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Atmospheric Chemistry and Physics
Discussions

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 #1

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OVERVIEW

The work of Ohata and colleagues presents MAC values of Arctic BC aerosol calculated from absorption coefficient and BC mass of various filter based photometer. Considering the extensive use of MAC values in modelling and observational studies, this topic is of great scientific interest. The manuscript has a dominant technical imprint, Thus, I wonder if the manuscript wouldn't be more appropriate for AMT. Alternatively, in order to publish on ACP, the atmospheric processes controlling the MAC variability should be asses in more details.

MAJOR COMMENTS

The paper has a very strong technical declination. In the introduction very few words are dedicated to explain why MAC is important in climatic studies and what are the current subsequent uncertainties in RF estimations. The subsections 3.2-3.4 merely report slope and correlation coefficient of babs and Mbc. In Section 3.5, the calculated MAC values are compared to previous studies but no reasoning on the year-to-year variability, seasonality and spatial variability is presented. Considering the remarkable amount of data, this work could provide more climatic relevant information. From this specific thought I encourage the authors to develop a climatic relevant discussion before resubmission to ACP or submit the manuscript to AMT.

The whole MAC calculation is based on the fact that COSMOS provides mBC similar to the SP2. It is thus important to prove that this assumption is valid for Arctic conditions. Although Mbc(COSMOS) and Mbc(SP2) nicely agreed (slope 1.02) in Alert, the COSMOS-SP2 Mbc ratio in Fukue varies from 0.92 to 1.14. The last value being very close to previous SP2-COSMOS Arctic comparison (Zanatta et al., 2018), but being 10% higher than in Alert. What is the overall uncertainty, in Arctic conditions, of assuming that mBC (COSMOS) is equal to mBC (SP2)? A second critical assumption is that the SP2 provides accurate BC measurements. The SP2 might report considerably different Mbc values compared to the thermal-optical technique, as shown in Pileci et al. (2020). As a matter of fact, mEC values were 1.3-1.7 times larger than mBC (SP2) in Alert (Sharma et al., 2017). Moreover, Mbc(COSMOS) observed at Barrow might overestimate by 5 ng/m3 Mec in summer (Sinha et al., 2017). This values might be negligible in continental locations, but very important for pristine summer Arctic condition (mEC<20 ng/m3). Considering that this is the first time a filter-based absorption photometer is used as BC mass concentration reference to calculate MAC values on relatively long time series and at multiple Arctic sites, the above mentioned points should be addressed. This, in turns, will increase the technical nature of the manuscript and makes it more suitable for AMT.

SPECIFIC COMMENTS (L: line of text; F: figure; S: section)

S2: A description of the station and measuring period must be presented here rather than in the intro.

S2.2.2: I strongly believe that this section does not fit very well in the storyline of the manuscript. Mostly because the actual impact of iron oxides is not quantified for absorption-photometer other than the COSMOS. As summarized in the conclusion, the effect of FeOx on COSMOS babs is even negligible. Thus, I strongly suggest removing this section.

S3.2.1 Similar to previous section; in the context of Arctic observations I do not think that Fukue values need a self-standing subsection.

S3.2-3.3-3.4 These sections read like a list of numbers as such could be completely replaced by a table. I suggest the authors to rearrange these subsections not as function of instrument but rather as function of location. In this way instrument-error analysis, comparison with previous observations and climatologic aspects can be nicely organized for each station. Currently, Section 3.5 is a bit self-standing and is the only non-fully-technical section of the paper.

L75-78: If I understand correctly, COSMOS is a filter based instrument. Reading Section 2.2.1, it appears that MBC is calculated from babs with a MAC. As every other filer-based absorption photometer. The description given here is a bit misleading.

L83-101: the description of stations does not belong here, but rather to the method.

L145: What MAC (COSMOS) is used in the present work, Sinha et al., 2017 or Irwin et al., 2015?

L315-316: How much mass is underestimated by the EC-SP2 ? Isn't it a larger uncer-

tainty source compared to calibration and density issues discussed afterwards?

L318-332 : How do the pre- and post- campaign calibrations agree? Considering the Aquadag scaling factor to be applied. Anyhow, this are just details considering that SP2-COSMOS comparison is still very good.

L357: please use one single name: Pallas or Pallastunturi. Or define this in the methodology and adjust the following text and sections title.

L363: the slope calculated for the 1h or 24h average observations?

L365: is 12.1 m2/g the average between MAC calculated at Pallas and at Fukue? Why should this be relevant?

L368-369: I would be careful in suggesting dust as potential source of interference, since there is no evidence in this case.

L370: . . . low by a factor of two IN ARCTIC CONDITIONS.

L429-437: write down somewhere the resulting Aethalometer MAC.

L500-505: FeOx is irrelevant in the context of this study. Such detailed description in the conclusion is not needed.

F2: This figure, despite being really interesting, does not fit in the overall discussion of MAC variability in the Arctic, like all Section 2.2.2.

F3,4,5,8,9,10,11: the paper has many figures, the time series shown n these figures are not strictly needed, since all the information of interest are already contained in the scatterplots. I suggest the authors to remove the time series panels or at least move them in the supplementary.

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