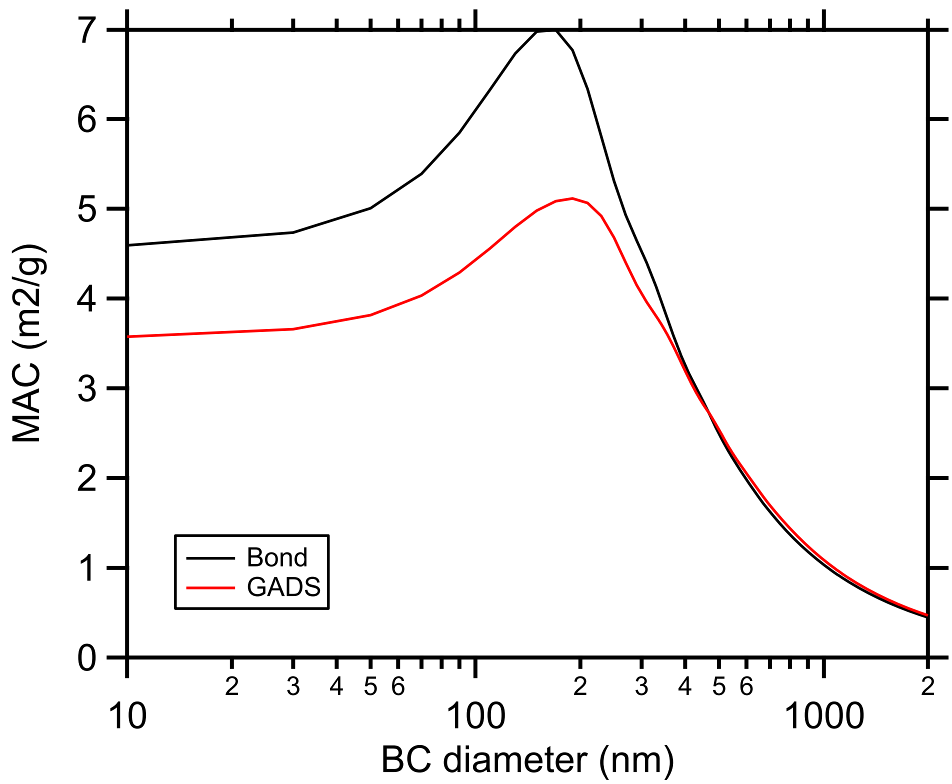
I appreciate the effort the authors have put in to their revisions, including the additional figures and calculations. However, I believe a number of concerns remain regarding the authors interpretation and some of their new results that must be resolved prior to publication. My comments follow.

Response 5: I appreciate that the authors have added results using a more realistic refractive index for BC. However, I still have strong concerns about how they present their results in the context of the GADS RI's that are used. The discussion of MAC's in Section 3.2 uses only the GADS RI values. The maximum MAC for bare BC when an RI of 1.75 + 0.45i is used 5.1 m2/g, assuming a material density of 1.8. This is substantially lower than the 7.5 m2/g that the authors refer to from Bond et al. for "freshly emitted BC." Thus, when the authors state their "Allcore" results for Axel are "are more consistent with the MAC value estimated for "freshly emitted" BC this is, in my opinion, an apples and oranges comparison. A calculated MAC of 7 m2/g using the GADS RI values implies an enhancement of at least 7/5.1 = 1.37 and for the NW Alert values the modeled enhancement must be at least 1.86. Applying these enhancements, which are much less sensitive to the BC RI used, to the "freshly emitted BC" implies a much larger MAC for the model if more appropriate RI values had been used. I believe that such issues need to be factored in to a much greater extent when interpreting the increase in the calculated MAC with decreasing BC mass concentration (around L315). Given the BC size distributions used, it is possible to calculate a maximum bound on the "lensing" enhancement (assuming a non-absorbing coating). This can then allow for bounds to be placed on the dust contribution, or on the contribution from OA. The discussion here is very speculative, but it should be made more precise. The authors can disentangle the contributions from lensing, absorption by the organics (since these are slightly absorbing in the model) and absorption by dust. To a reasonable extent, their dust concentrations are relatively constant with BC mass, around 0.8 ug/m3 or so. This, given the RI reported, corresponds to an MAC of around 0.2 m2/g for dust. Given 0.8 ug/m3 and this MAC, the calculated absorption by dust is around 0.16 m2/g. It is then trivial to show that the calculated MAC should increase with decreasing [BC] simply due to dust contributions. In fact, I suspect that the modeled MAC co-varies with the absolute dust contribution in the model (or even better, the dust/BC ratio). All this is to say that there seems no reason for the authors to be so speculative here, or to invoke unproven increases in lensing with decreasing [BC]. As the authors have a model, they can attribute absorption specifically to dust or BC, and I suggest they do so for this discussion. I'll also note that the authors statement on L524 that dust is not a factor at low BC does not seem to hold up, in light of the model behavior. Dust is most certainly a contributing factor.

