**Atmospheric
Measurement
Techniques
Discussions**

# *Interactive comment on* "A new algorithm for brown and black carbon identification and organic carbon detection in fine atmospheric aerosols by a multi-wavelength Aethalometer" *by* F. Esposito et al.

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Esposito et al.: A new algorithm for brown and black carbon identification and organic carbon detection in fine atmospheric aerosols by a multi-wavelength Aethalometer, Atmos. Meas. Tech. Discuss., 5, 1003–1027, 2012 Review General comments The paper first presents an algorithm for estimating the absorption by organics in the UV wavelength channel 370nm of a 7-wavelength aethalometer. Then the method is applied to measurements conducted in southern Italy at a site not far from an oil pretreatment plant that emits high concentrations of light absorbing carbon aerosols. In

short: first the absorption by non-organics at $\lambda = 370$ nm is calculated so that an absorption Ångström exponent is calculated by fitting to data from the other wavelengths of the aethalometer and then using it absorption is extrapolated to $\lambda = 370$ nm. The difference between the actual absorption at $\lambda = 370$ nm and the so extrapolated absorption is then interpreted as absorption by organics, which makes sense, nothing wrong with that. But essentially the only difference between this method and that presented in the aethalometer manual is that in the latter it is simply assumed that the absorption Ångström exponent = 1 in the wavelengths other than 370. This naturally makes a clear difference in the estimated absorption by organics. A general comment is that the data from that site is in principle interesting, especially the change of the Ångström exponent when the refinery was out of order and when it was in use. Mainly due to this observation I am not going to reject this paper but I have to say that the method itself is not a very strong advancement compared with the method presented in the aethalometer manual, even though the authors use (unnecessarily) long text to describe it. And since the method improvement is not that significant, the paper might suit better to ACP or Atmospheric Environment. Anyway, I won't reject this.

> First of all we would like to thank Anonymous referee for his useful suggestions to be used in the revised version of this paper. > The innovation of the technique presented in this paper is related to the obtained results rather than to algorithm in itself. In fact, when alpha $\approx$ 1.3, the equivalent UVPM shown in figure 7 and estimated by MAGEE procedure gives values as higher as twice those presented in the paper. In cases of alpha values higher than 1.3 the over estimation of UVPM will increase according the exponential law. —-

On literature use I have some wishes. The authors cite and use some equations presented by Fialho et al. (2005) but they should mention that in Fialho's paper a method almost similar to that presented in the present paper was used for estimating the absorption by desert dust, so did Müller et al. (2009). When they write on p. 1014 that "The capacity of our algorithm to identify the aerosol type ... is remarkable since this re-

sult could not be obtained with a standard analysis of Aethalometer measurement, that gives only the concentration..." they mean that since they have calculated the Ångström exponent of absorption this is something remarkable. I don't want to discourage the authors but this is not quite true. There are several papers on the Ångström exponent of absorption, see for instance Bergstrom et al. (2007) and their table 2, and a lot of the papers I added to the references below.

> Once again, the authors are conscious that the estimation of Ångström exponent of absorption in itself is not something of remarkable. What will be remarkable is the possibility to avoid systematic errors in the equivalent UVPM estimation when alpha is greater than 1. —-

One thing the authors should mention is that condensable organic gases may change the optical properties of the filter material itself, affecting the results as shown by Lack et al (2008), even though they were discussing a PSAP but the same definitely applies to an aethalometer. A general but small point still is that in most literature in aerosol optics the symbol ïA̧ť (tau) is used for aerosol optical depth. For absorption most people use either bap or $\sigma$ap. The field is already so mixed up with symbols that I strongly suggest you use symbols that have been adapted by others also. Review of Esposito et al.: Atmos. Meas. Tech. Discuss., 5, 1003–1027, 2012

> The authors will take into account these last suggestions. —-

P1005L26-28 "...in BrC the imaginary part of the refractive index depends on the wavelength, causing the Angstrom coefficient $\alpha$ to become greater than 1 " Well, simply the fact that the imaginary refractive index is dependent on wavelength does not make $\alpha > 1$. It could also result in $\alpha < 1$. It depends on what the wavelength dependency of the refractive index is. Rephrase.

