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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We sincerely thank both reviewers for their valuable feedback and constructive comments. Their input has greatly helped to improve the quality and clarity of the manuscript, to our opinion. We hope that the reviewers share this notion. Below, we aim to respond to the issues they raised on a point-by-point basis. We decided to provide the replies to both reviewers within one review letter. Reason is that some comments were raised by both reviewers. We took the liberty to point to a given reply if a reviewer comment is similar to a previous one.

Throughout our reply letter, we use the following abbreviations:

– R#C$: Comment number $ by Reviewer number #
– AR#-$: Authors’ response to comment number $ of reviewer number #

Response to Reviewer #2

Major:

R1C1 (splitted into individual parts):

A) the text needs a lot of rewriting, both structurally and for a better understanding of a few sentences (which I will specify in the specific comments). In my opinion, the new method is not explained satisfactorily. I am missing a detailed explanation on why you chose to divide the spectrum into 5 parts, and how you are dividing the spectrum, which has not been mentioned anywhere. I do not know which 5 species/shapes you want to cover with that, I can only think of rimed particles, aggregates, plate-like crystals, columnar crystals. Of course you can also have super-cooled liquid water, but for that we know the shape very well, so no retrieval is necessary. I would also like to know if you are trying to "track" the different species throughout the different heights, or if the division of the spectrum is just random.

B) Also, please explain the main peak method better, for me it was difficult to understand that. Other more "trivial" retrievals such as the vertical wind velocity are explained in great detail, even though it is not that relevant to the study and many institutions are running wind retrievals on an automatically basis. I would suggest to rewrite the paper the following way:

– I like Figure 6 and 7, perhaps you can move that to the method section? Then it might be clearer how the retrieval works. If you include the full RHI scan in Figure 6 (without the separation into the 5 parts) and 7, you can first explain the main peak method and then the spectral approach in a clearer way. I would first explain in detail which peak is used in the main peak approach, indicate that in the figures and then continue on to your new approach and show how that is different and how you are dividing into 5 parts. How are you then using the 5 parts? Are you averaging along the Doppler velocity of each part? If so, it might be helpful to provide the averaged elevation dependencies of ZDR and RhoHV either in addition to the Doppler spectra parts or instead of them. Also you should specify what the polarisability ratio and the degree of orientation are and provide the formulas. This has of course been mentioned in Myagkov et al. 2016, however, these are not standard variables and I think it is therefore necessary to explain that again. You can even use panel a and c of their figure 13 to show the polarisability ratio and orientation. When you are explaining polarizability ratio you can also mention that the shape retrieval can be used with sZDR and sSLDR.

AR1-1: Thanks for these detailed comments and suggestions.

AR1-1A: Our main objective is to extend the technique developed by Myagkov et al. 2016 to probe cloud layers for the co-occurrence of multiple hydrometeor types. Thereby, we don't focus on any specific hydrometeor type. We basically decided to evaluate the Doppler spectra for the occurrence of a maximum of 5 different hydrometeor populations. We selected 5 parts because it would to our opinion cover the maximum of different hydrometeor types, namely droplets, pristine ice, rimed ice, aggregates and rain/drizzle. To achieve the spectral separation, we identified the starting and ending points of the Doppler spectra for each pair of range and elevation angle and then divided the spectra's width into five equal parts. Subsequently, we calculated the average ZDR and RHV values for each part to provide a single representative value for the next stage of analysis (comparison stage), similar to the main-peak approach. This explanation has been included in the text. The motivation and introduction of the spectrally resolved approach in the manuscript has been extended and now spans across lines 245-282 of the revised manuscript.

A tracking of particles across different height levels is not performed, but is a very good point for future improvements of the technique. This point was thus added to the conclusions section.

AR1-1B:

In order to improve the explanation of the main-peak technique, we modified the text (lines 187 to 202) and expanded on the concepts of polarizability ratio and degree of orientation in Section 3.1. This includes incorporating the relevant formulas to review these concepts as presented in Myagkov et al. (2016). While we acknowledge the suggestion of R#1 to move Figs. 6 and 7 to the methods section, we decided to not do so. Both figures require a lot of introduction which is much better placed in the results section.

We included RHI scans of SNR, ZDR, and RHV for the main-peak approach in both case studies (Fig. 5, Fig. 11). RHI scans for the spectrally resolved approach can be found in Fig. 7 and Fig. 15. An important observation from the RHI scans for both case studies is that the ZDR and RHV profiles in the main-peak approach closely resemble those in parts 1 to 4 of the spectrally resolved approach. However, the distinct ZDR and RHV signatures in part 5 of the spectrally resolved approach indicate the presence of a different hydrometeor type. In the main-peak approach, we select the peak of the SNR spectrum, as illustrated in the Fig 1a. For the spectrally resolved approach, the spectrum is divided into five parts, as shown in the Fig 1b. It is important to note that the main peak in the main-peak approach does not necessarily correspond to any of the five parts in the spectrally resolved approach, since the spectra are split into equally spaced (by means of Doppler velocity) parts, without considering the occurrence of individual peaks. Based on experiences made in the course of the development of the spectrally-resolved approach, we were convinced that individual spectral peaks are hardly detectable at large off-zenith angles due to spectra-broadening effects by the horizontal wind.

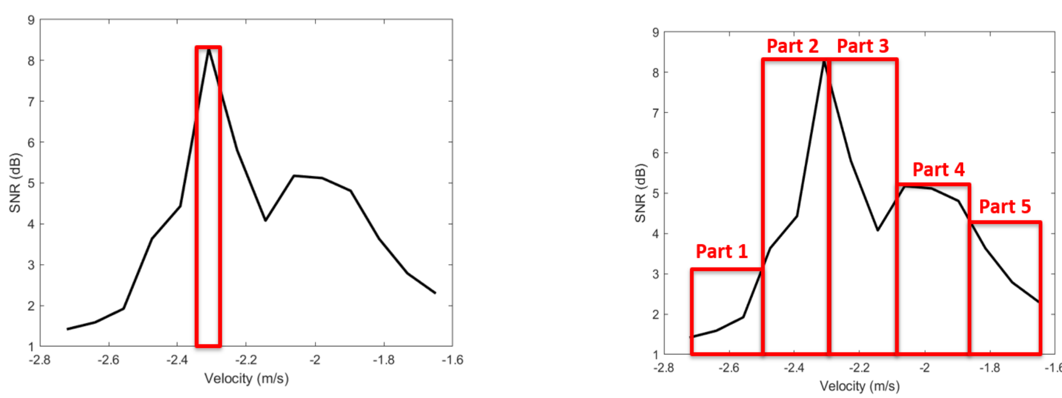

Fig 1. (a): main peak selection in the main peak approach. (b): 5 parts of Doppler spectra after splitting in the spectrally resolved approach

