Reply to Reviewer #2:

We gratefully thank the reviewer for the detailed review and her/his valuable suggestions to improve the manuscript. Detailed replies on the reviewer's comments are given below. Our replies are given written with indention. Citations from the revised manuscript are given in italic and quotation marks.

Page 3, caption table 1: There are various ways of defining BC. Please include a reference to for example Petzold et al. (2013) to clearly define your use of BC. Also please mention how EC values compare with BC values.

> Thanks to the reviewer to bring this up. We are aware that the definition of BC in literature is not consistent. However, Petzold et al., 2013 provided an excellent overview on that topic. Since the terminology depends on the different measurement techniques, we added the applied measurement method in Table 1.

Table 1. Typical values of the black carbon mass concentration in snow pack observed in different regions and seasons in the Arctic. Note, that Pedersen et al. (2015) and Forsström et al. (2013) derived the mass concentration of elemental carbon applying a thermal-optical measurement method.

Location	Season	BC mass concentration (ng g^{-1})	Method	Source
Svalbard region	March/April	13	filter transmission	Doherty et al. (2010)
Arctic Ocean snow	Spring	7	filter transmission	Doherty et al. (2010)
Arctic Ocean snow	Summer	8	filter transmission	Doherty et al. (2010)
Northern Norway	May	21	filter transmission	Doherty et al. (2010)
Central Greenland	Summer	3	filter transmission	Doherty et al. (2010)
Svalbard region	March/April	11 - 14	thermal-optical	Forsström et al. (2013)
Corbel, Ny-Ålesund	March	21	thermal-optical	Pedersen et al. (2015)
Barrow	April	5	thermal-optical	Pedersen et al. (2015)
Ramfjorden, Tromsø	April	13	thermal-optical	Pedersen et al. (2015)
Valhall, Tromsø	April	137	thermal-optical	Pedersen et al. (2015)
Fram Strait	April	22	thermal-optical	Pedersen et al. (2015)

> Further, we cited Petzold et al. (2013) in the introduction:
>
> *"Black carbon (BC) aerosol particles, which mostly originate from incomplete combustion of organic material (Bond et al., 2013; Petzold et al., 2013), absorb and scatter solar radiation in the visible wavelength range and, therefore, influence the atmospheric solar radiative energy budget."*
>
> and revised the manuscript accordingly:
>
> *"The numbers given in Table 1 were derived from different measurement methods. More precisely, thermal-optical techniques were applied in Forsström et al. (2013) and Pedersen et al. (2015) provide the elemental carbon (EC) mass concentration, while filter transmission methods result in BC concentrations (Doherty et al., 2010). As a consequence of the different measurement methods, the ratio of the BC to EC concentration in snow can reach values of 1.3 as reported by Douet al. (2017). A full discussion of the EC/BC terminology can be found in Petzold et al. (2013)."*

Page 4, lines 17-19: Sentence is unclear. Please reformulate.

> We rephrased the sentence:

"In this paper, measurements from the PAMARCMiP 2018 observations conducted from 10 March to 8 April 2018 were analyzed. The research flights, starting from Station Nord/Greenland, were performed above the sea ice in the Arctic ocean north of Station Nord and the Fram Strait."

Page 5, line 15: The reference to Evans (1998) and the SHDOM code appears to be out of place. Should it be Stamnes et al. (1988) instead?

We cite Stamnes et al., (2000) here which explicitly refers to DISORT2.0 as also indicated in Mayer and Kylling (2005):

"As a solver for the radiative transfer equation, the Discrete Ordinate Radiative Transfer solver (DISORT) 2 (Stamnes et al., 2000) routine running with 16 streams was chosen."

Page 5, lines 12-18: 1. How many streams was used for DISORT? 2. The solar zenith angle is large for all regions considered. Did you make any spherical corrections? If not, why not, and how do you expect this to affect your results? 3. What was the vertical resolution of your model atmosphere?

The number of streams (16) is given in the sentence before. Further, the reviewer raises a good question concerning the plane-parallel assumption we applied here. For testing the effect, we compared pseudo-spherical and plane-parallel for SZA ranging between 60 - 75° giving an uncertainty of less than 0.7% in downward irradiance. We added:

"*For the calculations, a plane-parallel atmosphere was assumed, which is justified for the Arctic conditions during the three campaigns. Using a pseudo-spherical geometry in libRadtran would change the broadband downward irradiance by less than 0.1 % (0.7 %) for calculations with SZA = 60° (75°). The vertical resolution of the simulated irradiances was adjusted to the measured BC profiles, ranging between 100 m and 1 km."*

Page 6, Fig 1: The profiles shown are averages. Please also include the standard deviation (or other measure of variability) of the profiles to give an idea of how the profiles varied for the different campaigns.

Thanks for the suggestion. We added the standard deviation in Figure 1 and included profiles of the relative humidity in a second panel as suggested by the other reviewer.

