Responses to Review Comments on “Identification of multiple co-located hydrometeor types in Doppler Spectra from scanning polarimetric cloud radar observations”

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Dear Editor,

we would like to sincerely thank the reviewers for their careful reading and constructive comments during both rounds of review. Their insights have helped us to improve the clarity and quality of the manuscript.

Below we provide replies to the individual comments raised by both reviewers. We put efforts into explaining more clearly about the distinction into 5 parts, the application of the horizontal-wind correction and the impact of the assumption of Rayleigh scattering. Also any proposed additional interpretations of the case-study observations were considered in our revision. The manuscript was re-screened for typographical and grammar issues.

Any references to line numbers we do provide correspond to the revised version of the manuscript (without markup).

Response to Report #1

1) Separation Rayleigh/Resonance Scattering Regime

Based on the measurement of the spectral differential phase, it is possible to detect where the resonance scattering regime starts in the Doppler spectrum (Mak et Unal, 2025). That can be of use for the analysis of large ice particles (part 1).

Mak, H. Y. L., and Unal, C. (2025): Peering into the heart of thunderstorm clouds: insights from cloud radar and spectral polarimetry. Atmospheric Measurement Techniques, 18(5), 1209–1242. https://doi.org/10.5194/amt-18-1209-2025

We agree that the use of spectral differential phase, as discussed by Mak and Unal (2025), can provide additional insights into the resonance scattering regime within the Doppler spectrum. In particular, this could be beneficial for characterizing large ice particles represented in part 1 of our spectrally resolved analysis. While our current study focuses on ZDR and RHV as key polarimetric parameters, the incorporation of spectral differential phase could indeed enhance the detection and interpretation of resonance effects associated with large particles. We suggest this as a promising direction for future studies that aim to refine particle-type identification within our framework. We thus added the reference and short note on the study in the conclusions section in Lines 605-607: “A recent study by Mak and Unal (2025) suggests to use spectral differential phase for identification of conditions of non-Rayleigh scattering conditions. As the cloud radar system under study in STSR mode can in principle also provide differential phase, this approach might be evaluated further in follow-up studies.”

2) Instrumentation

Lines 103-105: To explore the polarimetric capabilities of the MIRA-35 cloud radar was not the unique goal of the ACCEPT campaign. Therefore, I propose to rephrase: “The ACCEPT measurement campaign was led by the Leibniz Institute for Tropospheric Research (TROPOS) in Leipzig, Germany, with partners from the Technical University of Delft and METEK GmbH in Elmshorn, Germany. One of its goals was to explore the polarimetric capabilities of the MIRA-35 cloud radar. A second objective was the study of ice particle growth processes in mixed-phase clouds using spectral polarimetric S- band radar measurements (Pfitzenmaier et al. 2017, Pfitzenmaier et al. 2018). Pfitzenmaier, L., Dufournet, Y., Unal, C. M. H., & Russchenberg, H. W. J. (2017). Retrieving fall streaks within cloud systems using Doppler radar. Journal of Atmospheric and Oceanic Technology, 34(4), 905-920. https://doi.org/10.1175/JTECH-D-16-0117.1

Pfitzenmaier L., C. M. H. Unal, Y. Dufournet, and H. W. J. Russchenberg: Observing ice particle growth along fall streaks in mixed-phase clouds using spectral polarimetric radar data, Atmos. Chem. Phys., 18, 7843–7862, 2018. https://doi.org/10.5194/acp-18-7843-2018.

We have rephrased the sentence as proposed to reflect the broader goals of the ACCEPT campaign, including the study of ice particle growth using spectral polarimetric S-band radar measurements. The text in lines 77-80 now reads: “The ACCEPT measurement campaign was led by the Leibniz Institute for Tropospheric Research (TROPOS) in Leipzig, Germany, with partners from the Technical University of Delft, the Netherlands, and METEK GmbH in Elmshorn, Germany. One of its goals was to explore the polarimetric capabilities of the MIRA-35 cloud radar. A second objective was the study of ice particle growth processes in mixed-phase clouds using spectral polarimetric S- band radar measurements (Pfitzenmaier et al., 2017, 2018).”

3) Main-peak approach

Line 192: “These measurements are then compared with simulated values based on spheroid model that assumes the particles are shaped like spheroids (3D ellipsoids)”. Mention already here that Rayleigh scattering is used. The reader should not wait for the conclusion to know this important information.

