Responses to the AMT-2021-148 Manuscript RC1 comments

The paper presents the measurement results from the airborne RadSnowExp campaign which offers near-simultaneous and coincident triple-frequency radar observation and in-situ (in-cloud) ice particle characterization. The observational setup is rather unique as it basically shifts what is commonly done on the ground (e.g. BAECC campaign in Finland, von Lerber et al 2017) on an airplane allowing for direct in-cloud particle imaging, but posing new challenges regarding the observational constraint given by the airborne platform.

The analysis of the dataset focuses on the connection between triple-frequency radar signatures at the X-Ka-W band and particle properties which is a very relevant subject for snow microphysical studies. The study confirms the existence of such connection as it is predicted by various modeling studies which are presented in the paper introduction. The results of the study are supporting the idea of using multifrequency radars for microphysical retrievals.
Given the significance of the dataset presented I think the paper constitutes a valuable contribution to AMT. However, I have a few major comments that I suggest to be addressed before the paper is published.
We would like to thank the referee for these very helpful and constructive comments. We have significantly revised most of the figures to improve their appearance and the overall presentation of the manuscript to enhance its clarity and readability. We have also addressed the various deficiencies pointed out by the referee throughout the paper. Please find our detailed responses to your comments as follows.

1) Figure quality. I do not think that the presentation quality is sufficient for a final publication. Many figures are very hard to evaluate due to the fact that they are quite compressed in terms of the range of values. Also, text and labels are often hardly visible. Some significant work must be put on the figure quality.

The following are just suggestions connected to the aforementioned "readability" point, but it is up to the authors to take it or not. The number and size of the figures are significant, perhaps some work can be done also in this direction to rationalize the figure-load and

facilitate the reading. As an example, Figures 12, 15, and 18 occupy an area comparable to the one occupied by Figure 13 and not equivalently discussed. Perhaps they could be probably be accommodated as subpanels of their respective "event-dashboards" (figures 13, 16, and 19) making this a complete overview of the measurements.
Figure 6 can be combined with Figure 5 giving a single general overview of the flight path and atmospheric conditions which connects well to the description given in the text.
Finally, I see a little relevance of figures 1 and 6 which are not necessary for the paper and can be moved to supplementary material or even left out. The study focuses on the measurements taken during the flight of 22 November and should only present data from that flight in my opinion. I basically had problems during the reading in following the various hierarchical groups: campaign->flights->segments->sections(A, B, C ...).
We completely agree with the referee on this comment. In the revised manuscript, we have increased the font size in the figures and rearranged the panels in Figs 13, 16, and 19 to improve the figure quality and readability (see the response in detailed comment below). We have considered accommodating Fig. 12 as a subpanel of Fig. 13 (similar to Figs. 15 and 16; 18 and 19) but the resultant figure became too “busy”. We have also removed Fig 1, 5, and 6 as they are not necessary for the paper. With this major modification we believe the revised manuscript is now easier to read.

2) Data availability. I did not find the data availability section. There are occasions where the paper specifically states the importance of the presented dataset which makes the data availability not only highly recommended, but quite essential to deliver the value of the study to the scientific community.
During the submission process, we entered this information to the Data Availability section on AMT “The data used in this analysis are stored at the National Research Council database. Please contact the lead author for access to the data.” Our plan is to publish the whole dataset when all the flight data are quality controlled accompanied by a paper to ESSD describing the details of all flights in this project.

3) Scope and Uncertainties. The abstract (and in part also the summary) states that there is a "close relationship" between triple-frequency and particles' bulk density, level of riming, aggregation, and characteristic size of the PSD.

The degrees of aggregation and riming are not evaluated if not only qualitatively, but I do not understand from this paper how to use DFR to make a quantitative estimation of aggregation and riming degree.

We have revised the text in the abstract and conclusion sections to remove this confusion on the quantitative estimation of aggregation and riming degree. The accompanying paper (Mroz et al., 2021) presents nice results on the estimation of level of riming using the same dataset mentioned in this manuscript.

Regarding bulk density, I do not see such close relation. Judging from figures 14, 17, and 20 it seems that bulk density is connected to mean size but can take various values at the same DFR range. Looking at Fig 21b it seems that high-density values are found for small DFR and on both left and right sides of the histogram. The "rotation" feature in the triple-frequency plot is not really evident. The range of bulk density values is very limited and skewed towards low densities which suggests a general problem in estimating this quantity. This also suggests that higher density values are found at the borders of the histograms due to problems of statistical representativeness (rare values are found in small samples). Density values in Fig 21 seem to correlate mostly with MVD rather than DFR.

