<u>Response to the Reviewer #1</u>

Dear Editor,

We sincerely thank Reviewer #1 for their insightful and constructive comments, which have significantly improved the quality of our manuscript.

We hope that the comments and suggestions provided by reviewer #1 and reviewer #2 have been thoroughly addressed in the reply letter and the updated manuscript. 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 reviewer #1 and reviewer #2. In our replies, all references to modified lines are given with respect to the tracked-changes version of the manuscript.

Thank you for considering our work,

Best regards,

Audrey Teisseire, Patric Seifert, Kevin Ohneiser, Maximilian Maahn, Robert Spirig, and Jan Henneberger

Review of Evaluation of the VDPS method with in-situ measurements and assessment of the impact of non-Rayleigh scattering

General comment:

The study evaluated the VPDS method with the VISSS instrument during the CLOUDLAB campaign. It is shown that the retrieved polarizability ratio and consequent interpretation of the particle type are consistent with the on-ground observations of the VISSS. The study further tries to evaluate the Rayleigh assumption that is used in the VDPS method, concluding that for large aggregates, at Ka or W-Band deviations can be expected. I think this study is a nice addition to the series of papers on the VDPS method, as for the first time the method is evaluated. I however have several suggestions that would make the evaluation of the Rayleigh assumption stronger, especially a discussion about non-Rayleigh scattering from ice crystals at non-zenith observations (see comment 6) has to be added in, if you want to evaluate the Rayleigh assumption correctly.

We acknowledge the suggestion of Reviewer #1 to extend the discussion of non-Rayleigh effects and the potential influence of the elevation angle on the latter. Our attempts to account for the suggestions in the revised manuscript are outlined in our responses to the individual comments.

I. Major comments:

<u>Comment 1:</u>

If you want to evaluate non-Rayleigh scattering effect wouldn't it be best if you simulate SLDR with existing scattering databases such as the Lu (2016) database? They have aggregates and ice crystals, since you are looking at the main SNR peak, both particle types are possible. Forward simulate SLDR for the different particle types at different radar elevation angles and see if non-Rayleigh effects are visible (i.e. dips, differences between X-Band, Ka-Band and W-Band). If you know that the variable you use for retrieving the shape has Rayleigh-scattering effects, then probably also your retrieval is influenced by it. In any case, I would be very interested to see how SLDR varies with increasing sizes and particle types, as I have thus far only seen similar calculations for ZDR.

Response 1:

Thank you for this suggestion. Indeed, this topic was already discussed during the review process of the first paper Teisseire et al., 2024 as follows:

(https://amt.copernicus.org/articles/17/999/2024/amt-17-999-2024-discussion.html, see response #3 to Reviewer #2.)

The conclusion of the discussion was, that although individual large ice particles may produce resonance effects beyond the Rayleigh approximation, such signatures are rarely observed in radar measurements because scattering arises from heterogeneous particle populations whose properties average out these effects. The analysis proposed in the response 3 from the review confirms that strong polarimetric oscillations are exceptional in deep ice clouds.

We added additional passage to the conclusion of the manuscript in lines 524-528: "*Furthermore, since the main SNR peak used in the VDPS method may include contributions from both aggregates and individual ice crystals, both types of particle are plausible within the observed signal. A natural next step would be to forward-simulate SLDR for different particle types across a range of radar elevation angles and frequencies (e.g., X-, Ka-, and W-band) to assess the potential visibility of non-Rayleigh scattering effects (Lu et al., (2016),Wells et al., (2024)). Such an analysis would help to better constrain the conditions under which deviations from Rayleigh scattering may become detectable.*"

Comment 2:

You are calculating DWR integrated over the entire spectrum, and then compare it to the retrieved shape of the particles populating the main peak. Usually the largest particles drive DWR, which might not be the particles populating the main peak. I would suggest calculating the DWR of the main peak, and to not use the variable integrated over the entire spectrum, because otherwise you are likely comparing two different populations.

