Response to the reviewer 1

In this paper the authors describe a statical method to identify cloud microphysical growth regimes based on radar reflectivity and Doppler velocity measurements, particularly for cirrus clouds. The method uses the slope defined in the space of the two variables, which indicates either depositional growth or aggregation growth tends to be dominant in an area or temperature range. The global observation of Doppler velocity however includes errors associated with intrinsic limitation of the hardware, observation window modes, mis-pointing of the radar. The authors analyzed the errors globally, and improved the method to use median Doppler velocity given a radar reflectivity range. Throughout the analysis, they employed datasets from a high-quality ground-based Doppler velocity to create synthetic satellite observation. They also report minimum duration required for statistically meaningful analysis with the two measurements.

The paper is well written and the analysis is thoroughly conducted. I believe that this paper contributes to the science community, including climate modeling since they show usefulness of the Doppler observation for the first time, and possibility of identifying dependency of the cloud microphysical process on specific area of interests.

Thank you very much for reviewing our manuscript. We sincerely appreciate your careful and detailed reading, which greatly improved the clarity and readability of the paper. Your insightful interpretation of the non-Gaussian characteristics in the ground-based Doppler velocity was especially helpful, as it resolved a point we had been uncertain about. Thanks to your comments, the overall quality of the manuscript has been significantly enhanced.

Minor points

Page 3, Line 73: I think that when the difference in the vertical wind speed of two samples is negligible eq. (5) holds as well. If so, " or constant over the samples" may be added.

I initially had the same thought. However, due to the nonlinearity of the logarithmic transformation, the differential values of $\Delta\log(v_t)$ and $\Delta\log(v_t + w)$ with a constant value of vertical wind w are quite different:

$$\Delta\{\log_{10}(v_t + w)\} \simeq \frac{1}{(v_t+w)ln10}\Delta v_t \neq \frac{1}{v_t ln10}\Delta v_t.$$

In particular, when the vertical wind speed is much larger than the terminal fall velocity (i.e., $|v_t| \ll |w|$), the denominator of the coefficient is dominated by the inverse of the vertical wind speed. This significantly reduces the sensitivity of the differential value to the particle size (i.e., terminal fall velocity). For instance, if an upward wind of 1 m/s is superimposed on an ice particle with a terminal fall velocity of 5 cm/s, the sensitivity of the difference reduces to approximately 5/105 (~5%).

This issue also connects to the proposed improvement method discussed in Section 4, where the median of v_d is taken for each Z_e in the joint PDF. The use of the median helps reduce the influence of random vertical winds, which can occur naturally.

Page 3, Line 89: "… by differentiate Doppler velocity …" should be "… by differentiating Doppler velocity …".

Thank you for pointing it out. We fixed the typo.

Page 4, Line 111: "… microphysical sensitivity from the cloud top toward cloud base, …" Do you classify the vertical profiles into cloud layer objects and see the same characteristics? If not, it is a bit misleading.

No, we did not, as you mentioned.

Therefore, we have modified the sentence as follows:

"*This indicates that the microphysical sensitivity transitions from the upper to middle troposphere in a manner that is strongly constrained by atmospheric temperatures.*"

Figure 1, caption: "… by multiplying by the bin area, enabling resolution-independent interpretation." I think this statement is correct. For instance, when you construct a one-dimensional histogram, changing the bin width affects number of samples stored in each bin. So, the estimates of frequency depend on the bin width. But, if you divide by the width, then the frequency is normalized (see attachment).

We clarified that the joint PDF depends on bin width and explained the motivation for converting it into probability mass:

"Here, the *joint PDF varies with bin width and was therefore* converted into probability mass …"

Page 5, Line 142: Have you tried the hypothesis test on the correlation coefficient for statistical significance? The null hypothesis would be to assume correlation is zero.

Yes, we did examine the statistical significance of the correlation coefficients. However, due to the very large sample size, even relatively small correlation coefficients tend to be statistically significant. Despite this significance, the slope of the principal components derived from PCA can still vary substantially due to the influence of noise. In other words, hypothesis testing is not useful for evaluating the reliability of the slopes discussed in Figure 2. For this reason, we deliberately chose not to include the results of such tests in the manuscript.