Concerning the request to mention that the shape retrieval can be also used with sZDR and sSLDR, we acknowledge this fact now at the end of Section 3.1 (lines: 229-231)

R1C2:

I am missing in this paper a few clear statements about the problems of the method. i.e. you should critically discuss the scattering properties that you are using. You did not specify which spheroidal method you are using, in Myagkov et al. the retrieval is based on Rayleigh. It is well known that spheroidal methods have issues of representing the scattering of ice particles accurately, especially in the Mie-region (which is reached at Ka-Band for aggregates) and for low density ice particles such as aggregates. I understand that generating a large DDA database which is necessary to do such a retrieval is difficult. However, I know that other working groups have done that and you could collaborate with them. I think that especially now that you want to retrieve the shape of multiple species a different scattering method should be taken into account. In my opinion it would have been beneficial to collaborate with other groups who have sophisticated scattering databases based on e.g. DDA. Another thing to discuss: can you be sure that along all elevation angles the species in one height doesn't change? For larger heights this spans quite a large cloud area

AR1-2: We did not modify the spheroidal model used in the main-peak approach. In fact, our goal was to take the scientifically very exciting step to extend the existing main-peak approach by analyzing the full Doppler spectra. For this study, we restricted the analysis to cases where the larger particles in part 1 were likely to be small, based on their fall speed.

The evaluation of alternative models, such as the Discrete Dipole Approximation (DDA), to enhance the analysis, shall be left for future studies. We personally doubt that the current retrieval framework would work in case of DDA. Reason is, that the spheroidal model used by Myagkov et al. (2016a) was able to use a direct relationship between the measured parameters ZDR and RHV and the microphysical properties of polarizability ratio and degree of orientation, respectively (two observables, two unknowns). This is only possible because the spheroidal model is independent of particle size and concentration. As DDA requires also size and concentration as input parameters, a completely new approach would be required in order to relate the observables with the microphysical parameters. E.g., one could imagine that such an extended, DDA-compatible retrieval approach might be possible if all novel technical developments from the recent years would be included, such as spectrally-resolved reflectivity and dual- or triple-wavelength ratio of the latter for size information, and (spectral) KDP for density and number information. All of these for different elevation angles. While multi-frequency polarimetric datasets do meanwhile exist (e.g, TRIPEX-pol; von Terzi et al., 2022), they are not widely available (Kneifel et al.,2011; Leinonen et al., 2013; Chellini et al., 2024), and are usually obtained for fixed elevation angles, only. The issue was extensively discussed in the course of the publication process of the study of Teisseire et al. (2024, see the discussion at https://amt.copernicus.org/articles/17/999/2024/amt-17-999-2024-discussion.html) and was also previously communicated in a study of Matrosov, (2021), and shall not be repeated here.

One of the assumptions of the main-peak approach is homogeneity at each height, and this assumption was retained in the spectrally resolved approach. However, the consideration of five distinct hydrometeor types at each height actually already introduces an implicit assumption of inhomogeneity of the particle habits within the volume. The approaches to evaluate horizontal homogeneity, which we applied, were (1) to require a minimum amount of data points at each elevation angle of an RHI scan, and (2) to evaluate monotonic behavior of the elevation-dependency of ZDR and RHV. As can be seen in the theoretical model of the relationship between ZDR, RHV, polarizability ratio and degree of orientation, the elevation dependency is always required to be monotonic. When no monotonic slope of the elevation dependency of ZDR and RHV is detected, then the presence of inhomogeneities is likely. (3) The applied correction of the horizontal wind field on the elevation-dependency of the Doppler spectra can be used as a measure of cloud homogeneity. We added these three measures of homogeneity to lines 232 - 243 of section 3.1. To conclude the given statements, the evaluation of the horizontal homogeneity is in practice still subject to experienced-eye check, taking into account the above-mentioned 3 constraints. Cases, where the above-mentioned criteria are not met shall be excluded from the analysis.

We would like to note here, aside to the given evaluation of the homogeneity in our retrieval, that retrievals that are applied to standard-measurements of precipitation radar network instruments are much more relaxed regarding the assumption of horizontal homogeneity. One example is the prominent approach of obtaining so-called quasi-vertical profiles (QVP) by averging over a range of azimuth (of up to 360°) of fixed-elevation (down to between 6.4° and 28°) precipitation radar scans (Ryzhkov et al, 2017). QVP approaches thus assume horizontal homogeneity over hundreds of square kilometers.

R1C3:

Along with comment 3: aggregates have a small polarimetric signature, especially if you are assuming a spheroidal scattering approximation. How much more insight do you even gain when you are considering the spectral lines with aggregates? I am expecting that similar to rimed particles they will just have a polarizability ratio of close to 1. Can you even distinguish between these different particles then? If you can only distinguish between them because they have a different fall velocity is it even worth splitting the Doppler spectra into 5 parts? Would it not be beneficial to just separate between ice crystals and aggregates? I would suggest to show the theoretical polarizability ratio you would expect for all your 5 different species (which I am assuming to be rimed particles, aggregates, dendritic ice crystals and columnar ice crystals and something else). This would show that a separation into 5 parts is actually necessary and that not i.e. 2 parts would suffice.

AR1-3: It is correct that by using only the spectrally resolved approach, it is not possible to distinguish between rimed and aggregated particles, because the polarizability ratio for both is approximately 1. Additional information is required to differentiate between them. One approach is to check for the presence of liquid droplets in regions where the polarizability ratio approaches 1. If liquid droplets are detected, it can be inferred that riming is occurring. Otherwise, the most likely interpretation is aggregation. Furthermore, dual- or triple-wavelength ratios from multi-frequency cloud radar observations can help to distinguish riming from aggregation. Teisseire et al., (2025) combined a modified version of the main-peak approach with liquid-detection retrievals and dual-frequency radar observations to show the added value of the shape and orientation retrieval for cloud microphysics retrievals. As already outlined before in this reply letter, the initial version of the spectrally resolved approach, that is presented in this study, aims on a general evaluation of the potential presence of different hydrometeor types, without the demand to apply any a-priori assumption about which particle types are present. An application of the spectrally-resolved shape retrieval for an actual (microphysical) hydrometeor type classification is subject to future studies. It is important to clarify that we did not intend to associate each part with a specific hydrometeor type. Depending on the location within the Doppler spectrum, the parts may be close to 0 m s^{-1}, indicating small particles, or further from 0 m s^{-1}, representing larger particles. As a result, associating each part with a specific hydrometeor type is not feasible. The polarizability ratio and degree of orientation for each part reflect the shape and orientation of the particles that constitute that part. Overall, while the spectrally resolved approach enhances our understanding of clouds by enabling the retrieval of multiple hydrometeor types, its greatest strength lies in its ability to significantly improve the interpretation of microphysical processes when combined with additional data inputs. We added a passage to the conclusions section which discusses the focus of the in-here presented first/plain version of the spectrally resolved approach.

