Response to the Editor

Dear Editor,

We once more want to thank the reviewers for taking the time for a second review and the provision of new valuable comments. Comments and suggestions presented by both of reviewers 1 and 2 have been considered in the new version of the manuscript.

Specifically, we took note of the concerns both reviewers raised again about the application of the Rayleigh scattering theory in the VDPS retrieval. We provide further explanation for our decisions below in our replies to the specific review comments (see R1-C1, R1-C2, R2-C5, R2-C6 below). We decided to put some general statements already here. First, we'd like to emphasize once more that we introduce the VDPS method as a versatile, additional tool for the characterization of the microphysical structure of mixed-phase cloud systems, which can be applied to data from meanwhile widely available scanning SLDR cloud radars. This is highlighted already in the introduction section of the manuscript. Similar to many other radar observables (such as even the radar reflectivity factor Z), it relies on certain assumptions (such as the assumption that Z is always given with respect to liquid water, even in ice clouds). Second, the sole application of SLDR for estimating the polarizability ratio (as an indicator for the shape) of hydrometeors relies on the availability of a simple model. Much more complex measurements (multi-frequency, spectrally resolved, symmetrically scanning observations of radars with very accurate reflectivity calibration) would be needed in order to constrain the degrees of freedom introduced by complex T-Matrix or DDA scattering models, as these models depend strongly on assumptions on size and specific shape of the scatters. When a wrong size distribution is assumed in DDA or T-Matrix calculations, resonance effects can lead to strongly varying simulations of backscattering and polarimetric results. Third, we inform and elaborate in detail about the possible deficiency of the Rayleigh-scattering assumption in our manuscript. By doing so, we provide the expertized reader with information about possible uncertainties in specific cloud situations while hoping to provide motivation for future improvements of the approach, which is beyond the current state of the VDPS method as it is presented in our study.

Additionally, we would like to complete our answer to one comment of the second reviewer from the first revision cycle, which was "*Can you explain better how you think a vertical profile of shape can be useful for model development? I am very skeptical since most bulk schemes assume a constant ice particle habit throughout the entire temperature range. Only very few experimental models currently exist which take particle habit into account*." We would like to refer to a recent manuscript well submitted to Journal of Advances in Modeling Earth Systems by the German Weather Service (preprint available at DOI:10.22541/essoar.168614461.18006193/v1). In the manuscript the authors use the polarizability ratio to improve the ice growth characterization for the explicit habit prediction in the Lagrangian super-particle ice microphysics model McSnow. We added this information to lines 58-64 of the introduction section of the updated version of our manuscript: "*However, since the polarizability ratio depends on both variables, it can be used to track the evolution of the ice particles from pristine state to aggregates and rimed particles in observational studies. Polarizability ratio profiles are also valuable for modeling studies since the profiles can be used to constrain microphysical processes of ice growth. The first attempt to utilize polarizability ratios to improve ice characterization in models was recently done by Welss et al. (2023). Based on polarizability ratios the authors have updated the ice growth characterization for the explicit habit prediction in the the Lagrangian super-particle ice microphysics model Mc-Snow developed by German Weather Service (DWD, Brdar and Seifert (2018)).*"

Consequently, we would like to submit the revised manuscript and the diff-version of the revised manuscript together with our responses to all the comments provided by the two reviewers. In our replies, all references to modified lines are given with respect to the final diff-version of the manuscript (with differences) to facilitate the lecture to the editor and/or reviewers. Comments are enumerated by the term “Reviewer_1/2_Comment_X which means e.g. R1-C1”. In the corresponding answers, the C (“comment”) is exchanged by an A (“answer”). Thank you for considering our work.

Best regards,

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Response to reviewer 1# :

R1-C1: My question on Rayleigh scattering is yet addressed... I am concerned with the non-Rayleigh scattering effect in presence of large aggregates at Ka band, but you assume Rayleigh at all conditions. I am questioning the applicability of this assumption.

