Response to "Review of Global identification of dominant ice-particle growth in cirrus clouds using EarthCARE satellite observations"

The study applys an ice-particle growth identification algorithm that the authors developed in a pre vious study. The algorithm uses the relationship of radar reflectivity and Doppler velocity to discriminate between aggregation and depositional growth in clouds. The algorithm is applied to EarthCARE CPR data, and possible errors, namely the long integration time required to reduce noise in the Doppler velocity observations, as well as errors from pointing uncertainties in the CPR data are discussed. The method is then applied to different latitude regions, and conclusions about the prevailing ice microphysical process are drawn.
I am disappointed by the poor scientific standard of this paper. The authors do not discuss prior work relevant to their method. Ze–vd diagrams and related retrievals, such as vertical wind, particle type, and latitudinal variability, have been employed in numerous previous studies; however, the authors present their approach as if they were the first to adopt this concept. Furthermore, the method is insufficiently described. Error bars and statistical analyses of the slopes derived from the data are absent, rendering the resulting conclusions about microphysical processes unsubstantiated. In addition, as noted by reviewer Bernat Puigdom`enech Treserras, there are several major issues in how CPR pointing has been accounted for. Again, the authors appear to disregard previously published methodologies related to CPR. I find that, as presented, the method and subsequent analysis do not provide a meaningful contribution to the scientific community. Unless major revisions are undertaken, including appropriate citation of prior literature, comparison with existing studies, and addressing both my major and minor concerns and those raised by Bernat Puigdom`enech Treserras, the study is not suitable for publication.

Thank you for your comments and suggestions. We appreciate your expert perspective and the opportunity to place our study more clearly within the context of previous work on cloud microphysical processes and radar-based analyses. Your comments helped us improve the manuscript by clarifying the relationship between our approach and earlier studies using Ze–vd relationships.
In the revised manuscript, we have expanded the discussion of prior studies and clarified how the present method differs from and complements existing approaches. We have also improved the description of the methodology and addressed the issues related to CPR pointing and observational uncertainties raised in your comments and in the open review by Dr. Bernat Puigdomènech Treserras.
A significant portion of the manuscript was devoted to explaining the data characteristics and processing steps because EarthCARE observations are still very new, and at the time of the initial submission there had been no published studies using the JAXA Level-2 CPR_ECO product. In particular, the Doppler velocity observations contain substantial noise, which required careful

treatment and detailed explanation of the data processing. As a result, the scientific implications of the analysis may not have been sufficiently emphasized in the original version.

Nevertheless, the results demonstrate that information on cloud microphysical sensitivity can be extracted from Ze–$\log_{10}\upsilon$d observations from the EarthCARE CPR with a quality comparable to ground-based radar analyses, and that this diagnostic can be extended to a global scale through satellite observations. We believe that these results highlight the scientific potential of the CPR Doppler observations for studying ice cloud microphysical processes.

In the following point-by-point responses, we address each of the reviewer's comments and clarify the points that were previously unclear.

Major points:

1. The manuscript would benefit from a more comprehensive discussion of relevant prior studies that have employed Ze–vd relationships. For example, Kalesse and Kollias (2013); Protat and Williams (2011) used the relationship between Ze and vd to retrieve vertical wind velocities in clouds. Matrosov (2023) examined the dependence of Vd–Ze relationship on different ice habits in Antarctica and the Arctic, and, most closely related to the present work, Kalesse et al. (2013) applied Ze–vd relationships to investigate microphysical processes in clouds.

I encourage the authors to consider these studies and to place their method within this broader context. In particular, I would suggest to discuss how the proposed approach differs from existing methodologies, what advantages it offers, and how the findings compare critically with prior results. Given that the retrieval is applied across multiple latitude regions, a comparison with the Arctic focused results of Matrosov (2023) and the midlatitude analysis of Kalesse et al. (2013) would be especially valuable.