Regarding the request to present polarizability ratios for different types of hydrometeors, we'd like to point the reviewer to Fig.1 in the study of Myagkov et al., (2016a). We added a reference to this figure into our manuscript in line 237.

R1C4:

Why do you need to retrieve the actual air motion? Do you use the Doppler velocity for anything and therefore a knowledge of the fall velocity of the particles are valuable? Otherwise you could just move the Doppler spectrum to 0m/s. This would save a lot of effort and space in the paper.

AR1-4: The Doppler spectra correction includes three components: folding/aliasing correction, elevation-angle correction, and horizontal wind correction. Just to move the spectrum to zero m/s would not lead to the same result and would lead to Doppler spectra whose width is also influenced by the elevation angle. This would in turn potentially affect the number of selected data points per Doppler spectral part. In addition, as written already in AR1-2, we consider it valuable to use the homogeneity of the Doppler-spectrum width at different elevation angles as one additional measure for evaluation of the cloud homogeneity. These aspects motivated us to apply the general wind-field correction, which we want to get published in addition as guideline for future studies.

Additionally, retrieval results can be obtained based on the mean Doppler velocity of each part. Figure 2 below illustrates the results for November 7, 2014, during the period from 09:16 to 09:18 UTC. We did not use this representation in the current manuscript as it is planned to be used in a future publication.

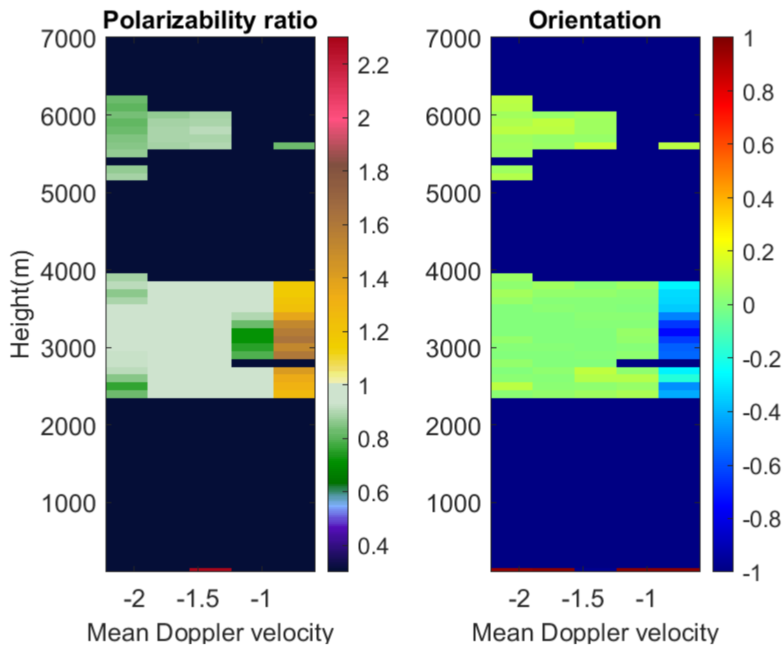

Fig 2. Illustration of the spectral shape retrieval for a RHI scan from Cabauw, NL, observed on November 7, 2014, 09:16-09:18 UTC. Shown are profiles of (left panel) polarizability ratio and (right panel) degree of orientation as a function of mean Doppler velocity of each of the 5 parts of the spectral shape retrieval. Left-hand columns represent part 1, right-hand columns represent part 5.

Minor:

R1C5:

discuss your figures better/more in the text. Especially Fig 4 and Fig. 9 are not discussed enough. Most readers are not familiar with all the Radar and Lidar variables and can not draw their own conclusions.

AR1-5: We improved the discussion of the figures as requested. This hopefully will also help the future reader to get a better introduction to the case studies.

R1C6:

In this regard: why do you show the Lidar measurements? Due to the rain the Lidar doesn't even penetrate into the relevant cloud regions, so you can just omit the plots.

AR1-6: In the first case study, the lidar is very helpful. In the course of this revision we carefully introduce that the lidar information is key to identify the layer of supercooled liquid water. For the second case study (now Fig. 10) we followed the suggestion of the reviewer and removed the lidar panels. There, we only note in the text, that the lidar measurement does not provide valuable information due to strong attenuation of the signal above the melting layer.

R1C7:

You need to refine your naming in the equations better. For example, before equation 1 you say that E_h is the horizontally polarized plane of the received wave. You never specified what E dot means. Then in equation 4 you say that E_h is the copolar element of the backscattering matrix. Please be specific with your definitions, and stick with one! (also co and cross-polar are not the same thing as horizontal and vertically polarized)

AR1-7: The dot above each word indicates that the parameter is complex. Additionally, we removed the definition of RHV, as it was also mentioned by the second reviewer. Additionally, we removed the explanation of RHV including co-polar and cross polar.

R1C8:

In your conclusions I am missing a clear outlook and a discussion of the method and its caveats.

AR1-8: As suggested by the reviewer, we extended the conclusions and outlook section. Especially with respect to the caveats which were also elaborated on in the course of this review.

R1C9:

The colormap in your Figures looks like jet, perhaps you could consider moving towards a colormap which does not have so many deficiencies, if you prefer rainbow you could use e.g. turbo or something similar.

AR1-9: We evaluated all figures with the colorblind tester https://www.color-blindness.com/coblis-color-blindness-simulator/ and could not find any loss of information by using the current color scheme.

R1C10:

why are you only analyzing the first time period of 7.11.2014 with your spectral retrieval? The second time period would have been interesting, since there the polarimetric variables are large!

AR1-10: We only show results for the first time period because only there multiple hydrometeor types were located. For the sake of saving figure space, we thus presented only the main-peak result of the second time period, which could easily be added as sub-panels to Figs. 5 and 6.

Specific comments:

R1C11:

Line 47-48: you say peak signal of the Doppler spectrum, could you specify which variable you mean? Spectral reflectivity? Spectral SNR? Spectral ZDR?

AR1-11: We selected the peak of the spectral SNR and do mention this now at this location in the text (lines 73-74).

R1C12:

Line 61: you say "the polarimetric variables exhibit sensitivities to specific fall velocities" This is not true please specify this sentence

AR1-12: We removed this statement from the manuscript text.