Response 2:

The comment is right that when integrating DWR over the entire spectrum, the signal is often dominated by the largest particles. Indeed, in this study, the DWR is qualitatively used in Section 5 as an indicator of the presence of large particles, in order to assess deviations from Rayleigh scattering and to test the applicability of the VDPS method under non-Rayleigh conditions. Additionally, the reflectivity factor (Z_e) from LIMRAD-94 and MIRA-35 is calculated by integrating over the entire Doppler spectrum, which is not fully consistent with an approach to compute the DWR along the main spectral peak only. Recomputing Z_e for both radars based exclusively on the main peak in order to derive a peak-resolved DWR would require substantial additional processing and could introduce cumulative uncertainties in the retrieval, which is not in the scope of this study. Finally, calculating spectral DWR with two radars with two range resolutions, such as LIMRAD-94 and MIRA-35 used in this study, remains a real challenge (Kneifel et al., 2016). Implementing a PeakTree-based spectral decomposition would allow the DWR to be calculated consistently for the main peak, thereby improving the comparison with the VDPS method and enabling a more robust quantitative interpretation of DWR. We have to admit that such complex adaptions are out of the scope of the current study. But we acknowledge the general suggestion, which led to the addition of a dedicated passage to lines

516-523: "*In the present study, DWR is used as an indicator of large particles and is computed by integrating over the entire Doppler spectrum. This approach is not fully consistent with the VDPS retrieval, which is based solely on the main Doppler spectral peak. In future work, implementing a PeakTree-based spectral decomposition would allow for a peak-resolved calculation of DWR (Radenz et al., (2019)), ensuring methodological consistency between both approaches. Such an improvement would strengthen the comparison with the VDPS method and enable a more physically consistent and quantitative interpretation of DWR. It must however be noted that calculation of spectral DWR, especially for selected spectral ranges, are a technical challenge using two radars with two different range resolutions (bins) which can introduce biases or smooth out important spectral features and requires careful data handling and sophisticated retrieval schemes (Kneifel et al., 20216).*"

Comment 3:

I would also suggest looking at spectral DWR, find the regions of high DWR and look at the corresponding LDR (SLDR) spectrum in this region to see how SLDR looks like there.

Response 3:

It would be interesting to calculate spectral DWR and to compare it with spectral SLDR. However, we use in this study two different radars (LIMRAD-94 and MIRA-35) with differing range and Doppler spectral resolutions (bins) which makes complicated the calculation of spectral DWR. When the range bins and/or the Doppler velocities don't align, a direct point-by-point comparison is impossible, so we must interpolate or average the measurements to a common grid. These manipulations can introduce biases or smooth out important spectral features. The complexity to calculate spectral DWR is detailed in Kneifel et al., 2016 where multi-frequency radar observations and their processing challenges are discussed. As DWR is only used qualitatively in this study, spectral DWR is not calculated but we find important to mention this in the conclusion section.
We added in lines 521-523: "*It must however be noted that calculation of spectral DWR, especially for selected spectral ranges, are a technical challenge using two radars with two different range resolutions (bins) which can introduce biases or smooth out important spectral features and requires careful data handling and sophisticated retrieval schemes (Kneifel et al., 2016).*"

Comment 4:

Your comparison against in-situ seems to be done "visually", is there a possibility to analyse it more objectively?

Response 4:

This is an interesting idea; however, it is outside the scope of this study, as the primary objective here is to present a proof of concept. A visual comparison was chosen as the most appropriate approach to assess the consistency of the VDPS method and its ability to reproduce the observed structures. Indeed, the focus of this paper is the qualitative validation of the VDPS method, which solely requires comparing the particle shapes obtained from the VDPS method with the particle types observed by VISSS. However, we agree the reviewer that it exists possibilities to analyze the in situ-measurements more objectively using simulation of VDPS parameters based on in situ measurement. We could also refer to Pasquier et al., (2022) where PAMTRA is used for simplified reflectivity (Z_e) simulations, as this represents a promising framework. However, extending such an approach to the combined use of VISSS and SLDR would require implementing SLDR scattering within

PAMTRA and developing a method to incorporate VISSS-observed particle properties into the model, which is beyond the scope of the present study.
We added in outlook in lines 530-532: "*Finally, the comparison in this study is conducted visually, as the primary objective is to present a proof of concept of the VDPS method. A comprehensive statistical comparison between in situ measurements and particle shapes derived using the VDPS method would constitute an interesting and valuable direction for future research.*"

Comment 5:

In my opinion more information from the radars and other instruments can be used to make your discussion of e.g. riming stronger. The mean Doppler velocity is a variable used in most studies to show riming, as riming strongly increases the fall velocity of ice particles. Also, I find it very hard to actually determine super-cooled liquid droplets in the Doppler spectra you showed. Just because there is a signal around 0 m/s does not mean that these are droplets, to me it rather looks like your spectrum gets shifted there due to updrafts. In your Figure 5d, also the region above your rectangle has the same criteria as you described in your text, why did you not classify this as droplets then? Is there a LIDAR or ceilometer available at the side? Since this is a snowfall case it would be easy to use the LIDAR to detect the height at which droplets are expected. If this is not possible then I would suggest to use values from literature that define where it is likely to have a droplet peak in the spectrum (i.e. Ze smaller than a certain threshold)

Response 5:

Unfortunately, calibrated lidar measurements are not available for most of the discussed case studies, due to strong signal attenuation by low clouds or precipitation. However, the detection of supercooled liquid droplet is not solely based on the fall velocity of particles. Indeed, supercooled liquid droplets are characterized by the absence of a detectable signal in the cross-polarized channel; consequently, no corresponding depolarization values are attributed to the low signals measured in the co-polarized channel. This configuration is well represented in the black box in Fig. 5 a-c, where low values of SNR calculated in the co-polarized channel (Fig. 5a) are associated with no depolarization values (Fig. 5b and 5c).
Concerning the particles observed above the black rectangle in Fig. 5d, they are already classified as supercooled liquid droplets in lines 223-227 as follows: "*As a starting point, the low-LDR layer that is present in both case studies depicted previously in Fig. 3c is examined. Regarding the Doppler spectrogram depicted in Figs. 5a and 5d, a layer with low values of SNR_{co} associated with a fall velocity $v \approx 0$ m s−1 is observed from 1.2 to 1 km height. The absence of detectable signals in SNR_{cross} and SLDR at this height, as shown in Figs. 5b and 5c, respectively, indicates that these particles are likely supercooled liquid droplets at the top of the cloud.*"
If Reviewer #1 refers to the region below the black rectangle (and not above), this is correct. Particles located between 650 m and 600 m height exhibit characteristics consistent with supercooled liquid water (low values of SNR_{co} associated with no detectable values of SNR_{cross} and SLDR). The presence of drizzle in the upper layer was sufficient to indicate an ongoing riming process. We redefined the black rectangle in Fig. 5d-f to show only the supercooled liquid droplets detected at around 0.6 km height and added in Lines 236-238: "*Finally, supercooled liquid droplets are detected between 0.7 and 0.6 km height as indicated by the black rectangles in Fig. 5a and 5d, where no detectable signals are observed in SNR_{cross} and SLDR, as shown by the black boxes in Fig. 5b, 5c and 5e, 5f.*"

Comment 6:

I am missing a discussion on non-Rayleigh scattering of crystals at non-zenith viewing angles. The scattering properties of the particles depend strongly on the viewing angle, as these particles are typically highly asymmetric. When viewed zenith, the electromagnetic (EM) wave only travels through a small amount of mass, as the particles are oriented with their largest dimension approximately horizontal. However, when viewed from the side, the mass (size) the EM wave has to travel through is much larger, thus causing non-Rayleigh effects also for ice crystals. This effect can be observed when DWR from non-zenith observations is analysed. In your discussion you are only looking for regions where DWR is large when looking zenith, thus only looking at cases where aggregates can cause non-Rayleigh scattering. However, your method uses different elevation angles, thus also having ice crystals that are non-Rayleigh scatterers. You could investigate this behaviour by calculating LDR (SLDR) of ice crystals at different elevation angles as described in comment 1.

Response 6:

We thank Reviewer #1 for pointing this out. Unfortunately, the RPG-94 did not perform scanning during the campaign, which makes it impossible to calculate the DWR at off-zenith viewing angles.
Indeed, we focused in the discussion section on zenith observations where DWR is large and aggregates dominate non-Rayleigh effects. Our method uses different elevation angles, meaning some ice crystals may exhibit non-Rayleigh scattering even when not observed at zenith. To further investigate this, we could calculate the SLDR of ice crystals at different elevation angles, and compare non-Rayleigh effects as a function of elevation angle.