> We will rephrase the sentence according to this clarification. —-

P1007L2-4 "...one deriving from the aerosol particle absorption, as described by the

Mie theory, the other deriving from the specific spectral behavior of organic compounds present in the atmospheric particles..." This is not quite right. Also the absorption by OC is ".deriving from the aerosol particle absorption", if you are talking about OC in particles but not some condensable gases, which I don't think you mean here. You use this discrimination in other parts of the paper, too, so correct that everywhere.

> The purpose of this sentence was to highlight the effect of organic compounds on the aerosol optical properties. Also this and other sentences will be rewritten according to this suggestion —-

P1008L16-17 You have set the aethalometer to change the filter spot once an hour. How long time did it take for the internal calibrations etc. Typically it takes even 20 minutes so your hourly data contains 40 min of data. Is this true?

>Our data have been averaged over 45 minutes, because the internal calibration time is 15 minutes. —-

You only present the hourly averages, but how much did ATN grow during this time? > Tipically, the ATN at the end of the hour goes from 4 to 16 at 470 nm which are satisfying values for our purposes because the filter automatically advances when ATN470 =100. —-

The concentrations are unrealistically high when I look at some of your data figures 2, 5, 6: there are some turbidity coefficients of 0.01 m-1 which is 10000 Mm-1. Using your formula (5) with $\lambda 0 = 1$ $\mu$m and assuming $\alpha = 1$, I can estimate that at 880 nm absorption = 10000 Mm-1 * (880 nm /1000 nm) = 8800 Mm-1. Typically even in polluted air absorption coefficients are in the order of some tens of Mm-1. so either you have made some mistake or your site really is extremely polluted, straight at the exhaust pipe. Check that. And if you have not made any mistake, during one hour ATN may have gone very high. Make some calculated estimate of how much the shadowing may have affected your results.

> Regarding this point Referee 1 is right because we plotted ATN instead of ïĄťaer. We will re-plot figures 1, 2, 5, 6 using turbitdity coefficients. —-

Section 2.1 is unnecessarily long. Lines 1 – 19 are essentially the same as in Fialho et al (2005), and also in some other papers so you could simply give the formula abs = (A/Q)*(dATN/dt), in your selected symbols.

> This part has been inserted in the paragraph only for sake of completeness. We would eliminate it if referees suggests to move in this sense. —-

P1011L7-8 "...is the difference between measured and computed aerosol absorption coefficient. .." What is the computed absorption coefficient? Also the "measured absorption" has been calculated somehow, hasn't it?

> Measured absorption is intended as the absorption obtained by formula (2) starting from measured ATN. Computed absorption is intended to be the result of the best-fit procedure applied to the spectral range 470 nm – 980 nm. The residual is the difference between these values. —-

P1012 _"ïĄťuv =_ïĄť($\lambda$=0.37) =ïĄťaer +ïĄťoc , where _OC is the contribution to the absorption in the UV channel due only to the organic compounds ..." Again, also the OC is in aerosols. Unless you mean some gases condensing in the filter which would mix up the whole analysis. Anyway, the main difference between your eq (7) and that presented in the aethalometer manual is that you use here $\alpha$ that has been fit from the data whereas in the aethalometer manual they use $\alpha = 1$. You should mention this.

> As said before referring to organic compounds absorption was a way to highlight the effect of these compounds on aerosol absorption. Moreover, since one of the purposes was to improve the OC detection, we have considered the best fit procedure to verify the hypothesis ïĄą=1 and correct the systematic error introduced by the software when a $\neq$ 1. We will clarify this passage —-

Figure 1. Why do you have "arbitrary units" in the y axis even though it is obviously

1/m?

> see answer to comment P1008L16-17. —-

Figure 7. The y axis unit is gr/m3 and the graph shows that the concentrations rise to about 800 gr/cm3. Air weighs about 1.29 kg/m3. So your OC concentrations are truly high. Check the units.

> The units have to be corrected to ng/m3. —-

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