R1C-13:

Line64: Large aggregates do not fall faster than 1m/s, their fall velocity saturates around 1m/s. The Doppler velocity is therefore often used to distinguish between rimed particles and aggregates. Please correct!

AR1-13: Thanks for the hint. We corrected the passage accordingly. See lines 89-91.

R1C14:

The super-cooled liquid water layers are not visible in your case studies because the lidar doesn't penetrate the rain

AR1-14: As stated in AR1-6, we see good reason for presenting and discussing the lidar observations in Fig. 4. In the case of Fig. 9 (Fig 10 in the revised version), we removed the lidar panels.

R1C15:

Eq. 1-4: see minor comments

AR1-15: We applied the correction and explained it in the minor comment section. See AR1-7.

R1C16:

Line 125-126: your statement that prolate particles have a negative ZDR is not true. This is only valid if they are oriented in a very specific way. In my experience I have never seen negative ZDR that is only associated with prolate particles, it is mostly attributed to differential attenuation or conical graupel

AR1-16: Thank you for the hint. You are right. We corrected the text passage: "At zenith-pointing direction, ZDR is zero. A positive ZDR value may indicate ice particles that are horizontally aligned, whereas a negative value might suggest particles aligned vertically."

R1C17:

Eq. 7 and paragraph below that: why do you describe LDR in such a detailed way? Your retrieval is based on ZDR and RhoHV and those are the relevant parameters.

AR1-17: We shortened the text as suggested.

R1C18:

Line 166: please specify the scattering model you use!

AR1-18: We added information on the scattering model and the respective references to Section 3.1.

R1C19:

Section 3.1: as mentioned in the major comments: please introduce polarizability ratio further, how do you calculate it? How are sZDR and RHV used for that?

AR1-19: In the middle of Section 3.1 (lines 197–225), we added a detailed explanation along with Equations (8) to (11) to describe the polarizability ratio and the degree of orientation, as well as how ZDR and RHV can be derived from these parameters

R1C20:

Line 187: Also small particles form distinct peaks in the Doppler spectrum (as you can see in your Figure 12)

AR1-20: We rephrased the passage to achieve a more general description: "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 winds that cause additional broadening of the spectrum Radenz et al (2019)." See lines 250-253 of the revised manuscript.

R1C21:

Line 189-192: please rephrase this sentence, hard to understand

AR1-21: We rephrased it like this in the text: "This study extends the main-peak shape and orientation retrieval through the spectrally resolved approach, which assumes that different hydrometeor types in a cloud volume are separated by their distinct fall speeds, as explained in the previous paragraph." See lines 254-256 of the revised manuscript.

R1C22:

Line 198: if you say not more than 5 shapes can be present please name those shapes. I would even refer to this as particle types, because if you consider dendritic particles, they can have many different shapes

AR1-22: Separating into five parts does not imply there are exactly five distinct hydrometeor types. This number was chosen as the best estimate for the number of hydrometeor types and also to effectively represent the existing shapes (see also AR1-1). We added additional text to the beginning of Section 3.2 to improve the introduction and motivation of our approach (as was also requested in the major comments).

R1C23:

Line 211-212: I don't understand why you need to "harmonize" the Doppler spectra in order to derive the vertical wind, isn't it the opposite way around? You retrieve the wind from the PPI scans in order to match the Doppler spectra across different heights/elevation angles?

AR1-23: See AR1-4**.**

R1C24:

Figure 1: Specify which Doppler spectrum (sSNR? SZDR?). Usually Doppler spectrum refers to spectral Ze, which you are not using here correct? Also this sketch is too idealized. I have never seen a Doppler spectrum with 5 distinct peaks. How do you know that columns are falling faster than small dendrites but slower than large dendrites? In my opinion you don't need that plot, but rather a good explanation of how you separate into 5 parts!

AR1-24: We replaced the figure with a more realistic one. The illustration of different shapes is intended as a visual aid to show that distinct shapes can be separated based on their velocities. It does not imply that a specific shape consistently has a higher or lower speed.

R1C25:

Figure 2: it is nice to have a block diagram, however, you should also describe it in the text! e.g. in the text it is not mentioned that you are using minimum square error function which is an important detail.

AR1-25: The description of Fig. 2 was extended and now covers lines 284-295. We added the description of the minimum mean square error function to the text.

R1C26:

Line 221-224: I don't see how you depicted turbulence in Figure 1.

AR1-26: Turbulence can be represented by the upward motion of particles, indicating a positive velocity. In addition, it leads to the 'smearing' of individual Doppler peaks. Both aspects are considered in the revised version of Fig. 1.

R1C27: Equation 8: what is Vw? What is Vh?

AR1-27: V_h represents the velocity of the horizontal wind and V_w represents the map of horizontal wind to radar line of sight direction. We calculated V_w to be able to remove the effect of wind from the radial velocity of hydrometeors measured by radar (line 292-303).

R1C28:

Figure 3: I do not understand this figure! Since the wind retrieval is something that is frequently done, this is not necessary. But if you want to leave it in you have to work on that. A few things that I don't understand are: What are the dashed lines? Why is Vh in the dashed line not the same as the grey Vh line? I don't understand the beta angle or the alpha angle in this context.

AR1-28: We revised and cleaned the figure, removing beta, which indicated the azimuth angle of the radar. We focused on retaining only the parameters related to wind. Alpha also represents the wind direction angle.

R1C29:

Paragraph about aliasing: how do you determine n?

AR1-29: In this study, since the wind speed was not very strong, the aliasing issue was consistently resolved by setting n = 1. In this case, folding occurs at low elevation angles (between 30-60◦ or 120-150◦). However, in the presence of stronger winds, folding begins at elevation angles closer to the zenith (between 60-90 ◦ or 90-120◦), requiring the consideration of n > 1 (lines 339-343).

R1C30:

Paragraph horizontal wind: do you need to know the true fall speed? See major comment 4

AR1-30: No, knowledge of the true fall speed is not required. See AR1-4 for more details.

R1C31:

Figure 4: Why is the 0° isotherm not at the same height as the melting layer? Looks like a 500m difference here! Also: if you want to include the lidar measurements please change the colormap you use, I can not see anything in panel f

AR1-31: The temperature information is taken from a model (gdas1) which can therefore deviate to some extent from the true temperatures. Also, it should be noted that melting usually occurs at heights below the height level of the 0°C isotherm. This is the case because melting generally only starts when the wet-bulb temperature approaches temperatures greater than 0°C. This can also explain up to a few hundred meters of height difference to the 0°C air temperature. See Ryzhkov and Krause, 2022.

R1C32:

Line 294-296: How do you know that there is liquid all the way to the cloud top? The lidar doesn't penetrate through the rain.