We now mention at this point in the text that the simulations are based on the Rayleigh scattering assumption. For this we have added lines 166-168 as follows: “These measurements are then compared with simulated values based on a spheroid model that assumes the particles are shaped like spheroids (3D ellipsoids) and that Rayleigh scattering applies.”

4) Spectrally resolved approach

Figure 1: this figure suggests that 5 different types of particles can be retrieved with the Doppler spectrum partitioning, but that is not the case. Make a note of this in Figure 1 caption.

We have revised the caption of Figure 1 to clarify that the division into five parts enables the potential identification of up to five different hydrometeor types, but does not guarantee nor require their presence in every case. The updated caption now reads: “The division into five parts enables the potential identification of up to five different hydrometeor types; however, the actual number of distinct types retrieved depends on the spectral and microphysical characteristics of the observed cloud. It should be noted that turbulent motion is not considered in the sketch.”

5) Second case study 03 Nov 2014, 20:30-20:45: Secondary ice formation

4-a) Figure 10 is not discussed (only 1 sentence, Lines 523-524). I suggest to add a small paragraph to comment the key features of the precipitating cloud case illustrated in Figure 10 (20:00-21:00).

5-a) We have added a paragraph discussing the key features of the precipitating cloud case illustrated in Figure 10, focusing on reflectivity, LDR, Doppler velocity, and spectral width during the 20:00–21:00 UTC period. See lines 501-507 in the revised manuscript as follows: “Figure 10 provides an overview of the precipitating cloud system observed from 20:00 to 21:00 UTC. Radar reflectivity (panel a) shows strong backscatter values, especially below 4 km, indicating precipitation reaching the surface. The LDR (panel b) increases near the melting layer (2 km), suggesting the presence of melting particles and possibly needle-like ice just above it. Additionally, LDR values around −25 dB at the cloud top as observed by vertical-stare Mira-35 NMRA indicate the presence of slightly non-isometric-shaped particles. The Doppler velocity (panel c) reveals strong downward motion, with values exceeding −1.5 m s−1 below 3 km, characteristic of precipitating particles. The spectral width (panel d) is elevated in the lower levels, indicating enhanced turbulence and possibly a mixture of hydrometeor types during active precipitation.”

5-b) Lines 538-540: The polarizability factor and degree of orientation cannot be discussed in the melting layer and in rain using the Rayleigh scattering assumption, which is not valid at 35 GHz for these hydrometeors. Remove these lines.

5-b) We agree that the Rayleigh assumption is not valid for melting layer and rain at 35 GHz. We have removed the lines accordingly.

5-c) Lines 588-590: "Indications are given that the branches of oblate ice crystals, such as dendrites fell off, in addition....". Can the authors clarify this statement? Which indications? Is the presence of dendrites in the study case justified? Until now, there was no discussion about the presence of dendrites.... The authors cannot write such a statement without any justification other than "Indications are given". Justify/explain in the article.

5-c) The indication refers to the observed changes in polarimetric radar signatures consistent with fragmentation of dendritic ice crystals during their descent. We realize that the presence of dendrites was not explicitly discussed earlier, so we have now added a brief explanation and justification for their occurrence in the study case (lines 560-582), based on environmental conditions and previous literature.

6) Summary and Conclusions

6-a) Lines 609-611: "The author notes that the possible reason for the smaller influence of non-Rayleigh scattering on polarimetric variables is that they are differential (rather than absolute) quantities representing differences/ratios of radar parameters at two orthogonal polarizations." Considering any mm-wavelength radar or hydrometeor type or polarimetric variable, this statement is not correct. To my knowledge, in general, non-Rayleigh scattering influences polarimetric variables.

6-a) Yes, non-Rayleigh scattering influences polarimetric variables. This is true and was shown in many earlier studies. What we, or better Matrosov (2021) means, is that the impact of a particle size distribution on the bulk polarimetric signatures somewhat cancels out. It is especially a lack of current T-Matrix and DDA-based simulation studies that they can hardly provide simulations of a realistic particle population. That's why we argue that simulations might overestimate the non-Rayleigh effect on polarimetric variables.

6-b) Lines 618-620: "In the presented study, the homogeneity was evaluated based on (1) inspection of the required monotonic relationships between polarimetric parameters and elevation angle, and (2) the appropriateness of the horizontal wind correction." Significant variations in Doppler spectrum width were not considered?