Associated, I do not agree that the circled data points in Fig. 7 show "nothing other than an increase in MAC with decreasing BC" (L329). As the authors have no error bars on these numbers it is not possible to know whether this is simply noise (as I suspect it is) or some real trend. Also, I suggest that the authors distinguish between the POLAR 6 and Alert measureements in Fig. 7b, as they do in other figures. It seems evident, based on comparison with Fig. 5, that the cluster of points at low BC are all from Alert while the points at higher BC (including the "zero dust" points) are all from POLAR 6, and from Fig. 5 it seems that the precision (single point uncertainty) is much lower for the POLAR 6 data.

Fig. 10: I appreciate the authors have added calculations using a more realistic RI value for BC. However, I find the new results somewhat confusing. Below is a plot of the calculated MAC versus size for BC assuming either the GADS RI or Bond et al. RI. The Bond et al. values are larger for nearly every size. This remains true if one accounts for coatings. However, in Fig. 10 there are a number of altitudes where the GADS results yield greater absorption than the Bond results. This is not true for all altitudes, however. It is important that these differences in model configurations/results are explained more thoroughly than is done currently. Why should the absorption be lower for the GADS RI values? Did the authors, perhaps, also make some different assumption regarding the OA RI value?



L333: The authors now cite Yu et al. (2019) as showing that absorption is increased by 3-16 times in the Arctic. This is a misunderstanding of the Yu et al. paper, which makes various unconstrained assumptions regarding the optical properties of the coating material. Only when they assume the coatings are notably absorbing do they get the largest enhancements. This important aspect is not evident in what the authors have written here. The Yu et al. paper is ultimately just a Mie theory based calculation exercise using as a constraint observed coating-to-core ratios. Associated, I do not think the citation of Yu et al. (2019) on L341 is justified. Yu et al did not actually observe enhancements, and the authors here seem to misunderstand what Yu et al. showed.

L373: The deficiency in rBC mass concentrations would be a function of the particle size distribution, with a larger bias likely when the mass mode diameter is smaller and closer to the 85 nm threshold. The 7.5% cited here assumes a constant bias, but the authors should consider that the bias varies dependent on the mean particle size.

Response 30 and Figs. 12 and 13: The authors have added a comparison of size distributions, as suggested, as support for their contention that differences in the scattering efficiency between model and measurement at the lower layers might result from model/measurement size differences. I appreciate this addition, however I am not convinced by the argument here. The authors' state that "Model underestimation of submicron particle sizes may contribute to the lower modelled volume scattering efficiencies" (L473) but then a few sentences later that "The modelled distributions…are shifted to slightly lower sizes relative to the average of the observations." These points seem to me to conflict.

The authors now mention that the sampled air stream warms significantly (by up to 50 deg C). Are the authors concerned that this might impart some bias on the measurements of the more volatile particle components?

The caption for Fig. 6 is incomplete, not referencing panel b.

Fig. 7: What do the authors mean by "confidence level" for the slopes. Is this from a t-test? I am familiar with confidence intervals, but this is not the same as a confidence level.