We agree to the suggestion of Reviewer #1 that the topic of elevation-dependent occurrence of non-Rayleigh-scattering should be further discussed within our study. We therefore added in the discussion in lines 484-490: "*However, the scattering properties of the particles depend strongly on the viewing angle, as these particles are typically highly asymmetric. When observed at zenith, the electromagnetic (EM) wave propagates through only a small amount of mass because the particles are generally oriented with their largest dimension approximately horizontal. In contrast, when viewed from the side, the EM wave traverses a much larger effective mass (or path length), which can lead to non-Rayleigh scattering effects even for ice crystals. This behavior could be assessed by analyzing the DWR RHI profiles which are not available for this study. Alternatively, simulating SLDR at different elevation angles would allow quantification and comparison of non-Rayleigh scattering effects as a function of viewing angle.*"

II. Other comments:

Comment 1:

How well do you trust your temperature information from IFS? Especially in case of an inversion, IFS is known to not represent the temperature well. I don't know how well IFS performs in mountain areas. While the temperature information is not that essential for your paper, you do use it to discuss the shape retrieval and possible microphysical processes that happen, so in my opinion it would be worth to check if the temperature from IFS is performing reasonably well.

Response 1:

The applied temperature information is sometimes not very precise. For our cases, the temperature is used as a supplementary tool and doesn't require high precision. To assess the reliability of the IFS temperature profiles, we compare them with Radiosonde profiles, which are based on local observations. This comparison shows generally good agreement but also highlights differences on the order of several tenths of a Kelvin which is precise enough for our analyses. Finally, all IFS vertical profiles were compared with on-site radiosonde and microwave radiometer measurements, as well as with radiosonde observations from Payerne, showing good agreement with the temperatures obtained from IFS. For example, on 8 January 2024 at around 10:00 UTC the IFS profile shows a temperature inversion in Fig. 3. This case study has already been analyzed in Ohneiser et al. (2025), and the temperature radiosonde in Payerne shows good agreement with the IFS data.

We acknowledge that, for the first case study on 22 February 2024, we observed a temperature deviation between the IFS calculation and the radiosonde measurement in Payerne exceeding one kelvin. As a result, we decided to remove the temperature contour from Fig. 1, since it is not used in the interpretation.

A paragraph addressing the limitations of the IFS has been added to the Instrumentation section in lines 120-128: "*Finally, the temperature information used in this study is obtained from the European Centre for Medium-Range Weather Forecasts (ECMWF) Integrated Forecasting System (IFS). Indeed, IFS temperature profiles represent grid-box averages and rely on model physics and data assimilation, which may limit their ability to capture local-scale and small-scale temperature variability, particularly in the boundary layer and under stable conditions. However, Illingworth et al. (2007) report generally good agreement between IFS and ground-based observations, while also highlighting differences on the order of several tenths of a Kelvin, which is sufficient for the purposes of our analysis. However, the temperature contours are compared with the Payerne atmospheric soundings for all case studies in order to verify the reliability of the measurements and the associated interpretations. For this reason, the temperature contour has been removed from Figure 1 due to the poor agreement between the IFS and radiosonde profiles.*"

Comment 2:

Line 30-31: the fall velocity of ice crystals is not necessarily smaller than -1m/s (see Karrer et al. 2020), The fall velocity depends on the size, and for larger sizes particles such as plates or columns actually fall much faster than aggregates, whose fall velocity saturates at -1m/s.

Response 2:

We deleted the end of the sentence in Lines 30-31 "*whereas small pristine crystals fall more slowly with velocities of* $v > -1$ *m* s^{-1}.". We replaced ". *In contrast*" with ", *while*" in Line 28. Finally we added in lines 32-34 "*Karrer et al. 2020 showed that large aggregates approach fall velocities in the range from 0.8 to 1.6* $m.s^{-1}$ *depending on the aggregate type*."

Comment 3:

Line 117: you are saying you use a spheroidal scattering model? I thought the method is based on Rayleigh right? Spheroidal can mean many things (e.g. T-matrix, Mie,..), I would be more specific here.

Response 3:

Myagkov et al, (2016a) describe the spheroidal scattering model like this: "*For the polarimetric scattering model it was assumed that spheroids have the same volume and the geometrical axis ratio as the hexagonal prisms used for the calculation of the apparent ice density* ". The method is fundamentally based on Rayleigh scattering. When we mentioned "spheroidal," we meant that we approximate the ice crystals as spheroids to account for their non-spherical shape in the forward simulations (Myagkov et al 2016a). We are not using a full T-matrix or Mie solution here but a simplified Rayleigh-based model with spheroidal geometry. We added *in lines 132-133: "The VDPS method is based on a simplified Rayleigh scattering-based model with spheroidal geometry approximation and …"*

Comment 4:

Line 126 (and other places): you are saying that the peak of your spectrum represents the dominant hydrometeor population, however, how can you be sure it is only one hydrometeor type in this Doppler bin? From my understanding there is no clear way to determine that there is not a mixture of particles present in each Doppler bin.

