Referee comments

October 14, 2024

1 Referee 1

1.1 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.

The authors express their gratitude to Referee 1 for the thorough and meticulous review, which provided many valuable suggestions to enhance and improve the article. Referee 1 can find our responses in blue (italic) below.

1.2 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.
   The ZDR values are not corrected for differential attenuation. The differential attenuation relating to the presence of oblate raindrops leads to a decrease of the measured ZDR values and can even lead to negative measured ZDR values, which is an issue, particularly if we want to detect the presence of conical graupels. Therefore, we added the following small paragraph for more information:
   Lines 572-581: "The presence of liquid water introduces an additional challenge, namely differential attenuation, which influences the sZ_{DR} values. While no direct measurements of the Rain Drop Size Distribution (RDSD) are available, a simulation can provide an estimate of the differential attenuation. For this purpose, the convective RDSD typical of the Netherlands, based on disdrometer data from Gatidis et al. 2024, is considered. The corresponding intercept parameter

N_w equals 1300 mm^{-1} m^{-3} and the mass-weighted mean diameter D_m is 2.2 mm. The shape parameter, derived using the μ-λ relationship from the same study, along with the shape-size relationship from Unal and van den Brule 2024, is applied. Consequently, in rainfall, the differential reflectivity is estimated at 0.15 dB, and the one-way differential attenuation at 0.06 dB km^{-1}. Except near the edges of the precipitation, Z_{DR} measurements show an increase from 0 dB to 0.2 dB as height decreases from 3000 m to 2200 m. Thus, the two-way differential attenuation contribution is expected to be low, at less than 0.12 dB, and does not significantly affect the interpretation of the results discussed.

2. There is a good variety of past literature included within the introduction section. The authors should consider an additional discussion section to the 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?
ZDR values are not directly comparable because the frequency differs (Lund, S-band). Previous studies have not investigated the time that vertical alignment of ice occurred. Lund 2009 analysed ZDR at certain instants representative of particular stages of the thunderstorm, Sokol 2020 matched high LDR with a period with intense lightning, Mattos 2016 looked at mean profiles of ZDR / KDP at times with different lightning intensities. We have not seen any studies that analysed the vertical alignment of ice at the second level like we do.

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.
The authors consider important to discuss all the possible causes of particle alignment in the first study case (thunderstorm cloud 1). However, the paragraphs between former lines 393 and 437 (45 lines) have been shortened to 32 lines.

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.
Added in Lines 4-5: "The time and location of individual lightning strikes are determined using the BLIDS system, operated by SIEMENS, which is based on the Time-Of-Arrival principle."

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

Line 28: Include a diagram to supplement the explanations of the "numerous charging mechanisms".
The authors are generally in favor of illustration or diagram. However, we think that there is already a good summary of the three major categories of charging mechanisms, with references.

Lines 32-36: Some explanation of the "inconsistencies" and "quantitatively unrealistic ineffective" mechanisms of charging would be appreciated here.
Expanations are added in Lines 34-35 and Lines 37-38.
Lines 34-35: "However, numerous investigators such as Chiu and Klett (1976) have found inconsistencies between this mechanism and observations, such as opposite cloud polarity if the cloud forms close to the ground."
Lines 37-38: "However, many studies have shown that these mechanisms are quantitatively unrealistic or ineffective since it is only effective when the electric field is below typical values for the initiation of

lightning in thunderstorms (Pruppacher and Klett, 1980; Wang, 2013)."

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".
Lines 46-47: "where the temperature is low" is changed to "where temperature is below T_R".

Line 46: What are the terminal velocities of the ice crystals that are "thrown upwards" in updrafts?
Lines 47-49. The sentence is revised to:
"The negatively charged graupel will fall at the periphery of the column where the updraft is weak, while the positively charged ice crystals with negligible fall velocity will be thrown upwards."

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.
Lines 50-53. The sentence is revised to:
"Although non-inductive charging due to the collision of graupel and ice crystals best explains tripolar cloud structure, it should be noted that all charging mechanisms above could contribute to certain extent at some time to cloud charging even though certain mechanisms alone would produce inadequate or reversed charges (Pruppacher and Klett, 1980)."

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.
The temperature information is added.
Lines 86-88: "With Ka-band cloud radar, Sokol et al. (2020) identified a mixture of hydrometeors at an elevation of 4–7 km (from 6.6 to 27°C) with a predominance of ice and snow particles and graupel based on the terminal velocities of different hydrometeors."

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 88-89.The sentence is revised to:
"The coexistence of different types of hydrometeors is supported by the measured high Doppler spectrum width."

