Review for “Peering into the heart of theunderstorm clouds: Insights from cloud radar and spectral polarimetry” (10.5194/egusphere-2024-1232)

General comments:

This paper uses the technique of spectral data from a polarimetric cloud radar to study the passage of convective cells with lightning activity. The study is novel in that spectral polarimetry analysis in the Ka-band is used to study a thunderstorm cloud, so as to identify the different scattering regimes and the suitability of interpreting sZDR extrema. The inclusion of scattering simulations using the T-matrix method supplements the explanation of peaks and troughs in the spectral polarimetric data. In particular, the study makes convincing interpretations for the presence of conical graupel and vertical alignment of particles prior to a lightning strike, which are consistent with past literature. The phenomena of supercooled liquid water, chains and strong updrafts associated with in-cloud turbulence are also explored.

The manuscript is mostly well written, with a detailed set of results and scientific explanations that make it suitable for the scope of AMT. The novelty of using spectral polarimetry of a cloud radar on a thunderstorm case is well appreciated, especially when supplemented with sufficient results that ultimately lead to valid interpretations of the hydrometeor type and the sound conclusions reached regarding their presence in convective cells with lightning. There are several clarifications that are needed in the first two sections of the manuscript, alongside suggested reduction to the results section. Although the specific comments are mostly minor, they combine to push me to recommend a major revision.

Specific comments:

Major

1. Although the authors have made it clear that there could be differential attenuation due to attenuated reflectivity from the presence of rain drops for 17:24 to 17:30UTC, there still seems to be areas of enhanced calibrated ZDR just above 2000m of height in Fig. 19a. Could the authors explain whether they have taken into account differential attenuation when calibrating ZDR? If so, how? If not, what is the estimated magnitude of differential attenuation? How would this change the calibrated ZDR values and alter the interpretation of the results in Sections 5.2.1 and 5.2.4 regarding the evidence of conical graupel and possibility of chains.

2. There is a good variety of past literature included within the introduction section. The authors should consider an additional discussion section to the manuscript to discuss how results in this manuscript both qualitatively and

quantitatively compare with past studies if possible. The authors could consider the following questions including, but not limited to: How do the ZDR values of vertically aligned particles in this study comparable with those in Lund et al. (2009)? How does vertical alignment of ice up to 4 seconds before lightning comparable with past studies?

3. I appreciate the depth of the scientific deductions and explanations provided for the physical interpretation of the alignment of particles in Section 5.1.1. However, this has made the section rather long. To focus the discussion on the most probable reason for vertical alignment, the authors could consider shortening the paragraphs between lines 393 and 437 and potentially including some of the Doppler flipping figure and explanation as supplementary material.

Minor

Line 2: I agree that using a cloud radar to study thunderstorms mitigates the need to fly into one to take in-situ measurements. However, there is no mention of arrival time difference networks for lightning detection in the abstract. Yet, the authors do take advantage of the BLIDS system. Please mention this in the abstract.

Line 15: “reflected by the...” is confusing, especially since you mention spectrum flipping later on. I would just us “shown by the...”

Line 28: Include a diagram to supplement the explanations of the “numerous charging mechanisms”.

Lines 32-36: Some explanation of the “inconsistencies” and “quantitatively unrealistic ineffective” mechanisms of charging would be appreciated here.

Line 44: Which part of the updraft column? Consider giving an indication of a height/temperature range, which would quantify what is meant by “where the temperature is low”.

Line 46: What are the terminal velocities of the ice crystals that are “thrown upwards” in updrafts?

Lines 48-49: The authors mention that “all charging mechanisms above could contribute to certain extent at some time to cloud charging”, but they also comment that some of these mechanisms are unrealistic earlier in the same paragraph. Please rewrite this paragraph so it is not self-contradicting.

Line 79: Provide temperature information if possible for the height range of 4-7 km in the Sokol et al. (2020) study, so as to be consistent with the descriptions of Mattos et al. (2016) earlier in the paragraph.

Line 81 and 558: A high Doppler spectrum width is not a sufficient condition to highlight the coexistence of different hydrometeor types. Such a measurement could hint at strong shear and/or turbulence. See Feist et al. (2019) for details before clarifying the interpretation of results from Sokol et al. (2010).

Lines 107-109: Great to see the authors have considered past work in light of the novelty of using the spectral polarimetry technique in the Ka-band to study storms. However, I would like to see at least a brief literature review that mentions how polarimetric Doppler spectra have been used in other past studies. Were they for non-thunderstorm cases? Used other radar wavelengths? Did they also use the spectra to identify the transition between Rayleigh and Mie scattering regimes? Answering these questions will strengthen this paper on how novel the work is!

Line 111: Add a paragraph to introduce the remaining sections of the manuscript.

Line 116: Refer to Figure A1 immediately after constant azimuth of 282deg is mentioned.

Line 128: Consider adding a short explanation regarding how SLDR is an appropriate proxy for LDR.

