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Atmospheric Measurement Techniques Discussions

Interactive comment on "Correcting aethalometer black carbon data for measurement artifacts by using inter-comparison methodology based on two different light attenuation increasing rates" *by* Y.-H. Cheng and L.-S. Yang

Anonymous Referee #3

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Correcting Aethalometer black carbon data for measurement artifacts by using inter-comparison methodology based on two different light attenuation increasing rates

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By Yu-Hsiang Cheng and Li-Sing Yang

The authors present an interesting measurement setup using two Aethalometers in parallel, each with a different flow (face velocity), causing a different deposition rate and

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Discussion Paper

consequently different attenuation of light through the filter tape loaded with particles. The concept has been used in the newly developed Aethalometer AE33 (Drinovec et al., 2014). The approach presented in the reviewed manuscript uses a simple linear model to determine the factor k, which accounts for the loading effects in the filter tape.

The authors assert that the presented method results in accurate determination of BC and that the results can be made independent of the aerosol scattering. They propose their method as the empirical method for the determination of BC in situations similar to the one they describe. The authors need to address several issues:

1. Accuracy

The claim that the determination of BC is accurate is not correct. The authors show that the BC's determined from the Aethalometer using 2 l/min and the one using 6 l/min are the same (Fig. 7). This is not true, what they show is that the two Aethalometers give precise results. The criterion for accuracy is missing. Given the fact that the cite Park et al. (2010) and Drinovec et al. (2014), they could have used the algorithms described there to determine the accuracy. This should be added to the manuscript. It would be highly advantageous for their argument if they showed the slopes in the regression plots and mention them in the text. The uncertainty (or confidence interval) of the fit parameters should be shown as well.

2. Definition of attenuation

Attenuation (ATN), as defined in Eq. 1, is a measure of how much light is transmitted though the filter. The subsequent definition of the "corrected" or "true" ATN is, simply put, wrong! There is no "wrong" or "right" ATN, and no "corrected" one either – ATN is defined in Eq. 1. The under-reporting of BC due to loading effect in the filter is not due to the measurement of ATN, but due to the non-linearities (higher order effects), which are not described by Eq. 4. The loading of the tape definitely influences the ATN measurement, it causes the ATN to increase with more and more (absorbing) particles being loaded on the filter. The relation is on-linear and additional terms need to be

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taken into account to convert the measurement (!) of ATN into absorption or mass concentration. Eq. 5 is therefore wrong, and needs to be rewritten so that it shows the relationship between the "raw" BC measurement and the "corrected" BC – taking a form of Eq. 3, only with BC from Eq. 4 (see also comment P2856, L7). The authors need to take this in to account in the revision of the manuscript.

3. Immunity to scattering

The immunity to scattering has not been demonstrated in the manuscript – it is an interesting working hypothesis which needs to be tested. The authors need to test their method in environments with different concentration ranges and different values of single-scattering albedo. The scattering coefficient needs to be measured alongside the Aethalometer measurements. The sensitivity of the determination of the parameter k needs to be carried out. At low ATN on a unloaded piece of filter tape, Eq. 9 will feature a difference of two small numbers being divided by a difference of two small numbers. Noise in the measurement will significantly contribute to the value of the parameter k. In urban environments this might not be as critical (although this needs to be proven!) as in clean ones, but the stability of the determination of the parameter k needs to be shown.

The manuscript is often unclear and very sparse with information. For example, the following information needs to be added:

- Time resolution of the determination of the parameter k.

- Units need to be shown in all graphs.

- The assertion of wood burning contribution to BC needs some substantiation.

- Consistent notation should be used in the figures and the text of the manuscript.

- The underlying concept of Eq. 4, that is that all BC, accumulated on the filter (that is integrated from t=0), contributes to the loading, and that the parameter k is determined from this integrated BC, is fundamental to the manuscript. This needs to be expanded

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and emphasized. This is the central point of the manuscript (Drinovec et al. (2014) make the same point, only with higher time resolution)!

Other comments, by page/line numbers:

Page 2852, Line 2: "Recently" – for example, the Weingartner et al. (2003) paper was published 12 years ago, this is not recent.

P2853, L23: ". . .the decrease in light transmission through the aerosols on the filter is measured." – No, the measured quantity is the intensity of light transmitted through the filter loaded with particles.

P2854, L7: ". . .the measured BC concentration decreases with an increase in the filter load. . ." – Virkkula et al (2007) have shown an increase (negative parameter k). A reference is missing for both claims in this sentence.

P2854, L9: "Recently" – see above.

P2856-2858, Section 2.1: The discussion and definition of "true ATN" needs major revision, see comments above and below.

P2856, L7: "optical mass cross section" – this is mass attenuation cross-section. In fact, this is the parameter, showing "loading effect", as ATN (defined with Eq. 1) change is converted into mass.