Response 4:

Indeed, the reference to the 'main' particle population, we don't mean that only one hydrometeor type is contained in the Doppler bin but that the hydrometeor population of potentially multiple particle types falling with the same terminal velocity is dominating. We propose to replace this sentence in lines 144-145 with "*the dominant hydrometeor population, potentially composed of multiple hydrometeor types sharing the same terminal velocity.*"

Comment 5:

Table 1: you write frequency and wavelength in row 1, however, you only mention the frequency in the following rows.

Rsponse 5:

We thank the Reviewer #2 for the hint. We removed "*Wavelength*" from the row 1 of the Table 1.

Comment 6:

Line 171: you say ice crystal population, I would rather refer to it as ice particle population, because you do not know what particle type it is.

Response 6:

We changed *"ice crystal" to "ice particle"* in line 190.

Comment 7:

Figure1: I found it hard to compare DWR to Ze and LDR, because of the different colormap used, where red in DWR means small, but red in Ze means large. For simplicity I would suggest to use the same colormap for all variables.

Response 7:

We thank Reviewer #1 for this comment, we have changed the DWR colormap as suggested in all panels.

Comment 8:

Line 198-200 and line 234: where are the dendrites coming from? Dendrites usually form at -15°C, at -10°C it is typically assumed that plates are forming, not dendrites.
Figure 3: the -10°C is displayed twice, is this correct?

Response 8:

The panel was replotted with more temperature contours showing now also the temperature inversion. The temperature is below -11°C in the inversion layer which is corresponding to the formation of dendritic or stellar plates (which could correspond to VISSS measurements) formation at high humidity rate. Indeed, the humidity rate is used to be high between 90 and 100% which allow at this temperature range dendrite (or stellar plates) formation, but their branches are shorter or partially melted. However, in this case, dendrites were observed at the ground by eye by people who conducted the campaign, which is a strong argument for the presence of dendritic crystals in the cloud layer. Finally, as showed in Fig. 4c VISSS detected rimed dendrites and the partially melted branches are visible which induce that dendrites were already present in higher cloud layers.
We modified in lines 214-215: "*where temperatures are approximately between -10°C and -11°C…*" and in lines 216: "*… rimed dendritic or stellar plates…* "

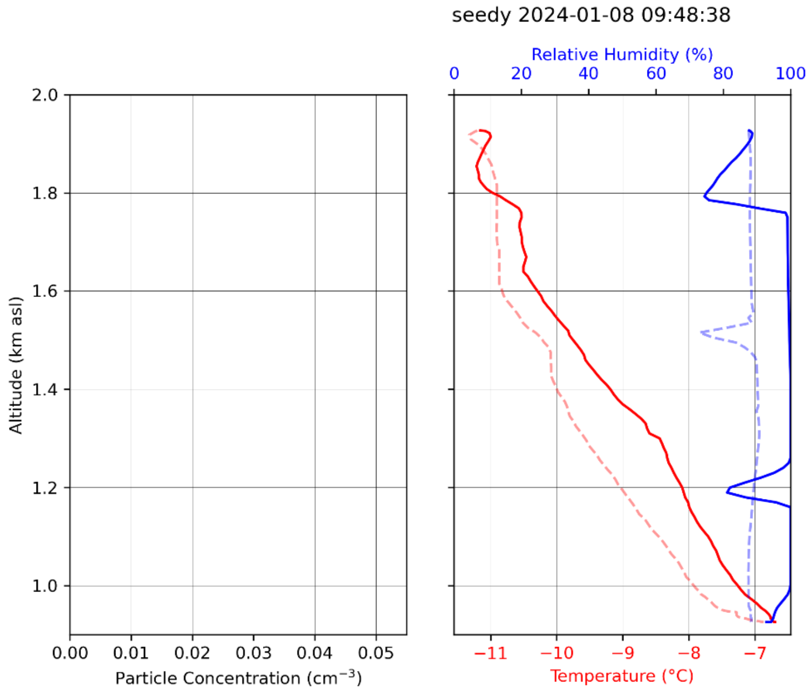

Figure 2: Atmospheric sounding of temperature and humidiy on 8 January 2024, 9:48 UTC at Erwisil during the Cloudlab campaign

Comment 9:

Paragraph from Line 337 to 344: I did not understand everything here, perhaps you could explain it differently?