Thank you for your expert advice. As also mentioned in our response to the general comment above, this suggestion significantly improved the value of our manuscript. Because the points raised here are particularly important for placing our study in the context of previous work, we added two paragraphs in the Introduction to discuss the relevant prior studies (Lines 42–58).

In addition, we briefly describe the differences between the analysis method used in this study and those employed in previous studies at the beginning of Section 2.1. These additions clarify how the present approach differs from previous methodologies and highlight the advantages of the proposed analysis for diagnosing cloud microphysical growth regimes.

2. ***Several aspects of the Ze-log10(vd) method are not clear to me and need revision/clarification.***

(a) why do you use log10(vd) for your study? The logarithm has several limitations: you are excluding any positive velocities. When you fall velocities of the ice particles are large enough this

might not cause a problem. However, when the particles are small, their fall velocity is close to 0, depending on small updrafts they can even have negative velocities. By cutting of the distribution at 0m/s, you are potentially biasing your values significantly. Also, by fitting a line in log-space you are weighting your small vd significantly more than the large vd. Is that something you want to do? Previous studies have always fitted a power law directly to Ze-vd, why do you not do that?

Thank you for your comment. The issues you pointed out are indeed closely related to the limitations that should be carefully considered when applying this method to EarthCARE observations with relatively large observational noise. As discussed at the beginning of Section 3.1, we explicitly identify this issue as an important point when analyzing EarthCARE data. The impact of random errors on the analysis is also quantitatively evaluated using Fig. 6.
The reason for using log10(vd) is described in Section 2.1. By formulating the relationship based on the microphysical dependencies of radar reflectivity and terminal fall velocity, the covariance structure in the Ze–log10(vd) space acquires a direct physical interpretation related to cloud microphysical processes.
Furthermore, the concern you raised regarding the potential bias associated with small Doppler velocities is addressed in the revised analysis method presented in Section 4.
Unlike approaches that directly fit a power-law relationship to observational data, the formulation adopted here derives the Ze–log10(vd) relationship from the microphysical dependence of reflectivity and terminal velocity on particle size.

(b) looking at your distributions, the correlation between Ze and log10(vd) is really small, usually, two datasets are considered to be linearly related if the correlation is higher than 0.5. Especially for your data at Temperatures colder than 223K I do not see a linear correlation between Ze and log10(vd). Can you comment on why that is the case and why you still chose the linear fit?

Thank you for your comment. There appears to be a misunderstanding regarding our analysis. We do not apply a linear regression to the Ze–log10(vd) distributions, nor do we assume that a single slope should represent the entire dataset. The joint probability density function (Joint PDF) in the Ze–log10(vd) space contains information from multiple cloud layers and microphysical regimes, and therefore can be regarded as a superposition of many different slopes. Accordingly, the validity of the method does not depend on the existence of a strong linear correlation between Ze and log10(vd).
In fact, Seiki et al. (2025) examined how Ze and log10(vd) evolve along a single vertical profile within an individual cirrus cloud (see Fig. 3 in Seiki et al., 2025). In addition, the variability of the slope ΔZe/Δlog10(vd) was evaluated by constructing its frequency distribution over one year of observations (Fig. 5 in Seiki et al., 2025), which demonstrated the diversity of microphysical growth

regimes.

In this framework, a low correlation between Ze and log10(vd) indicates the coexistence of multiple particle growth modes, whereas a high correlation implies that particle growth is constrained to a more uniform regime. To extract the representative growth mode from this diversity, a principal component analysis is applied, and the leading principal component vector is shown as the black line in the Joint PDF. This vector represents the dominant particle growth tendency.

Seiki et al. (2025) also showed that numerical simulations can produce correlations as high as 0.84 at temperatures below 223 K. Such high correlations indicate insufficient diversity in particle growth processes in the model. Therefore, the magnitude of the correlation itself can serve as a useful metric for evaluating cloud microphysical representations in climate models.

