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Responses to reviewer 1

Highly supercooled riming and unusual triple-frequency radar signatures over Antarctica

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We thank the reviewer for his efforts and time for reviewing as well as constructive comments which greatly helped to improve the manuscript. All our point-to-point answers are highlighted in red below according to the following sequence: (i) comments from referees/public, (ii) author's response, and (iii) author's changes in manuscript.

General comments:

In this manuscript, the authors quantitatively interpret triple-frequency radar signatures in snow and ice clouds observed in Antarctica. Their results indicate that riming starts at lower temperatures in Antarctica than at mid-latitudes and that observed triple-frequency radar signatures with extreme values of DWR_Ka,W (dual-wavelength ratio of Ka and W band reflectivities) can only be explained by approximating the particle size distribution by gamma distributions with high shape parameters mu (called a 'narrow' particle size distribution). Overall, the manuscript is well written and presents the main points clearly. Nonetheless, some parts of the manuscript could still benefit from more careful editing to improve the grammar and the clarity of the arguments made (the 'Technical corrections' suggested below can provide a starting point). I think, while some of the points discussed in the manuscript (lengthy discussion of Fig. 4, entire section 4.6, see specific comments below) may add value to the study, they are also difficult to follow and therefore dilute the main results. One important point that could still be discussed in a revised version is how realistic the narrow particle size distributions are that form the basis for the retrieval of ice properties from the triple-frequency radar signatures (see also comment regarding Section 4.4, 4.5 below), given the somewhat extreme values of the PSD shape parameter and the missing information about other commonly used parameters of the size distribution(s). Considering the high quality of the study and the novelty of the results, I would therefore suggest to publish the manuscript after these points are addressed.

Specific comments:

1) **l. 137 ff:** (i) Can the authors quantify these disparities, because these disparities seem to form the motivation for the entire analysis? For example, a very basic method would be to compare (i) by how much mean or median DWR_Ka,W increases from -25 °C to -15 °C for both locations (also in relation to the width of the distributions with width interpreted e.g. as range from 10th to 90th percentile or standard deviation) and (ii) how much of the DWR_Ka,W distributions is 'much higher than the common maximum of 12 dB'. The general goal here should be to provide some reasonable parameter(s) for how large the disparities are and how (statistically) significant they are for the given datasets. No need for a detailed statistical analysis.
   (ii) (iii) We added this statistical information and modified the text and figures.

2) **l. 144 ff:** (i) Could this low aerosol concentration or a different type of aerosol found over Antarctica not also cause the initiation of aggregation at a lower temperature? What about characteristic differences in the typical wind field, could those affect aggregation/riming temperatures and lead to the initiation of aggregation at a lower temperature in Antarctica? Maybe the authors can present a few (more) arguments to support their conclusion that the differences in observed triple-frequency signatures can only be attributed to the riming process.
   (ii) The main argument supporting that the differences are not due to the aggregation process are the triple-frequency signatures themselves: as recalled in section 3.1, aggregation would cause an increase in $DWR_{X,Ka}$ practically equal to the increase in $DWR_{Ka,W}$ while no increase in the AWARE $DWR_{X,Ka}$ is seen above the -15°C level (i.e., level from which aggregation is expected to be efficient). We are therefore confident that the differences in the triple-frequency signatures are due to riming. What is less certain is the reason why riming is happening at a lower temperature in the AWARE dataset. We agree that we cannot exclude the potential effect of wind due to the complex topography around McMurdo station. (iii) We have added this hypothesis and we modified the text in order to better explain that the uncertainty is on which mechanism leads to riming at lower temperature but not on the occurrence of riming.

3) **Fig. 4 and section 4.2:** (i) Sooooo many plots in a single figure. In my opinion, the discussion of this figure also follows many trains of thought that probably all make sense but I could not grasp all of them. To appropriately make all points that the authors intend to make here, more (con)text and multiple separate figures would be needed. I would therefore suggest to focus only on the most

relevant points of this entire discussion and omit the rest.

(ii) (iii) In our opinion, all the points discussed are important. In order to make them more understandable, we separated the Fig. 4 in 3 different figures of 3 panels (i.e., we were able to remove one panel), and we describe them in 3 different subsections (4.2.1, 4.2.2 and 4.2.3), and we reworded some of the text.