R1-A1: We acknowledge the concerns raised by reviewer 1. But we would also like to note that we extensively discussed the possible effects of non-Rayleigh scattering in our reply to reviewer 1 and we already added a comprehensive discussion to section 2.3 of the 1st revision of the manuscript. Nonetheless, we now also elaborate on the potential shortcomings of the Rayleigh approximation in the conclusions section of the manuscript. We added to lines 511-515: "*Finally, it remains subject of future discussions to investigate the applicability of more sophisticated scattering theory in a quantitative determination of hydrometeor shape. The scattering model underlying the VDPS method only requires information about the axis ratio, apparent density and canting angle distribution. T-Matrix or DDA methods provide much more degrees of freedom concerning the microphysics of the scattering hydrometeors, specifically the number size distribution and fine structure of the hydrometeors*"

Unlike the VDPS method which requires only two parameters (namely polarizability ratio and degree of orientation), T-matrix and DDA require additional characterization of particles. In the case of T-matrix, size is required. Depending on the exact size-wavelength relations, scattering properties of a single particle can indeed deviate from the Rayleigh solution due to resonance effects. However, in the radar scattering volume we have a huge number of particles with different sizes and therefore, resonance effects are averaged out. In the case of DDA, the exact geometric structure of a particle must be characterized. In this case as well, there can be scattering properties deviating from Rayleigh, but again here due to a number of particles with different sizes and shapes the resonance effects are averaged out. In addition, these additional degrees (size, fine geometric structure) of freedom cannot be constrained by observations at only one wavelength. A combined analysis of the radar polarimetry and multi-frequency observations might add such a constraint, but at the moment even though such datasets exist (TRIPEX-pol von Terzi et al., 2022), they are not widely available (Kneifel et al.,2011, Leinonen et al., 2013).

R1-C2: In the response, comments 5 & 7 were poorly addressed.

Comment 5:

I believe that the Rayleigh condition at Ka-band should be considered. The theoretical basis is based on the assumption of Ka-band Rayleigh scattering which is satisfied for pristine ice. However, in presence of large aggregates which usually occur at -5 to 0 C or -15 C, this assumption is violated and the retrieval should be made with caution.

Comment 7 :

The authors claim that rimed or aggregated ice particles are isometric and speculate that the retrieved polarization ratio around 1 indicates riming or aggregation. I feel that this statement should be used with caution. Firstly, the method may be limited to pristine ice at ka band. The Rayleigh condition at Ka band may not have reached, for example at 1.6 km in figure 10. Therefore, the basic assumption is broken. Secondly, very heavily rimed ice can be isometric. Lightly rimed ice or aggregation has a characteristic aspect ratio.

R1-A2: The proposed method is applied to spectrally resolved polarimetric measurements. This way we reduce the likelihood of having considerably different particle types affecting the measurements. When we have pristine ice particles, VDPS is sensitive to particle's shape and density. When we have aggregated/rimed particles, polarimetric signatures are weak, which indicates that these particles are either isometric or only slightly non-spherical or have low density (due to air inclusions) or both. In order to illustrate this, we provided measurement statistics in our previous reply. Here we show it again in Figure 1 representing a single scatter plot of 80 deep precipitating clouds randomly chosen from the analyzed campaign. For each cloud we took profiles of the attenuated reflectivity and SLDR, at 60° off-zenith angle. Small particles (Rayleigh scattering, low reflectivity) are in the left part of the plot. These particles often produce strong polarimetric signatures (large SLDR up to -5 dB). In the right part of the plot we have big particles. We do not know the type of these big particles but they produce SLDR mostly below -10 dB which is equivalent to 0.9 dB ZDR. When these values of SLDR (< -10 dB) are used for VDPS, the resulting polarizability ratio is in the range of 0.8 and1.2, Values in this range indicate that particles are isometric (see Table 2), i.e. do not produce strong polarimetric signatures. And this is the correct result. We do not see big violations of the retrieval applied to these big particles as suggested by the reviewer.

Concerning the prolate particles detected at 1.6 km in Figure 10 of the first version of the manuscript, there were artifacts caused by the calculation of SNR based initially on the spectral line in the cross channel, and increasing values of the cross-correlation coefficient describing irregular shapes but not especially columnar crystals. In the revised version of the manuscript, we can see that VDPS derives isometric particles at this layer.