P2856, L19: "Therefore, determining the true ATN value was the main objective of this study." – No! "True ATN" does not exist, loading effects are a consequence of non-linearities between the mass loaded on the filter and the ATN – please see above.

P2858, L24 – P2859, L1, discussion about drying and data processing: Using a dryer to reduce RH is a good and recommended practice. Since the RH did not change in winter compared to summer, not using the dryer in winter could have led to large variations of RH in the sampling lines due to operation of the air conditioner in the shelter. This might need more explanation. More importantly, negative values in the

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data could be noise, induced by cycling of the RH (as induced by the air conditioner). The noise in this case is a combination of a negative spike, upon the occurrence of the perturbation, and a positive one when the conditions return to the initial ones. A positive/negative pair is also possible. Removing negative values is therefore wrong – the data need to be averaged with a window, long enough to take into account the air conditioner cycling!

P2859, L12-20: Fitting method needs to be specified.

P2859, L24: Why does ATN in Fig. 2 extend to very large values? ATN=400 in the channel used to calculated BC (880 nm) represents a completely black filter. ATN at which the tape advance is triggered automatically is usually at ATN=120 in the detection channel at 370 nm.

P2860, L6: "...value could fall to 3 : 1." – This is unclear and needs to be explained in more detail.

P2860, 12-17: The Drinovec et al. (2014) compensation parameter k is not calculated according to the Eq. 10 presented here! This comparison needs to be completely revised. However, the comparison show identical results. Comparison with the Weingartner et al. (2003) algorithm shows identical results as well. Therefore the necessity of the second Aethalometer is questionable, if a post-processing scheme, such as Weingartner et al. (2003), is shown in this manuscript to be applicable and produces identical results.

P2861, Section 3.2: This section needs to significantly revised using scattering measurements or changed completely. Most of the text is speculation nor, at best, a working hypothesis – see above for more.

P2862, L24: "...to 1.17+/-0.44 (estimated from corrected ATN)..." – From which Aethalometer 2 l/min or 6 l/min?

P2863, L3: Same as just above.

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P2863, L14: The discussion of Fig. 5 needs to include more quantitative information, for example the value of k at large ATN/Q. Is Fig. 5 essentially Fig. 4 with a log scale? Why is the ATN/Q not extended to larger values?

P2863, L14: prediction of the parameter k. this is interesting, but an empirical relationship might hold for this particular site and Aethalometer only. The authors suggest that these relationships can be applied more widely – this needs to be shown, by using them somewhere else.

P2864, L6: The discussion of Fig. 7 needs to include the slopes. Figure showing the relationship for the measurement channel 470 nm needs to be added – it was used to determine the Angstrom exponent!

P2864, L11: the fact that the two measurements agree is not necessarily the proof that the derived BC value is accurate – the criteria for accuracy are missing and should be included here. Please see above for more.

P2864, L22: How were the parameters f for the Weingartner et al. (2003) algorithm determined? This is crucial for the discussion. Also, the results are identical, although the slope in Fig. 8 is missing. Please include more quantitative information (slopes. . .) in the discussion and in Fig. 8.

P2865, L1: Same as just above.

P2865, L2 and onwards: Please add the discussion on the potential differences of post-processing the data with Weingartner et al. (2003) algorithm and the principle presented in the manuscript using two Aethalometers. Advantages and disadvantages should be presented, including the obvious ones (need for more instruments).

P2865, Conclusion: Conclusion needs to be revised according to the comments above. The message about scattering and wide applicability needs to be either toned down or experimentally proven. Please see above for more information.

All Figures: Please use larger font.

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Figure 2: Why do the ATN reach to above 400? This is never used with Aethalometers!

Figure 4: Units are missing.

Figure 5: Is this fig 4 with log scales? If so, why does the x-axis stop at ATN/Q=20 or 40? Make the x-axis identical for (a) and (b), preferably same as Fig. 4. See also comments to the manuscript text!

Figure 6: Why is the y-axis stretched between 0.5 and 1? The values of (1-k.ATN) all lie between 0.8 and 1!

Figure 7: The slopes need to be included on the figure. The axes maximums need to be reduced (potentially "breaking" them to include higher values) to show more detail at low BC concentrations. What about other wavelengths, especially 470 nm, since it was used to determine the Angstrom exponent?

Figure 8: The axes maximums need to be reduced to show more detail at low BC concentrations.

While the drawbacks of the manuscript are by no means negligible and presentation should be improved significantly, the idea to combine two Aethalometers is interesting. I believe that the manuscript results could be better presented with appropriate major revisions as described above. These would make it suitable for publication in AMTD, most probably as a Technical Note.

References

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