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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characterizing the Arctic absorbing aerosol with multi-instrument observations" *by* Eija Asmi et al.

Anonymous Referee #1

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REVIEW OF CHARACTERIZING THE ARCTIC ABSORBING AEROSOL WITH MULTI-INSTRUMENT OBSERVATIONS

OVERVIEW The work of Asmi et al. presents optical properties of Arctic aerosol measured with a wide array of instruments. Considering the global intensive use of the considered instruments, understanding and quantifying the issues of each instrument in order to optimize its performances is an essential task. However, the present manuscript lays between a technical assessment of the performances of filter-based absorption photometers and a survey of arctic aerosol optical properties. Thus, the objectives of the manuscript are not very clear nor are the scientific and the technical conclusions. The dataset is of undoubtable value, the authors have, nonetheless, clarify their technical or scientific message. I do not recommend the publication of the manuscript in its

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Discussion paper

present form. However, with the hope that my comments will be helpful to the authors, I suggest a major rethinking of the manuscript.

MAJOR COMMENTS My biggest concern is represented by the overall “take home message”, which is hard to grasp. The “Campaign overview” and “Absorbing aerosol characteristic” are not of scientific relevance, since similar results have been widely presented in previous and more comprehensive works. Hence, the characterization of aerosol properties and airmass origin, which does not have a clear impact on the instrumental comparison, adds only confusion. As an example, the distinction between period 1 and period 2 is not used in the more technical part. The 5 filter-based absorption photometers agree to a variable degree. The actual causes are, however, not clear or not investigated. As a matter of fact, Section 3.5 provides, citing the manuscript, “unambiguous evidence” on the impact of mixing and size on optical measurements.

The SP2 is used to provide the degree of internal mixing of rBC particles. Why this is done with the lag time technique and not with the LEO fit. Although both are prone to large uncertainty, the first is only qualitative. If my understanding is right, the Mode1 Mode2 classes are based on a lag time distribution. It appears that the lag time analysis was applied to all BC particles. This might cause substantial biass in the fraction of thickly and thinly coated particles. By limiting the analysis to BC cores falling in the detection range of scattering detector, the fraction of thickly coated particles should degrees (see specific comments below). Hence, the very interesting and also surprising results shown in 3.5 might be wrong.

SPECIFIC COMMENTS

L: line of text; F: figure; S: section

Title: from the title the reader might expect a soot optical characterization on a large Arctic scale. I suggest to slightly modify the tittle specifying the location of measurements.

L32: Worth citing the NILU report : https://www.amap.no/work-area/document/3058

L49-51: I find the statement about MAC and eBC a bit out of place and might generate confusion. eBC should be rather mentioned in the filter-based instrument paragraph. Since the eBC and MAC are mostly used for the filter-based instruments, I suggest to mention them a bit earlier.

L78: It would be appropriate to shortly summarize the goal of the manuscript in this last paragraph, or clearly state that this work was performed within the framework of EMPIR BC, if this is the case.

L95-100: description of goals and objectives does not belong to method sections, more to introduction.

L100-109: in this subchapter there are many abbreviations of the various instruments, which might become overwhelming and confusing to a non-expert reader. I suggest to move this plumbing description in a separate subchapter after the instrumental description.

L145: I would not use the abbreviation MAC to describe the coefficient used internally by the AE31.

L110-126: only the SSA is really described. The MAC is briefly described elsewhere in the text. I suggest to rework a bit this part in order to provide a more systematic and inclusive description of all optical properties.

L159-160: This is actually a very good point

L192: Continuous soot monitoring system . . . capital or non capital?

L193-195: Many periods in this sentence, writing could be smoother and more enjoyable. This is a constant feature of the paper. I suggest the authors to work a bit on it.

L206: not sure if capital letters for Black Carbon are needed.

L210: The work of Lim focussed on SP2 measurement in snow. I think there are better references: (Laborde et al., 2012).

L211-213: as stated: “This technique is very sensitive but does not measure particle light absorption as such, and therefore, a direct comparison with other absorption measurement techniques is not straightforward.”. So, what the SP2 is used for in this work?

L215: provide CAPS full name

L220-221: Despite very recent and in review, I suggest giving a look to (Modini et al., 2020).

