Referee comments

October 17, 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.*

2 Referee 2

The authors study two lightning cases (the first and fourth cloud of a thunderstorm event) using a dual frequency radar (with unfortunately only one usable frequency of 35 GHz). The authors combine polarimetry and Doppler spectra to present an interesting and novel study which looks at the presence of different hydrometeor types and their alignment in thunderstorm clouds around the times when lightning is occurring. Other processes such as turbulence are also considered.

Although the manuscript is interesting and novel, I think it will benefit from the edits described in this document. In particular, I believe some of the analysis is based on scattering simulations which are not representative of the problem. I would like to see improvements related to that, or at least some clarification on why the authors believe conclusions can be drawn from these scattering simulations. Because of this, my recommendation is a major revision.

The authors express their gratitude to Referee 2 for the thorough and meticulous review, which provided many valuable suggestions to enhance and improve the article. Referee 2 can find our responses in blue (italic) below.
Regarding the scattering simulation, for this initial study of thunderclouds based on radar observations at mm-wavelengths and spectral polarimetry, the authors aimed to use existing scattering codes or databases. The use of the T-matrix code was intended to aid in interpreting the spectral polarimetric measurements, and we are aware of its limitations. Specifically, we sought to gain a preliminary understanding of the variability and sensitivity of sZ_{DR} and $s\delta_{co}$ in relation to axis ratio, ice fraction, and canting angle.

2.1 General comments

1. I feel like the section/subsection organisation in section 5 could be better. The current organisation is:

   5 Case analysis

   5.1 First cloud

   5.1.1 Alignment of particles

   5.1.2 Supercooled liquid water

   5.2 Fourth cloud

   5.2.1 Evidence of conical graupel

   5.2.2 Alignment of particles

   5.2.3 Strong updraft and turbulence

   5.2.4 Possibility of chains

   In terms of the subsections (5.1 and 5.2) I find it slightly confusing to refer to the first and fourth cloud. Without reading the paper in depth, one might expect to find sections for the second and third clouds too. Perhaps it's worth changing to something like cloud A and cloud B rather than first cloud and fourth cloud, and then just pointing out which clouds they are.

   Considering the figures showing the four thunderstorm clouds (Figs 2-5), which relate to the weather radar images in Appendix A, the authors would like to keep the terminology first cloud and fourth cloud for clarity.

   Moreover, I think it might be better if the subsubsections were consistent in subsections 5.1 and 5.2. For example, something along the lines of:

   5 Case analysis

   5.1 Cloud A

   5.1.1 Alignment of particles

   5.1.2 Interesting microphysical properties

   5.2 Cloud B

   5.2.1 Alignment of particles

   5.2.2 Interesting microphysical properties

   The authors agree with the proposed restructure of the subsections, which are now:

   5 Case analysis

   5.1 First cloud

   5.1.1 Alignment of particles

   5.1.2 Interesting microphysical properties

   5.1.2.1 Supercooled liquid water

   5.2 Fourth cloud

   5.2.1 Alignment of particles

   5.2.2 Interesting microphysical properties

   5.2.2.1 Evidence of conical graupel

   5.2.2.2 Strong updraft and turbulence

   5.2.2.3 Possibility of chains

2. In various places you refer to "Mie scattering" and the "Mie scattering regime". I understand that you are talking about non-Rayleigh scattering, and to me describing this as the "Mie scattering regime" is ok (although you could consider "resonance regime"). Referring to "Mie scattering" on its own is slightly questionable to me, as Mie theory is applicable only to spheres. I recommend that in places where you say "Mie scattering" you could change to resonance or non-Rayleigh scattering (unless you are talking about spheres in any of these places…).

   The authors kept "Mie scattering regime" and replaced "Mie scattering" by "non-Rayleigh scattering" or "resonance" in function of the context.

2.2 Specific comments

Line 11: these thunderstorm clouds, or all thunderstorm clouds?

The sentence was rewritten as: "From the results, there is a high chance that supercooled liquid water and conical graupel are present in the investigated thunderstorm clouds."

Consider adding Saunders and Wahab reference for lab measurements of chains forming due to electric fields at -12C and -8C: Saunders, C. P. R. and N. M. A. Wahab, 1975: The influence of electric fields on the aggregation of ice crystals. J. Meteorol. Soc. Jpn., 53, 121–126.