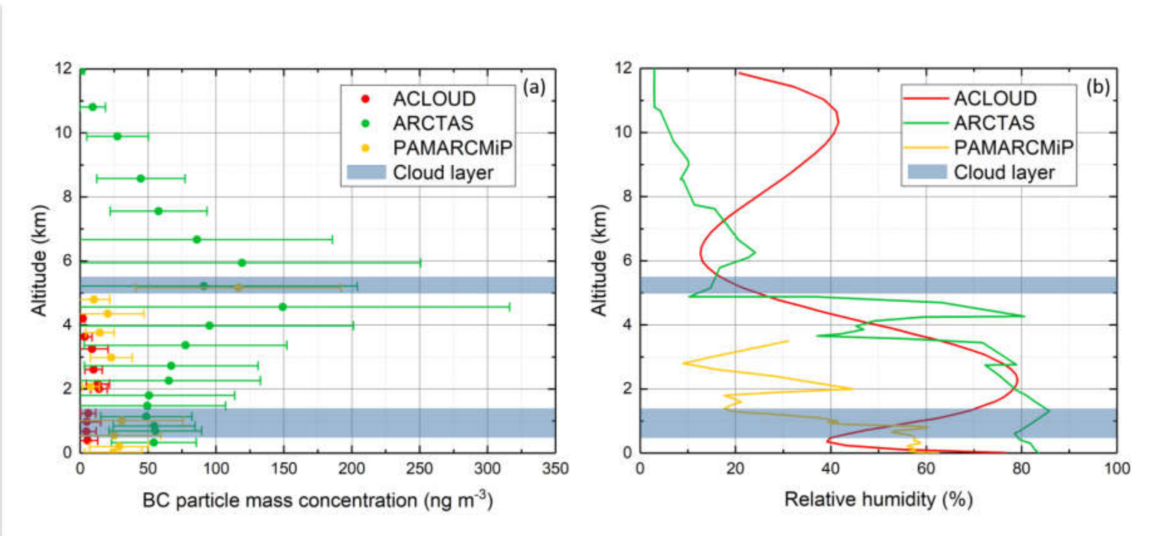

Figure 1. Mean profiles of atmospheric BC particle mass concentration (a) and relative humidity (b) averaged for each the three campaigns (ACLOUD, ARCTAS and PAMARCMiP) as used for the radiative transfer simulations. Horizontal bars indicate the standard deviation. The positions of the two implemented cloud layers (blue shaded area) are marked.

Page 7, line 2: In the snow a two-stream model is used. Presumably more streams were used for the atmospheric radiative transfer. Why is it sufficient to use only two streams in the snow pack?

The number of streams is related to the number of angles where the radiance is calculated. For up- and downward irradiance calculations often two-stream models are applied. In particular, for the radiative transfer simulations snow models apply the two-stream approximation. Dang et al. (2019) compared the DISORT calculation using 16 streams as benchmark with three two-stream models to identify the uncertainty of albedo simulations. Figure 2 from Dang et al. (2019) shows the simulated snow albedo for the tested models, illustrating the sufficient accuracy of the two-stream approximation.

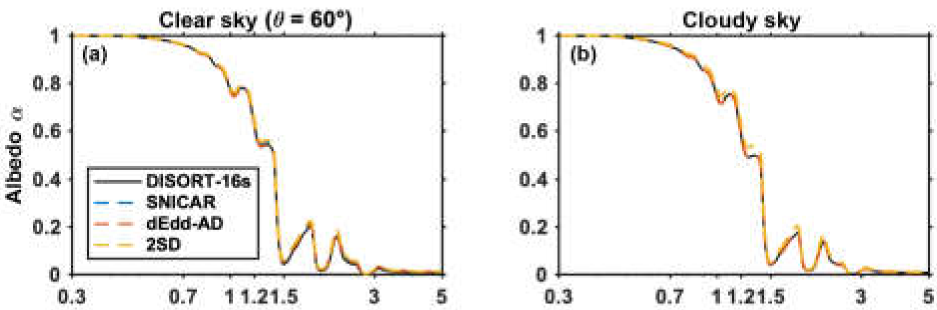

Figure 2 from Dang et al. (2019)

They conclude: “Compared with a 16-stream benchmark model, the errors in snow visible albedo for a direct-incident beam from all three two-stream models are small (<±0.005) and in-crease as snow shallows, especially for aged snow. The errors in direct near-infrared (near-IR) albedo are small (<±0.005) for solar zenith angles θ <75°, and increase as θ increases.”

We are aware that for SZA > 75° the uncertainty by using the two-streams approximation might be higher than 0.005.