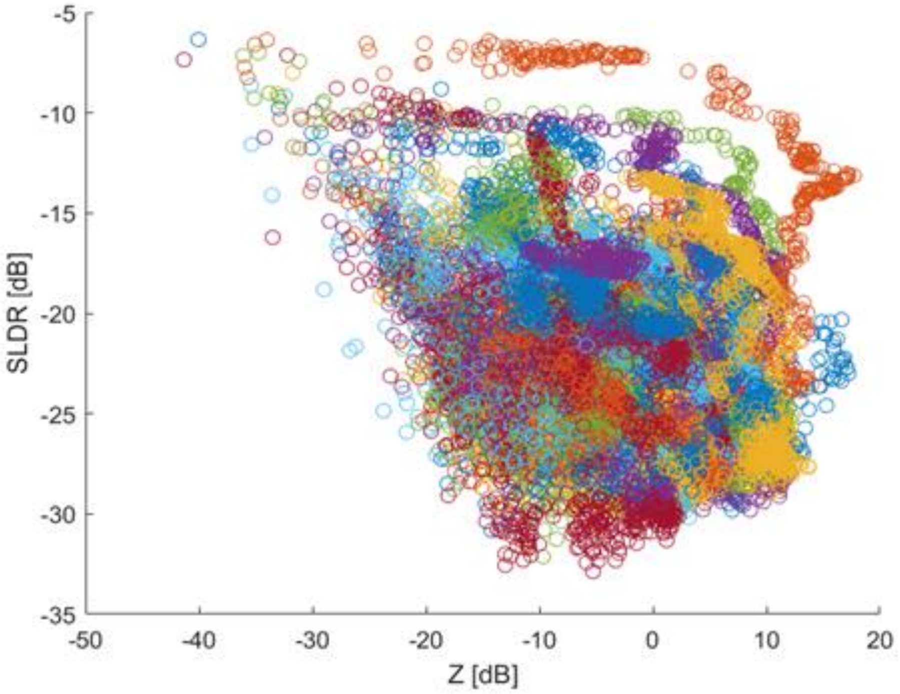

Figure 1: Attenuated radar reflectivity Z and SLDR of 80 deep precipitating clouds as observed at 60° off-zenith angle in the course the CyCARE campaign.

R1-C3: Discussion on the weakness of this work is missing in conclusions. For example, no direct validation with in-situ observations

R1-A3: Thanks for highlighting, that the discussion seems to be still missing to elaborate on the weaknesses of the study. We added that so far validation with in-situ observations was not possible. While we pointed to this issue already in the reply to the first review, we missed to include it into the actual manuscript. We now did so, see lines 485-487: "*It's important to* *highlight that we could not validate the method using in-situ observations throughout the two campaigns. It is nevertheless the goal of the authors of this study to aim on deployments of the SLDR-mode scanning cloud radar in campaigns where in-situ observations are available*."

R1-C4: A fatal error in response 9 and 10. Spectral broadending will definitely affect the values of spectral peaks at both channels, since the peak value is affected by adjacent data during the convolution, namely smoothing. Therefore, your SLDR will be affected. You could simply follow Kollias 2011 and do forward simulation.

R1-A4: We apologize for the imprecise reply. What we meant is, that a convolution of several spectral lines with the same polarimetric signatures does not change these polarimetric signatures. For instance, at a cloud top where a high turbulence is expected, we have likely pristine ice particles with the same shape. These particles produce similar polarimetric signatures and these signatures are still the same in the case of broadening.

When we analyze a volume in the middle of a deep precipitating cloud, the maximum spectral line typically corresponds to large aggregated/rimed particles. Here again neighboring spectral lines characterize similar types of particles. And therefore, the spectral broadening would not considerably change observed polarimetric signatures.

The broadening would only have an effect when particles with different polarimetric signatures are "mixed/convolved" into a single spectral line. But in this case, we argue that most of the time particles with different polarimetric signatures have considerably different terminal velocities and this difference is too large for turbulence to mix these particle populations into the same spectral line.

However, in order to acknowledge the possible impact of spectral broadening on the polarimetric signatures, we added a text passage to lines 505-511 of the conclusions section, which discusses the potential of spectral broadening and provides the reference to the study of Kollias et al, 2011: *"In addition, turbulence, horizontal wind, and radar beam width, especially at large off-zenith pointing angles, can lead to a broadening of the Doppler spectra, which has the potential to impact the spectral peak values in both channels (Kollias et al., 2011). Spectral broadening becomes noteworthy when particles with distinct polarimetric signatures are blended into a single spectral line, and it becomes particularly relevant when substantial turbulence is present (typically on the order of several meters per second). However, the spectral broadening would not considerably change observed polarimetric signatures in the case of pristine ice crystals at the cloud top, or when only one type of hydrometeors is present in a cloud volume. "*

R1-C5: You should give specific definition for isometric particles, or use other descriptors, since isometric is rarely used in cloud physics communication.