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!
The authors believe this is not the ideal section for a brief literature review on the use of polarimetric Doppler spectra. Our focus remains on thunderstorms (the medium) and polarimetric Doppler spectra (the methodology). Nonetheless, we have improved this section and included an additional reference.

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

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

Line 128: Consider adding a short explanation regarding how SLDR is an appropriate proxy for LDR.
SLDR explanation added.
Lines 148-149: "Compared to L_{DR}, SL_{DR} in the STSR mode loses the direct mean canting angle information due to the inability to acquire cross-polar measurements, but retains information on the

variance of the canting angles and axis ratios."
This statement is also added in the conclusion.

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.
The outputs are available hourly. The output at 16h for 1600-1700 and the output at 17h for 1700-1800 are used. Details are added in lines 180-181 and lines 586-587.
Lines 180-181: "The mean vertical velocity in Fig. 5(a) eliminates from the measured mean Doppler velocity the contribution of horizontal wind in the same hour obtained from ECMWF model forecast initialised at 17 June 2021 12:00 UTC."
Lines 586-587: "The vertical velocity is estimated by assuming uniform horizontal wind predicted by the ECMWF model in the same hour."

Line 136: Include a citation to the vertical coordinate system used by the IFS.
Added.
Lines 155-156: "The model uses an eta-coordinate system, with vertical resolution of the first 10000 m ranging from around 20 m near the surface to around 300 m at the top."

Lines 137-138: I don't think it is necessary to mention the act of "clicking on the lightning stroke".
This is removed.

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 169-170 are added:"However, after 17:15, a brief indication of a melting layer can be observed using the radar variables, Z_{DR}, SL_{DR} and ρ_{hv}."

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?
The authors think that this visual effect is due to the different velocity scaling in Fig. 5a and Fig. 5b.

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.
Lines 188-190: "This could mean that there is a wide variety of particles within the radar resolution volume or the Doppler spectrum is broadened by turbulence or shear (Doviak and Zrnic, 2006; Feist et al. 2019)."

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?
The text is revised.
Lines 194-198: "For the first cloud, lightning occurred near the line of sight at more than 10 km away from the radar. For the second cloud, lightning occurred at the ranges 3 to 8 km with a cross-range varying from 1 to 10 km. The third cloud only produced two lightning strikes after passing through the line of sight of the radar. The fourth cloud produced a large number of lightning strikes near the radar line of sight from less than 1 km to more than 15 km along-range."

Line 187: Justify why integrated radar variables are computed from Level 0 data, rather than directly using Level 1 data.
The paragraph is rewritten.
Lines 207-210: "This research utilized spectral polarimetric radar variables derived directly from the Level 0 data. Consequently, the majority of the integrated radar variables were also computed from Level 0 data. This approach facilitates consistency checks between Level 0 and Level 1 data, enables spectral domain filtering when necessary, and allows for the dealiasing of Doppler spectra prior to the

calculation of Doppler moments."

Line 202: What were the original errors without the extra polarimetric calibration?
The original errors are now indicated.
Lines 224-225: "This procedure resulted in reducing the expected error associated with Z_{DR} and Ψ_{DP} from 0.18 dB to 0.05 dB and from 1.6° to 0.6° respectively."

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.
True North. That is explicitly mentioned in the text.
Lines 237-238: "D is the wind direction and ϕ is the azimuth angle of the radar beam, both being relative to True North."

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.
The authors leave the sentence unchanged. This Doppler bin does not always correspond to the highest positive Doppler velocity value. It can correspond as well to a negative Doppler velocity.

Lines 251-252: Convert the relation of psi_DP with phi_DP and delta_co into an equation.
Done in Line 277.

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.
The sentence was improved.
Lines 278-279: "In the Rayleigh scattering regime, where δ_{co} is zero, the spectral differential phase shift at a fixed range remains constant because the electromagnetic wave at both polarizations has passed through the same particles in all preceding ranges."

Lines 287-297: Consider summarising the details of your experimental configurations in a table.
The authors think that the simulation configurations are clearly mentioned in the text and the figure captions.

Lines 290-291: Add relevant citations and justify how these axis ratio and ice fraction values were chosen.
Citation added in the second sentence of the paragraph.
Lines 313-315: "The axis ratio and ice fraction of the particles in the simulation experiments were chosen according to the data given in Spek and al. (2008)."

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.
The caption is rephrased with details of the two frames:
"Definition of Euler angles α and β. The xyz coordinate frame has the z axis aligned with the radar's zenith direction. The rotated frame is denoted as $x'y'z'$, corresponding to the particle's orientation. Starting from the xyz frame, a rotation by angle α around the z axis results in the intermediate frame $x'y_1z$. This is followed by a rotation by angle β around the x' axis to achieve the final $x'y'z'$ frame."