Although the magnitude of the correlation contains meaningful information and was discussed in Seiki et al. (2025), it cannot be utilized in the revised method proposed in Section 4 of the present study. This is because the revised method computes the median Doppler velocity for each Ze bin, which removes the covariance information between Ze and log10(vd). Since the correlation-based metric is more suitable for low-noise ground-based radar observations, we did not elaborate on it in the present manuscript.

(c) I do not understand the step from equation 1 to 2. What is the change in Ze? Do you mean temporal change in Ze at the same height? Or change of Ze with height? This is an important aspect. Also, what is e in equation 2? Where is it coming from? Same for equation 3.

We appreciate the reviewer pointing out that the explanation of the differential operation and the appearance of "e" was not sufficiently clear in the original manuscript.

Thank you for your comment. First, if your comment refers to a typographical error in the coefficient of the second term on the right-hand side of Eq. (1), you are correct. The coefficient was mistakenly written as $(60-10b)10$, and it has been corrected to $(60-10b)$.

Equation (2) is obtained by taking the total differential of Eq. (1). This operation is purely a mathematical manipulation and does not assume a specific physical interpretation such as a temporal change or a vertical variation at a fixed location. For example, one may interpret it as a change with height at the same horizontal position, but the derivation itself simply represents the differential relationship between the variables.

For completeness, the derivation is summarized as follows. Starting from Eq. (1):

$\Delta Z_e = \Delta(10\log_{10} M) + \Delta\{(60-10\mathrm{b})\log_{10}\overline{D}\}$,

When differentiating logarithmic functions, the common logarithm (base 10) is converted to the natural logarithm (base e). Using the base-conversion relation,

$\log_{10} M = \frac{lnM}{ln10} = \log_{10} e \, lnM,$

the differential of the first term becomes

$\Delta(10 \log_{10} M) = 10 \log_{10} e \frac{\Delta M}{M}.$

Similarly,

$\Delta\{(60 - 10\mathrm{b}) \log_{10} \bar{D}\} = (60 - 10b) \log_{10} e \frac{\Delta \bar{D}}{\bar{D}}.$

Combining these expressions yields Eq. (2).

We also recognize that introducing $e$without explanation may reduce readability in a section where several variables are newly defined. To avoid this confusion, we added the following clarification in the manuscript:
"*When computing the total differential, the base of the logarithm is converted from the common logarithm to the natural logarithm, introducing the Euler number e (the base of the natural logarithm).* Thus, a change in Z_e is attributed to a change in M and a change in $\bar{D}$ as follows:"

(d) how do you do the fitting of the line to your Ze-vd data? Is this a least-squares method? In the description of your method you are saying that the slope of deltaZe/delta(log10(vd)) depends on deltaM/deltaD. Why do you know fit a line into the Ze-log10(vd) space? Also, you are then talking about variation in slope, which variation do you mean? Between different heights? This is not clear to me.

Thank you for your comment. Although part of our response overlaps with our reply to comment (b) above, this is a very important point, and we therefore provide a detailed explanation here.
First, we do not fit a line to the Z_e–$\log_{10} v_d$ data. Instead, we first construct the joint probability density function (Joint PDF) in the Z_e–$\log_{10} v_d$ space, and then calculate the principal eigenvector from its covariance matrix. In other words, we apply principal component analysis (PCA). This is explicitly stated in the final paragraph of Section 2.1.
Under the constraint of cloud microphysical processes, the mass concentration M and mean particle diameter $\bar{D}$ vary in a physically related manner. Since both Z_e and terminal velocity v_t depend on M and $\bar{D}$, Z_e and v_t also vary in a characteristic manner depending on the governing microphysical process. This dependence appears in the right-hand side of $\Delta Z_e / \Delta \log_{10} v_d$ (Eq. 6). The fact that Z_e and terminal velocity exhibit characteristic variations associated with cloud microphysics through constraints imposed by the particle size distribution moments has also been pointed out in the pioneering study by Kalesse et al. (2013), which you kindly brought to our attention.