Dang, C., Zender, C. S., and Flanner, M. G.: Intercomparison and improvement of two-stream shortwave radiative transfer schemes in Earth system models for a unified treatment of cryospheric surfaces, The Cryosphere, 13, 2325–2343, https://doi.org/10.5194/tc-13-2325-2019, 2019.

line 6: Stamnes et al. (1988) is not a reference for the delta-Eddington approximation. Maybe rather cite Joseph et al. (1976)?

Thanks for identifying this mistake. We changed the reference as suggested:

“To solve the radiative transfer equation, the delta-Eddington approximation (Joseph et al.,1977) is used.”

Page 7, line 14: In the snow the BC optical properties are from Bond et al. (2013) while in the atmosphere they are from Hess et al. (1998). Hence, the BC particles are different in the atmosphere and the snow. What is the rationale behind this choice other than what is available in the models used?

The refractive index of BC can vary a lot and is reported differently in various publications. Bond et al., 2013 writes exemplarily: “A variety of values for the refractive index of BC has been used in global climate models including the OPAC value of 1.74 +- 0.44i [Hess et al., 1998].” In this study we decided to use the data of BC optical properties as proposed by the two separate models for radiative transfer simulations in snow and in atmosphere, respectively.

Page 8, Table 3: Should the first row in the table be named „Thickness" instead of „Depth"?

In literature we found both terms. However, we stick to the term “depth”. It makes sense following the explanation on https://wikidiff.com/depth/thickness: “As nouns the difference between depth and thickness is that depth is the vertical distance below a surface; the degree to which something is deep while thickness is (uncountable) the property of being thick (in dimension).”

Page 8, lines 20-21: The sentence „This procedure is repeated until the deviation between previous (step n) and revised surface albedo decrease below 1 %" is unclear. Please reformulate.

The sentence is revised as follows:

“This procedure was repeated until the deviation of the surface albedo calculated in the previous step (n) and calculated in the revised step (n+1) decreases below 1 %.”

Page 10, Fig. 3: May it be concluded from the plot that the iteration procedure has no impact on the surface albedo in the wavelength region where BC absorbs?

The reviewer is right. The coupling is of minor importance for the surface snow albedo in the wavelength range where BC in snow absorbs. In TARTES, the calculation of the snow albedo requires the direct-to-global ratio as boundary condition. The difference of the calculated snow albedo from one iteration step to the next depends strongly on the change of the direct-to-global ratio. As indicated in Fig. 3, the initial step assumes a ratio of 0, which is more appropriate for the visible spectral range than for the near-infrared. For cloudless conditions the direct-to-global ratio is almost one in the near-infrared. Therefore, the largest effect of coupling is observed in the near-infrared spectral range.

We added:

“The assumption of a pure diffuse illumination in the initial run caused no significant difference of the calculated visible snow albedo to the first and second iteration step. In contrast, the iterated direct-to-global ratio adjusts the snow albedo in the near-infrared, because the direct fraction is quickly approaching unity in this spectral range.”

Page 11, lines 1-2: The upward and downward irradiances were averaged and from these the averaged heating rates were calculated. This appears as a rather unusual and unphysical approach. Would it not be more appropriate to calculate the instantaneous heating rates and then average these?

In this study we were focused on the daily averaged heating rates. The averaging gives exactly what the unit of heating rates, K/day, is expressing. From the mathematical point of view, there is no difference between the temporal averaging of the irradiances and calculating the mean heating rate out of it, or averaging the temporal resolved heating rates over the day.

Since the numerator is a linear term and the arithmetic mean has linear correlations, the results will not change when swapping the order of operation. We try to illustrate that by the following equation:

$$\overline{HR(z)} = \frac{\frac{1}{n}\sum_{i=1}^{n}(F_{net}z_t,i - F_{net}z_b,i)}{\rho(z)c_p(z_t - z_b)} = \frac{\frac{1}{n}\sum_{i=1}^{n}(F_{net}z_t,i) - \frac{1}{n}\sum_{i=1}^{n}(F_{net}z_b,i)}{\rho(z)c_p(z_t - z_b)} = \frac{1}{n}\sum_{i=1}^{n} HR(z),i$$

Page 11, line 15: The enlargement of Fig 4. seems to be missing. As Fig. 4 is, it does not make sense to have many overlapping lines. Please provide a zoom in of the visible wavelength region (lambda < 700 nm).