AR1-32: Thank you for pointing us to this typo. We meant 'ice phase' and corrected the position accordingly.

R1C33:

Please discuss the Figure in more detail. I am missing more discussion of LDR, what does that mean, mean Doppler velocity, what can that tell you about the particles,...

AR1-33: We thank the reviewer for pointing us to the lack of detailed introduction to the case study. As suggested, we extended the introduction and discussion of the 7-November 2014 case study considerably. We now involve all sub-panels from Fig. 4 and provide already at this introduction stage some suggestions about the associated microphysical cloud properties. See lines 360 to 389 of the revised manuscript.

R1C34:

Line 303: what does "transition towards strong spherical particles" mean? Also: I barely see a tendency if I look at Figure 5 I would like to see plots of your RHI scans, similarly to how you have done it in Figure 6. I would suggest to either discuss the radar observations of Figure 4 in more detail to show how they are relevant, or only show Ze, LDR and then the RHI scans.

AR1-34: Indeed, the increase of polarizability ratio between 4 and 2.5 km height is too weak to motivate the given emphasize on sphericity. We therefore decided to remove this statement.

R1C35:

Figure 5: in your Figure 4 you show that you have the temperature information, It would be helpful to have that also in this figure to draw conclusions about the ice particles (i.e. ice particle habits are strongly dependent on temperature)

AR1-35: Thanks for the suggestion to improve the usability of the results plots. We now added the GDAS1 temperature levels to all figures showing profiles of polarizability ratio and degree of orientation.

R1C36:

Line 314: I would not say it “effectively” identifies the shape, since you are not comparing against other measurements you do not know it that is actually true

AR1-36: The performance of the main-peak approach has already been evaluated with laboratory results, which showed good agreement. Based on this evaluation, we assume that the current results are also reliable.

R1C37:

Line 315 and following: I don’t understand the discussion here. The sentences are really hard to understand. Do you mean that because there are prolate shaped particles in the time between 09:46 and 09:48 they also have to have been present earlier? What do you base that on? You are saying that the cloud changes drastically, therefore I would not compare the two time periods and draw conclusions on the microphysics that are happening.

AR1-37: What we meant with ‘the cloud changes drastically’ was that its vertical structure changed from a seeder-feeder constellation (09:15-09:30) to a single-layer stratiform mixed-phase cloud (09:30-10:00). Based on the observations we consider our conclusion true that the same layer of supercooled liquid water with precipitating columnar ice crystals, which was observed from 09:30-10:00 (see Fig. 4), was also present between 09:15 and 09:30 when the cloud was deeper and the second, more isometric particle shape was present. We see this constellation as an evidence, that the prolate ice formed heterogeneously in the thin layer of supercooled liquid water. No seeding by the higher-level cloud and thus no rime-splintering was required to explain the observed mode of prolate ice crystal shapes.

R1C38:

Figure 6: I would suggest to also show the RHI of the complete spectrum (see also major comments). I would also adjust the colorbar of ZDR and RhoHV, because I can not see any tendency in the variables. ZDR for part 1-4 looks like it is close to 0 for all heights and elevations. Would it not be nice to show the method on a case that exhibits larger polarimetric signatures? Then the benefit of having a spectrally resolved retrieval would be more obvious. Here even the slowest falling particles show barely any ZDR.

AR1-38: We added the RHI scan for the main-peak approach. Additionally, we would like to point out that, using Figure 6, we aim to show that the profiles of parts 1 to 4 are similar to the main-peak profile. The only noticeable difference is in part 5, where the ZDR and RHV above the melting layer at low elevation angles exhibit a different signature, as shown in this figure.

R1C39:

Line 328: You say dealing with noise is a challenge, yet you do not say how you deal with it! when the SNR is too low.

AR1-39: We modified the text ackordingly: "Managing noise can be challenging in the spectrally resolved approach, when the SNR is too low.". What I meant is that if the SNR is too low (in this work, less than 10 dB), the data becomes heavily contaminated, making retrieval impossible. However, in these cases, the SNR was not excessively low. By using RHI scans, we can assess the extent of noise contamination in the data. In these instances, while the data is contaminated, it is not to a degree that would lead to incorrect results.

R1C40:

Line 330-331: please rephrase the sentence "this diminished SNR … fails to reflect in ZDR and RHV" how do you know that?

AR1-40: We rephrased the statement using the following text: "At higher altitudes, the low SNR in part 1 (representing the fastest falling particles) of the Doppler spectra prevents the complete representation of ZDR and RHV profiles." At higher altitudes, which are not shown here, the SNR profile exhibits very low values. However, at the same altitude, there is no detectable signature of ZDR and RHV.

R1C41:

Line 337: ZDR is always really close to 0, I do not see a tendency, so I would just assume that the particles are nearly spherical. Perhaps if you change the colormap it will be visible.

AR1-41: Yes, it was typo. We replaced with spherical shape.

R1C42:

Line 341: ZDR and RHV look nearly exactly the same to me as for part 1,2.

AR1-42: Yes, that's true. We replaced with spherical shape.

R1C43: Figure 7: While it looks nice to have all the separate doppler parts and it helps the understanding, it is really hard to see the elevation dependency of ZDR and RHV here. I would suggest to add another figure with that (I assume that you average the different parts over the Doppler velocity to obtain one value of ZDR (RHV) per elevation, so you can show that in the figure)

AR1-43: Our goal was to highlight that the elevation dependency of ZDR and RHV is only noticeable in part 5. The text regarding the other parts, as mentioned in the last two comments, was incorrect. We believe the figure is now presented more clearly.

R1C44: Line 365: do you mean part 2 and 3? Part 1 is barely existing here.

AR1-44: Yes, we corrected it and rephrased the text like this: At an altitude of approximately 6 km, the retrieved polarizability ratio for parts 2 and 3 is 0.9 and 1.1, respectively..

R1C45:

Line 372: are your particles transition into spherical particles? Or is turbulence removing the small ZDR signal that was present at higher altitudes? How can you tell the difference? Or are the largest prolate particles aggregating, therefore leaving only the small prolate particles which have a smaller ZDR? I do not think that particles can change their shape if they have already developed into distinct prolate shapes. For this analysis, again the temperature information would help

AR1-45: We don't mean that the shape is changing. Our interpretation is that as the polarizability ratio approaches 1, the shape is becoming more spherical, which is due to microphysical processes like aggregation or other factors. In fact, we observed that changes in shape are associated with microphysical processes, but we cannot specifically identify which processes were involved.