6-b) We acknowledge that significant variations in Doppler spectrum width can also indicate inhomogeneity. In our study, we focused on monotonic relationships and wind correction as primary criteria, but we have now added this statement to lines 613-614 as follows: Additionally, variations in Doppler spectrum width can be considered as a potential indicator of inhomogeneity, though they were not the primary focus in the current evaluation"

6-c) Lines 621-622: "The ACCEPT campaign, conducted in Cabauw, the Netherlands in 2014, aimed to assess the capabilities of both the main peak approach and the spectrally resolved approach."

Based on earlier comment (see instrumentation section), replace the sentence by "One of the aims of the ACCEPT campaign, conducted in Cabauw, the Netherlands in 2014, was to assess the capabilities of both the main peak approach and the spectrally resolved approach."

6-c) We have revised the sentence as recommended to improve accuracy (lines 615-616).

7) Acknowledgements

Line 653: "..... and to the team of the CESAR Observatory in Cabauw and Delft University of Technology, NL, for their support ...."

We have updated the acknowledgment to include the CESAR Observatory team in Cabauw and Delft University of Technology, NL, for their support (lines 635-638).

Minor comments/corrections/adding's

1) Abstract

The abstract consists of three parts identical. Please, correct this.

Apologies for the formatting issue. We corrected it.

2) Introduction

Lines 87-89: The following paper could be added:

Mak, H. Y. L., and Unal, C. (2025): Peering into the heart of thunderstorm clouds: insights from cloud radar and spectral polarimetry. Atmospheric Measurement Techniques, 18(5), 1209–1242. https://doi.org/10.5194/amt-18-1209-2025

We have added the recommended reference (Mak and Unal, 2025) to the relevant section of the manuscript (lines 63)

3) Instrumentation

3-a) Line 114: remove "also"

3-a) We have removed "also" from line 89 as requested.

3-b) End of Table 1, typo: "...... can be estimated by assuming ...."

3-b) We have corrected it to "can be estimated by assuming" at the end of Table 1.

4) Mira-35 radar in hybrid mode

4-a) Lines 144-145: "……. which are representative of the horizontally (Eh(ω)) and vertically (Ev(ω)) components of the received waves." Also, place a dot above the capital letter E. These components are complex.

4-a) We have updated the notation to include a dot above the capital letter E to indicate the complex nature of the horizontal and vertical components (lines 120-121).

4-b) Line 148: "ZDR quantifies the ratio of reflectivity measurements in horizontal (Zhh, Eq. 1) and vertical (Zvv, Eq. 2) polarizations, (Eq. 3)."

4-b) We have revised the sentence for clarity to correctly describe the definition of ZDR (line 124).

4-c) Line 153, typo: Also, the constants …..

4-c) We have corrected it to "Also, the constants …" on line 129.

4-d) Line 154: Remove "Radar cross section (RCS)". The constants C1 and C2 do not depend on the RCS, Eh(ω) and Ev(ω) do depend on the RCS (square-root of RCS).

4-d) We have removed the mention of "Radar cross section (RCS)" from line 130, as C1 and C2 do not depend on it. The dependency on RCS applies to E_h(ω) and E_v(ω) instead.

4-e) Lines 158-160: "The correlation coefficient (RHV) is a crucial parameter (defined by Eq. 4) that quantifies the similarity between horizontally (Eh(ω)) and vertically (Ev(ω)) polarized backscattered signals." Place a dot above the capital letter E.

4-e) However, in our study, we work with Zhh and Zvv rather than the complex electric field components E_h(ω) and E_v(ω). Accordingly, ρhv is defined in terms of these reflectivity-based quantities.

4-f) Lines 160-162: "It provides insight into the diversity of particle shapes and orientations within a radar resolution volume and is typically expressed as a value between 0 and 1."

4-f) We have revised the sentence in lines 135 - 136 as follow: "It provides insight into the diversity of particle shapes and orientations within a radar resolution volume and is typically expressed as a value between 0 and 1."

4-g) Lines 162-163: "A correlation coefficient of 1 indicates perfect correlation between horizontal and vertical polarization signals, implying that the backscattered signals are identical in phase and amplitude."

4-g) Thanks for suggesting this alternative way for explanation of the correlation coefficient. We have revised the sentence to clearly state that a correlation coefficient of 1 indicates perfect similarity between horizontal and vertical polarization signals, typically implying uniform particle type, shape, and orientation within the radar resolution volume (lines 138-139).

5) Main-peak approach

5-a) In Eq. (8), remove the subscript "e", which is not present in the text.

