

***Interactive comment on* “Estimates of mass absorption cross sections of black carbon for filter-based absorption photometers in the Arctic” by Sho Ohata et al.**

Anonymous Referee #2

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Review of the manuscript entitled “Estimates of mass absorption cross sections of black carbon for filterbased absorption photometers in the Arctic” submitted to Atmos. Chem. Phys. by Ohata et al.

This manuscript addresses observations of black carbon aerosol in the Arctic. Knowing spatio-temporal variation of black carbon (BC) concentration and resulting light absorption is of general atmospheric and climatic interest as motivated by the authors. They present multi-annual and multi-site data sets of parallel measurements with two or more methods. This is a great effort and of large value as starting point for many studies. The methods and data treatment are sound and the manuscript is well written.

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The only major question I raise concerns overlap with journal scope: “The journal scope is focused on studies with general implications for atmospheric science rather than investigations that are primarily of local or technical interest. . . .” This manuscript here certainly has broader and longer coverage than “just local interest”, whereas it has a strong technical focus (as appropriately reflected in the title). On the one hand, it does provide ample and important information towards consistent interpretation of observations made with different instrument types commonly deployed to quantify black carbon mass concentrations, and also provides approximate values for the conversion factor between light absorption coefficient and black carbon mass concentration, i.e. the MAC value, as far as achievable with filter-based methods. On the other hand, it only provides limited additional results and interpretation around these quantities. Therefore, a journal such as Atmos. Meas. Tech. would certainly be a valid if not more suitable choice. Journal choice is to be answered by the handling editor. As for myself, I can only stress that this manuscript warrants publication after addressing a couple of minor comment listed below.

Minor and technical comments: I recommend to emphasize the peculiar set-up and purpose of the COSMOS even more clearly, as readers may overlook the fundamental difference between COSMOS and e.g. aethalometer. The heater included in the COSMOS setup aims at denuding particulate matter internally mixed with BC from these BC cores. The very purpose of modifying the aerosol in this manner is to stabilize the mass attenuation coefficient of the BC deposited on the filter tape at a well-defined and constant value independent of original BC particle mixing state. This makes the instrument response proportional to BC mass, thereby preparing the ground for a BC mass measurement with good accuracy. However, a consequence is that the COSMOS does not provide information on the light absorption coefficient of the untreated aerosol. By contrast, the aethalometer provides a priori provides a signal that is proportional to the absorption coefficient of the aerosol (neglecting dependence of the C-value on BC microphysical properties), whereas proportionality to BC mass concentration typically is smaller due to variations in MAC of BC (this point is made in the introduction as

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very motivation behind this manuscript). The consequence of stabilized versus variable MAC for filter-based measurements of equivalent black carbon mass could also be put in context of terminology recommendations by Petzold et al., 2013. I.e., it seems justified to omit the “equivalent” in COSMOS derived BC mass, however, the reason for it should be laid down.

Strictly speaking, another consequence of the previous comment is that the detour via absorption coefficient when inferring M_{BC} from b_0 measured by the COSMOS instrument is obsolete (Eqs. 1-3). Instead, one can directly obtain M_{BC} as b_0 divided by the mass-specific attenuation coefficient in the filter matrix. This is another way of conveying the message provided on lines 148 through 153. The absorption coefficient of the modified aerosol is not of interest, hence no need to go through absorption coefficient.

L234: The MAC of 6.6 m²/g for the MAAP is the default factory setting, which doesn't necessarily equal a recommendation. It could be said that this value applies to fresh uncoated BC, in which case the default output of the MAAP for equivalent BC mass concentration agrees well with actual BC mass concentration. It is also known to be too low for internally mixed BC thereby leading to eBC mass being larger than actual BC mass in such cases.

L263: No direct corrections were applied to compensate for non-linear dependence of attenuation on filter loading. Please specify whether the correction was not applied as not required, which can well be the case for aged aerosol. Plotting the absorption coefficient as a function of attenuation directly reveals whether correction is necessary. In principle, the correction could be implicitly included in the MAC value for deriving BC mass concentration. However, this would hinder interpretation and comparability of MAC values and it would make them dependent on setting chosen for filter tape advances.

Eq. 7 and text around it: I almost got confused by the two symbols B_{abs} and b_{abs} . I

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wouldn't necessarily say that they are different (line 272), which sounds like two different physical quantities, but rather just make the point that filter-based measurements of b_{abs} always remain tainted with some uncertainty for the reasons laid down on line 273ff. (Equation 7 isn't really needed then, is it?).

Fig. 2: I suggest to emphasize that the Mie curves shown in this figure only apply for bare BC particles (or FeOx) particles. Accordingly, I suggest to be more specific regarding the notation of the diameter, i.e. D_{BC} and D_{FeOx} instead of D_m (the latter could be misinterpreted as total particle diameter in case of internally mixed particles).

Fig. 4c: There seems to be a systematic high bias of the data points relative to the fit line in the range of lower concentrations (however, it is hard to just from a graph which heavily overlapping data points; please consider alternative visualizations). The slope of a line fitted through the origin is always much more sensitive to data points at high concentrations than to those at low concentrations. Therefore, I suggest as an alternative to calculate the ratio between the two measurements for every data point and then run stats on these ratios (and show them as histogram). Comparing the resulting mean ratio with the fitted slope gives an idea on the robustness of the analysis (note: some pre-averaging before taking the ratio may be required in case of poor signal-to-noise ratio). And the width of the histogram of ratios carries information that is easier to interpret than the correlation coefficient, which is a blend of variations in ratio and variations in absolute concentration. The same should be done for the analysis of other "mean ratios".

Figures 5a, 9a etc: please find better ways to visualize these data. As is, the black traces simply obscure the red traces.

Table 1 caption: Looks like a typo for the MAAP wavelength.

Table 8: This table shows MAC values across the wavelength spectrum covered by the aethalometer. Such values must only be presented with very clear caveats on how

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to use and interpret them. The light absorption coefficient at near UV wavelength is, simply speaking, the sum contributions from black carbon and brown carbon. Therefore, the MAC reported here at e.g. 370 nm is not a property of the BC particles, instead it simply is a conversion factor that provides correct BC mass concentration “on average”. By contrast, the MAC reported at red and near IR wavelength, where BC dominates absorption, is both a property of the BC particles and a conversion factor to provide correct BC mass concentration. Or in other words, the apparent MAC at near IR should not change when a biomass burning plume passes by, whereas the apparent MAC (absorption divided by BC mass) at near UV wavelengths should increase. Given this, it is actually surprising that the correlation coefficients are found to be comparable in these two wavelength ranges. Does this indicate that brown carbon plays a minor role or that it makes a rather constant relative contribution? I also suggest that longer wavelength are preferentially to be used to infer eBC mass concentrations whenever possible, whereas the measurements at shorter wavelength should only be used to infer the spectral dependence of the absorption coefficient, which gives hints on presence of brown carbon.

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