In principle, values of $\Delta Z_e/\Delta\log_{10} v_d$, which are sensitive to cloud microphysics, can be evaluated for any pair of cloud layers, and each choice has its own physical meaning. We do not intend to restrict the applicability of the method to a single definition. In the present study, however, for practical reasons, we statistically analyze all pairs of cloud layers belonging to cirrus systems within a given temperature range. This is because Doppler velocity observations from CPR are too noisy to extract meaningful signals unless a sufficiently large number of samples is used.

In addition, the phrase "variation in slope" in Line 121 was intended to mean that the characteristic slope changes from one temperature range to another. We agree that this expression was unclear, and we have revised the manuscript to make this point explicit as follows:
"Based on Seiki et al. (2025), *the systematic increase of the representative slope with temperature* is explained using microphysical theory."

(e) You are saying that during aggregation the mass concentration does not change while D increases. This is only correct if your fall velocity does not change during aggregation. If the fall velocity increases, your mass flux increases and thus mass concentration reduces. Can you comment on that?

Thank you for this insightful comment. The process you describe is indeed important. In this study, however, we treat this mechanism not as aggregation but as gravitational sedimentation. This distinction follows the formulation commonly adopted in cloud microphysical schemes used in weather and climate models, where aggregation and sedimentation are treated as separate processes and solved numerically.

Because gravitational sedimentation cannot be expressed in the same analytical form as Eqs. (11) and (13), its effect cannot be interpreted in terms of a simple increase or decrease of the theoretical slope derived in our formulation.

In Seiki et al. (2025), the contribution of gravitational sedimentation was evaluated numerically. The results showed that its contribution is considerably larger than that of aggregation, leading to the conclusion that vapor deposition and gravitational sedimentation are the dominant processes within cirrus clouds.

Note that the mass concentration considered here represents the average mass within a radar sampling volume rather than the flux of particles through that volume.

(f) in line 123 you say that the slope is estimated as a fixed value as equation 11. I do not understand what that means. Why is (60-10b)/β considered to be fixes? And 60/β is not?

Thank you for pointing this out. The description in the manuscript may have been unclear. In Eq. (6), the relationship was written in a total differential form with M and D treated as variables. In contrast,

the expression referred to as a "fixed value" in line 123 was intended to indicate coefficients that do not include the differential terms ΔM or ΔD.
In this sense, both $(60-10b)/\beta$ and $60/\beta$ should be regarded as fixed coefficients in the formulation. The use of the terms "fixed value" and "constant value" in different places may have caused confusion, and we have revised the manuscript to clarify this point.
Note that these coefficients contain the parameters b and β, which are related to the nonsphericity of ice particles. In reality, these parameters may exhibit some variability. However, in the present formulation they are treated as constants for simplicity of the analytical derivation.

(g) you are also stating in line 133 that your method only works if no sublimation or ice nucleation is not playing a key role. If that is the case then you need to restrict your dataset to cloud regions where you can exclude the two processes.

Thank you for your comment. As stated in the manuscript, the applicability of the method assumes that particle growth occurs while particles fall downward and that nucleation or sublimation are not dominant processes.
The dataset used in this study is already restricted to such conditions through the sampling criteria described in Eqs. (8)–(10). In particular, Eq. (9) represents the increase in particle size with decreasing height, which implies particle growth during sedimentation. Together with Eq. (8), this condition suggests that particle mass also increases downward. In addition, Eq. (10) ensures that the terminal fall velocity exceeds the vertical air motion, indicating that the particles are falling relative to the air. Under these conditions, active ice nucleation is not expected to dominate the microphysical evolution. This point is already described in the original manuscript (Lines 132–134), where the sampling conditions are explained.