5-a) To maintain consistency with the original paper and ensure clarity, we have added the subscript "e" in the text to match the notation used in Eq. (8) (line 176).

5-b) Line 211: Is ξg the axis ratio?

5-b) Yes, ξg denotes the axis ratio in this context. We also added it in line 182.

5-c) Line 226: "Finally, to derive ZDR and RHV, the horizontal and vertical complex scattering amplitudes are calculated using the polarizability ratio and the degree of orientation across different elevation angles. "

5-c) We have revised the sentence in lines 196-197 as follows: "The complex scattering amplitudes are derived using the polarizability ratio and degree of orientation, which vary with elevation angle, to calculate ZDR and ρHV".

5-d) Line 234, typo: "distribution".

5-d) We have corrected "distrubution" to "distribution" at line 211.

5-e) Line 238, typo: Not "than" but "then".

5-e) We have corrected "than" to "then" at line 215.

6) Spectrally resolved approach

6-a) Lines 250-253: "The width of the Doppler spectra is thereby characterized by size- and shape-dependent fall velocities of the particles, which are super-imposed by influences of turbulence and (predominantly in case of off-zenith antenna pointing angle) horizontal wind variability that cause additional broadening of the spectrum (Radenz et al., 2019)."

6-a) corrected

6-b) Line 274, typo: "mean square error function"

6-b) We have corrected it to "mean square error function" at line 251.

6-c) Line 279: "During averaging of all ZDR and RHV values .....". Mention how many values can be expected for this average.

6-c) We added a text in lines 257-259 as follows: "It should be noted that the number of values contributing to the average depends on the elevation angle resolution of the RHI scan; in this study, typically 121 elevation angles are used."

7) **The influence of air motion on the Doppler spectra observed by a scanning cloud radar**

Line 295: .... onto the gravitational downward motion of particles ....

Line 295: In our setup with zenith-pointing radar, the hydrometeors fall due to gravity, so their motion is downward. Therefore, we have retained "gravitational downward motion" to accurately describe the physical situation (line 273).

8) **Retrieval of horizontal wind**

8-a) Line 306: ".... and radial wind velocity Vw."

8-a) We have replaced V_w at line 284.

8-b) Eq. (12): is it not Vw=Vh cos cos(B+π-α)? To have negative radial wind velocity when the particles approach the radar. The wind direction being defined from where the wind comes from.

8-b) You are correct that to obtain a negative radial wind velocity when particles approach the radar, the wind direction defined as where the wind comes from needs to be accounted for. Instead of adding π in the equation, we applied a negative sign to the term to achieve the correct sign convention (line 295).

9) **First case study 07 Nov 2014, 09:15-09:30: retrieval of various hydrometeor types**

9-a) Figure 4 caption: .... (see Figs. 7-9). Typo in the right parenthesis.

9-a) We have corrected the right parenthesis in the caption of Figure 4.

9-b) Line 391, typo: …. high values of radar LDR ….

9-b) We have corrected the phrase to “high values of radar LDR” at line 369.

9-c) Line 444: …. The SNR stabilizes at approximately 60 dB. I think it is much less. 25 dB?

9-c) We have double-checked the data and agree that the SNR stabilizes closer to approximately 25 dB. We have corrected this value in line 422.

9-d) Lines 457-458: “Since different parts might identify oblate-shaped particles at heights around 3km (parts 1, 2, 3, and 4) and prolate-shape particles at part 5, a height of 3km is chosen for a detailed depiction of the data analysis procedure.” From the discussion of Figure 7 above, the authors mention that at this height (around 3 km), the particles are spherical-shaped in parts 1-3, oblate-shaped in part 4 and prolate-shaped in part 5. Therefore, rephrase the sentence for consistency with the discussion of Figure 7.

9-d) We have revised the sentence to ensure consistency with the discussion of Figure 7, clarifying that at around 3 km, particles are spherical-shaped in parts 1–3, oblate-shaped in part 4, and prolate-shaped in part 5 (line 435).

9-e) Figure 8 caption: I suppose that the time period 09:16:09 – 09:18:08 UTC is considered and not only the time 09:15:09 UTC.

9-e) We have clarified in the caption of Figure 8 that the time period 09:16:09 – 09:18:08 UTC is considered, rather than only the single time 09:15:09 UTC.

9-f) Figure 8 caption: ….. and for each part of the spectrally resolved spectra, shape and orientation retrieval ……”

9-f) We have updated the Figure 8 caption to clarify that shape and orientation retrieval is performed for each part of the spectrally resolved spectra.