R1-A5: We added an additional description of the primary shape classes at lines 171-177 of the manuscript: "*In this study, we will sort particles into three primary categories based on their shape: Oblate particles, which have a polarizability ratio less than one, prolate particles, characterized by a polarizability ratio greater than one, and isometric particles, where the polarizability ratio is ranged from 0.8 to 1.2, depending on the radar calibration (see Table 2). With respect to the definition in this study, we consider particles as isometric when they do not produce considerable polarimetric signatures. Such particles have either spherical or just slightly-non-spherical shape. In addition, non-spherical particles with low density (low-refractive index) also appear to be isometric. "*

Response to reviewer 2# :

I would like to thank the authors for carefully revising the manuscript. In my opinion it has greatly improved and I find it now much easier to follow. I think the manuscript is now ready to be published after some minor comments and changes listed below are taken into account.

General comments:

R2-C1: One advantage of the method which the authors might consider is that the SLDR method is probably relatively immune to attenuation effects. For ZDR one has to consider effects of differential attenuation. RHI scans at multiple frequencies suffer from the need to estimate the dual-frequency relative attenuation along the slant path. I guess this is all much less relevant for SLDR.

R2-A1: Note, that in the STSR mode (simultaneous transmission and simultaneous reception) in which ZDR is measured, the transmitted signal is exactly the same as in the SLDR mode in terms of polarization, i.e., linearly polarized at 45 degrees electromagnetic wave is transmitted. Effectively, such a wave is a superposition of two waves of which one is horizontally polarized and one is vertically polarized with the 0°- phase shift.

Therefore, in both STSR and SLDR modes, the signal is transformed in the atmosphere in the same manner by scattering and propagation effects. The difference between STSR and SLDR modes is only the basis in which the signal is received, but in both modes the received signal is still affected by the atmosphere in the same way. It could be that partially some of these effects are mitigated in SLDR but this would require investigations which are beyond the focus of the current manuscript.

R2-C2: The four case studies are quite interesting and demonstrate the potential of the method. But honestly, I would not consider them as a true evaluation of the method. I agree that they are very valuable consistency checks but without in-situ truth, I think it is not a real evaluation.

R2-A2: Indeed, with the statement 'evaluation', we did not mean to do an actual validation but more an application. We thus implemented two modifications into the new version of the manuscript.

We rephrased Lines 480-485 to: "*A fourth case study demonstrated the potential of the VDPS method for tracking of the evolution of the ice crystal shape between top and base of a deep cloud system. Before application of the VDPS method to the case studies, the algorithm was tested and calibrated with success based on observational datasets from two field campaigns...*"

We added in addition another statement on the lack of actual evaluation data to Lines 485-487: "*It is important to highlight that we could not validate the method using in-situ observations throughout the two campaigns. It is nevertheless the goal of the authors of this study to aim on deployments of the scanning SLDR radar in campaigns where in-situ observations are available*."

Specific comments:

R2-C3: Table 1: The sensitivity of a radar is range dependent. Please provide range and averaging time used to estimate the sensitivity.

R2-C3: We now provide the averaging time (1s) and range (5 km) in Table 1.

R2-C4: L. 106: “receiver antenna”. Why specifically receiver antenna, I thought the same antenna is used for RX and TX? As such also the TX polarization is turned by 45°, or?

R2-C4: Indeed, apologies for the typo. We removed the term ‘receiver’ from line 103.

R2-C5: Discussion of scattering in section 2.3: I thank the authors that they discuss the potential issue of non-Rayleigh scattering for this method. I still think the paper would be much stronger if some sample comparisons of polarizability ratios from idealized plates and columns would be compared from the standard Rayleigh spheroidal method and some existing DDA simulations (for example in a short supplement). However, I suggest to add some example references to those statements “It is well known that the Rayleigh approximation is not always applicable to simulate scattering from large ice particles. Often the direct dipole approximation (DDA) is used to simulate scattering of individual ice particles having a complex shape.” I suggest especially mentioning studies looking at non-Rayleigh scattering effects of pristine ice particles and scattering databases that contain the full scattering information of single ice crystals.

R2-C5: A similar question was already raised by reviewer 1 in R1-C1, that is why we kindly refer to our response R1-A1.