R1C46:

Paragraph below Line 373: I do not agree with this analysis. First of all, your SNR was too low to retrieve the shape of the particles which seeded into the region below 4km. This does not mean however that there where no prolate particles that seeded. In addition, your argument that in the later period you see prolate particles is in my opinion not an argument that the particles you had 15 minutes earlier were generated the same way. The cloud clearly changed drastically between the two time steps. In the second time period it is likely that ice particles where formed via a mixed layer at cloud top. However, if the same process was present I would have expected much higher spectral ZDR values to be present at a similar height in the first time period. Especially since in the second period the LDR is really large for the newly generated ice particles! For this discussion it would be really helpful to have the retrieval also for the second time period, so that it is possible to compare the polarisability ratio for the two cases. So if you want to draw any microphysical conclusions I would strongly suggest to include the retrieval of the second time period. I agree with your statement on SIP, however only because ZDR of the slowest falling part of the Doppler spectrum is so low. If there was SIP I would expect much larger values.

AR1-46: We acknowledge that Reviewer #1 shared his concerns about the interpretation of our first case study. Similar to as we replied in AR1-37, we however consider our interpretation valid. The application of the spectrally resolved approach definitely identified the co-location of a prolate-particle-bearing layer and a vertically extensive seeder cloud, which would suggest the occurrence of of secondary rime-splintering processes. In our case study, corroborated by lidar observations of low volume depolarization ratio, however found that the prolate particles were formed in a layer of supercooled liquid water, independent of the presence of any seeder cloud from above. We consider it scientifically relevant to highlight such an observation, as current studies in general interprete the co-location of a seeder cloud and prolate particles as an indication for the presence of secondary ice formation. To corroborate our interpretation, we extended the discussion of the case study (lines 483-490) where we also provide references.

R1C47:

Paragraph below 380: Nice discussion about SIP and the melting layer (ML), however, how is that relevant here? You do not have two LDR layers within the ML. I would rather discuss the large number of papers that have found elevated LDR/ZDR above the ML in the needle growth regime than that very special case that Dmitri Moisseev had.

AR1-47: In Figure 11, We showed that during the time interval from 20:40 to 20:47, there are two melting layers below the height of 1.85 km.

R1C48:

Line 396-397: Why are they not able to infer any information about the “background population”? They also have the Doppler spectrum so they are able to do that. In the Doppler spectrum the particles separate due to their different velocity

AR1-48: They use a vertically pointed radar and analyze LDR values to obtain general information about hydrometeors, incorporating other parameters like temperature. Based on this setup one cannot distinguish between (horizontally aligned) oblate and spherical particles (such as aggregates), since both produce low LDR due to their similar spherical cross-sections with respect to the radar line of sight. In contrast, our approach involves a scanning radar, where we analyze Doppler spectra at all elevation angles. This allows us to present a more detailed and comprehensive analysis of the hydrometeor shapes. Especially oblate particles can be better discriminated from spherical ones, as the cross-sections of both particle species differ strongly when observed at low elevation angles. Additionally, we rephrased with this text: “their method is not able to detailed information about the background population of ice crystals from which the secondary ice formed.”

R1C49:

Figure 9: do not include lidar here, in this case it really has no valuable information for your case study. And please discuss the figure in detail in the text, do not assume that the reader can deduce all necessary and important information by themselves.

AR1-49: As already requested/suggested above, the lidar panels were removed from Fig. 10 (previously Fig 9)

R1C50:

Line 409: Why do you use the time period between 20:01 and 20:11 for the main peak approach? Did you not specify that you are using the time period from 20:37 until 20:47? In the earlier time period there is barely any LDR signal, it is an unfair comparison then to use the main peak approach on a time period where the polarimetry is expected to be low. I would suggest to use the same time period as you are using for your spectral retrieval

AR1-50: We revised the text, updating the times to 20:30 and 20:45. Additionally, we corrected the title of Figure 12.

R1C51:

Is that even significant?

AR1-51: It’s not significant; it’s simply an explanation of the changes in the polarizability ratio.

R1C52:

Line 413: is your scattering approximation suited also for liquid?

AR1-52: No, we just wanted to demonstrate how this technique, using the spheroid scattering model, retrieves a very low polarizability ratio for liquid, which is incorrect. Of course, we focus only on ice, not liquid.

R1C53:

The main point is you have a second, slow falling mode which might indicate multiple ice species, especially since the slow falling particles show a different LDR!

AR1-53: We removed the references to figures from other papers and replaced the text below line 540 with the text below.

"Elevated radar reflectivity levels at 2.9 km and within the lower velocity range of the Doppler spectrum suggest the presence of different hydrometeor types. Additionally, a significant increase in LDR values, reaching about −17 dB, was observed from above the melting layer to 2.9 km in the slow-falling velocity range, indicating the dominance of prolate-shaped ice particles in this part of the spectrum."

R1C54:

Figure 11: not necessary, you can get all the information from the figure 9

AR1-54: We added a zoom-in into Fig. 13 to highlight the two melting layers.

R1C55:

Fig. 14 and 15 are nice, however, there are already many figures and by now the reader has understood how the retrieval should work. So focus on the figures with the polarisability ratio and degree of orientation!

AR1-55: These figures are essential for a better understanding of the spectrally resolved approach. Therefore, we believe it is important to present them before showing the final results.

R1C56:

Line 462-464: I do not see the indication of fragmentation of dendrites. What are you basing that on?

AR1-56: The second case study aims on revealing the shape of the seeding particles which are the prerequisite for the observed secondary ice formation / ice multiplication, which was observed just above the melting layer (as indicated in Fig. 18 by the sudden switch of the polarizability ratio toward prolate particles in Part 1 at 2.5 km and below). As the shape retrieval identified, all other parts show oblate structures, with a polarizability ratio of around 0.7. This is the reason for our conclusion that the seeding particles likely were dendritic/oblate ice crystals. Our motivation for this case study was simply that the vertical-stare approach of Li et al. 2021 was not able to distinguish whether the seeding particles (seeding the layer of secondary ice formation) were oblate or spherical. This is simply not possible, as both particle types produce low LDR in vertical-stare mode. We would thus propose to keep the conclusions for this case study (as shown in lines 574-585).

R1C57:

Line 481-484: while I agree with the statement, my opinion is that with the provided analysis you can not draw that conclusions (see comment above)

AR1-57: We hope that we provide the Reviewer #1 sufficient reasoning for our conclusion by means of our statements given in AR1-37 and AR1-46.

Response to Reviewer #2

Thanks for your comments and suggestions. As some of the comments have already been addressed in our responses to the comments of Reviewer #1, we took the liberty to point to the respective responses where applicable.