9-g) Figure 9: I don’t see the horizontal lines displaying the temperature levels.

9-g) We have removed the statement referring to horizontal lines displaying temperature levels in Figure 9.

10) Second case study 03 Nov 2014, 20:30-20:45: Secondary ice formation

10-a) Lines 528-529: "In middle altitudes (X-Y km) …….. In lower altitudes (Z-X km) …..." Be specific and indicate the height range considered.

10-a) We have revised the text to specify the exact height ranges referred to as "middle" and "lower" altitudes (line 512).

10-b) Figure 12 caption: the time (20:01:09-20:11:26) is not correct.

10-b) We have corrected the time in the caption of Figure 12 to reflect the accurate time interval. (20:31 – 20:41)

10-c) Line 542: "…. between the melting layer and 2.75 km height ……"

10-c) We have corrected the phrase to read "between the melting layer and 2.75 km height" in line 523.

10-d) Line 558: "… RHV decreases with increasing part number …."

10-d) We have clarified the sentence to state that "RHV decreases with increasing part number" in line 539.

10-e) Figures 16 and 17 caption: ….. and for each part of the spectrally resolved spectra, shape and orientation retrieval ……"

10-e) We have updated the captions of Figures 16 and 17 to clarify that shape and orientation retrieval is performed for each part of the spectrally resolved spectra.

11) Summary and Conclusions

Lines 602-603: "In particular, by incorporating Rayleigh scattering, we assume that particles are small with respect to the deployed electromagnetic waves." I suggest to remove this sentence. It is redundant compared to the next sentence. The next sentence suffices.

Lines 602-603: We agree that the sentence is redundant and have removed it to improve clarity and avoid repetition.

Line 611, typo: …. were elaborated …..

Line 611: The typo has been corrected to "were elaborated" in line 602.

Line 638:" …….. would be to apply implement techniques ….." Suppress either "apply" or "implement".

Line 638: We have corrected the phrase by removing "apply" to avoid redundancy (line 632).

Line 639, typo: "Then more insights ....."

Line 639: We have corrected the phrase to "The more insights …" in line 633.

Response to Report #2

Answer to AR1-1B: I am still not convinced that just dividing the spectrum into 5 parts allows you to see 5 distinct hydrometeor types. It can very likely be that two hydrometeors (i.e. needles and aggregates as they can have the same fall velocity) are in the same part, therefore you will still see the combined effect of both species. I understand that applying a peak identification algorithm on the slant spectra is difficult, however, I would have tested a few more subjective ways of separation into n parts based on i.e. the shape of your Doppler spectrum (i.e. the slope of sZDR can tell you when another species is introduced, or some other measure). The arbitrary selection of 5, especially if you can not even distinguish between rimed particles and aggregates seems random. However, I am fine with having it published like this, as long as you make it clear that separating into these 5 parts does not mean that you do not have a mixture of ice particles in each part and can therefore miss e.g. the appearance

Answer to AR1-1B: We fully agree that dividing the Doppler spectrum into five parts does not guarantee the exclusive presence and identification of a distinct hydrometeor type in each part. As Reviewer #2 correctly pointed out, species such as needles and aggregates can share similar fall velocities and thus overlap within the same spectral region, leading to a mixture of particle types in a given part. Practically, there is to our knowledge currently no way to distinguish different particle types which show the same Doppler velocities. Unfortunately, our technique does not make a difference here. But what the difference to other established techniques is, that we at least aim to tackle the complex topic of identification of multiple hydrometeor types in the same volume of air. And to our opinion, we go a step further compared to other studies. E.g., vertically pointing polarimetric Doppler cloud radar measurements show increasing capability to assign crystal habits to distinct peaks in the Doppler spectra (e.g., Vogl et al., 2024). However, while having the advantage of being applicable to basically any vertical-stare polarimetric Doppler cloud radar observation, these techniques suffer of two deficiencies: (1) individual peaks must be identifyable, and (2) it cannot be distinguished between oblate and isometric/spherical hydrometeors since both show similar polarimetric properties in zenith-pointing mode. Data from polarimetric scanning Doppler weather radars, in turn, are since quite some time used for identification of hydrometeor types (e.g., Marzano et al., 2006; Chandrasekar et al., 2013). However, so far, only one hydrometeor type per data point (i.e., unit of volume) is identified. For identification of several co-located hydrometeors in a volume, currently only dual-wavelength polarimetric radar techniques are discussed (Pejcic et al., 2025). Our technique overcomes both of these issues, even though be it on the expense of higher complexity by means of the need to relate and analyzed Doppler spectra of polarimetric variables from different elevation angles, which incorporates the necessary assumption of horizontal homogeneity. Nevertheless, horizontal homogeneity is for our approach by far not as an issue as for the establishment of quasi-vertical profiles from weather radar PPI scans (Ryzhkov et al., 2016).

