Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-148-RC1, 2021

Comment on amt-2021-148

Anonymous Referee #1

Referee comment on "Coincident In-situ and Triple-Frequency Radar Airborne Observations in the Arctic" by Cuong M. Nguyen et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-148-RC1, 2021

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.

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 ...).

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.

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.
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.
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).

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?

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

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

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.
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.

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.

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.

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

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).

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.

Line 480 Figure 22. What are the black lines?

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

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

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?

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.

Minor Points

Line 17 Please introduce the CPI acronym

Line 18 DFR acronym is introduced later at line 21

Line 22 Double period ..

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.

Line 56 Mismatched parenthesis))

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]

Line 232 Missing year in Heymsfield et al.

Line 288 misspelled Gans?

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

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

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

Line 497 The MEAN particle diameter.

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