L262-270: these two subsections (2.4.10 and 2.5) are very short. I suggest combining them together with the plumbing description (L100-110) into a unique subsection: “Additional tools and methods”

L273-277: these numbers are not very useful without any reference for comparison. Are these pristine, background, polluted conditions for Finnish Arctic? Considering the influence of different airmasses and, thus, different aerosol loads and properties, averaged values are definitely not of interest. I suggest removing this paragraph.

L278-279: the distinction of the two periods in not very clear. Especially considering the backtrajectories shown in F3 (see related comment).

L288-291: “The aerosol optical size related parameter”, is confusing. Simply use the symbol or “Angstrom exponent”. I would be careful to jump into conclusions: the absence of precipitation and thus wet scavenging (both from nucleation and impaction) might cause increase of number concentration and diameter decrease. Moreover optical diameter measurements are not available.

L292: typically observed. . .add reference and potentially a value.

L296-298:What do you mean with “average σAP630nm”? Average between all instru-

ment? Unclear. Same at L319

L307-309: lag-time description. . .move this to technical section. Is the lag applied to all rBC signal or a to a specific rBC diameter range? Although this measurement does appear to be only qualitative here and does not need supreme robustness in this case, applying a lower limit to rBC particles diameter (let's say above the detection limit of the scattering signal) will reduce the number of thickly coated BC cores (the weak incandescence signals with no coating (total particle size below 150-200 nm) will not be seen by scattering detector). Not compulsory, but worth trying. This might change your statement at L310.

L321-322: from my understanding COSMOS directly provide a "eBC" with the cinstant COSMOS-MAC value. Here absorption coefficient is presented, which MAC was used. Worth specify in the respective technical section.

L320-334: These paragraphs so not provide relevant information; Or, at least, it is hard to understand what the authors want to show.

L337: in the equation there is a ">". Is this correct or it should be " * "? L361-365: Why ? Is the low sensitivity the sole explanation to the bad correlation between EMS and MAAP ? This is quite interesting since CAPS and nephelometer should not suffer from filter matrix-effect.

L379-380: What do you mean with "a clear tendency". I suggest plotting the AE31 and AE33 results in figure 8. Same for figure 9.

L393-395: the increase of absorbing organic carbon could be seen with tha absorption angstrom exponent.

S3.1-3.2: Since the main focus is absorption I suggest merging these two sections

S3.4: is the analysis done on the full campaign or on a selected period?

F1: this figure is partially needed to understand the scientific message of the paper. I

would move it in the supplementary.

F3: I find the legend and caption a bit confusing. These are the 2 considered periods: Period 1 (June 19 – July 7) and Period 2 (July 7 – July 17). The legend does not reflect this partitioning

F5: what the meaning of bins is?

F6: Define the difference between panels

F7 the colour scale does not provide useful additional information. Scattering coefficient is not even mentioned in the text. I wonder if SSA might provide a more info.

F8 the x-axis label is a bit confusing, I suggest to use a more understandable label "Fraction of thickly coated rBC"

REFERENCE Laborde, M., Schnaiter, M., Linke, C., Saathoff, H., Naumann, K.-H., Möhler, O., Berlenz, S., Wagner, U., Taylor, J. W., Liu, D., Flynn, M., Allan, J. D., Coe, H., Heimerl, K., Dahlkötter, F., Weinzierl, B., Wollny, A. G., Zanatta, M., Cozic, J., Laj, P., Hitzenberger, R., Schwarz, J. P. and Gysel, M.: Single Particle Soot Photometer intercomparison at the AIDA chamber, Atmos Meas Tech, 5(12), 3077–3097, doi:10.5194/amt-5-3077-2012, 2012. Modini, R. L., Corbin, J. C., Brem, B. T., Irwin, M., Bertò, M., Pileci, R. E., Fetfatzis, P., Eleftheriadis, K., Henzing, B., Moerman, M. M., Liu, F., Müller, T. and Gysel-Beer, M.: Detailed characterization of the CAPS single scattering albedo monitor (CAPS PMssa) as a field-deployable instrument for measuring aerosol light absorption with the extinction-minus-scattering method, Atmospheric Meas. Tech. Discuss., 1–56, doi:https://doi.org/10.5194/amt-2020-292, 2020.

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