REFEREE 2 - Alexander Myagkov

Summary

The manuscript under revision presents a comparison of simulated and measured spectral polarimetric variables at W-band. The analysis is done for rain assuming well known sizeshape-velocity relations for raindrops. The manuscript clearly shows that more investigation is required for cloud radars to accurately simulate the spectra. The study is of a great importance for the cloud radar community. I have one major comment and several minor comments. I believe addressing these comments can considerably improve the manuscript.

Major comment

Major comment: 1. Even though the Sec.2.2 is in general clear, there is a lack of explanation why it is necessary to generate noisy spectra using I/Q components. In general, average spectra can be used. These can be derived simply by adding spectral noise power to Svv and Shh (as it is done in Eq. 14 of the manuscript). Assuming no correlation between noise in the two orthogonal channels, on average there is no effect on Shv. The variance of spectral Svv, Shh, and Shv taking into account the number of averaged spectra can be found as demonstrated in Myagkov and Ori 2022 (https://amt.copernicus.org/articles/15/1333/2022/). My question is, what are the benefits of generation of random individual spectra instead of the average ones? Please clarify this in the manuscript.

The reason we chose to generate noisy spectra using I/Q components, instead of working with average spectra with added noise power, is to explicitly investigate whether the use of random individual noisy spectra can help explain or reproduce the variability and degradation often observed in measured spectral polarimetric variables, particularly in variables that rely on cross-channel correlations like S_{hv}, at low SNR and low correlations where approximated formulas as proposed in Myagkov and Ori 2022 (https://amt.copernicus.org/articles/15/1333/2022/) tend to fail. We will include such considerations in the revised version.

By simulating the noisy spectra from I/Q components, we aimed to test whether noise characteristics contribute to the spectral variability seen in observations and whether this could help explain the persistent discrepancies between simulated and observed spectral polarimetric variables. In this sense, our work seeks to fill a gap in the literature and offer an alternative angle to understanding the role of noise in radar polarimetry.

Minor comment

1. L. 2 Change "spectral differential correlation coefficient" to "spectral correlation coefficient"

Will be done in the revised manuscript.

2. L. 6 "W band millimeter-wavelength radar" keep either W-band or millimeterwavelength, these two terms are kind of redundant

Will be done in the revised manuscript.

3. I have a feeling that some sentences in the introduction are not well connected to each other. I recommend reformulating the text to improve the reading flow:
a. L12-18 are about cloud radars. L.18-20 start with "Additionally" and emphasize advantages of polarimetry in precipitation radars, and then afterwards there is a jump back to cloud radars.

Rephrasing will be performed for improving the reading flow.

b. L24-29 I understand what is meant here, but for a general reader this might be confusing. I would recommend the following sequence: Integrated variables at centimeter-wavelength are very informative. At millimeter-wavelengths signatures in integrated polarimetric variables become less pronounced. Spectral polarimetry in cloud radars is better because different particle sizes are observed independently.

This part will be rewritten.

"This configuration has the critical advantage that particles with different sizes are separated in the spectral domain (because they have different sedimentation velocities), which allows to disentangle the contributions of different particle types. While vertically pointing radars can also achieve this separation, radars in slant polarization mode additionally exploit polarimetric measurements. At higher frequencies, where multiple resonances occur across the particle size distribution (PSD), the polarimetric variables—resulting from integration over the entire PSD—tend to average out the characteristic features of single-particle scattering, often balancing positive and negative contributions. Consequently, these variables exhibit low sensitivity to PSD variations. Further, they reflect both scattering and propagation effects. A way to mitigate these challenges at millimeter wavelengths is to analyze polarimetric variables in the spectral domain."

c. L29-38 I recommend making a separate paragraph and to indicate that these are some of advanced applications of polarimetric measurements at W-band.

A separate paragraph will be made.

4. The introduction section does not explain the novelty of the study, although the manuscript definitely shows novel results of comparison between state-of-the art simulations and real measurements. In the sentence (L50), there is one sentence stating that the goal is to describe the simulation. But I think the goal is much more than that, the study shows a comparison between an advanced spectral modelling based on empirical knowledge about rain drops (including size-shape relations, size-velocity dependence, turbulence, orientation etc) and real observations. And I would put the goal of the study in the end of paragraph, i.e. after existing simulation studies have been discussed.

The rephrased goals of the study will be placed at the end of the paragraph as recommended by the reviewer.

