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Comment on acp-2022-263

Anonymous Referee #2

Referee comment on "Ice microphysical processes in the dendritic growth layer: a statistical analysis combining multi-frequency and polarimetric Doppler cloud radar observations" by Leonie von Terzi et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-263-RC2, 2022

Review of “Ice microphysical processes in the dendritic growth layer: A statistical analysis combining multi-frequency and polarimetric Doppler cloud radar observations” by Leonie von Terzi, José Dias Neto, Davide Ori, Alexander Myagkov, and Stefan Kneifel.

Summary:

This manuscript examines the microphysical processes of ice particles growing above the melting layer with complementary spectral polarimetric and vertically pointing multi-frequency radar measurements. Measurements for a specific case are first analyzed to provide context for the relevant microphysical processes of vapor deposition and aggregation and to demonstrate the utility of the radar measurements in understanding these processes. Statistics of the measurements are then analyzed over a collection of cases to examine the prevalence of radar features primarily within the dendritic growth region. Profiles from the set of cases are separated into groupings of aggregate size (determined from multi-frequency measurements) and cloud top temperature. Additionally, polarimetric spectra show both a persistant updraft near -15 degrees C and the appearance of secondary mode of ice particles at this region that produce a reduction in the apparent falling motion of the ice particles measured from Doppler velocity profiles. This paper is generally well written and provides novel insights into the microphysical processes occurring within the dendrtitic growth layer. The authors do a good job contextualizing their results within the prior work on this topic, and this paper will be a valuable addition to this body of literature once some of the issues highlighted below are addressed.

General comments:

One improvement that should benefit this manuscript is using more quantitative methods or language to illustrate differences between radar profiles that are grouped by the aggregate size class or cloud top temperature. For example, differences between the median profiles of a given radar variable from two different classes could be described relative to the standard deviations of the profiles within each class. Or, for example, the lower quantile of one class exceeds the upper quantile of another class at a certain height (or temperature). As the descriptions in section 4 are currently presented, it is unclear how robust the presumed relations between the radar profiles and these classes (aggregate size and cloud top temperature) are without knowing whether they could be explained simply by random variability from subsetting the data. Adding more quantitative language to these areas of the manuscript (especially section 5.2) should help better qualify whether the relations are physical or incidental.

I also think that the evaluation of the aggregate contribution to KDP requires more discussion of the impact of particle orientation. The single-particle calculations of KDP shown in Appendix C are acceptable for the purposes of illustrating the scattering behavior with respect to size, but likely overestimate the KDP of natural aggregates if a fixed horizontal orientation is used rather than a distribution that accounts for flutter or tumbling. As such, the claims in section 5.1 regarding the aggregate contribution to KDP should also be qualified as highly uncertain.

Specific comments:

- Line 102: I believe this relation should be reversed; the Rayleigh regime is valid for particles with size much smaller than the wavelength.
- Lines 180-183: Is the spectral mask simply the region outside of the spectral edges determined by the bins exceeding the noise floor? Please clarify.
- Line 252: Please add what specific measurements this correlation refers to.
- Line 268: What is the maximum range used for the W-band measurements taken at 30-degree elevation angle? Please add what the horizontal distance is between data at this maximum range and the location of the vertically pointing radars.

- Line 305: I think using "slower than" is a bit more confusing than "greater than." Maybe if there is a mention in the text of negative MDV corresponding to motion towards the ground, comparisons to specific values of MDV would be more appropriate than indirectly referring to the absolute value of MDV.
- Lines 313-315: Please add that the ZDR at an elevation angle of 30 degrees will always be less than that measured at side incidence. It's important to mention this difference because other studies of ice microphysics often observe ZDR at elevation angles closer to side incidence (i.e., < 5 degrees).
- Line 338: It is preferable to say that the spectrum shifts rightward or towards larger values since there are weakly positive velocities in Fig. 3e near -15 degrees C.
- Line 340-341: The wording here is a bit unclear. Do the authors mean something like: the main mode contributes more power to the spectrum than the secondary mode and therefore shifts of the main mode with respect to Doppler velocity dominate changes in MDV with height? Please clarify.
- Line 342: At -12 degrees C?
- Line 345: How much of these oscillations in KDP are due to noise in the PhiDP profiles compared to a microphysical signal?
- Lines 351-359: I largely agree with this assessment. However, the lack of layered KDP enhancements observed in this study may also be due to a lack of strong forcing associated with mesoscale snowbands. In these snowband cases, there may be more particles and/or more rapid dendritic growth leading to more intense aggregation and a thus a more rapid depletion of pristine ice crystals. The higher sensitivity radars used in this project would be able to detect weaker vapor growth and aggregation cases where the ice crystal depletion is slower, extending the KDP enhancement farther down. KDP observations of these weaker cases at S-band or X-band would likely show near-zero KDP throughout the profile. So in order for the low-frequency radars to detect measurable KDP enhancements in snow, there may need to be more substantial vapor growth and subsequent aggregation. Please address this potential for selection bias with respect to low-frequency radars in the text.
- Line 369: Does continuous here refer to profiles without any masked regions?
- Line 388: Please use "dB" instead of "dBZ" for differences in reflectivity values.
- Lines 396-397: Please change to "magnitude of MDV increases."
- Line 405: Please change "upwind" to "updraft."
- Line 422: How are the slow and fast edges of the spectra determined? Are they the first and last bins above the noise threshold?
- Lines 426-427: Does this assumption that new particles decrease the slow edge of the spectrum require that these particles have a minimum fall velocity? For example, if the new particles only become detectable once their fall speed is 0.5 m/s, isn't it possible that they would have no effect on the slow edge velocity?
- Line 499: Given that the example case study seems to have much higher skewness, KDP, and maximum spectral ZDR (near 4 dB according to Fig. 3f) compared to the bulk statistics, there should be some brief discussion of the uniqueness of that case relative to the others in the dataset.
- Line 525: ZDR also tends to saturate as dendritic growth occurs because of the generally decreasing effective density of the particles with size. Please add some mention of this effect.
- Line 558: The uncertainty in this value for the aggregate KDP contribution needs to be more clearly stated. For example, the orientation behavior of the aggregates can have a large impact on the measured KDP.
- Lines 561-562: Please add a caveat here that there may be non-Rayleigh effects on KDP at larger size parameters than those examined in this study.
- Lines 612-613: Please reword this sentence for clarity.
- Lines 612-616: The comparisons between the radar variable profiles with different cloud top temperatures need to be more carefully stated in terms of how significant they are relative to sampling errors between the different groups. In other words, are the differences in particular radar variable profiles beyond what would be expected

from randomly grouping the profiles into different classes?

- Line 654: Please clarify what properties of the sedimenting particles are being considered to have no effect on KDP and sZDRmax here.
- Lines 752-754: What orientation assumptions are being used to calculate the aggregate KDP? Does the simulated ZDR for the aggregates with these PSD assumptions agree with the measurements?