Also Stith et al 2002 for chains observed in clouds: Stith, J. L., J. E. Dye, A. Bansemer, A. J. Heymsfield, C. A. Grainger, W. A. Petersen, and R. Cifelli, 2002: Microphysical Observations of Tropical Clouds. J. Appl. Meteor. Climatol., 41, 97–117, https://doi.org/10.1175/1520-0450(2002)041<0097:MOOTC>2.0.CO;2.
The authors would like to thank Referee 2 for providing complementary references. Lines 65-70 are rewritten to include the results of these studies.

54/55: Riming doesn't necessarily form large, dense, near spherical particles. For example, if you have a single dendrite monomer which experiences light riming, it won't become much bigger or "near spherical". Perhaps you could rephrase this in a less extreme way, such as "…generally resulting in increased particle size, density and sphericity".
Rephrased. Lines 58-59.

60 and 63: You refer to aggregates as spherical. The appropriate shape approximation of aggregates is something that is still debated (e.g. see the introduction of this paper: https://doi.org/10.5194/amt-14-6851-2021), but usually a spheroidal or ellipsoidal shape would be assumed rather than a sphere. Anyway, in this instance it is probably sufficient to make very slight edits like:
- On line 60 you could maybe just make it more general, e.g. "…form larger particles that tend to be more spherical in shape". On line 63 you could just remove the word "spherical".
Thank you for the proposed edits.
Lines 64-68: "Aggregation occurs when ice crystals collide with each other and form larger particles that tend to be more spherical in shape. Various lab measurements have demonstrated that when an electric field of more than around 50 kV m^{-1} is present, aggregation of ice crystals may be enhanced due to attractive electrical forces induced between neighbouring conducting crystals, forming elongated chains rather than almost spherical clusters (Conolly et al. 2005)."

81: ", which also implies the existence of collisions of hydrometeors." I'm not sure what you mean here. Do you mean that collisions are more likely because there are mixtures of particles with different velocities?
We suppressed this part of the sentence.

118: Is the 94 GHz completely un-usable? The combination of 35 and 94 would be beneficial for determination of particle size. In fact, unless you are referring to something else, this is precisely thanks to "complications due to Mie scattering". Can you not just correct for attenuation; I think you can correct for liquid water attenuation because you know the LWP?
The authors agree with Referee 2 and may revisit the combination of 35 and 94 GHz using Dual-Wavelength Ratio. The main issue encountered was a significant decrease of sensitivity for the Doppler spectra measurements at 94 GHz at large heights.
Lines 135-137: "In this study of thunderstorm clouds, only the 35 GHz data is used since there are numerous issues associated with the 94 GHz data including significant attenuation, less sensitivity at large heights, Doppler aliasing and complications due to resonance."

112 (Section 2): In Figure 2 you have numbered clouds to identify which ones you are referring to. In the other figures (Figs. 3-5) it would be useful if you highlighted the regions of interest somehow, e.g. by drawing lines or a box around the specific times/heights you are looking at.
The authors think that based on Fig. 2, the clouds 1, 2, 3 and 4 can be clearly identified in Figs. 3-5.

150: does the high SLDR occur slightly later than the negative ZDR? Hard to tell on this axis.
This case was discarded and not studied in details. The authors focused on selecting appropriate times with no precipitation reaching the ground to avoid misinterpretation.

240-244: it would be helpful to include approximate values such as Doppler velocities close to 0m/s

rather than just referring to the right and left part of the spectrum.
The authors did not change the formulation. The Doppler velocity measurement contains two contributions, one caused by the fall velocities and one due to the radial wind. Therefore, it can occur that Doppler velocities related to the smallest particles have significant positive values, while the Doppler velocities related to the largest particles have values near 0 m/s.

245/470: Your references Lu et al 2016 a and b are the same paper
Did you extract the conical graupel from the database, or does it have the information about negative ZDR and conical graupel in the reference you give?
Duplicated reference is fixed. The information about negative ZDR and conical graupel is not given in the paper Lu et al. 2016. Therefore, we obtained Zdr of conical graupel from the database. This is made clear in the following lines:
Lines 61-63: "Scattering simulations carried out by Oue et al. (2015) and data from the scattering database created by Lu et al. (2016) indicate that conical graupel can produce negative differential reflectivity (Z_{DR}) values at X-, Ka- and W-band."
Lines 270-271: "On the other hand, based on the database described by Lu et al. (2016), negative Z_{DR} for large particles only may indicate the presence of conical graupel."