Page 11, Line 252: "(cf. Fig. 3)" is this "Fig.5"?

We are sorry for the typo. It is Fig. 4. We have fixed it.

Page 13, Line 306: "(cf., Section 3a and 3b)" do you mean "(cf., Section 3.1 and 3.2)"?
We are sorry for the typos. It is Section 3.2 and 3.3. We have fixed them.

Page 14, Line 308-309: "… resembles a Gaussian distribution …" According to Fig.9, the PDF sems to take on Student's t distribution. It is probably reasonable because corrected Doppler velocity is an average of Doppler velocity. Have you considered fitting Student's t distribution instead of Gaussian PDF?
Thank you for the valuable suggestion. While we had not previously considered fitting a Student's t-distribution, we agree that the PDF shown in Fig. 9 appears to resemble a t-distribution, which may reflect the nature of the averaged Doppler velocity. However, the primary goal of this analysis is to estimate the underlying standard deviation (σ) of the corrected Doppler velocities, which is a physically meaningful parameter in this study. Since the standard deviation is a direct parameter in the Gaussian distribution but not in the Student's t-distribution, we employed Gaussian fitting for consistency with our objective. We have added a brief explanation in the manuscript to clarify this point.
"*In general, the PDF resembles a Student's t-distribution with a mean close to zero, which is characterized by heavier tails compared to a Gaussian distribution. This shape is likely due to the use of moving averages in the corrected Doppler velocity, where outlier effects tend to propagate to neighboring values and persist, resulting in a broader distribution. Nevertheless, since the objective of this analysis is to estimate the underlying population standard deviation—a parameter directly associated with the Gaussian distribution—Gaussian fitting was employed to obtain a representative range of residuals under various conditions.*"

Page 14, Line 327: What is the minimum error observed in the 16-km mode called sigma_{1km}? Does "1 km" mean the integration length? Please clarify it.
We have added the explanation about that as follows:
"*Here, "1 km" refers to the along-track integration length used in the standard Doppler velocity product, which averages observations over approximately 1-km intervals. This product integrates the original radar footprints (~700–800 m in size) and therefore reflects small-scale variability such as instrument-induced fluctuations, fine-scale atmospheric perturbations, and variations in particle size distributions (e.g., Hagihara et al., 2022). The estimated σ_{1km} thus captures the baseline variability intrinsic to the 1-km integrated observations.*"

Page 19, Line 426: "Section 3c" do you mean "Section 3.4"?
We are sorry for the typo. It is Section 3.4, as you mentioned. We have corrected it.

Page 21, Figure 14: Not sure what these lines denote in each subplots. I think that the multiple lines for corrected and default cases correspond the latitudinal bands, but not clearly described which is which.

We have added a brief explanation about that in the figure caption and the main body as follows:
"*Figure 14. Probability density functions (PDFs) of Doppler velocities with (solid lines) and without (dashed lines) the correction, shown across different temperature ranges. Six lines are shown for each case, corresponding to 30°-wide latitudinal bands from 90°S to 90°N. The coefficient of variation (CV) is also indicated.*"

While the individual latitudinal bands are not identified in the figure, the lines are included to illustrate the general difference in distribution width (standard deviation) between the corrected and uncorrected cases.

Page 23, Line 511: "Note: …." I think this sentence seems to be out of place.

I agree that the supplementary information appeared out of place. That part was removed from the main text. Instead, we have added following explanation.
"*Direct measurements using a precision level confirmed a similar pointing bias. Since July 2025, the tilt of the HG-SPIDER radar has been corrected and the system has been operating under long-term test observations.*"

Page 27, Line 566-567: This sentence is confusing. Do you mean that ground-based Doppler radar observation provides supplementary dataset for the analysis of satellite observation? Please clarify it.

We have clarified the meaning as follows:
"*To evaluate the impact of random noise in satellite observations, ground-based Doppler radar data with minimal noise were used as a reference. By comparing with pseudo-observations created from the ground-based data, the effect of noise on the analysis can be assessed.*"