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Interactive comment

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.

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We are grateful to the Reviewer for their time and for providing constructive comments. Our responses follow the individual comments.

Reviewer comment: The topic of the paper (BC vertical profiles in the Arctic) is importat for climate application. However some issues has to be solved before publication. One of the most important and main lack of the paper is its aim. It just reports data and a comparison with model results but with a poor discussion concerning the origin of the big differences reported. Please first of all details very well the goal and aims of the paper.

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Response – We agree that the main objective of the paper may not have been clearly stated. In the last paragraph of the introduction we stated “By constraining the discussion to values of ïĄşsp less than 15 Mm-1, we address the largest component of Arctic haze exclusive of the direct influence from strong plumes. Since most Arctic pollution in April is from long-range transport, the lower ïĄşsp suggests that these particles on average spent longer times in the Arctic atmosphere and thus are more indicative of the “chronic” Arctic haze discussed by Brock et al. (2011). Further, the SSA for particle populations that fall within this constraint have been found to decrease more sharply with decreasing ïĄşsp (e.g. Targino et al., 2005; Andrews et al., 2011), making these populations more efficient at warming of the atmosphere.“ To clarify our objective, we add, after the above, the following sentence: “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: 1-Introduction lines 55-78: most of the reported references (even good) are quite all and the final statment "in part due to the lack of observational data on the distribution of BC with altitude (e.g. Samset et al., 2013)" should be changed considering all the BC vertical profiles reported in the Arctic during the last ten years. They are not reported here. Some examples come from Schwarz et al. (2010), Wofsy et al. (2011), Spackman et al. (2010), Ferrero et al. (2016), Markowicz et al. (2017).

Response – The suggested references has been added. It now reads as follows: “Despite profiles of black carbon and optical properties in recent years (e.g. Brock et al., 2011; McNaughton et al., 2011; Schwarz et al., 2010; Wofsy et al., 2011; Spackman et al., 2010; Ferrero et al., 2016; Markowicz et al., 2017) there remains a shortage of such observational data that limits evaluation of models of Arctic BC and light absorption (e.g. Samset et al., 2013), because the Arctic is subject to transport from many pollution sources at southern latitudes during winter and spring, and variability exists with altitude, with location and from year-to-year.” In addition, we have added a reference to Ferrero et al. (2016) in our discussion of the vertical profiles in Section 3.3 as follows: ” Also, the lower part of the profile concentration data (<1 km) is similar to the springtime low-level profile BC concentrations from Ny-Ålesund measured by Ferrero et al. (2016).”

Reviewer comment: 2- Introduction lines 88-90: "Airborne measurements of ïAËŻsÂÿap that are based on transmission of light through a filter, as used here, are constrained by instabilities during changes in pressure (i.e. altitude) and generally higher detection limits (DL) associated with flight conditions". The sentence here is not clear and generate confusion in the reader. Better to remove and details in the method section.

Response – We feel that this statement is reasonably clear. It summarizes an issue with sampling for light absorption based on light transmission through a filter. The statement is appropriate here, because it is fundamental to our objective.

3- Introduction lines 91-100: this part is a methodological part. Please move to the method section.

Response – Again, this short discussion of methods is fundamental to defining the objective, and therefore maintained here.

4- Section 2.1 lines 115-116: "All airborne and model data presented here are referenced to a temperature of 20oC and pressure of 1013.25 hPa". Please remember that are ambient concentrations that determined the final radiative effect. Please add also data in ambient concentrations (at the real T and p) at least in the supplementary.

Response – We agree that the radiative effects are based on ambient concentrations, and we have added the following statement: “As discussed in Section 2.1, the profile data, including ïĄşap and ïĄşap (SSA is dimensionless), have been adjusted to a standard temperature and pressure (20oC and 1013.25 hPa) for purposes of comparisons. We note that in-situ values of ïĄşap and ïĄşap are appropriate for calculating radiative effects.“

5- Lines 193-198: "Model 1.129 measures particles larger than 0.25 ïAËŻ m, but only the coarse particle concentrations are used here. As shown by comparisons with a Particle Measuring Systems FSSP-300 probe operated under one wing of the POLAR 6, the coarse particles tend to be sampled less effectively than the submicron particles, but they are still an indicator of the presence of coarse particles, and, more importantly, the coarse particles entering the POLAR 6 sample manifold". There is no reason to avoid the use of submicron data from Grimm OPC. I would suggest to compare the Grimm data with the UHSAS ones on the overlapping measuring region.

Response – The Grimm data were used because those particles were sampled inboard the aircraft, and therefore better represent the inboard aerosol that is the subject of the measurements. Although we have compared the UHSAs and Grimm data, with reasonable results, there is no reason to draw that comparison here.

6- Lines 234-235: The model assumes a refractive index for BC of 1.75-0.45i in the mid visible (Hess et al., 1998). Hess et al. (1998) data are old. Bond an Bengstrom (2006) reported new and accepted values of BC refractive index. There is no reason to use the oldest refractive index. Please, redo the calculations considering the Bond and Bengstrom (2006) data.

Response – In response to comments from Reviewer 1, we have done exactly as you ask. These new results are shown in Figures 10 and 11 of the current revision. Perhaps surprisingly, perhaps not, there is relatively little difference.

7- Section 3.1. Dust episodes in the Arctic are quite important. Please compare your results to other literature papers.

Response – We discuss other results in Section 3.1, and we mention how our dust optical properties with the results of Hallar et al. (2015). We completely agree that dust is important, and we mention that in the conclusions, but the paper is not about dust.

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8- Line 289: "Removal of points with modelled dust concentrations greater than 1.5 ïAËŻ g m-3 (arbitrary value)". Removing data based on an arbitraty choice can influence results without any scientific criteria. Please details the reason of the 1..5 ïAËŻ g m-3 choice.

Response – While the 1.5 value is arbitrary, we demonstrate in Figure 6 that this point of discrimination is at the lower end of the modelled absorption values. Therefore, changing that value would not result in a significant difference.

9- Section 3.2: I see a serious problem here related to the fact that modelled results from which MAC are calculated are based on the hold Hess et al. (1998) refractive index. I suggest to redo the calculations (see my question 6).

Response – As above, we did re-evaluate with the refractive index recommended by Bond and Bergstrom.

10- Figure 6: please also add panels in which only the mass concentrations (either measured and modelled) are plotted one versus the other.

Response – Profile plots of the measured and modelled BC mass concentrations are shown together in Figure 9.

11-Figures 9 and 10: the reason of using half of absorption coeff or doubling it is not clear from the manuscript text. Please details it better.

Response – Agreed. In our response to Reviewer 1, we detail the changes we have made to address this problem.

12- Lines 409-410: "The modelled scattering efficiency (scattering coefficient per unit volume) is significantly lower than the efficiency based on the observations. Near the surface (>900 hPa), the median of ïAËŻsÂÿsp/Volume from the observations is 12.1 ïAËŻ m-1" Something appears wrong from a dimensional analysis. Scattering coefficient unit is usually in Mm-1, and volume in m3. How results can be in a lenght at -1 (um-1)? Moreover, the scattering efficiency is a dimensionless parameter (Seinfeld and Pandis, 2006).

Response – Thank you. We have corrected the above sentence “"The modelled scattering efficiency (scattering coefficient divided by volume concentration) is . . .” The volume is actually a volume concentration (ïĄ■m3/cm3), as shown in the profile plot of the volume concentration (Figure 12 of current revision), which results in the indicated units.

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