We are sorry for the confusion. We included the wrong image file in our first version. It is updated as follows:

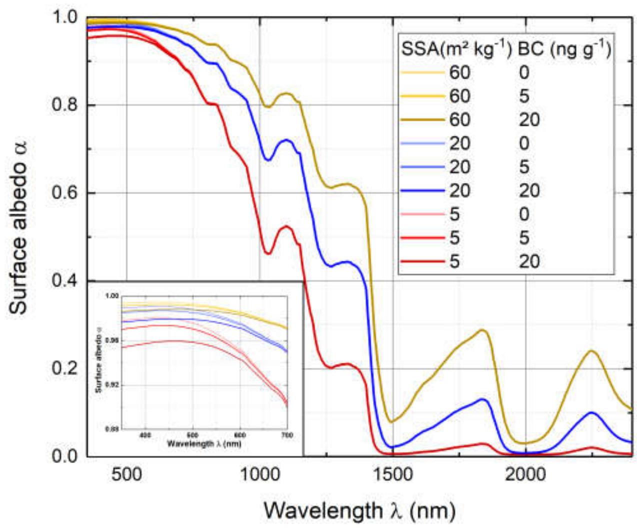

Figure 4. Spectral surface albedo of snow for cloudless conditions and a SZA of 60° for different SSA and BC particle mass concentrations. The inlay shows an enlargement of the spectral albedo between 350 and 700 nm.

Page 11-12, line 31-1: In the introduction it is stated that „the radiative effects of atmospheric BC particles and BC suspended in snow shows an opposite behavior" and „these two effects balance each other". Here it says „the impact of BC particles suspended in the snow pack is assumed to be of minor importance for Arctic conditions". These statements appears to be contradicting each other. Please clarify.

Indeed, our statement was misleading. We adjusted the introduction part and removed the "balance statement" which has not been properly expressed.

"Many regional and global climate models do account for the opposite radiative forcing of atmospheric BC particles and BC particles embedded in snow (Samset et al., 2014). However, estimates of the total net forcing rely on the accuracy of the distribution of the BC particles assumed in the particular model."

Page 12, lines 1-2: The paper by Warren (2013) discussed remote sensing of BC in the snowpack. I can not see how it justifies the claims made here?

We weakened our statement here that BC in snow is of low importance by relating directly to the snow grain size effect. The numbers of albedo reduction due to BC in snow given in Warren (2013) are in good agreement with our calculation. Therefore, we cited the paper here. For clarification we added some more details as follows:

"Therefore, for Arctic conditions, the impact of BC impurities on the broadband snow albedo is of minor importance, compared to the impact of modifying the snow grain size. Also Warren and Wiscombe (1980) and Warren (2013) found only a small reduction of the broadband albedo between 0 - 1 % for fresh snow and 0 – 3 % for aged snow when adding BC with a mass concentration of 34 ng g^{-1} to the clean snow."

Page 14, line 2: Is the factor of about 3 mostly due to differences in solar zenith angle?

Yes, the difference between the three cases is the diurnal pattern of available radiation.

We stated in the original manuscript: "This difference is caused by the lower maximum Sun elevation during PAMARCMiP (location in higher latitude) resulting in a lower amount of available incoming solar irradiance compared to ACLOUD and ARCTAS (see range of SZA in Tab. 2)."

Page 16, line 32: Sentence starting with „Absorption in the ..." is unclear. Please reformulate.

We rephrased the sentence:

"The absorption in the ice cloud is less pronounced, and the increase of $HR_{tot}(z)$ is significantly lower."

Pages 19-21: In the conclusions please discuss how the results from this study compare with previous studies mentioned in the introduction.

We compared the derived atmospheric heating rates due to BC already with findings from other regions to relate the numbers to more polluted conditions:

"For example, studies investigating strong pollution conditions in northern India or China reported on BC heating rates in the atmosphere larger than 2 K day^{-1}, which may significantly influence the lapse rate and the atmospheric stability (Tripathi et al., 2007; Wendisch et al., 2008). For the rather pristine Arctic, this study showed significantly lower daily mean BC heating rates of maximum 0.1 K day^{-1}, which have not the potential to significantly modify the atmospheric stability."

Further we added a comparison of BC radiative effects with Wendling et al. (1985) for atmospheric BC and Dou and Xiao, 2016 for BC embedded in snow:

"The magnitude of the atmospheric BC radiative forcing at the surface derived in this study (up to -0.2 W m^{-2}) agrees quite well with findings from Wendling et al. (1985). They reported a BC induced solar cooling in the range of 0.0 to -0.5 W m^{-2} for spring measurements in the Svalbard area. Further, the solar surface radiative effect due to BC embedded in snow has shown solar warming between 0.05 and 0.7 W m^{-2} depending on the BC mass concentration and incident solar irradiance. For comparison, Dou and Cun-De (2016) deduced an averaged solar warming over Svalbard in spring of 0.54 W m^{-2} based on a BC mass concentration of 5 ng g^{-1} in snow."

Language corrections

Page 2, line 7: change 'of suspended' to 'suspended'.

Changed.

Page 2, line 34: change 'will warming' to 'will warm'.

Changed.

Page 3, line 16: remove '.' after 'quantified.'.

Changed as suggested.

Page 9, line 1: Should it be „converge" instead of „conversion"?

We changed it to: *“This quick convergence of …”*

Page 11, line 6: Remove “(SSA respectively)".

Changed.