Maybe the Aydin and Seliga (1984) paper could be useful for you: https://journals.ametsoc.org/view/journals/atsc/41/11/1520-0469_1984_041_1887_rpbpoc_2_0_co_2.xml
Thank you for the reference. The paper provides zdr of conical graupel at 10 cm wavelength.

255: could you add some references for "This part of the spectrum is often referred to as the Rayleigh plateau"?
A reference was added.

285: as you are saying m_eff can be determined, would it be better for the equation to be m_eff=sqrt(eps_eff)?
The equation was modified to better relate to the text.

291: "different types of particles" is a bit vague, please elaborate
The text was modified.
Lines 315-317: "In the first experiment, the axis ratio of spheroids with a zero mean canting angle was varied from 0.1 to 1.2. This range encompasses the axis ratios of plates, dendrites, aggregates, and graupel. The ice fraction was held constant at 0.6, representing the average ice fraction for the aforementioned ice particles."

295: Why did you choose these particular values of axis ratio and ice fraction? E.g. aggregates are often considered to have aspect ratio 0.6.
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)."
Further, the authors replaced "aggregates" by "slightly oblate aggregates" in Line 321 and caption of Figure 10.

313: Can you provide an explanation of how a horizontally aligned prolate spheroidal model is different to an oblate spheroid?
Thank you for spotting this inconsistency. In section 4, "horizontally aligned spheroids" has been replaced by "spheroids with zero mean canting angle" in the text, the figures caption and Table 2.
We added the following sentence in the subsection 4.3 (canting angles):
Lines 358-360: "A zero mean canting angle corresponds to oblate spheroids being horizontally aligned and prolate spheroids being vertically aligned. To represent prolate particles as horizontally aligned, they are modeled with a mean canting angle of 90 degrees."

319 (and elsewhere): Figure caption – change "scatterers" to spheroids
Done.

320: Figure 9 and Figure 10 - You don't seem to refer to the reflectivity plots here, either include a reference to them in the discussion, or remove them. This could even be something simple like pointing out that the Mie minimums can be seen in the reflectivity plots.
There is now reference to Figures 9-10 first row (reflectivity) in Lines 347 and 357.

325: You are talking about ice fractions of 0.8 and 1, right? Thus, I think "For a radius of larger than 2.5 mm representing large aggregates such as graupel" is a bit misleading, as presumably such large ice fractions would only represent graupel / some heavily rimed particle and not any other unrimed aggregate type? If you agree, you could just rephrase e.g. "Particles of this size and ice fraction could represent graupel…".
Rephrased.
Lines 350-353: "When ice fraction is large (0.8 and 1), the sign of Z_{DR} *flips soon after reaching the first extremum, and the trend is rather unpredictable. For particles of this ice fraction with radius larger than 2.5 mm, which could represent graupel, significant negative (positive) values could be obtained, which increases the interpretation challenge."*

366: why small but not large? Do you just mean that large particles usually aren't oriented by the field? Please elaborate.
We rephrased the paragraph to improve its clarity.
Lines 396-402 : "From Fig. 11(a) and (b), intriguing polarimetric signatures can be observed within the cloud. Fig. 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."*

368: What is the temperature here, can you get any information on the particles from that? Perhaps you only have temperature measurements in the vertical.
The temperature from 7600 to 9300 m measured by the microwave radiometer is around -24 to -41 °C. However, it is challenging to get information on the particle types from the temperature since particles formed at certain temperature ranges are moved around by updrafts and downdrafts, and the temperature is measured vertically, so it is not the actual temperature in the measured cloud at 45 deg. elevation.

381: this is interesting, how did you obtain the "lightest 10%"?
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."*

387: Be consistent with units, either m or km
Corrected.
Lines 420-422: "Negative K_{DP} *appears at a distance of 7600 m to 9300 m away from the radar, but the lightning strikes occurred at least 13000 m away from the radar."*

390: You mention Fig. 16 here but I think you have not yet discussed Figs 14 or 15, maybe the figures should be reordered.
Fig. 16 is dedicated to the section 5.1.2.1 Supercooled liquid water. Therefore, we must have it after Figs. 14-15.