We agree with this referee's observations and address his remarks in the section below.

Regarding MVD I think that a correlation with DFRs is clear. However, Fig 21a only shows the mean MVD for a combination of DFRs and does not show other important quantities such as the variance of MVD which I believe is essential for the retrieval study of Mroz (2021).

We have added the variance plot for MVD and ρ_e estimation to Fig. 21.

Detailed Points

Line 115. Figure 2 - it is very difficult to connect the curves to the legend symbols. Perhaps enlarge the legend fonts or group the legend labels in different blocks according to their respective main group (already color-coded). Also, it is not totally clear to me how this is used in the study. If it is only for illustrative purposes or it is actually an attempt to connect with microphysical properties?

For example, It would be nice to connect the triple-frequency characteristic of these modeled particles with microphysical quantities as they are defined in section 3.2 lines 230-240. What are the MVD and bulk density of these modeled particles? How do they compare with the mean values measured for the same DFRs?

Fig. 2 has been introduced here only for illustrative purposes. A more quantitative analysis that discusses the values expected for the microphysical properties (Dm and density) in correspondence to a given pair of DFR values (DFR Ka-W, DFR X-Ka) is provided in a companion paper (Mroz et al. 2021, in particular see Fig.2).

The figure has been modified by increasing the symbol size and the legend fonts and identifying for some of the symbols the corresponding MDV values.

Line 161-168 If the Ka and W band radars are absolutely calibrated, their return for Rayleigh ice particles should be around 1.2 dB and not 0. This is because of the frequency-dependent difference between the dielectric factor K for ice and water. The radars cannot be simultaneously calibrated in an absolute sense and have DFR=0 for small ice particles. Please clarify the calibration procedure.

This point is also discussed in Dias Neto et al. 2019 and Ori et al 2020

In the calibration constants, we use dielectric factor (|Kw|^2) of 0.93, 0.88, and 0.7 for X, Ka, and W-band at T=0° C, respectively. The cross calibration between frequencies is done at the region of small ice particle sizes (e.g. near cloud top at the beginning (19:07 – 19:32 UTC) of the 22 Nov flight). For small ice particles (median size less than 300 um), according to Matrosov et al. (1993), differences between Z_X and Z_{Ka} should be negligible, and differences between Z_{Ka} and Z_W is small, $Z_{Ka} - Z_W \sim 0.2\ dB$. We have updated the radar data and re-generated all of the related figures. The text in this paragraph has been revised for clarity.

https://psl.noaa.gov/people/sergey.matrosov/1993%20-%20jgr.pdf

Line 185 Figure 4 It is very difficult to evaluate a bias of 0.8 dB on a small scale that spans over 100 dBZ. Considering the objective of the figure I would cut it between -15 and 10 dBZ focusing on the upper part only.

We have removed panel (a) and added a “zoom-in” plot of the upper part to more clearly show the mismatch in reflectivities at the close range. In the left panel, we prefer to retain the profile reaching to the ground to show the good alignment of the data with the ground return as a reference.

Line 236 It would be nice to include a formula also here like it is done for the other quantities. Usually it is define as,

int_0^MVD V(D) N(D) dD = int_MVD^inf V(D)N(D)dD

where V(D) is the volume as a function of size.
Since the video disdrometer cannot measure the volume of snowflakes (which is also ill-defined considering that snowflakes' shapes are irregular) it is better to say also how volume is calculated here. Is it still assumed to be a spheroid with a 0.6 aspect ratio?
The given citation seems inappropriate to me. Leroy (2016) describes a methodology to calculate Median Mass Diameter (MMD) and it is not clear how this is connected to MVD.

We agree with the referee on this comment. In the revision, we added a formula for MVD as suggested. In the calculation of V(D), each particle is approximated as an oblate spheroid with an aspect ratio of 0.6 similar to that in the calculation of effective bulk density. We added text to clarify this point.
We also thank the referee for pointing out an error in the citation. Since the formula for MVD is added, we removed the reference for MVD.