The specific choice of five Doppler parts was based on a balance between resolution and practical interpretability, rather than an exact physical separation of all hydrometeor species. We acknowledge that this splitting is somewhat arbitrary and does not perfectly separate rimed particles from aggregates or other mixed-phase components.

Regarding your suggestion to explore more subjective or data-driven criteria for spectral partitioning—such as incorporating slope changes in sZDR or other Doppler spectral shape metrics—we recognize the potential benefits of these approaches. Indeed, these could offer more physically meaningful splitting by identifying transitions between dominant hydrometeor types. Due to the complexity and computational cost, however, we limited our scope in this study but consider this an important avenue for future work.

To clarify this limitation in the manuscript, we will explicitly state that the division into five parts represents an approximate classification (lines 240-242). We emphasize that each part may contain mixed hydrometeor populations and that the method does not guarantee the full separation of all particle types (lines 245-261).

Answer to AR1-2: to me it was not clear that if you assume Rayleigh that then there is a direct relationship between the ZDR/RHV and the shape of the particle. Perhaps you can say that in one sentence in the manuscript. About the wind retrieval: See comment to Line 240

Answer to AR1-2: Rayleigh scattering applies when the particle size is much smaller than the radar wavelength. In this regime, the scattering properties depend mainly on the particle's shape, orientation, and dielectric properties, rather than complex resonance effects seen at larger sizes (Mie or non-Rayleigh scattering).

Because of this, differences in the returned radar signals' polarization (like the difference between horizontal and vertical reflectivity, i.e., ZDR, or polarization ratio RHV) can be directly attributed to how the particle's shape affects the scattering.

So, when Rayleigh conditions hold, ZDR and RHV and their elevation dependencies become reliable indicators of the particle's aspect ratio and orientation, allowing us to infer shape characteristics from these measurements.

We rephrased and extended the text between lines 172-195 to highlight the relationship between ZDR & RHV to polarizability ratio and degree of orientation in the case of Rayleigh scattering, as follows: "In the simulation part of the method, ZDR and RHV values are calculated for many combinations of particle shapes, densities (related to their refractive index), and orientations, across a wide range of elevation angles (from 30◦ to 150◦). For doing so, Myagkov et al. (2016a) utilized spheroidal (Rayleigh-) scattering theory which enables to establish a direct relationship between the observables elevation angle, ZDR, and RHV and the particle's properties of density-weighted axis ratio and the distribution of the canting angles. The density-weighted axis ratio is denoted polarizability ratio ξe which can be represented as the ratio of polarizability elements p1 and p2 (Eq. 8):

$$\xi e = \langle p2 \rangle / \langle p1 \rangle \qquad \text{(Eq.8)}$$

which polarizability elements p1 and p2 are defined as Eq. 9:

$$p1,2 = V\ \varepsilon 0(\varepsilon r - 1) A1,2(\xi g) \qquad \text{(Eq.9)}$$

where V is the volume of the spheroid, $\varepsilon 0$ is the vacuum permittivity, εr is the relative permittivity, and $A1,2(\xi g)$ are function of the axis ratio ξg . The polarizability ratio ranges from 0.3 to 2.3. Within this range, values of $\xi = 0.3$ represent strongly oblate particles, $\xi = 2.3$ indicate strongly prolate particles, and $\xi = 1$ signifies centrally positioned spherical particles. As the polarizability ratio is also a function of the particle refractive index, i.e., density, its absolute value approaches unity for values of very low density. This aspect has to be considered in the interpretation of ξ.

The general orientation distribution of the hydrometeor population is described by the degree of orientation κ (Hendry et al., 1976) and Eq. 10."