406/408: make the vertical wind shear units the same.
Done.
Lines 436-439: "For a 10 $m\ s^{-1}$ *spectrum width and* $V_t = 2\ m\ s^{-1}$ *corresponding to the upper bound of the terminal velocity of plates (Spek et al. 2008), a shear of approximately 25000* $m\ s^{-1}\ km^{-1}$ *would be required to flip the spectrum, much larger than the 4* $m\ s^{-1}\ km^{-1}$ *observed from 7500 m to 10000*

m in ECMWF data shown in Fig. 15(c)."

417: here and in the rest of the subsubsection, you refer to "a lightning" or "lightnings". I believe these should be "a lightning stroke" and "lightning strokes".
"stroke" was changed into " lightning strike".

450: What frequency? The caption of Fig 18 says 94 GHz but line 130 says LWP comes from 31.4GHz?
Thank you for noticing the discrepancy. That is now corrected in the caption of Fig. 18 and the text (Line 150). We used the embedded 89 GHz passive channel.

468: Can you explain what you mean by "Since small particles are more easily aligned by an electric field and they are not aligned in this case,"? Do you just mean that the part of the spectrum corresponding to small particles has sZDR close to zero, implying that the negative sZDR corresponding to the larger particles is not caused by vertical alignment?
We rephrased the sentence:
Lines 542-546: "When negative sZ_{DR} appears on the left part of the spectrum, the sZ_{DR} on the right part of the spectrum is close to zero. The observed negative sZ_{DR} values on the left part of the spectrum may suggest the presence of conical graupel (Lu et al. 2016), as smaller particles, which are typically more easily aligned by an electric field, do not appear to be aligned in this case, as indicated by the absence of slightly negative sZ_{DR} values."

There is also lower correlation coefficient in this region, implying mixture of particle shapes.
That is true. We have therefore mentioned this observation.

477: You hypothesize conical graupel, but you then model conical graupel with a spheroid. Please provide an argument for why you think you can get any information about conicality if you are using a spheroid. Perhaps you could just refer to results/simulations in the literature for conical graupel.
To strengthen the discussion related to the simulation, we analysed as well Lu et al. 2016 database results for conical graupels. We used 8 points corresponding to the radius, 0.2 mm, 0.3 mm, 0.4 mm, 0.5 mm, 1 mm, 1.5 mm, 2 mm and 2.5 mm and an approximate density corresponding to ice fraction 0.6. For the cone angles we selected 40 and 50 deg. In that case, simulations (T matrix prolate particles), (conical graupels Lu database) and observations provide comparable results in terms of trends for Z_{DR} and δ_{co}. Fig. 22 includes now Lu results about conical graupels.

486: supported by low correlation coefficient
added in the text.
Lines 574-576: "This suggests that the two peaks in spectral reflectivity represent two particle populations, the left peak corresponding to conical graupel and the right peak relating to nearly spherical smaller ice particles. This hypothesis is supported by a lower co-polar correlation coefficient."

Figs 22 and 28: It's quite hard to compare the measurements and simulations here due to different y-axis scales and different variables on x-axis. Consider at least making the scale of the y-axis the same for observations and simulations. Can you calculate the fall speeds of your model particles and plot that on x-axis? Also, are your labels and units accurate? E.g. are you plotting reflectivity [dBZ] or spectral reflectivity [dBsZ]?
Fig. 22: we reduced the y-scale of the modelled reflectivity and add different axis ratio for the T matrix simulation. The goal here is to obtain comparable trends between simulations and the three observations to identify the possibility of conical graupels. The analysis emphasizes spectral polarimetric observations, where polarimetric measurements provide greater insight compared to Doppler velocity data. The Doppler velocity does not express yet the fall velocity of the particles.
*The linear spectral reflectivity (sZ) is expressed in mm^6m^{-3}. We consider 10*log10(sZ), which is then expressed in dBZ.*
Fig. 28: The simulations are removed.

578: I think you should rephrase this or expand it a bit. At the minute it might be misinterpreted as if chains can only be found if the temperature is below -40. I think what you are trying to say is that

although aggregation is usually associated with slightly warmer temperatures (maybe above -25), the presence of electric fields allows chains to form in colder temperature regions, which has been observed at temperatures below -40 (Stith et al 2002; Connolly et al 2005; Gayet et al 2012)? Maybe this could be mentioned around line 65 when you are discussing chains in cold temperatures.
Rephrased.