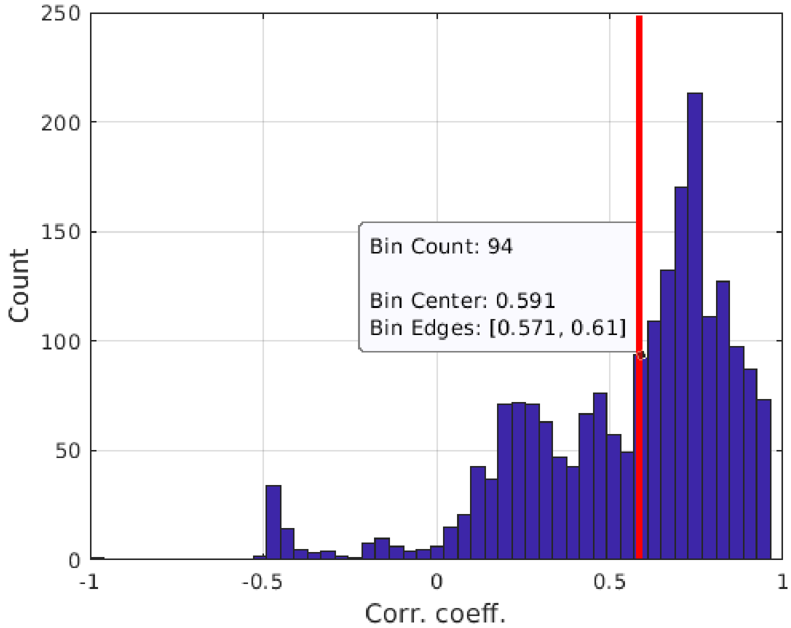
Finally, the statement "This is the characteristic diameter that contributes most to cloud liquid water or mass" is confusing and incorrect to me. By definition, the size contributing the most to the mass should be the one that maximizes the function m(D)N(D)dD (i.e. the mode of the mass distribution). Even considering the volume equivalent to mass (by assuming constant density) stating that MVD is the size that contributes the most to the total volume would be again incorrect. The mode and the median value of distribution are in general diverse, this is especially true for multimodal distributions such it is the case in the presented case studies.

We thank the referee for pointing out this error. The text was used in an older version of the manuscript for other parameter and should be changed. In the revision, we replaced it with "The in situ derived MVD will be used to evaluate the relationship between the characteristic size of the PSD and the DFRs (Kneifel et al., 2015). "

Lines 291-294. I am not really sure if I can understand these sentences. First, a 10-minute running window corresponds to roughly 6 km considering the average ground speed. Is homogeneity important for this thresholding technique? How is the threshold of 0.6 identified? What do the authors mean by "accurate analysis"?
I guess that a good correlation is one easy indicator that the authors can use in order to connect measurements on-board of the aircraft and apart from it, but I wonder if this analysis could be biased by the characteristics of the measurements. As an example: If the cloud field analyzed is very homogeneous both measurements would result in a signal mostly dominated by random noise and thus even if the two signals are connected in reality the correlation coefficient would be close to 0.

We tried several ways to determine what cloud segments and which direction (up or down) would be used in the study cases and found (empirically) that the simple correlation method works best. The reason we chose a 10-minute (or 60 km) window is exactly as pointed out by the referee: to avoid the case where the cloud field is homogeneous. In the environment we flew (this Artic storm), the likelihood of the cloud being homogeneous over a 60 km scale is utterly negligible. On the other hand, if a longer window is used, the results will be smoothed out, possibly leading to an inaccurate selection. We mention in passing that it is well known (e.g. Guillaume et al. 2018 and references therein) that atmospheric fields generally exhibit strong spatial scale dependence; this complicates the matter of computing their unbiased averages and other statistical properties (e.g. Selvam 2009 and references therein).

The figure below shows the histogram of the nadir correlation coefficient. The histogram show a main mode with correlation coefficient greater than 0.6. Hence, we selected a threshold of 0.6 for the decision of a good match between the in situ measurements and the radar data.



Line 320 Figure 10 This figure is not readable. I suggest the authors make much better use of the page real estate; increasing the vertical size of the figure, allowing for a better evaluation of the various curves, and significantly enlarging the font sizes.

We thank the referee for this suggestion. We have increased the vertical size of the figure and the font size of figure 10 to improve the readability.

Line 338 Figure 11 Enlarge axes font size of the legend.

Correct as suggested.

Line 346 Is it possible that the 30um peak is due to the shattering of ice particles at the probes? Shattering is not discussed in the text. The reference list includes Lawson (2011) but that reference is not present in the text (Line 600).

To minimize hydrometeor shattering the probes are equipped with anti-shattering tips (Korolev et al., 2013) as it might be possible to see in Fig. 3b. Therefore, the shattering events are expected to be relatively rare and wouldn't be visible on an averaged size distribution such as the one in Fig. 12. We have removed Lawson 2011 from references and added Korolev et al., 2013. We also amended the sentence in the manuscript for clarity: "The probes are equipped with anti-shattering tips (see Korolev et al., 2013 for details) and were calibrated with glass beads and a spinning chopper before the campaign and re-evaluated in NRC's altitude icing wind tunnel after the campaign."

