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 pre-treatment plant that emits high concentrations of light absorbing carbon aerosols. In short: first the absorption by non-organics at λ = 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 λ = 370 nm. The difference between the actual absorption at λ = 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.

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 result 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 Ångstrom 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.

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 τ (tau) is used for aerosol optical depth. For absorption most people use either b_{ap} or σ_{ap}. The field is already so mixed up with symbols that I strongly suggest you use symbols that have been adapted by others also.

Detailed comments

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

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.

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? You only present the hourly averages, but how much did ATN grow during this time? 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 λ_0 = 1 μm and assuming α = 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.

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.

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?

P1012 "$\tau_{uv} = \tau(\lambda=0.37) = \tau_{aer} + \tau_{oc}$, *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 α that has been fit from the data whereas in the aethalometer manual they use α = 1. You should mention this.

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

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/m^3. So your OC concentrations are truly high. Check the units.

References to be studied properly and added to the paper

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