters. Parameter Symbol Unit Optical-attenuation ATN unit less Attenuation coefficient  $b_{ATN}$   $Mm^{-1}$  Multiple scattering effect C unit less Shadowing effect R unit less Absorption coefficient  $b_{abs}$   $Mm^{-1}$  Mass absorption efficiency (MAE)  $\bar{\kappa}_{abs}$   $m^2g^{-1}$

P1238, L1: Again inconsistency with the use of notations. The term “ $I/I_0$ ” is usually defined as transmittance while “ $\ln(I_0/I)$ ” is defined as absorbance or attenuation (ATN).

P1238, L4-5: Delete “the extinction coefficient, here referred to as”. Please see an earlier comment.

P1238, 3-6: Please amend the uses of attenuation and attenuation coefficient in the text accordingly.

Equation 3; P1238, L16-24 and P 1239, L1-4: The measurement of ATN and other optical properties in Ram and Sarin (2009) paper is based on the measurement of intensity of transmitted light ( $I$ ) and intensity of incident light ( $I_0$ ). The absorbance or attenuation is calculated when the sample is just put in the oven and sample is not heated. Thus, all the derived optical properties are independent of what happens to laser transmittance when sample is heated in an inert and oxidizing atmosphere. It would be interesting to know how the transmittance and optical properties changes when sample is heated. This is an important finding of the paper and authors should utilise this fact, in details, to discuss the changes in optical properties of aerosols.

Table 1 and Fig. 2a: What is the uncertainty in the measurement of attenuation coefficient? Authors are suggested to provide details of the uncertainty in their measurement, though they have provided standard deviation in real-time measurement. The numbers given in table 1, except at MCOH, are statistically indistinguishable and may be same within the error of two measurement techniques. What could be reason (s) for differences in calculated optical properties at MCOH, especially when the two tech-

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niques agree well at other locations? Could it be explained on the basis of mixing state (internal) due to aging/chemically processing?

Authors are suggested to add ATN (calculated using transmission signal) vs EC concentration (in  $\mu\text{g cm}^{-2}$ ) plot of their own data (to validate the Beer-Lambert's law and use of the methodology).

Technical corrections: P1234, L10: pleas replace "in-organic" with "inorganic" P1235, L4: Please replace "opposed" with "compared" as Brown carbon also has a weak absorption like BC. P1235, L14: Punctuation is needed after "setup". P1235, L24, 26 and other places: Please replace "extinction" with "absorbing" throughout the text. Extinction means the sum of scattering and absorbing properties of aerosols. P1236, L1: Delete "PSAP" as authors have already defined it at P1235, L11-12. Table 1: Please change the units of EC and OC to  $\mu\text{g m}^{-3}$ . Also, the use of unit  $\mu\text{m}^{-1}$  gives wrong impression as it means  $(\mu\text{m})^{-1}$ , if written in this way. I would urge the authors to use  $\text{Mm}^{-1}$  instead.

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 1233, 2011.

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