Comments/corrections/adding's

1) Introduction

R2C1:
Line 39: "Cloud Doppler radars, introduced by Wakasugi et al. (1986), provide backscattered signal...." Rephrase because cloud Doppler radars were not introduced by Wakasugi et al.
AR2-1: We have removed the reference Wakasugi et al. (1986).

2) Instrumentation

R2C2:
Can the authors provide a clear set-up of the measurements? The mode RHI is mentioned, but later in section 3.3.1 the retrieval of the horizontal wind using the PPI mode is discussed. Therefore, it is not clear to the reader what the measurement sequence is: a combination of RHI and PPI? Also, the rotation speed of the radar for RHI and PPI measurements should be provided.
Table 1: add the Doppler velocity resolution for both MIRA-35.

AR2-2: The requested information, including the PPI scan, the scanning speeds for both RHI and PPI, and the Doppler velocity resolution has been added to the text and table.

3) Mira-35 radar in hybrid mode

R2C3:
Line 116: "....as a function of Doppler velocity · ..." My suggestion: either "....as a function of Doppler angular frequency ..." or "....as a function of Doppler velocity v ..."

AR2-3: Since v is used for multiple velocity-related parameters, we have chosen to use ω for Doppler velocity.

R2C4:

Lines 116-117: No point above the capital letter for Eh and Ev?

AR2-4: The dot above Eh and Ev denotes that these parameters are complex-valued. This comment is identical to Reviewer #1's R1C7; please refer to our response **AR1-7** there for further clarification.

R2C5:
Equations (1) and (2): Equations (1) and (2) are not correct. The reflectivity is not directly equal to the average modulus square of received complex amplitudes. A constant is missing.

AR2-5: We added C1 and C2 in equations of 1 and 2. Also, added this text in lines 153-155: Also, The constants C1 and C2 depend on the radar system parameters such as transmitted power, Radar cross-section (RCS), radar geometry, wavelength of the radar signal, and system gain

R2C6:
Line 121: "ZDR quantifies the difference between reflectivity measurements in horizontal (Zhh, Eq. 1) and vertical (Zvv, Eq. 2) polarizations, expressed in decibels (dB) (Eq. 3)." Eqs. 1-3 are not expressed in decibels. Be consistent with the text and equations.

AR2-6: We have revised the text to clarify the definitions in both linear and dB units (line 155).

R2C7:
Line 125: "At zenith-pointing direction, ZDR is zero. At slant-pointing direction, a positive ZDR value ...."

AR2-7: We modified the text as requested by the Reviewer (lines 155-156).

R2C8:
Line 127: "The correlation coefficient (RHV) is a crucial parameter that quantifies the linear relationship between the Zhh and Zvv." Rephrase this statement, which is now not correct.

AR2-8: We rephrased it like this: The correlation coefficient (RHV) is a crucial polarimetric parameter that quantifies the similarity between horizontally and vertically polarized backscattered signals. It provides insight into the diversity of particle shapes and orientations within a radar resolution volume.

R2C9:
Lines 128-129: the sentence is not clear and that is not useful to describe Eq. 4 in terms of ratio, sum, square root, product… because that can be directly seen in Eq. 4.
AR2-9: Thank you for the suggestion. We have removed the explanation accordingly.

R2C10:
Line 131: remove the point after 1.

AR2-10: Removed.

R2C11:
Lines 131-132: "… a correlation coefficient of 1 indicates perfect correlation or alignment between horizontal and vertical polarizations, suggesting consistent scattering behavior." Rephrase. What is "alignment between horizontal and vertical polarizations"? What is "consistent scattering behavior"?

AR2-11: we rephrased the text like this: A correlation coefficient of 1 indicates perfect correlation between horizontal and vertical polarizations, implying that the backscattered signals are identical in phase and amplitude. This suggests consistent scattering behavior, often associated with isotropic scatterers such as spherical particles.

R2C12:
Line 135: "…. raindrops, with a spherical shape and ….". Replace "spherical" by "spheroidal".

AR2-12: We replaced "spherical" with "Slightly spheroidal."

R2C13:
Line 137: "…. a parameter frequently detected by cloud radars ….". Rephrase. A parameter is not detected.

AR2-13: We replaced "measured" with "detected."

4) Main-peak approach

R2C14: Is the main peak approach code by Myagkov et al. available online?

AR2-14: The central retrieval code is still owned by Alexander Myagkov (as deployed by Myagkov et al., 2016a) and is available upon request.

R2C15:
Line 163: "This analysis provides insights into particle habits by utilizing a spheroidal scattering model". "A spheroidal scattering model". Which scattering model is used?

and “spheroidal scattering model” is not the appropriate name.
Provide the equations of the polarizability ratio and degree of orientation. Explain how they relate to the ZDR and RHV measurements.

AR2-15: These comments and questions have already been addressed in our response to the first reviewer (AR1-2).

5) Spectrally resolved approach

R1C16:
I recommend to the authors the extension of the block diagram of Figure 2, where the main peak approach block would appear. Further a zoom of the main peak block, with inputs and outputs, can be worked out in a second Figure. Presently, without reading in detail the papers Myagkov et al., it is challenging to understand the spectrally resolved technique. The reader should be able to understand the paper without having to read preceding papers.

AR2-16: We understand the concerns of the reviewer that the technique shall be better introduced again in our manuscript. But we have to emphasize that the actual retrieval technique requires a very extensive introduction, as it was done by Myagkov et al., 2016a. We therefore kindly ask the Reviewer to accept that we introduce the basic retrieval only briefly. We added a short statement to section 3. where we emphasize the importance of the work of Myagkov et al., (2016) for a full introduction of the (main-peak) shape retrieval technique. Nevertheless, as w also requested by Reviewer #1, we extended the introduction of the general retrieval in Section 3.1 to provide the reader a more detailed introduction.

R2C17:
There is no information on the error analysis.

AR2-17: In the spectrally resolved approach the same error calculation is used as for the main-peak approach. The values of polarizability ratio and degree of orientation where the best fit between simulated and observed ZDR and RHV are identified are selected as the result. We treat the error discussion similar as was done by Myagkov et al (2016b). There, they focused on visualization of the 2 times the standard deviation of the retrieved values of polarizability ratio and degree of orientation, respectively, in a range gate. This standard deviation results from the variability of the ZDR and RHV data points that are incorporated into each range gate and elevation angle interval used in the retrieval.

R2C18:
How is Mie scattering regime accounted for? For example, for Part 1.
AR2-18: This point has already been addressed in our response AR1-2 to the first reviewer.

R2C19:
Line 185: "Consequently, the Doppler spectra observed with a vertically pointing cloud radar offer insights into the variability of sizes and shapes of the ice particles". Information on the shapes of the ice particles for zenith-pointing cloud radar cannot really be obtained.

