Referee comments

December 16, 2024

The authors study 2 clouds within a thunderstorm case, using a 35 GHz cloud radar with 45 degree elevation. They use spectral polarimetry to study particle properties and other processes. Valuable improvements have been made to the original manuscript.

I recommend the following **minor corrections** before publication.

The authors express their gratitude to the Referee for this second thorough review. The Referee can find our responses in blue (italic) below.

I'm not sure if you mention the radar you are using, is it the CLARA radar?
The radar name is now mentioned.
Lines 132-136: "The cloud radar used in this study is a dual-frequency scanning polarimetric frequency-modulated continuous-wave (FMCW) radar produced by Radiometer Physics GmbH located at Cabauw, the Netherlands (51.968° N 4.929° E). It is named CLoud Atmospheric RAdar (CLARA) and operates at 35 GHz (Ka-band) and 94 GHz (W-band) in Simultaneous Transmission Simultaneous Reception (STSR) mode and measures at a constant elevation of 45° and constant azimuth of 282° (see Fig. A1) at some selected periods."

Abstract line 2: change "thunderstorm clouds" to "them"
Done.

Sub-sections: I appreciate you following my suggestion to re-structure, however I believe the AMT guidelines state that three sectioning levels are allowed, i.e. 5, 5.1,5.1.1, thus the new sectioning you have chosen might not be allowed.
Thank you for this information. For the moment, we leave the sub-sections as they are.

Line 170: link to the relevant figures (Figs. 3 and 4?)
Done.
Lines 175-176: "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} in Fig. 3(a) and Fig. 4.

Line 251: Can you comment on the accuracy of the vertical air velocity estimation?
Lines 257-260: "Then, the vertical air velocity w can be estimated by solving Eq. (6) with $V_t = 0$ and v_H and D estimated from the ECMWF model data. The latter estimation may influence the accuracy of vertical air velocity measurements. The ECMWF model supplies an average horizontal wind profile, whereas the cloud radar observations are associated with thunderstorm clouds, where local dynamic variability is anticipated."

Line 394: "The centre of the cloud that contained lightning activities was more than 10 km away from the radar, thus the radar could only see the edge of the cloud." Could you elaborate on this slightly, e.g. What is the maximum distance that can be seen? Are you saying it's 10km, or is it the value of 14969.9m given in Table 1?
The sentence is removed and a better introduction of the first cloud is provided.
Lines 406-408: "The weather radar images presented in Fig. A1 indicate heavy precipitation occurring at a distance of 10–15 km from the radar between 16:20 and 16:25. Correspondingly, owing to its 45°

elevation angle, the cloud radar observes the thundercloud at altitudes between 6 and 10 km, and not the precipitation below."

Line 398: Also provide the time here (I think you include it later - 16:20:11 to 16:21:37 UTC?)
Done.
Lines 412-413: "Conversely, Fig. 11(b) reveals a cluster of negative K_{DP} values between 7600 m and 9300 m and within the time period 16:20:11 to 16:21:37 UTC, suggesting the alignment of non-spherical small ice particles.

Line 403: "mainly shows downdrafts from 16:15-16:18" I think you mean that within the time period examined, these are the times when downdrafts occur. However, saying the cloud "mainly shows downdrafts" could be misinterpreted as you saying there are mainly downdrafts, as opposed to updrafts. At 16:15-16:18 there are updrafts and downdrafts (at different heights)
Rephrased.
Line 417: "From Fig. 11(c), downdrafts occur in the first cloud from 16:15 to 16:18 UTC and after 16:22 UTC.

Line 404: In the second paragraph of section 5.1.1, there is a discussion about updrafts and downdrafts on the edge of the thunderstorm compared to the core, saying that you would expect updrafts in the core. You say that at 16:18-16:22 the core is in sight, while before and after that period (at 16:15-16:18 and after 16:22) the radar sees the edge of the thunderstorm. In Fig A1 it is obvious that at 16:15-16:18 the radar is looking at edge, but is 16:22 – 16:25 not looking at the core?
We discussed the cloud radar observations in the second paragraph of section 5.1.1. We added the following sentences for the comparison cloud radar data - weather radar data:
Lines 422-424: "Cloud observations between 6 and 10 km height generally show good agreement with the precipitation patterns and intensity measured by the weather radar at lower heights in Fig. A1. However, timing differences on the order of 1 minute may arise due to the differing temporal resolutions of the two radars."

Line 409: The first two panels are before the negative KDP is observed, right? If so, it would be helpful to the reader to mention that explicitly here.
Added.
Line 425: "Figure 12 shows the spectral Z_{DR} across the period when negative K_{DP} is observed (panels 3-4).

