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# *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.

**Anonymous Referee #2**

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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. Other major comments follows. MAJOR COMMENTS: 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). 2- Introduction lines 88-90: "Airborne measurements of ïĄşap 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 3- Introduction lines 91-100: this part is a methodological part. Please move to the method section. 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. 5- Lines 193-198: "Model 1.129 measures particles larger than 0.25 ïĄ■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. 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 an Bengstrom (2006) data. 7- Section 3.1. Dust episodes in the Arctic are quite important. Please compare your results to other literature papers. 8- Line 289: "Removal of points with modelled dust concentrations greater than 1.5 ïĄ■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 ïĄ■g m-3 choice. 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). 10- Figure 6: please also add panels in which only the mass concentrations (either measured and modelled) are plotted one versus the other 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. 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 ïĄşsp/Volume from the observations is 12.1 ïĄ■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).

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