AR2-19: Doppler spectra from a vertically pointing cloud radar provide insights into the variability of ice particle sizes and shapes but do not allow for their direct quantification.

R2C20:
Lines 198-199: "The amount of 5 parts was empirically chosen for this study, because usually not more than that amount of different particle shapes can be expected in a cloud volume". Can you provide a reference for this statement?

AR2-20: This point has already been addressed in our response to the first reviewer (AR1-1).

R2C21:
Lines 199-200: "Increasing the number of parts would result in a reduced amount of available data points per Doppler spectrum part which would lead to increased uncertainties. This statement should be developed. "Which amount of data points for the spectrally resolved approach is recommended? Why? I missed a discussion on this point in terms of possible errors.

AR2-21: It depends on the number of FFT points. In this study (FFT = 256), we required at least one data point per part. For FFT = 256, we recommend using two data points per part.

R2C22:
Lines 203-204: "Instead, we assume that the fall attitude of the individual hydrometeor types contained in the cloud volume is similar at all elevation angles." Was the same assumption made in the main peak approach?
AR2-22: In the main peak approach, fall velocity is not considered at all.

6) Retrieval of horizontal wind

R2C23:
Figure 3: compared to Vf and Vh, VR is not well scaled. Correct this.

AR2-23: We have updated the figure and the included parameters.

R2C24:

Lines 241-242: “…. while the sine’s curve amplitude yields the wind velocity Vh multiplied by the cosine of the elevation angle, ”

AR2-24: Thanks for the hint. We corrected the passage.

R2C25:
Lines 242-243: “Additionally, the entire curve’s displacement from the zero velocity relates to the precipitation fall speed.”

AR2-25: Thanks for the hint. We corrected the passage.

R2C26:
Lines 243-244: “We used the approach of Baars et al. (2023) to derive the horizontal wind components.” Describe shortly this approach.

AR2-26: We added a short introduction to the technique deployed by Baars et al., (2023) to the last paragraph of Section 2.3.

7) Aliasing problems and effects of horizontal winds on the determination of the vertical velocity component.

R2C27: Line 257: mention what fn is.
AR2-27: We mentioned fn is pulse repetition frequency.

R2C28:
Lines 267-270: The methodology of dealiasing needs to be shortly extended for clarity and reproducibility.

AR2-28: In agreement to a request of Reviewer #1 we slightly updated the passage on the aliasing problem correction.

R2C29:
Eq. 10: VR should be replaced by VD.

AR2-29: We modified all of the variables in this section.

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

R2C30:
Figure 4 caption: ……. on November 7, 2014. Correct the date.

AR2-30: We updated the date from November 3 to November 7.

R2C31:
Lines 293-294: Between 09:15 and 09:30 UTC, a deep cloud .... which caused precipitation after 09 UTC. Check the time consistency. If it rains from 09:00 UTC, it means that the deep cloud is present before 09:15. Rephrase.

AR2-31: The first part of the sentence addresses the time period of the case study which will be introduced in more detail in the remainder of the section. The information about the onset of the precipitation is unrelated to the introduction of the case study period. We slightly updated the sentence and now state "...which caused slight precipitation already since 09:00 UTC."

R2C32:
Line 295: The evolution of the mixed phase in this deep cloud.... Why is this deep cloud a mixed-phase cloud? I miss the argumentation here.

AR2-32: We identified the deep cloud as a mixed-phase cloud as we were able to identify some presence of liquid water in its lower parts. This is now also mentioned in the text that introduces Figure 4.

R2C33:
Line 335: .... The SNR stabilizes at approximately 60 dB. I think it is much less. 25 dB?

AR2-33: Yes, thanks. We updated from 60 dB to 25 dB.

R2C34:
Figure 6 caption: there are errors in the sequence (a)-(o): .... (l) RHV in part 2.....(i) ZDR, and (n) RHV in part 4, ...

AR2-34: Yes, thanks. We updated i and f with l and i respectively.

R2C35:
Lines 345-347: Why is it possible to conclude that below 4 km based on RHV and ZDR the particles are prolate (part 5). Provide a short explanation and reference.

AR2-35: Based on the simulation, for prolate-shaped particles, ZDR is higher at non-zenith angles than at the zenith angle, while RHV is lower at the zenith angle compared to non-zenith angles.

R2C36:
Lines 358-361: provide a reference.

AR2-36: Similar to the previous comment, the conclusions are based on the simulation.

R2C37:
Figure 7 caption: error in the sequence (a)-(l): .... (g) RHV spectrum before splitting....

AR2-37: Yes, thanks. We updated h with g.

R2C38:
Lines 362-372: in this paragraph the retrieved polarizability ratio shown in Figure 8 is discussed. However, there is no word about the retrieved degree of orientation, part of Figure 8 as well. Why?

AR2-38: We added this text in lines 476-479: At an altitude of 3 km, the degree of orientation is at its lowest (around -0.75), suggesting that prolate-shaped particles are more horizontally aligned compared to other altitudes. Below 3 km, as the polarizability ratio decreases, the degree of orientation approaches 0, indicating a transition to randomly oriented prolate-shaped particles.

R2C39:
Line 363: "For the sake of readability, error bars are omitted in this case ...." OK, but some text related to the error bars should be written in section 3.2. How are the error bars estimated?

AR2-39: We kindly refer to our reply AR2-17, where we state how the uncertainties are determined and where they are introduced in the manuscript text.

9) Second case study 03 Nov 2014, 20:30-20:45: Secondary ice Formation

R2C40:
Figure 9 caption: The highlighted period.... are applied. Rephrase the sentence. Also, I don't see the highlighted period in the figure.

AR2-40: We now illustrated the highlighted period in the figure.

R2C41:
Figure 13 caption: there are errors in the sequence (a)-(o): .... (l) RHV in part 2.....(i) ZDR, and (n) RHV in part 4, ...

AR2-41: Yes, Thanks. We updated i and f with l and i respectively.

R2C42:
Figure 14 caption: error in the sequence (a)-(l): .... (g) RHV spectrum before splitting....

AR2-42: Yes, Thanks. We updated h with g.

R2C43:
Figure 15 caption: error in the sequence (a)-(l): .... (g) RHV spectrum before splitting....

AR2-43: Yes, Thanks. We updated h with g.

R2C44:
Lines 462-463: "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....

AR2-44: We would like to point to a previous reply AR#46 that addresses a similar question raised by Reviewer #1.

10) References

R2C45: The authors should review the reference list.
For example,
uncomplete reference: Melnikov and Sraka, 2013.
Spell-check: Hajipour, M et al. 2024: ....studies.....

AR2-45: We screened and modified the reference list as requested.

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