Fig 12 and Fig 16 seem like they could be combined somehow as they show overlapping time periods. (Though the times chosen in both figures seem a bit random, the time difference between consecutive panels is not the same)
These figures have different purposes. Fig. 12 displays the possibility of particle alignment and shows a time period of more than 3 minutes. For this time period, Fig. 16 provides a time zoom of 1 minute where the presence of liquid water is identified.

Line 423: In the 4th paragraph of section 5.1.1, you question whether the vertical alignment of particles (as seen by the negative sZDR values in the right part of spectrum at 16:21:05) is associated with cloud electrification before lightning. This is done by comparing the times and distances when negative KDP is observed (Fig 11b) to the times and distances of the lightning strikes in Fig B2.
I find some of the text confusing here. You say "negative KDP appears at a distance of 7600 m to 9300 m away from the radar", and "one would expect to observe negative KDP also for ranges beyond 9000 m". Do you mean height of 7600 m to 9300 m rather than distance from the radar? The range isn't shown on the KDP plot, right?
The authors thank the Referee for spotting this issue. Corrections are done.
Lines 437-439: "Negative K_{DP} are observed within the height range of 7600 m to 9300 m, whereas the lightning strikes occurred at least 13000 m away from the radar. If the electric field that caused these lightning strikes is responsible for the alignment of particles observed, one would expect to observe negative K_{DP} also for heights beyond 9000 m."
In the whole manuscript, for the profiles and spectrograms, the y-axis represents the height (and not

the range).

Line 428: The Wang et al 2019 paper you have cited doesn't have the vertical wind term you have included in Equation 12

Rephrased.

Lines 444-445: "By modifying the formulation of Wang et al. (2019) to incorporate vertical wind velocity, the horizontal and vertical particle velocities can be expressed as:"

Line. 432: For a radar

Corrected in Line 449.

Line 434: "Vt increases with particle size" – this statement is only broadly true

Line 451: "When V_t increases, a negative shear s causes the spectrum to widen as the left side shifts more than the right……"

Line 438: How accurate is the ECMWF forecast of vertical wind shear, and how do you think wind shear over Cabauw would compare to the actual values within the thunderstorm?

Rephrased.

Lines 454-459: "For a spectrum width of 10 m s^{-1} and a terminal velocity (V_t) of 2 m s^{-1}, corresponding to the upper bound for plate-like particles (Spek et al. 2008), a shear of approximately 25000 m s^{-1} km^{-1} would be required to invert the spectrum. This value is substantially higher than the observed shear of 4 m s^{-1} km^{-1} between 7500 m and 10000 m in ECMWF data, as shown in Fig. 15(c). While recognizing the limitations of ECMWF wind shear data in the context of thunderclouds, a wind shear of 25000 m s^{-1} km^{-1} is highly improbable. Therefore, wind shear is unlikely to account for the negative sZ_{DR} observed on the left side of the spectrum.

Line 440: I'm not sure what you mean here, it sounds like you are saying that large ice chains are more likely than other particles to be aligned by an electric field? Please consider rephrasing this paragraph.

Rephrased.

Lines 460-461: "Alternatively, the hypothesis is that the axis ratios of small particles are close to one and that the electric fields could align larger particles vertically, leading to negative sZ_{DR} on the left side of the Doppler spectrum.

Line 460: Have you already discussed somewhere the possibility that lightning may not be measured by the sensor? How likely it that?

We mentioned this in the text but we did not investigate the detection limitations of BLIDS for cloud-to-cloud lightning. In the future, we would like to use a more accurate information of lightnings.

Line 464: You could highlight here that the mode is particularly obvious in the 4th panel otherwise some readers may immediately be drawn to the 2 peaks between 6000-7000m that can be seen in the first panel.

Thank you. Added.

Line 486: "This separate mode is most clearly discernible in the fourth panel."

Line 465: think it's worth pointing out here that the temperature is measured vertically and not within the cloud.

The authors think it is clear that the microwave radiometer provides vertical profiles of air temperature. Later in the manuscript at the end of section 5.2.2.1, the following sentence is present: "Nonetheless, the temperature profile inside the thunderstorm cloud may be different from the temperature profile measured by the microwave radiometer looking towards the zenith, …..(Lines 637-639).

Line 486: on the line of sight → in the line of sight

Corrected in Line 507.

Line 488: why is sZDR more negative at the edges and closer to 0 for intermediate velocities?