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".
Thank you for rephrasing the sentence. Done in Lines 382-383.

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

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.

The reflectivity is plotted in Figure 2 and for further interpretation of ZDR, it is necessary to investigate sZDR (spectral ZDR). Therefore the authors think that it is not necessary to include an extra figure related to the reflectivity profiles.

We rephrased the first paragraph (Lines 396-402) for improved clarity:

"From Fig. 11(a) and (b), intriguing polarimetric signatures can be observed within the cloud. Figure 11(a) illustrates that Z_{DR} values are near zero with minimal variation. Conversely, Fig. 11(b) reveals a cluster of negative K_{DP} values between 7600 m and 9300 m, suggesting the alignment of non-spherical small ice particles. If these small ice particles are present in sufficient concentration, K_{DP} would become negative. The large ice particles, on the other hand, are expected to be slightly non-spherical, which leads to a small contribution to K_{DP}, and may not align with an electric field unless it is sufficiently strong. Because Z_{DR} is reflectivity-weighted, large ice particles significantly influence Z_{DR}, which likely explains why Z_{DR} does not exhibit significant negative values."

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

We replaced "lightest" by "smallest" in the text and Fig. 13.

Lines 413-415: Figure 13 shows the mean sZ_{DR} of the smallest 10% of the particles in each radar resolution volume at the three time instants. This is achieved by averaging sZ_{DR} over the rightmost 10% of the Doppler bins.

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

Eq. 13: γ is replaced by β, which is defined in Fig.7.

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

This is corrected.

Lines 454-455: "Using the vertical shear $s = \frac{dv_H}{dz} = 4\ m\ s^{-1}\ km^{-1} = 0.004\ s^{-1}$ and terminal velocity of 2 $m\ s^{-1}$, the canting angle is estimated at 0.05°, which is negligible."

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.

We added the following the sentences:

Lines 455-457: "Even considering underestimation due to model resolution, achieving significant canting would require a much higher shear of 4.9 s^{-1}, making wind shear an unlikely cause of the observed negative sZ_{DR} values."

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

The lightning sensor only record lightning strikes, not cloud charges. We already mentioned the case that there could be charges (i.e. electric field) that may not be strong enough to trigger lightning, and the case that some lightning strikes may not be measured by the sensor, which cover the presence of charges that does not give measurable as well as the limitation of the 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.

The air temperature at 6000 m is added.

Lines 463-465:"Another interesting feature observed in this cloud is the possible presence of supercooled liquid water. From 16:20:21 to 16:21:15 UTC, spectrograms of reflectivity show a separate mode of particles on the right side of the spectrum at around 6000 m (see Fig. 16(a)), where air temperature measured by the microwave radiometer is around −12.5 °C."

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

The authors removed "small".

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

The authors already included one cloud radar scan strategy in the conclusion.

Lines 683-684 : "A more appropriate radar measurement mode for studying thunderstorm clouds would be azimuth scan (PPI) with the constant elevation of 45°."

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?

Unfortunately, no information about this is given on the webpage `https://meteologix.com/nl/stormradar/utrecht/20210618-1650z.html`*. However, by looking through other products provided on the website, we find that the Radar SD Netherlands (2.5km) looks quite similar* `https://meteologix.com/nl/radar-hd/utrecht/20210618-1650z.html`*. Following this, marginal refers to 0.1-0.4 mm/h, light is 0.4-2.3 mm/h, moderate is 2.3-12 mm/h, heavy is 12-44 mm/h, very heavy is 44-146 mm/h and extreme/hail is 146-491 mm/h. However, there is no explicit mentioning that the source of the radar images we used is Radar SD Netherlands (2.5km), and there is also no information about the origin of the data of Radar SD Netherlands (2.5km) (we suspect it is a composite of the radars at Den Helder and Herwijnen* `https://meteologix.com/nl/radar-hd/utrecht/20210618-1649z_srcnl-zsweep-1km-hw.html`*. There is a similar dataset in KNMI* `https://dataplatform.knmi.nl/dataset/radar-reflectivity-composites-2-0`*, but we did not make the comparison). We chose the currently used radar images over the Radar SD Netherlands ones simply because they show the locations of lightning strikes on the same images.*
Summarizing we indicate now that the different classes of the colour bar relate to precipitation rate.

1.3 Technical corrections

Line 101: Add brackets around the citation. *Brackets added.*

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

Line 171: Citation should not be "GmbH". *Citation modified.*

Line 606: Add the symbol for spectral differential phase. *Symbol added.*