Field, P. R., A. J. Heymsfield, and A. Bansemer. " Shattering and Particle Interarrival Times Measured by Optical Array Probes in Ice Clouds", Journal of Atmospheric and Oceanic Technology 23, 10, 1357-1371, https://doi.org/10.1175/JTECH1922.1, 2006.

Korolev, Alexei, Edward Emery, and Kirk Creelman. " Modification and Tests of Particle Probe Tips to Mitigate Effects of Ice Shattering", Journal of Atmospheric and Oceanic Technology 30, 4, 690-708, https://doi.org/10.1175/JTECH-D-12-00142.1, 2013.

Line 380 Figure 13 (the same applies to figure 16 and 19). I like these overview plots, but the Figures are barely readable at maximum magnification on a screen. Also, the subpanels are not labeled and it is difficult to follow the discussion on them. I suggest significantly increase the size of the figures. An idea to make better use of the page surface could be to put all time-plots on the left column sharing the same x-time axis and the ABCDE-sections classification. The left column could take up to 2/3 or even 3/4 of the figure width. Then, the snow images could be arranged on the right column. Also, I suggest reducing the number of ice images including only a few significant ones.
We thank the referee for this suggestion. In the revised manuscript, we implemented this idea and it greatly improves the presentation of the figures.

Line 480 Figure 22. What are the black lines?
The black lines present data means and error bars of one standard deviation of the DFRs. We added a sentence for clarity.

Line 493 It is not clear to me where to find the relationships between ice particle properties and triple-frequency signature in this study. The paper presents a qualitative assessment of relations among these quantities

We agree with the referee on this comment. We have revised the sentence. It now reads “In this work, we focus on evaluating understanding the relationships between …”

Lines 503 and 505. I guess here it refers to Figure 21 and not 22

Thanks the referee for pointing out this error. It is now corrected.

Line 505 I actually see a very little sensitivity of estimated bulk density to triple frequency. From Fig 21b I do not see a transition from more reddish colors to blue/grey while "rotating" counterclockwise in the triple-frequency plot. Can the authors illustrate better this point?

We have reprocessed the Nevrozov data and recalculated the bulk density. The new data are slightly cleaner. We have also changed the colormap and display scale to better illustrate this observation.

Line 506 I saw that the "rotation feature" was much better illustrated in the first version of the manuscript uploaded. And I think that the text got it the other way around, or? A decrease in effective bulk density is expected when DFR X-Ka increases (counterclockwise rotation). For high values of DFR KaW and the low value of DFR X-Ka, we expect denser particles.

We thank the referee for pointing out this error. It should read “effective bulk density of ice particles increases as DFR X/Ka decreases and DFR Ka/W increases”. As mentioned above, figure 21 is revised and better illustrates the “rotation feature”

Minor Points

Line 17 Please introduce the CPI acronym

CPI acronym is added.

Line 18 DFR acronym is introduced later at line 21

This mistake is now corrected.

Line 22 Double period ..

Correction has been made.

Line 22 I guess the phrase was intended without the word "that", but I would suggest rephrasing it anyway to make it easier to understand.

The sentence has been rephrased in the revision.

Line 56 Mismatched parenthesis))

Thank you for pointing out this error. It is now corrected.

Line 95 I think there is a sign problem in the attenuation component of Eq 1. Assuming the attenuation to be semi defined positive such that the measured reflectivity Z=Ze-A then DFR = Z1-Z2 - (A1-A2) = Z1-Z2 + (A2-A1) [Lehrmitte 1990, Tridon 2020]

We apologize for the error. It has been corrected.

Line 232 Missing year in Heymsfield et al.

The missing year has been added.

Line 288 misspelled Gans?

We agree with the referee. It has been corrected.

Line 350 Figure 12. The caption refers to panels (a) and (b) but the figure panels are not labeled. The same applies to Figures 15 and 18.

We apologize for the error. The labels have been added.

Line 351 Text refers to left/right panels, but it is better to use panels labels (a) (b) according to AMT guidelines

We thank the referee for this comment. Figure 13 (also Figs. 16 and 19) is revised and panel labels have been added. We've also revised the text to reflect the change.

Line 436 Figure 15. I guess the caption refers to sections selected from figure 16

The figure number has been corrected.

Line 497 The MEAN particle diameter.

We agree with the referee. It now reads “The mean particle diameter increases …”

Line 546-550 I think that usually, 2020a comes before 2020b in the reference list.

We thank the referee for pointing this error. The reference order has been fixed and the corresponding reference in the manuscript has been changed.