We added the following sentences to explain this:

Lines 510-515 : "The sZ_{DR} values are predominantly negative across the entire spectrum, with an average value of -0.12 dB. Analysis of the spectrum at 8018 m, in comparison with the simulations presented in Fig. 10, indicates that sZ_{DR} aligns with the behavior expected for slightly oblate particles with a canting angle of $\beta = 90\,\text{deg}$. Specifically, negative values are observed on the right side of the measured spectrum, increasing with particle size before decreasing toward the left side coinciding with the first Mie minimum. At heights exceeding 8000 m, the spectra become broader and exhibit diminished resonance features due to enhanced turbulence."

Line 497: Don't you say elsewhere that large SLdr and small rho hv could be caused by low SNR?
Yes. The Referee probably refers to Lines 175-178 where the impact of important attenuation on the polarimetric variables is shortly discussed. In that case, we have another situation where the reflectivity is large, SLdr is small, rho hv has large values approaching 1, therefore we think of a possible alignment of the ice particles that decreases canting angle variance.
Line 524: "During this period, ρ_{hv} does not change significantly and is high (Fig. 21(c))."
We removed "which means that the decrease in SL_{DR} is not due to low SNR."

Line 501: 5.5 km is a large distance - You say in the intro that the magnitude of the electric field decreases to 3 kV m^{-1} within 5 km away from the cloud edge, and you only discuss alignment potentially occurring for fields of 10 kV m^{-1} and larger. So from those numbers it seems unlikely that there would be alignment here.
That was the largest peak current measured among 162 cloud-to-cloud lightning discharge. It corresponds well in time to the apparition of the sZDR negative values on the right side of the Doppler spectrum while apparently there is no change of particle populations. We still think that there is a correlation between these negative values of sZDR and this lightning. However, we would have liked to have quantitative values of the electric field in the line-of-sight of the cloud radar.

Line 510: other → a different
Corrected in Line 537.

Line 512: don't separate these paragraphs
The second paragraph is now removed.

Line 515/516: upper part of the spectrum → I think you mean the spectrogram?
We removed this paragraph.

Line 517: I think you are saying that at 17:19:09 there could still be the underlying vertical particles that would cause negative sZDR, but there are also other particles introduced into the same velocity bin which are not aligned, and these dominate the signal.
However, if the particles are in the same velocity bin wouldn't they be aligned too?
Yes, they should be aligned too. We agree with the Referee's comment. That is corrected by removing the paragraph and figure related to this discussion.

Line 519: is SLdr shown here?
No SLdr is not shown because at the heights of interest, its value does not really change versus time.

Line 523: would be good to be more specific with your summary here, e.g. did the alignment occur before/during/after the lightning?
We indicated time estimates for the two cases, and for the summary we added the following sentences:
Lines 544-545: "The vertical alignment of ice particles was observed 2 to 5 seconds prior to the lightning strike and dissipated 5 to 6 seconds afterward. These temporal estimates account for the measurement timing of chirp 3."
We modified the conclusion accordingly in Lines 715-716.

Line 526: spectrum → spectrogram or spectra
Corrected in Line 546.

Line 532: lightnings → lightning strikes
Corrected in Line 552.

Line 538: enhanced slanted linear depolarisation ratio and reduced copolar correlation not due to low SNR?
We added the following:
Lines 558-561: From Fig. 19 (c) and (d), this region has enhanced slanted linear depolarisation ratio and reduced copolar correlation coefficient. In fact, the SNR from 17:22 to 17:24 UTC at 4000 m to 6000 m ranges from 14.0 dB to 40.8 dB with a mean of 31.1 dB, suggesting that the enhanced slanted linear depolarisation ratio and reduced copolar correlation values are not due to low SNR.

Line 539, 595, and Figs 26 and 27 captions: spectogram → spectrogram
Corrected.

Line 550: An Evidence of differential attenuation ...
Corrected in Line 550.

Line 551: I haven't read that paper in great detail, but noticed a reference to mean axial ratios of 0.75-0.9 for sizes bigger than 1mm. I guess your definition of axial ratio is just the inverse of theirs? I think you define axis ratio but not axial ratio in your manuscript.
Corrected. We use the parameter axis ratio only in the whole article. It is defined in Lines 308-309. Line 572-574 : "From the literature, the theoretical axis ratio of conical graupel is 1.05, while measurements of mean axis ratios of conical graupels show values ranging from 1.1 to 1.3 for sizes in excess of 1 mm (Spek et al. 2008).