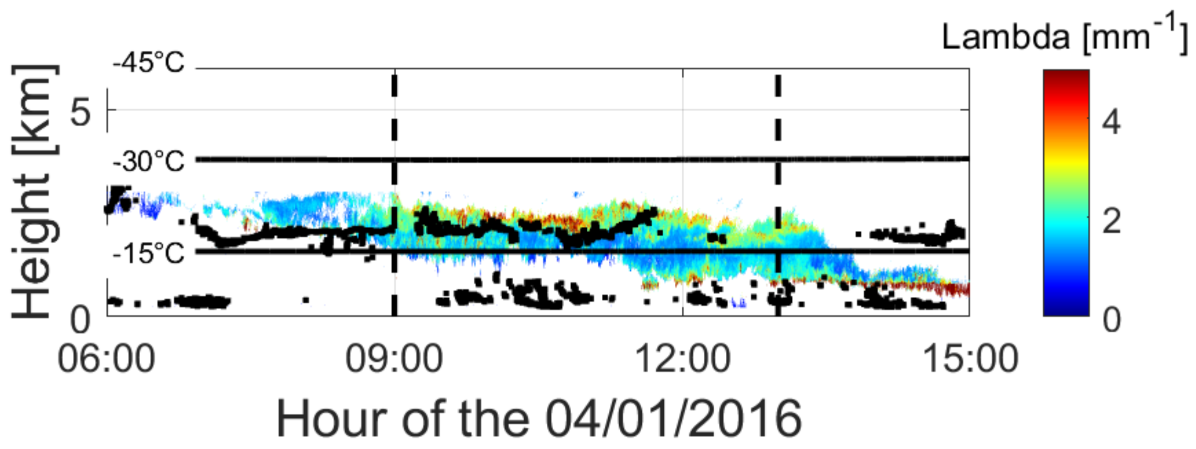
4) **l. 228 ff:** (i) I cannot follow this conclusion. Can the authors elaborate a bit or rephrase to clarify?

   (ii) (iii) We rephrased this paragraph (now first paragraph of section 4.2.3) to make it clearer.

5) **Section 4.4, 4.5:** (i) What range of Lambda values is used for the calculations to obtain the lookup table for the retrievals? Do all the retrieved parameters of the size distributions and the overall ranges used for the retrievals represent realistic size distributions in snow and ice clouds? For example, Brandes et al. (2007, DOI: 10.1175/JAM2489.1) rarely observed Gamma-PSD shape parameters of mu > 10 in their in situ observations at mid-latitudes. Gergely (2019, DOI: 10.1016/j.jqsrt.2019.106605) found a strong impact of the maximum snowflake diameter of the chosen particle size distribution on radar retrievals. Other studies also suggest that the slope parameter Lambda spans only a somewhat limited range of values. How do the values that the authors use and the ones they ultimately retrieve compare to those discussed in other studies? Are they similar? Or is there any reason why a different range of values may be appropriate to describe particle size distributions in Antarctica? Maybe the authors could also plot some of the size distributions that they retrieve or just some generic Gamma-PSDs to illustrate what a 'narrow' size distribution looks like vs. a 'wide' size distribution, as understood by the authors in the context of this study (confusion can arise here because the parameter Lambda can also be interpreted as a measure of the width of the distribution).

   (ii) One of the core result of our study is that the retrieved PSDs are indeed unusually narrow, and hence our retrieved shape parameter mu values are in the upper part of its usual range. Nevertheless, other parameters such as Dm, concentration and slope parameter Lambda lie in the range commonly found in literature.

   (iii) Because we think that there are some redundant information between Dm and Lambda, and to avoid a too large number of figures in the manuscript, we only show the Lambda values retrieved with the SSRGA-plates-M0.016 model in the Figure below. Nevertheless, we added a new figure in the manuscript (now Figure 12) in order to show examples of the gamma PSDs retrieved and their corresponding forward-modeled Doppler spectra. The figure demonstrates that the retrieved PSDs are compatible with the observed Doppler spectra. We now discuss these issues in several parts of section 4.5.



6) **l. 350 and following paragraphs:** (i) Substitute 'measurement uncertainties' and 'retrieval uncertainties', etc. for 'errors'. These are uncertainties, not errors.
   (i) We thank the reviewer for noticing this! (iii) Corrected

7) **Section 4.6:** (i) While this section describes an interesting exercise, it is not entirely clear to me whether this chapter adds anything substantial to this study, particularly because the results are mostly qualitative and can be interpreted as a type of consistency check. In my opinion, this does not add a lot of signifant results to the results presented in the previous chapter(s). I would instead (or additionally) prefer to see a brief discussion on how realistic the 'narrow' particle size distributions are, e.g., based on a comparison/discussion of studies that have used and obtained relevant parameters of snow and ice particle size distributions from in situ observations (see also comment regarding Section 4.4, 4.5).
   (ii) We respectfully disagree with the reviewer. We think that Section 4.6 is essential to this study, mostly because it addresses the reviewer's main concerns in this comment and in the general comment regarding how realistic our concluded PSD is. Comparisons to the literature are not relevant in this case for the following reasons:

   1. In-situ airborne observational datasets of Antarctic ice PSDs are scarce and contain most samples at temperatures greater than -15.
   2. The airborne instruments commonly used to obtain these ice PSDs (e.g., CPI, CIP) are limited to maximum dimensions on the order of 1.5 microns, smaller than the sizes derived here of a few to several microns, making studies using these instruments irrelevant.
   3. Lower-latitude clouds exhibit different characteristics (number concentrations, etc.) than Antarctic clouds so retrieved ice PSDs from such lower-latitude studies are irrelevant.
   4. Antarctic ground-based measurements and retrievals are confounded by commonly-occurring low-level ice sublimation (driven by Foehn winds over the

region) and blowing snow, and therefore, are challenging to objectively compared with our results.

Instead, we wish to provide physical context to our retrieval and results with this modeling exercise, in which we demonstrate that such narrow PSDs are realistic, and that the simulated scenario based on the full AWARE instrument suite measurement serves as a plausible explanation for the observed signatures.

(iii) We now explain that in the first paragraph of sect. 4.6:

"In-situ airborne observations of ice PSDs over Antarctica are relatively scarce and are commonly performed using instruments such as the Cloud Particle Imager (CPI Lawson et al., 2001) and the Cloud Imaging Probe (CIP Lachlan-Cope et al., 2016), which are limited to a particle maximum dimension of ~1.5 mm. This lack of a comprehensive observational database of Antarctic ice precipitation PSDs combined with instrument-detectable particle size limitations inhibits any pertinent comparison with the results presented here of ice particles with sizes on the order of a few to several mm generating the observed triple-frequency radar signatures. Moreover, the narrow and elevated altitude range depicting those radar signatures further impedes comparisons to ice particle properties derived from Antarctic ground-based observations, which are strongly influenced by low-level ice sublimation (e.g., Grazioli et al., 2017b) and blowing snow events (e.g., Loeb and Kennedy, 2021), frequently occurring over the region. Instead, we examine using the following modeling exercise whether a plausible riming scenario based on AWARE observations could develop the detected triple-frequency signature, thereby adding a physical context to this analysis, which exemplifies that such narrow ice PSDs are realistic."

We also refer to the instrument-detectable maximum ice size in the conclusions:

"*More studies are needed to validate the retrieval algorithm proposed here. This could be done … by using in-situ validation datasets from field campaigns …, while noting that commonly-used airborne imagers are limited to particles sizes smaller than those deduced in this study.* "

Finally, we also modified the section title accordingly: *"4.6: Bin model experiment: can a plausible scenario reproduce the observed narrow PSD of rimed ice particles?"*

Technical corrections:

- l. 69: 'data' instead of 'Data'? Done
- l. 144: 'this difference' or 'these differences', but not 'this differences' Done
- Fig. 2 caption: 'temperature profiles' Done
- l. 188: Do you mean ... 'refractive index' … Yes! Corrected

- l. 193: I do not understand ' where the temperature is comprised between -25 and -40_C', please rephrase, maybe you mean 'where the temperature is between -25 and -40 °C'. Yes. Corrected
- l. 212 ff: Can you rewrite this discussion, so it is more easily understood. For example, I get confused by multiple clauses starting with 'conversely' so close to each other. Done
- l. 214: Do you mean ... vertical 'bands' ... that alternate between blue and red? Yes. Corrected
- l. 224: spectrum width (singular) Corrected
- l. 226: Well, apparently they are not identical. Why not use 'very similar' or something along those lines? Done
- Fig. 7 caption: replace 'comprised between' with 'of' Done
- l. 388: delete 'notable' Done
- l. 394: Likewise, Fig. 9 c ... (instead of 'Likewise, the figure 9 c') Corrected
- l. 421: What does 'liquid-free layer' mean? Without liquid water present? Yes, changed to "cloud-free".
- l. 424: What is a 'geometrically-thick liquid water hydrometeor population'. Can you rephrase this to make it clearer? Sentence reworded: "*Based on this deduction and supported by indications of a geometrically-thick liquid water layer suggested by a consistent hydrometeor population observed in the KAZR spectra (not shown), …*"
- l. 508: maybe better to write something like 'These similarities are also evident from Fig. 11a which shows the spread ...' Text modified per the reviewer's suggestion.
- l. 545: ... allows us to constrain the microphysical properties of the ice particles … Corrected