As requested by reviewer 2, we did an additional literature survey and added further references to Section 2.3 of the revised manuscript, lines 159-167. We now cite the initial introduction into DDA simulations (Draine and Flatau, 1994, line 161), refer to one of the most-extensive currently existing scattering database (Lu et al., 2016, line 162) and also give several further references to studies which used DDA simulations and what they found about the backscattering and polarimetric signatures of hydrometeor populations.

R2-C6: L. 158: “In general, ice particles in a scattering volume have arbitrary shapes and the contribution of individual particles to the backscattering radar observables is averaged out.” I don’t understand why this is an argument of not using DDA particles. Also in your method, you assume that you have only one particle type in your volume.

A2-A6: In order to make a simulation of an individual particle with DDA one needs to characterize the fine geometric structure of the particle. In this case the scattering, which in case of large size is defined by resonance effect, strongly depends on the exact shape of the particle and its orientation relative to the radar. In reality there are many particles in a scattering volume and their shape is not known apriori. Since resonance effects on individual particles depend on their size, shape, and orientation and these parameters are likely different for all particles in the volume, the observed polarimetric signatures can hardly be represented by DDA scattering of an individual particle. One needs to at least simulate plenty of different particles and average their scattering properties. Ultimately, one would have to presume the distribution of particle fine structure over the entire hydrometeor size distribution. To our knowledge this has not been done yet at least not for polarimetric variables at mm wavelengths.

In a cloud we have a variety of particles different in size, shape and orientation. Each individual particle produces a specific polarimetric response. For instance, some particles produce large

ZDR some produce small ZDR etc. These differences are defined by exact geometry of individual particles and this is not known. Therefore, one way to handle this issue is to characterize the particles independent of their size and fine shape since there is no way to get this from available observations. And this is what we do with the Rayleigh assumption. We introduce a parameter characterizing a general shape of particles but size and fine structure effects are ignored. When more information is available, for example from multifrequency observations, we can constrain more microphysical properties of particles, but with the current SLDR mode scanning cloud radar setup, only a proxy for a general shape can be constrained.

As reviewer raised a similar issue in question R1-C1, we kindly refer to our answer R1-A1.

R2-C7: L. 211: “footprint of spaceborne remote-sensing meteorological instruments” is a bit vague. Maybe refer directly to MW passive sensors (which also often measure polarimetric signals). Active MW sensors usually have better footprints and instruments in the VIS/IR as well.

R2-A7: Thanks for this remark. We rephrased Lines 225-226: “*e.g., to a footprint of a space-borne passive microwave sensor.*”

R2-C8 : L. 332-333: “the derived values of ξ should be analyzed with care when the method is applied to rain and the melting layer” I would add here also “close to the melting layer” as one can expect large aggregates forming there. And as you mention yourself in section 2.3 the Rayleigh assumption is not increasingly invalid for larger particles.

R2-A8: Thanks also for this remark. We rephrased lines 346-348: “*the derived values of ξ should be analysed with care when the method is applied to rain and close to the melting layer.*”

R2-C9: Section 4.1: You assume in this case that rain is always spherical. This is of course only true for small drops. I wonder whether you can see in the SLDR-RHI scan the effect of increasing oblateness of larger raindrops (especially when observed at low elevation angles)? I suggest to add maybe a comment to the text, for example, if this would be possible given the expected uncertainties of the observations.

R2-A9: Indeed, we occasionally observed effects of oblateness on the SLDR at low elevation angles at Limassol. We added a short text passage to lines 383-386 to highlight this possibility: ”*With respect to the presented case it is noteworthy that it is likely that the observed rain droplets were small in size. This is corroborated by the absence of any elevation dependency of SLDR (Figure 9). In the case of strong rain, the oblateness of droplets would become apparent as SLDR increases from zenith pointing to 150° elevation angle, as we observed in some situations of moderate and heavy convective rain at Limassol during the CyCARE campaign.*”

Nevertheless, the effect of oblateness of large rain droplets basically is covered by the VDPS retrieval, since an increase of SLDR between 90° and 60° off-zenith is indicative of oblate particles, which is actually true for oblate rain droplets, as well.

Furthermore, it's important to note that we do not apply the VDPS method below the melting layer. It was carried out solely to provide an illustration pertaining to spherical particles such as droplets.

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