3. There are no error bars or other statistical analysis regarding the goodness of your fit to Ze log10(vd). Without the inclusion of this, the results in Figure 18 and 12 are not useful, as one does not know the spread of the data, and thus estimating if the difference of e.g. 30S-EQU in Figure 18 compared to the other regions is significant or not is not possible.

Thank you for this important comment. In the present analysis, the slope is not obtained through a regression fit between Ze and log10(vd). Instead, it is derived from the principal component vector of the joint probability density function (Joint PDF) in the Ze–log10(vd) parameter space. Because the slope corresponds to the direction of the dominant eigenvector of the covariance matrix, conventional error bars associated with regression fitting are not defined in this framework.

The spread of the distribution in the Joint PDF is instead represented by the corresponding eigenvalue of the principal component. As mentioned in our response to comment 2(b), quantities such as the correlation coefficient or the eigenvalue can provide useful information about the diversity of particle growth processes.
However, in the revised method proposed in this study, the analysis is based on the median Doppler velocity for each Ze bin. This procedure reduces the covariance information between Ze and log10(vd), and therefore metrics such as eigenvalues or correlation coefficients cannot be directly used to characterize the spread in the same manner as in the original method.
Figure 12 nevertheless illustrates how observational noise influences the estimated slope. In particular, the comparison between the cases with $\sigma = 1$ and $\sigma = 2$ indicates that larger noise levels tend to reduce the estimated slope. From this perspective, the relatively large slopes observed in the 60S–90S region may partly reflect the lower noise level associated with the 16 km observation window mode.
Since the winter of 2025, the EarthCARE CPR observation strategy has been updated, and the coverage of the 16 km observation mode has been expanded. As a result, lower-noise observations will become available over a wider range of regions. This improvement will make it possible in future studies to better distinguish between variations caused by microphysical differences and those caused by differences in noise levels.

4. Do you consider attenuation in your HG-SPIDER radar? At W-Band this is an important aspect, and you need to at least take gas attenuation into account, if not also attenuation by liquid and ice, depending on the situation (i.e. if rain was present, this needs to be corrected). Otherwise your Ze values are strongly biased and your slope is not correct.

Thank you for this important comment. We agree that attenuation is an important issue for W-band radar observations. In the EarthCARE Level-2 product (CPR_ECO), gas attenuation is already taken into account, while attenuation caused by clouds is not corrected in the current dataset. Therefore, the radar reflectivity values used in this study may still contain some attenuation effects. The same applies to the HG-SPIDER ground-based radar observations.
However, the analysis in this study focuses exclusively on upper-level cirrus clouds. Because these clouds do not contain liquid precipitation such as rain and are typically optically thin, strong attenuation effects are not expected to occur in most cases.
In addition, the objective of the present method is not to determine the absolute true value of the slope. Rather, the goal is to provide a diagnostic framework that can constrain the behavior of weather prediction and climate models using satellite observations. In Seiki et al. (2025), satellite simulators were used to generate attenuated radar reflectivity fields from numerical models, and these were compared with ground-based observations. In this sense, as long as both observations and model

simulations include comparable attenuation effects, the comparison remains physically consistent.

These points have been clarified in the manuscript as follows:

"In the EarthCARE satellite analysis, gas attenuation is corrected in the Level-2 CPR_ECO product, while attenuation caused by clouds is not corrected in the current dataset. Therefore, the radar reflectivity used in this study may still contain attenuation effects due to clouds. The HG-SPIDER ground-based radar data are processed in the same manner to ensure consistency between the satellite and ground-based analyses. In future comparisons with numerical weather prediction and climate models, satellite simulators can be used to generate attenuated radar reflectivity fields from model outputs, allowing a fair comparison between observations and simulations (e.g., Seiki et al., 2025).
"

5. How do you select your cirrus cases? You only state that the cloud must be at 16km or below. However, how can you be certain that you are using cirrus clouds? How do you exclude e.g. strong convection and other cloud types?