"Therefore, the first goal of this study is to explore how different assumptions that are related to atmospheric conditions (turbulence) and white and stochastic noise of a real radar spectrum, impact the simulated spectral polarimetric variables. The second objective is to present a novel comparison between simulated and observed data."

5. I am just curious, what is the reasoning to use a Eq.1 apparently based on studies before 2001 as a reference? And why using Thurai et al. 2008 as the second relation? Would not one be enough? Or is there a reason why two are needed, especially taking into account that the scattering simulations are often hard to distinguish in the figures?

We used two axis ratio parameterizations to demonstrate that the choice of parameterization does not have a significant impact on the polarimetric variables.

The first relation (Eq.1) was included as it has been traditionally used in scattering simulations, while the Thurai et al. (2008) relation was included because it is based on more recent measurements and is widely adopted in polarimetric studies.

To further illustrate the limited influence of the axis ratio parameterization, we plotted the polarimetric variables in Figures 3 and 4, showing that the differences between the two parameterizations are minimal.

6. Sigma on the y-axis in Fig. 2 should have VV as the subscript not just V.

Will be done in the revised manuscript.

7. L118-121 for me it is hard to follow these sentences. I would recommend to simply write that the broader the width of the canting angle distribution is, the lower the magnitude of the polarimetric variables.

This will be rephrased.

"In Fig. 3 (right), δ_{HV} remains near zero for small drop diameters, consistent with Rayleigh scattering. As the diameter increases, δ_{HV} departs from zero and exhibits oscillatory behavior, attributed to resonance effects and the transition from spherical to oblate shapes. These fluctuations become more pronounced at larger diameters. Variability in drop orientation within the radar sampling volume, described

by canting angle distributions, further contributes to the observed variations in δ_{HV}. The broader the width of the canting angle distribution is, the lower the magnitude of the polarimetric variables."

8. Instead of Fig3 right/left I would recommend marking the panels (a) and (b) and refer to panels using these marks.

We thank the reviewer for this recommendation, it is really appreciated. However, in order to maintain consistency with the style used throughout the manuscript (and in similar figures), we have chosen to continue referring to the panels as "left" and "right."

9. L124-125, elements Zij are not elements of the backscattering matrix but the Müller matrix, or as it is called in the manuscript, the phase matrix

Will be corrected.

10. L129-134 and Fig 4. Please mention that neither antenna pattern effects, nor antenna coupling for the quasi-bistatic radar configuration, nor multiple scattering, nor noise are included in the calculations of rhohv at this stage. One or a combination of these effects may drive rhohv below the stated minimum value.

Thank you for this clarification, which is will be mentioned in the article.

11. Sec.2.1.2 again here, why using 2 parameterizations?

As explained previously, we used two parameterizations to demonstrate that the choice of raindrop fall velocity model does not significantly impact the simulated polarimetric variables. This was important to show the robustness of the results against different commonly used relations.

12. L281 Why would one expect the opposite? If I understood correctly, the same Svv,Shh, and Shv were used for both methods. The difference is only in the randomness introduced by stochastic sampling. The averaged values are expected to be the same.

Yes indeed agreement is expected. We will rephrase the statement. The use of both methods was to ensure that the stochastic perturbations respect the physical relationships between the scattering elements. The fact that both methods demonstrated consistency when producing the polarimetric variables provided confidence in the turbulence generation on the simulations and that the discrepancies in the observations were not due to a simulation artefact.

13. L283 I recommend to avoid using the term correlation, when "agreement" is meant. Please check this throughout the manuscript

We will avoid the term correlation when discussing the results.

14. L287 L318 I see a significant difference between simulations and measurements in Fig. 8 at 5 m/s. Please check your conclusion about close alignment up to 7 m/s. Also, I do not see any noticeable differences at 3.5 m/s as written in the following sentence "For drops with terminal velocities up to 7 m/s, the simulations and the observations of sZ_{DR} and $s\delta_{HV}$ show reasonable agreement. Although, around velocities of 5 m/s, smaller values of sZ_{DR} and bigger values of $s\delta_{HV}$ are simulated relatively to the observations."

In section 4 - conclusions and ways forward, we will modify:
"The results reveal that the simulations closely align and show reasonable agreement with observations only within a limited area of the Doppler spectrum, approximately to terminal velocities up to 5 and 7 ms^{-1} (i.e. equi-volume diameters smaller than 1.33 and 2.25 mm), respectively."