Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-785-AC4, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical profiles of light absorption and scattering associated with black-carbon particle fractions in the springtime Arctic above 79° N" *by* W. Richard Leaitch et al.

W. Richard Leaitch et al.

leaitchs@gmail.com

Received and published: 20 January 2020

Reviewer comment: The paper is dealing with the very important topic of the vertical distribution of black carbon and vertical profiles of optical properties. Such measurements are still rare and urgently needed to answer the important questions connected to Arctic warming. The paper is based on a valuable data set which was analyzed in detail and complemented by model results. However, I miss some interpretation and real conclusions. The plots are mainly described by the authors but interpretation is sparse. The structure of individual sections could be a bit clearer, e. g. by introducing subsections. Thus, some more work needs to be done before presenting this valuable

dataset to the scientific community. Overall, motivation and conclusions are not clear to me. The paper needs to be checked also for consistency, different symbol or better different indices are used for the same variable if I understood correctly.

Response – We are grateful to the Reviewer for their time and for their constructive comments.

Reviewer comment: Various literature is given, missing articles are already mentioned by other reviews, thus I don't want to repeat this now. But I miss a clear motivation for doing this study. What is the open question after all these publications and experiments in the past? This has to be given in the introduction to arouse the interest of the reader.

Response – As in our response to Reviewer 2, we now specifically state "Our objective is to further our knowledge of the absorption by BC at these lower ïĄşsp in a region of the Arctic where relatively few airborne measurements have been made.

Reviewer comment: Obviously, there was no dryer used in the aerosol line? Was there any measurement of rH in the inlet line? Even small changes at low rH may cause changes in particle absorption, in particular in clean environments (Düsing et al., 2019).

Response – The aerosol was sampled at ambient temperatures of -20oC and colder. It was brought into the cabin near the front which was the warmest area of the instrument cabin, and the CLAP was situated beside the nephelometer. The temperatures of the air entering the nephlometer ranged from +14oC to +30oC, and therefore we expect the aerosol was very dry. In addition, we drew comparisons between two in-cabin instruments (both situated farther along the cabin where the air was slightly colder) that indicate the aerosol sampled in the cabin was dry: the in-cabin UHSAS measurements compared well with the Alert aerosol size distributions (which are very dry), as discussed by Wiliis et al. (ACP, 2019); the in-cabin Grimm OPC measurements were compared with the measurements from the outboard (underwing) FSSP-300. The FSSP-300 probe results showed evidence (in some cases) of distributions with

particle sizes shifted to larger values compared with the Grimm OPC, and (in other cases) the two agreed well. Also, as stated in the paper, the absorption measurements used in this paper were limited to in-flight pressure variations (as recorded within the nephelometer) of less than 2 hPa over a two-minute period encompassing each one-minute sample. Thus, we have no reason to expect that RH was a large factor in the discrepancy we find. We have added reference to Düsing et al. (2019) as follows: "Recently, Düsing et al. (2019) found that changes in water uptake by filter material used in particle absorption measurements can influence the light absorption measurement. In the present case, because the ambient temperatures were -20oC or colder and the temperatures at the intake of the nephelometer, which was situated next to the CLAP, ranged from +14oC to +30oC, we expect little influence of relative humidity on our results."

Reviewer comment: Line 140 ff. What was the filter medium used in the absorption photometers? How was the correction done? This is a very sensitive part of the data analysis in the Arctic.

Response - Glass-fiber filters (Pallflex type E70-2075W) were used. This has been added to the paper. They are similar to the PSAP filters except for size. As discussed in Section 2.2, the analysis was done using the algorithm described by Bond et al. (1999) and Ogren (2010). That analysis was done by the first author. The algorithm used for the POLAR 6 data was also applied to the Alert data, and it was found to agree with the Alert data analyzed independently using the NOAA algorithm.

Reviewer comment: Line 170 ff. Why was the volume compared? The surface area is more relevant for optical properties.