Response 9:

We changed the content of Section 5 in lines 353-370 as follows: "*This section aims to quantify the influence of the non-Rayleigh scattering on the performance of the VDPS method. Since the VDPS approach relies on a scattering model based on the Rayleigh approximation (see Sec. 3.1), deviations from the Rayleigh regime may introduce biases in the retrieved vertical distribution of the polarizability ratio. Such non-Rayleigh scattering occurs when hydrometeor sizes become comparable to, or larger than, the radar wavelength, leading to more complex scattering behavior than assumed under the Rayleigh approximation. These effects are particularly relevant at shorter radar wavelengths, such as the Ka- and W-bands (8.55 mm* *and 3.19 mm, respectively), and can affect radar observables including reflectivity and polarimetric variables.*

To assess the impact of non-Rayleigh scattering on the VDPS retrievals, we first identify time periods representing a worst-case scenario, characterized by the presence of large and dense particles in the VISSS observations and by high values of $DWR_{Ka\text{-}W}$. For these periods, the vertical distribution of the polarizablity ratio provided by the VDPS method is derived in order to evaluate the impact of the non-Rayleigh scattering regime on polarimetric variables such as SLDR and on the robustness of the VDPS approach in identifying the shape regime of large and/or dense ice particles. The cross-correlation coefficient ρ_{cx} introduced in Sec. 2.2 is additionally used to support and refine the advanced interpretation. In the following subsections, two case studies are represented, focusing on large aggregates and quasi-melted graupel particles, respectively.

Comment 10:

Paragraph from Line 355 to 365: I am not sure I agree here… DWR KaW should be stable at larger values than 5dB, especially if you reached 8dB before. The drop is not 3dB, when both are in the non-Rayleigh regime, but rather 1-2dB. Are you sure that your PSD stays the same in the analysed fall streak? Because DWR is also quite strongly dependent on the shape of your PSD…

Response 10:

We thank the reviewer #1 for this comment. We therefore do not calculate the PSD on the fall streak, as the VDPS method does not work in this regime. The departure of the MIRA-35 measurements from the Rayleigh scattering regime is evidenced by the high LDR values analyzed in Fig. 11d, requiring analysis along the fall streak. We acknowledge that we cannot strictly exclude variations in the PSD along the fall streak, and therefore the observed DWR variability should not be interpreted quantitatively. We modify the paragraph in lines 385-394 by: "*A small decrease in $DWR_{Ka\text{-}W}$ is visible around 18:08 UTC, coinciding with an increase in LDR values from MIRA-35 MBR7 (Fig. 11d) and the presence of particles exceeding 15 mm (Fig. 11e). While the PSD is assumed approximately stable over the fall streak, slight changes in particle shape or orientation could influence $DWR_{Ka\text{-}W}$. Therefore, the observed decrease in $DWR_{Ka\text{-}W}$ likely reflects the combined effects of both bands entering the non-Rayleigh regime and small variations in the PSD, rather than a large Rayleigh-to-non-Rayleigh transition. In this case, $DWR_{Ka\text{-}W}$ stabilizes around 5 dB along the fallstreak.*"

Comment 11:

Line 375: you say that because you don't see any clear separation of hydrometeor species in the Doppler spectra (I am assuming you mean any new peak appearing), there is only this one present. I don't think you can say that easily, just because there is no new peak doesn't mean that it is the same particle population in these bins.

Response 11:

We modified the sentence in Line 404-405 as follows: "*This indicates that only one hydrometeor population, which may be composed of multiple types of particles,…*"

Literature:

Lu, Yinghui, et al. "A polarimetric scattering database for non-spherical ice particles at microwave wavelengths." *Atmospheric Measurement Techniques* 9.10 (2016): 5119-5134.

Illigworth et al., 2007 “Cloudnet: Continuous Evaluation of Cloud Profiles in Seven Operational Models Using Ground-Based Observations“, Bulletin of the American Meteorological Society , Vol. 88, No. 6, American Meteorological Society: Boston MA, USA, p. 883 – 898.