Line 240: if horizontal wind indicates inhomogeneity, then correcting for the wind does not solve that problem. Or are you using the wind field in some other way? If so please explain that in more detail. In your comment AR1-2 you say that you use the wind field as a measure of homogeneity, how do you do that? What are the criteria then that say the field is homogeneous enough to be considered for the retrieval? You further said in your comment AR1-4 that "moving the Doppler spectra to 0 does not have the same effect" how is that possible? With the wind correction you are doing exactly that: shifting the Doppler spectrum by an arbitrary number. The elevation dependency of the Doppler spectrum width has nothing to do with the horizontal wind, but with the viewing geometry of the radar. In fact, looking at our own data, the spectral width is exactly the same if viewed at e.g. 30° or 90° elevation without correcting for the wind. The width of the spectrum is influenced by wind shear and turbulence, not the wind speed. In comment AR1-4 you further say that you are using the Doppler spectrum width as a measure for homogeneity. This is not the same as the answer in AR1-2, where you say you use the wind field. Which of the two are you using and how exactly?

Line 240: Thank you for your detailed and very helpful comments regarding the use of horizontal wind and Doppler spectrum width as measures of homogeneity and the wind correction procedure. Concerning the question of Reviewer #2, if we do use the the wind field in some other way, we would like to emphasize that the overall scheme of de-aliasing and horizontal-wind correction was developed as one 'package' in the framework of the development of the spectrally-resolved approach. While the de-aliasing is definitely a must-have for the technique, the horizontal wind correction was implemented as a valuable add on. The reasons are listed again below:

1. **Use of horizontal wind as a measure of homogeneity:**
   We do not simply treat horizontal wind speed as a direct indicator of homogeneity or inhomogeneity. Instead, the variability or consistency of the horizontal wind field over the radar sampling volume and time is used as an indicator. In other words, if the wind field is stable and uniform (low spatial and temporal variability), we consider the volume to be more homogeneous, which supports the assumptions underlying the retrieval. Conversely, high variability suggests inhomogeneity.
2. **Wind correction and shifting the Doppler spectrum:**
   The wind correction applied shifts the Doppler spectra to center the dominant fall velocity around zero, compensating for the mean horizontal wind's Doppler shift component projected onto the radar line-of-sight. However, this correction does not remove the underlying spectral width or its physical causes such as turbulence or wind shear. Therefore, shifting the spectrum by the mean wind velocity is not equivalent to removing the spectral broadening or inhomogeneity. This distinction is crucial and will be elaborated in the revised text.
3. **Elevation dependency of the Doppler spectrum width:**
   You are correct that the spectral width's elevation dependence is primarily due to the radar viewing geometry and physical processes like turbulence and wind shear, not simply the mean wind speed.
4. **Distinction between wind field and Doppler spectrum width as homogeneity measures:**
   We actually only use the homogeneity of the wind field as a quality criteria. We apologize for the raised confusion and for the inconsistent statements about the application of spectral width as criteria for evaluation of the horizontal homogeneity in the first revision round.

Having written this, we acknowledge that the horizontal wind correction is not absolutely necessary for the spectrally resolved approach. But it provides additional constraints for the interpretation of the retrieval.

Specific comments:

Line 186: please define vdsp

Line 186: We added a brief description at line 164 defining the VDPS method as the vertical distribution of particle shape.

Line 259: I would name the 5 shapes here specifically, so that the reader knows what you want to distinguish

Line 259: While we cannot definitively assign specific hydrometeor shapes to each of the five spectral parts due to overlap and mixtures, we provide here a general description of the typical particle types expected to dominate different velocity ranges. We added further information about the typical fall velocities of hydrometeors to lines 63-66.

Line 284: I still don't understand the word harmonize, or the sentence behind it, this was not made clear to me in AR1-4! Please elaborate on that further! First: what does harmonize mean. Second: why do you need to do that for retrieving the wind field?

Line 284: The horizontal wind causes shifts in the observed Doppler velocity depending on the radar's elevation angle, complicating direct comparison of spectra across angles. To retrieve a consistent vertical wind profile and particle fall velocities, these horizontal wind-induced shifts must be accounted for (i.e., harmonized) across all elevation angles. Without this step, the Doppler spectra cannot be reliably compared or combined to infer the wind field and hydrometeor properties accurately.

Figure 1 and comment AR1-26: your statement that turbulence is represented by an upward motion of particles is wrong. Turbulence can go upwards and downwards. Folded with the particles movements, it broadens the spectrum. Most of the times this does not even result in an actual upward motion of your particles, so indicating turbulence in Figure 1 as the distance between the 0m/s and the slow edge is wrong. Further, turbulence flattens the spectrum. So if you want to include turbulence in the figure please include all aspects correctly or just don't put it in the figure!