Response – The volume comparison was done initially for use in Willis et al. (ACP, 2019) that was examining mass concentrations. However, the comparison was initiated with number distributions that were converted to volume. Since the number distributions also compared well, it follows that the surface area distributions compare similarly

well.

Reviewer comment: Line 193 ff. Was there any correction for losses in the sampling line or inlet? It is just mentioned that coarse particles are collected less effectively, but this should be taken into account for the analysis.

Response – No corrections for inlet losses were applied, which is why all results are based on quantities measured inboard. Our assumption is that, because the Grimm instrument was inboard along with the CLAP, Neph, UHSAS, etc., the sampled aerosol was subject to the same inlet constraints. Coarse particles, and hence dust, may have been more abundant in the ambient air, but should not affect our observations-model comparisons, assuming that coarse-particle BC was small. If BC attached to coarse particles was significant, it would only exaggerate the observations-model differences.

Reviewer comment: Please check the symbols: the scattering coefficient is named with the index scat or sp or are these different parameters.

Response – Corrected to sp.

Reviewer comment: Mie model: Do I understand correctly that no measurements of aerosol number size distribution are used? Why?

Response – The model is a global chemical transport model. The particular simulations are described by Kodros et al. (ACP, 2018). As suggested by Reviewer 1, we have added a comparison of measured and modelled size distributions.

Reviewer comment: Figure 1: I see only one star, figure caption says "stars show the center: : :"

Response – The figure has been improved.

Reviewer comment: Figure 3: Figure caption contains < 2 m, while axis and text say > 2 m. I assume the latter one is correct. Are the zero-like number concentrations at the ground stations realistic or could this be also a result of inlet losses?

Response – Thank you. The caption has been corrected to > 2 m. The data points in Figure 3 are all from the POLAR 6 flights. “Alert and Eureka” refer to the flights conducted around those two locations. Similarly, “Inuvik” is for flights conducted out of the Inuvik airport.

Reviewer comment: Line 275: is spt the same as sp ?

Response – Yes, it has been corrected.

Reviewer comment: Line 284 ff. Why is the MAC observed here so different from any other Arctic studies? This should be critically discussed.

Response – The discussion of the differences and possible factors contributing to the differences has been expanded in the current revision; see the responses to Reviewer 1. However, the reasons for the differences are unclear, particularly since the absorption and SSA values from the POLAR 6 agree with the Alert Observatory results.

Reviewer comment: Figure 8 and 9: Model results: why do the model results show these structures? This is not really clear from the plot and text. Please explain!

Response – The work of Xu et al. (2017), using the same model as Kodros et al. (ACP, 2018), found much better agreement with the observations. The main difference between those models was that Kodros et al. injected all biomass burning emissions into the boundary layer. That was mentioned in the original manuscript (lines 359-363) and it is discussed in the current revision on lines 400-402.

Reviewer comment: Most of the figures are just described, I miss some more interpretation. Although the fact that the model does not generally underestimate BC, it is mainly in higher altitudes. This is an important fact and shows that the transport of anthropogenic pollution is by far not well understood and not covered by the models. This should be clearly stated and as a result more measurement for similar regions are needed to close this gap.

Response – The model-observation comparisons are done to provide one modelling

perspective on the overall uncertainties in our knowledge of BC and its impact on the Arctic atmosphere. It would be unfair to state here that all models are deficient in some way, even if it is likely. We think that our final conclusion is consistent with your recommendation: “This work typifies the large uncertainty that exists in our knowledge of the contribution from BC to direct warming of the Arctic atmosphere. It suggests a lower level of confidence in assessing direct absorption by BC, and the need for more detailed efforts if the impact of BC on Arctic climate is to be properly established. Those efforts include improved measurements of BC and absorption, and more vertical profiles of aerosol chemistry, microphysics and optical properties.”

Literature: Düsing, S., B. Wehner, T. Müller, A. Stöcker and A. Wiedensohler (2019). "The effect of rapid relative humidity changes on fast filter-based aerosol-particle light-absorption measurements: Uncertainties and correction schemes." Atmospheric Measurement Techniques 12(11): 5879-5895.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-785, 2019.