Myagkov et al., 2016a “Relationship between temperature and apparent shape of pristine ice crystals derived from polarimetric cloud radar observations during the ACCEPT campaign, Atmospheric Measurement Techniques, 9, 3739–3754.

Kneifel et al., 2016, “First observations of triple-frequency radar Doppler spectra in snowfall: Interpretation and applications”, Geophysical Research Letters , Vol. 43, No. 5, p. 2225-2233.

Radenz et al., 2019, “peakTree: a framework for structure-preserving radar Doppler spectra analysis”, Atmospheric Measurement Techniques , Vol. 12, No. 9, p. 4813-4828.

Pasquier, J. T. et al., 2022, “The Ny-Ålesund Aerosol Cloud Experiment (NASCENT): Overview and First Results.” Bull. Amer. Meteor. Soc., 103, E2533–E2558.

Ohneiser et al., 2025,“Impact of seeder-feeder cloud interaction on precipitation formation: a case study based on extensive remote-sensing, in situ and model data.”, Atmospheric Chemistry and Physics , Vol. 25, No. 23, p. 17363-17386.

Response to the Reviewer #2

Dear editor,

We sincerely thank Reviewer #2 for their insightful and constructive comments, which have significantly improved the quality of our manuscript.

We hope that the comments and suggestions provided by reviewer #1 and reviewer #2 have been thoroughly addressed in the reply letter and the updated manuscript. 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 reviewer #1 and reviewer #2. In our replies, all references to modified lines are given with respect to the tracked-changes version of the manuscript.

Thank you for considering our work,

Best regards,

Audrey Teisseire, Patric Seifert, Kevin Ohneiser, Maximilian Maahn, Robert Spirig, and Jan Henneberger

I. General Comments

This paper presents a validation of the VDPS method, introduced in an earlier paper by the same research group, expanding and illustrating its applicability to different case studies using independent VISSS observations recorded in the framework of the CLOUDLAB campaign. Two important contributions of the study are the use of SLDR based on cross-polarized Doppler spectra to detect hydrometeor populations and the assessment of non-Rayleigh effects trying to quantify biases at Ka/W band. Overall I think it is a valuable contribution to the field so, with a few clarifications and minor formal corrections (see Specific Comments), should be considered for publication – I suggest considering three general items in the text:

Comment 1:

Authors could describe more explicitly the contribution of the SLDR approach used (why is it important).

Response 1:

We added in the introduction Section in lines 40-42: "*The SLDR approach provides complementary information on particle shape and orientation that cannot be retrieved from co-polarized radar variables alone. This makes SLDR particularly valuable for identifying the particle shape and microphysical processes, which is a key aspect of this study."* and in VDPS method section in lines 134-136: "*Indeed, the SLDR approach is particularly advantageous due to its slanted geometry, which enhances sensitivity to particle shape while reducing the impact of particle oscillations while falling."*

Comment 2:

The particle size limits considered as thresholds for Rayleigh scattering in the two bands used (Ka and W) could be mentioned in the introduction (not in a results section).

Response 2:

Thank you for the comment, we added in lines 50-53: "*The Rayleigh scattering approximation is assumed to be valid for water droplet diameters smaller than approximately 300 µm at Ka-band and 100 µm at W-band (Bohren & Huffman, 1983/1998, Chapter 4). For less dense ice particles, the Rayleigh range covers larger size ranges, depending on shape and density.*"

Comment 3:

A brief discussion could be added on potential limitations of using ECMWF IFS temperature profiles instead of local measurements such as those obtained by tethered-balloons or from microwave radiometers (which in principle were also available during the campaign according to https://cloudlab.ethz.ch/the-project.html) – given the uncertainty associated to IFS profiles perhaps they could be plotted without one decimal digit as they are now.

Response 3:

The issue has already been raised by Reviewer #1. Unfortunately, temperature profiles from radiosonde or microwave radiometers are not available for all case studies during the CLOUDLAB campaign. However, the IFS temperature contours are compared with the Payerne radiosonde measurements to validate the agreement of the IFS temperature fields. We acknowledge that, for the first case study on 22 February 2024, we observed a temperature deviation between the IFS calculation and the radiosonde measurement in Payerne exceeding one kelvin. As a result, we decided to remove the temperature contour from Fig. 1, since it is not used in the interpretation.