Line 133: What is the initialisation time of the IFS forecast(s) used in this case study? Are outputs available hourly? Since the case spans longer than an hour, were outputs valid at different times used? Did the authors choose the closest valid time of the output to that of the radar observation and does the IFS forecast have to precede the radar observation time? Please include these details within the description of the IFS.

Line 136: Include a citation to the vertical coordinate system used by the IFS.

Lines 137-138: I don’t think it is necessary to mention the act of “clicking on the lightning stroke”.

Lines 148-149: It could be worth mentioning that although the bright band cannot be clearly seen in the Ze field, there are some signs of it in the dual-pol fields in Fig 3.

Lines 163-164: If vertical air velocity is obtained from the Doppler velocity bin with the smallest value, why do the colours in Fig. 5b hint at much larger vertical air velocity values than the mean vertical velocities in Fig 5a?

Line 169: Doppler spectra is not broadened purely by turbulence, but also by shear. See Chapter 5 of Doviak and Zrnić (1984) and Feist et al. (2019) for details.

Line 174: The use of “perpendicular distance” is potentially confusing here. Why not introduce this term earlier in line 173 when describing distance of the first cloud from the radar?

Line 187: Justify why integrated radar variables are computed from Level 0 data, rather than directly using Level 1 data.

Line 202: What were the original errors without the extra polarimetric calibration?

Lines 214-215: What is the definition of “North”? Grid North or True North? It is best to clarify this. From experience, there could be differences by up to a few degrees between grid north and true north, leading to significant differences in collocating radar echoes if the definitions are used interchangeably.

Line 227: Without having seen Fig 6 yet, it could be confusing as to what is meant by “rightmost valid bin”. Worth clarifying that this means bin with the highest positive Doppler velocity value.

Lines 251-252: Convert the relation of psi_DP with phi_DP and delta_co into an equation.

Line 254: Clarify what is meant by “same set of particles in all previous ranges”. I think the authors are referring particles within the Rayleigh scattering regime in all previous ranges, rather than the same particles existing in multiple ranges.

Lines 287-297: Consider summarising the details of your experimental configurations in a table.

Lines 290-291: Add relevant citations and justify how these axis ratio and ice fraction values were chosen.

Figure 7: Include details of the two frames of reference in the figure caption. The reader currently has no idea what the xyz and x’y’z’ coordinate systems mean.

Lines 352-354: Consider rephrasing this sentence to “Second extrema are challenging to interpret and measure, especially at high altitudes where the SNR is low”.

Figure 11: The height axis range is inconsistent in (c), compared to (a) and (b).

Line 368: Consider including the Ze field in Fig 11, since “reflectivity weighting” is mentioned in the interpretation of ZDR not showing significant negative values.

Line 380: How was the “lightest 10%” of the particles determined? Consider elaborating.

Line 431: Define the canting angle (gamma), or consider including it in Figure 7.

Line 434: Units of ms-1 are dimensionally inconsistent with the units of wind shear.

Line 434: Take care of the vertical resolution of the ECMWF IFS and thus the underestimation of vertical wind shear. Shear on the finer scales and turbulence associated with smaller eddies, could also enhance canting angle of a particle.

Line 439: Could there also be cloud charges that are not measured by the lightning sensor?

Lines 442-443: Provide the air temperature at 6000m measured by the radiometer, so readers could understand how cold the environment is with supercooled liquid water.

Line 447: What is the size of “small supercooled liquid water droplets”?

Line 644: Include possible radar scan strategies as ideas for future studies of similar convective storms involving the cloud radar.

Figure A1: Add details to the figure caption on the origin of these plan view radar images. Are these derived using a radar composite of operational radars? What defines the different classes of the colour bar? Is it precipitation rate or reflectivity?

Technical corrections:

Line 101: Add brackets around the citation.

Figure 2: Make the black rain rate lines thicker/darker.

Line 171: Citation should not be “GmbH”.

Line 606: Add the symbol for spectral differential phase.

References

Doviak, R. J. and Zrnić, D. S.: Doppler Radar and Weather Observations, Academic press, London, UK, 1984.

Feist, M. M., Westbrook, C. D., Clark, P.A., Stein, T. H. M., Lean, H. W., and Stirling, A.J.: Statistics of convective cloud turbulence from a comprehensive turbulence retrieval method for radar observations, Quarterly Journal of the Royal Meteorological Society: A journal of the atmospheric sciences, applied meteorology and physical oceanography, 145, 727–744, 2019.

Lund, N. R., MacGorman, D. R., Schuur, T. J., Biggerstaff, M. I., and Rust, W. D.: Relationships between lightning location and polarimetric radar signatures in a small mesoscale convective system, Monthly Weather Review, 137, 4151–4170, 2009.

Mattos, E. V., Machado, L. A. T., Williams, E. R., and Albrecht, R. I.: Polarimetric radar characteristics of storms with and without lightning activity, Journal of Geophysical Research: Atmospheres, 121, 14–201, 2016.

Sokol, Z., Minářová, J., and Fišer, O.: Hydrometeor distribution and linear depolarization ratio in thunderstorms, Remote Sensing, 12, 2144, 2020.