Fig. 25: I like the improvements made to this figure.
We also appreciate the improvements made possible by the Referee's suggestions.
Consider labelling the panels left to right instead of top to bottom, i.e. a, b, c in the top row rather than a, d, g.
For consistency that should be done for the whole article and text. Therefore, we left it as it is.
Also, you label some y-axes but not others, please label all yaxes.
All the y-axes are now labelled.
You could also consider flipping the x-axes on panels g, h, i, which would make it easier to compare to the simulations.
Again for consistency with all the other figures showing Doppler spectra, we don't flip the x-axis.

Line 561: In this paragraph please clarify which panels you are referring to, e.g. differential reflectivity of the simulated conical graupel is mostly negative (Fig. 25b?).
We added references to the different panels for clarity.
When you say "δ_{co} reaches a local maximum and decreases slightly before increasing again. ZDR increases and continues to oscillate." – which panels are you looking at here?
Thank you for spotting this. Some parts of these sentences relate to the figure of the previous version where the equal-volume sphere radius range was larger. This is corrected in Lines 586-587.
Differential backscatter phase (δ_{co}) is plotted in panel c *(right)*, but panels f and i show the differential phase shift (PhiDP), is that right? *panel f also shows (δ_{co}) while panel i shows the differential phase shift (PhiDP). Both simulations (panels c and f) provide δ_{co} and the measurement (i) supplies ($s\Psi_{DP}$). We wrote the following sentence for clarification: "Since the constant spectral differential propagation phase ($s\Phi_{DP}$) is nearly 0 deg. (Doppler velocities from 2 to -1 ms^{-1} in Fig. 25(i)), the spectral differential phase shift ($s\Psi_{DP}$) corresponds to the spectral differential backscatter phase $s\delta_{co}$." in Lines 588-590.*
The behaviour you describe is not obvious in panel c, I think you have changed the axis limits since the last version so maybe that's why.
That is indeed the reason. Corrected.

Line 561: earlier → at smaller sizes?
Modified in Line 585.

Line 570: suggests → highlights?
The sentence has been changed.

Line 573: could this difference be due to air motion?
The air motion affects the location of the first Mie minimum for the measurements sZ_{hh}, sZ_{DR}, and $s\Psi_{DP}$ in the same way. Here, the medium is complex, there are probably two types of ice particles and the first Mie minimum location difference may indicate the possibility of an extra dependency: shape. Lines 603-607: "Furthermore, the location of the measured first Mie minimum is influenced by both the equi-volume sphere radius and air velocity. However, a comparison of polarimetric spectra related to the same radar resolution volume reveals variations in the Mie minimum location, indicating an additional dependence on particle shape. Consequently, simultaneous consideration of the three parameters—sZ_{hh}, sZ_{DR}, and $s\Psi_{DP}$—at the same time and height is essential for a comprehensive analysis."

Line 585: data from Gatidis et al
Corrected in Line 616.

Line 590: Is this differential attenuation (in which case change dB → dB/km)? It might be clearer to integrate it and give an estimated value of two-way path integrated attenuation in dB.
Corrected.
Lines 621-622: "Thus, the two-way path integrated 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."

Line 592: Could you expand on this slightly please? What distance is the storm from the radar?
Corrected and expanded.
Lines 623-626: "It is worth noting from Fig. 24(a) and (b) that the population of graupel ends at around 4000 m height, which means the region with graupel is localised in the thunderstorm cloud. Since the radar is looking at an elevation angle of 45°, this suggests that graupel is not present closer than 5700 m from the radar. At this range, measurements cannot be obtained at lower altitudes due to the 45° elevation angle. Below 4000 m, graupel begins to melt."

Line 595: Might be worth mentioning that in the Fig 24 figure caption (vertical velocity rather than Doppler velocity)
Thank you. That is done now.

Line 628: is shown → are shown
Corrected in Line 662.

Line 630: At 17:22:57 you hypothesize that there could be chains at 10003m. This is because the entire ZDR spectrum is positive with values of about 0.2-0.6 dB. You also say that since there is a low copolar correlation coefficient, there could be a mixture of small ice and chains. Can you comment on why chains are the likely particles here rather than having for example plates or columns with varying aspect ratios?
The authors modified the section and propose an hypothesis with more consistency than the previous one. The trigger was the large increase of the differential propagation phase. The hypothesis is a mixture of two types of ice particles: oblate ice particles coated with liquid water (larger particles) and chains (smaller particles). We think that the entire ZDR spectrum is attenuated and expect higher values of ZDR. The same for the spectral reflectivity. Therefore, we think that chains are more probable than plates.