A paragraph addressing the limitations of the IFS has been added to the Instrumentation section in lines 120-128 ; "*Finally, the temperature information used in this study is obtained from the European Centre for Medium-Range Weather Forecasts (ECMWF) Integrated Forecasting System (IFS). Indeed, IFS temperature profiles represent grid-box averages and rely on model physics and data assimilation, which may limit their ability to capture local-scale and small-scale temperature variability, particularly in the boundary layer and under stable conditions. However, Illingworth et al. (2007) report generally good agreement between IFS and ground-based observations, while also highlighting differences on the order of several tenths of a Kelvin, which is sufficient for the purposes of our analysis. However, the temperature contours are compared with the Payerne atmospheric soundings for all case studies in order to verify the reliability of the measurements and the associated interpretations. For this reason, the temperature contour has been removed from Figure 1 due to the poor agreement between the IFS and radiosonde profiles.*"

II. Specific Comments

Comment 1:

Page 1, line 9. Typo: co channel -> co-channel (as in the rest of the text)? Please check.

Response 1:

Corrected as suggested in line 9.

Comment 2:

Page 2, line 43. Typo: Non-Rayleigh -> non-Rayleigh (as earlier in the same line and elsewhere).

Response 2:

Corrected as suggested in line 48.

Comment 3:

Page 3, end of Section 1. Section 5, unlike sections 2 to 4, is not mentioned here; please add a sentence to briefly describe it (I think it is ok to omit Section 5).

Response 3:

We thank Reviewer #2 for this comment. We modified the sentence in lines 60-64 by "*Section 4 is dedicated to the comparison of the VDPS results with in-situ measurements. Finally, two case studies featuring large aggregates and dense graupel are presented in Section 5, in order to evaluate the possible effect of non-Rayleigh scattering on SLDR measurements and the corresponding influence on the particle shapes derived by the VDPS method.*"

Comment 4:

Page 3, line 82. What does MBR mean? I assume is an internal naming convention, but readers might be curious (as I am) and a brief explanation can probably be added easily.

Response 4:

We added in line 89: "*… MIRA-35 MBR5 (MicroBlaze Radar with serial number 5) ...* "

Comment 5:

Page 4, line 90. Typo: 2016b)) -> 2016b)

Response 5:

Corrected as suggested in line 98.

Comment 6:

Page 4, lines 98-99. ... of type (?)... Please check sentence and rewrite.

Response 6:

We removed the parentheses following "*of type*" in line 107.

Comment 7:

Page 4, lie 104. Format: Tab. or Table? Please be consistent – I suggest using Table.

Response 7:

Corrected as suggested in all the manuscript.

Comment 8:

Page 8, Figure 1 caption. Please add something as 'Temperature levels plotted correspond to ECMWF IFS forecasts' or similar.

Response 8:

Corrected as suggested in all figures representing overviews.

Comment 9:

Page 11, Figure 4 caption. 10:10:17 -> 10:10:17 UTC

Response 9:

We added „*UTC*" in caption 4.

Comment 10:

Page 12, line 218. Please check meaning of this sentence. [and similarly Page 13, line 239].

Response 10:

We change the sentence in lines 239-240 in "*Next, the blue-framed case from 10:08 UTC (Fig.3), which represents an early stage of a riming process, is discussed."*

Regarding the sentence in line 260 ("*The following analysis is divided into two separate case studies."),* we have checked its meaning and confirm that it is correct.

Comment 11:

Page 16, line 275. Typo: the presence supercooled liquid droplets -> the presence of supercooled liquid droplets

Response 11:

Corrected as suggested in line 297.

Comment 12:

Page 23, line 405. ... that were... that was? Please check meaning.

Response 12:

In the sentence “*The second case study presents large graupel that were detected between 12:50 and 13:10 UTC on 24 February 2024*” we used “*were*” because “*large graupel*” is plural in this context.

Comment 13:

Page 26, Figure 18 caption. Suggest: Surface hydrometeor shapes detected with VISSS ...

Response 13:

corrected as suggested.

Comment 14:

References: some DOIs have duplicated ‘doi.org’ strings in the URL.

Response 14:

Corrected.

Comment 15:

Page 29, line 526. Typo: Journals -> Journal

Response 15:

Corrected as suggested.