581 onwards: Chains are modelled as prolate particles with axis ratio 7 and ice fraction 0.3. Can you put a reference here as to why you think 0.3 is suitable.
The simulation is removed.

At 17:22, From high ZDR and positive ZDR spectrum you hypothesize that there may be chain-like aggregates. The low correlation coefficient suggests a mixture of particles. If small particles and chains co-exist here, could you comment on why sZDR in Fig 28e would be positive. Do you think if there was a high E-field forming the chains, the small particles would be oriented vertically by the field giving negative sZDR?
Based on this comment and comment line 578, the text is rephrased:
Lines 629-634 : "The differential reflectivity of the Rayleigh plateau (Doppler velocity > 5 m s^{-1}) is around 0.2 dB, and the entire Z_{DR} spectrum is positive. One hypothesis is that the large particles with positive sZ_{DR} are chain-like aggregates that formed earlier under a strong E-field. The lower copolar correlation coefficient in Fig. 26(g) suggests a mixture of particles (small ice crystals and chains), but currently, there is no high E-field to vertically align the small particles. At that moment, the temperature above 9600 m is lower than -40 °C, and it is indeed possible for chains to be present at such temperatures, according to Conolly et al. (2005)."

You say that you should use axis ratio much greater than 1, but that the maximum you can use is 7 otherwise the code cannot converge. Can you provide more specific details on sizes and axis ratios here please? For example, what are the maximum sizes that have been observed before? You have an example in Fig 1 which is 721 microns. You also say that the individual monomers could be 15-20 microns. Are you then calculating the axis ratio as, for example, 721/15=48? I don't think you can get much information about a chain with axis ratio 48 by modelling it as a prolate particle with axis ratio 7, which you also point out in the next paragraph. You show simulations in Fig. 28 for particles with a much larger maximum dimension of up to 3mm, and then point out that chains of this size have not been observed. Can the code converge if you use smaller sizes less than 1mm with a larger axis ratio?
The authors agree that simulation result of prolate particle with axis ratio 7 does not give much information about a chain with axis ratio 48. However, the simulation does not converge even if we only use sizes less than 1 mm. (For axis ratio 15, we can reach at most 670 microns, but still it is far from axis ratio 48) So, here, we encounter a clear limitation of the Tmatrix method using spheroids. It cannot approximately simulate chains. Therefore, the simulation is removed.

I personally think the section is not very useful because the scattering simulations kind of go nowhere, then you say an alternative would be to look at data which isn't possible. I think you should either repeat simulations using more representative particles, or just remove the section and add a line saying that the high ZDR could be caused by chains.
We focus on the observation. The simulation is removed and the section is shortened.

640: Is there any scope to track thunderstorm evolution using steerable radars, like they did with weather radars during the WOEST campaign in the UK?
Delft University of Technology has definitively interest in this topic. There is work in progress regarding tracking using steerable radars (PhD position) and development of a fast-scanning phased-array radar at Ku-band with polarization diversity (PHARA).

2.3 Technical corrections

Line 2: a cloud radar. *Corrected.*

Line 3: a thunderstorm case. *Corrected.*

Line 4/5: maybe "in the millimeter band", or something similar. . . kind of sounds like its exactly 1mm now. *Corrected.*

Line 5: studies of thunderstorm clouds. *Corrected.*

Line 18: phenomenon. *Corrected.*

Line 38: pellets. *Corrected.*

Line 70: when the ice particle number, High concentrations. *Both corrected.*

Line 73: Evidence. *Corrected.*

Line 101: need brackets around citations. *Brackets added.*

Around line 276: sometimes use 1 sometimes one. *Corrected. We chose "1"*

Line 151: associated with. *Corrected.*

Line 216: dealias. *Corrected.*

Line 232: change would align to can align? *Corrected.*

Line 240: spectra. *Corrected.*

329 Fig 9 caption: change (a-c) to Panels (a-c). Same for (d-f). *Modified.*

365: is are close to zero and do not show much variations. *Corrected.*

473: Scattering simulations are (or a scattering simulation is). *Corrected.*

578: In the case being studied, the temperature. *Corrected.*

580: Scattering simulations are. *Corrected.*

592: in Fig 28c or (Fig 28c). *Corrected.*