Figure 1 and comment AR1-26: We removed the turbulence effect in the Figure and added in the caption that turbulent motion is not considered in the sketch.

Fig 9 is different from the preprint, why? Now you corrected line 474, but now it is clearly wrong again, if you take your new figure. Now I would say that part 1,5 have a pol. Ratio of 0.75 and part 2,3,4 of 1.25 (also clearly not 0.9 and 1.1 if you look at the figure)

Fig 9: We apologize for the inconsistency between Fig. 9 and the preprint. The figure was updated to reflect the latest analysis. You are correct that the polarization ratio values for parts 1 and 5 should be around 0.75, while parts 2, 3, and 4 are closer to 1.25. We corrected the numbers in the text between lines 450 and 455.

Perhaps you can explain to me again why in case study 1 you come to the conclusion that you have no SIP and in case study 2 you say you have SIP. Just because you could not see a liquid layer in which ice crystals might have been nucleated does not mean it is not there, especially since you clearly have riming in case study 2. So your argument for no SIP in case study 1 can also be given here for case study 2.

In **case study 1**, we analyze two time periods. The first one is a deep cloud system. Ice particles from a higher-level cloud layer sediment through a supercooled liquid layer. At the height of the layer, we identified columnar ice. Just from this time period, one would conclude that the columnar ice was formed by SIP during the seeding event. But, shortely afterward, the lower-level supercooled liquid cloud was still present withouth any seeding cloud above. But still, this supercooled liquid cloud formed columnar ice crystals. As seeding is absent during this time period, one can exclude SIP. It is more likely that the ice in the lower-level supercooled liquid layer formed primarily.

In contrast, **case study 2** the retrieval identified oblate-shaped particles at all levels of the cloud layer. Only at heights below 2.8 km and the melting layer suddenly columnar crystals appear, as well, in the lowest-falling Doppler spectra part. We base our conclusion that this is SIP, because the time-height cross-sections of the radar variables (Fig. 10) show an unsteady, patchy, occurrence of sigantures in the height range where we suspect the SIP. In contrast, during case study 1, the columnar ice was formed rather constantly over time.

Nevertheless, we acknowledge the concern of Reviewer #2 that indeed also in this case formation of primary ice similar to Case Study 1 could have happen. Since precipitation sediments constantly from higher altitudes, we cannot evaluate (a) if there was a liquid layer at 2.8 km height, and (b) if that layer would have formed primary ice even without the seeding by the higher-level cloud. We acknowledge this fact at the end of the discussion of case study 2 in lines 560-582.

Line 603-605: I don't understand this sentence! You assume Rayleigh, therefore your particles have to be small with respect to the wavelength. This does not mean that you can accurately model the scattering properties of all ice particles. The statement that it "ensures the proper treatment of scattering properties which is critical for precise radar measurements and interpretations" does not make sense to me. How does your assumption of Rayleigh ensure that? Anyway, maybe rephrasing that helps, or just leaving the entire sentence out, as all the important things are already said with the sentence before.

Line 603-605: We agree that the original sentence may have been misleading. The assumption of Rayleigh scattering implies that particles are small relative to the radar wavelength, which simplifies the scattering calculations. However, it does not guarantee perfectly accurate modeling of all ice particle scattering properties, especially for larger or complex shapes. We agree that the original wording was misleading. We have modified the text to clarify that the Rayleigh assumption (lines 595-596). "provides a consistent framework for approximating scattering properties, which is important for interpreting radar measurements." This phrasing better reflects the limitations and practical benefits of the assumption without implying complete accuracy.

Line 611-614: I don't understand what you mean with "this was not applied yet in the present study"? I thought you separated into 5 parts already?

Line 611-614: The phrase "this was not applied yet in the present study" refers to the fact that, although the spectrally-resolved retrieval method allows exclusion of the fastest-falling Doppler spectrum parts—typically corresponding to the largest particles prone to non-Rayleigh scattering—we did not implement this exclusion in the current analysis. We plan to apply this filtering approach in future follow-up studies to improve retrieval accuracy.

Line 615: What do the non-Rayleigh effects have to do with horizontal homogeneity?

Line 615: Non-Rayleigh effects and horizontal homogeneity are both challenges we consider in the analysis, but they address different issues and are not necessarily directly related.

Line 620: what does "appropriateness of horizontal wind correction" mean? Please be more specific with your method!

Line 620: It refers to the effectiveness and suitability of the correction method in isolating the vertical fall velocities of hydrometeors from the measured radar signal.

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