Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-166-RC1, 2021

Comment on amt-2021-166

Anonymous Referee #1

Referee comment on "Estimates of mass absorption cross sections of black carbon for filter-based absorption photometers in the Arctic" by Sho Ohata et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-166-RC1, 2021

Review of the manuscript entitled «Estimates of mass absorption cross sections of black carbon for filterbased absorption photometers in the Arctic» submitted to Atmos. Meas. Techn. 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 and it provides 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. Therefore matching the scope of this journal.

This manuscript was initially submitted to the journal “Atmospheric Chemistry and Physics” (https://acp.copernicus.org/preprints/acp-2020-1190/) where it underwent a first round of reviews, which resulted in transfer to the current journal (Atmos. Meas. Techn.). I already provided a review at that stage (https://acp.copernicus.org/preprints/acp-2020-1190/acp-2020-1190-RC2.pdf). The authors already implemented these comments in an appropriate manner. Therefore, I only have few additional minor and technical comments listed below. Other than that, I can only stress that this valuable manuscript warrants publication.

Minor and technical comments:

- Equation 1: The value(s) applied for f_{fil} should be reported. And/or the ratio “f_{fil}/MAC(COSMOS,SP2)”, which is the final and only factor applied to measured b_{0} for inferring M_{BC}.
- Caption of Figure 2: I suggest to add something along the line: “D_{m} is the mass equivalent diameter of bare BC or FeOx”.
- Section 2.2.3.3: It seems that actual wavelength of the MAAP instrument at Fukue was

measured and found to be 639 nm, which is a very slight difference from 637 nm reported in the literature. It might be worthwhile to mention that 639 nm is an actual measured value.

- Figure 3c: choosing logarithmic instead of linear axis scaling might provide better visualization of performance in the lower concentration range, where the majority of data points appear. In the current variant it is not perfectly clear whether the value of the regression slope, which is driven by the high concentration data points due to fixing the axis intercept at the origin, only matches data well at high concentrations or at low concentrations too.
- Choosing logarithmic instead of linear axis scaling might be preferable for several figures for the reasons laid down in the previous graph.
- Section 3.1.1: please put emphasis on the fact that quoted diameter values are BC mass equivalent diameter.
- Line 358: "These results show that on average, the agreement between MBC (COSMOS) and MBC (SP2) at Alert was within 10 %." – What exactly means "on average within 10%"?
- Figure 4: It is very good that the instrument comparison is presented in different ways. Furthermore, distinction of performance in different concentration ranges as done in panels 4e and 4f is valuable. Having said so, the threshold is chosen at a very low level (2 ng/m3), which separates the data set somewhere in the single digit percentile range. I'd rather suggest to split somewhere between 15th and 25th percentile. In any case, the histogram of the low concentration data only should also be added (besides all data and high concentration data, which are already shown). This comment also applies to several other figures).
- Line 393: "The babs, and therefore the MAC, for the PSAP and the two CLAP instruments (CLAP1, CLAP2) agree to within 8%". – Based on the values reported in Table 2, the difference appears to be somewhat larger?
- Caption of Table 3: please explain the variable "V" in the caption (i.e. inter-quartile range in relative terms). This comment may apply to other table captions too.
- Line 462: check consistency of quoted V-value with Table 3.
- Line 475: PSAP derived absorption coefficients are around a factor of 1.5 larger than aethalometer derived absorption coefficients at Ny Alesund. Zanatta et al. (2016) reported a systematic difference the commercial and ITM variants of the PSAP, which is approximately in this range (Sect. 2.3.2 and Table 3 in their manuscript). Which variant of the PSAP is operated at the Ny Alesund site?
- Line 586: repetition of "at Alert".
- The authors discussion that BC mass from MAAP default instrument output systematically differs from their direct measurements due to a difference in factory default and actual MAC value. A brief discussion for BC mass from aethalometer default instrument output compared to their direct measurements should also be included, e.g. in Sect. 4.