Thank you for pointing this out. We apologize for the omission of the definition of cirrus clouds in the original manuscript. To clarify this point, we have added the following description to the sampling procedure in Section 2.2:

"In this study, *the analysis is restricted to cirrus clouds. Based on previous combined CloudSat–CALIPSO analyses, clouds with a cloud-base temperature below 253 K are defined as cirrus (Seiki et al., 2019). Because the cloud base is sufficiently cold, these clouds are assumed not to contain liquid-phase hydrometeors. In addition, cirrus clouds are typically optically thin, and therefore attenuation effects caused by clouds are expected to be small.*"

6. You are stating that HG-SPIDER had a tilt of 0.7°. How did you correct for that? How have you checked the pointing of your radar? Did you do sunscans? Or what other method have you employed? Did you use the same data and just adjusted the vd accordingly? This is not described in a well enough manner.

Thank you for this important comment. The possibility of a pointing bias in the HG-SPIDER radar was identified through a comparison between the EarthCARE satellite observations and the ground-based HG-SPIDER measurements used in this study. Following this finding, the HG-SPIDER system was re-examined by the instrument operators at NICT.

During the summer of 2025, the pointing of the radar was measured using a high-precision digital level with an accuracy better than 0.05°. This measurement revealed that the radar had a tilt of approximately 0.70°. Since July 2025, the tilt has been corrected and the system has been operating with the corrected alignment during ongoing long-term observation tests.

As shown in Fig. 3, a sufficiently large sample size (on the order of a full year of observations) is required to obtain a stable estimate of the tilt from the statistical relationship between Ze and Doppler velocity. Therefore, the HG-SPIDER dataset used in this study still contains the misalignment.

A separate study led by NICT researchers is currently in preparation to provide an external calibration of the CPR_ECO Level-2 data, in which the HG-SPIDER pointing bias will be described in detail. For this reason, the present manuscript only briefly mentions this issue.

Following the reviewer's suggestion, we have added the following sentence to clarify the observational evidence in the manuscript:

"*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.*"

7. Figure 1 and all similar figures: the readability of the Ze-log10(vd) relation is really bad. Also, what are the colored lines?

Thank you for pointing this out. We agree that the description of the colored contours was missing in the original figure caption. We have added a clarification in the caption of Fig. 1 (and similar figures) stating that the colored contours represent the joint probability density function (Joint PDF) of Ze and log10(vd).

Regarding the readability of the figures, the purpose of these plots is to visualize the overall structure of the Joint PDF rather than to read numerical contour values. Increasing the number of contour labels would in fact reduce the visual clarity of the distribution. Therefore, the figures are designed to emphasize the relative density structure of the Joint PDF (i.e., regions of high and low probability density).

8. It is important to state what kind of meteorological conditions the data for Section 2.2 was from. Did you have mainly frontal systems? What kind of clouds? How many clouds? How many pixels of clouds went into the analysis? Perhaps it would be a good idea to show a Ze and vd time-height plot of a representative case study to give the reader some context.

Thank you for this helpful suggestion. The atmospheric data used for the analysis in Section 2.2 were obtained from the NCEP–FNL reanalysis dataset, in the same manner as for the EarthCARE satellite analysis presented in Section 5. We apologize that this information was not explicitly stated in the original manuscript. We have added the description about the NCEP-FNL reanalysis dataset in Section 2.2.

As mentioned in our response to Major Comment 5, cirrus clouds observed over Koganei, Tokyo were sampled over a one-year period. Therefore, the dataset includes a variety of cloud systems. In general, many of the sampled cirrus clouds are associated with frontal systems of midlatitude cyclones.

A representative example of the observed cloud structure was previously shown in Fig. 3 of Seiki et al. (2025), which includes the time–height structure of Ze and Doppler velocity. For reference, this figure has been reproduced in the response to the reviewer.

In addition, the sample sizes indicated in Fig. 3 of the manuscript represent the number of vertical profiles identified as cirrus clouds used in the analysis.

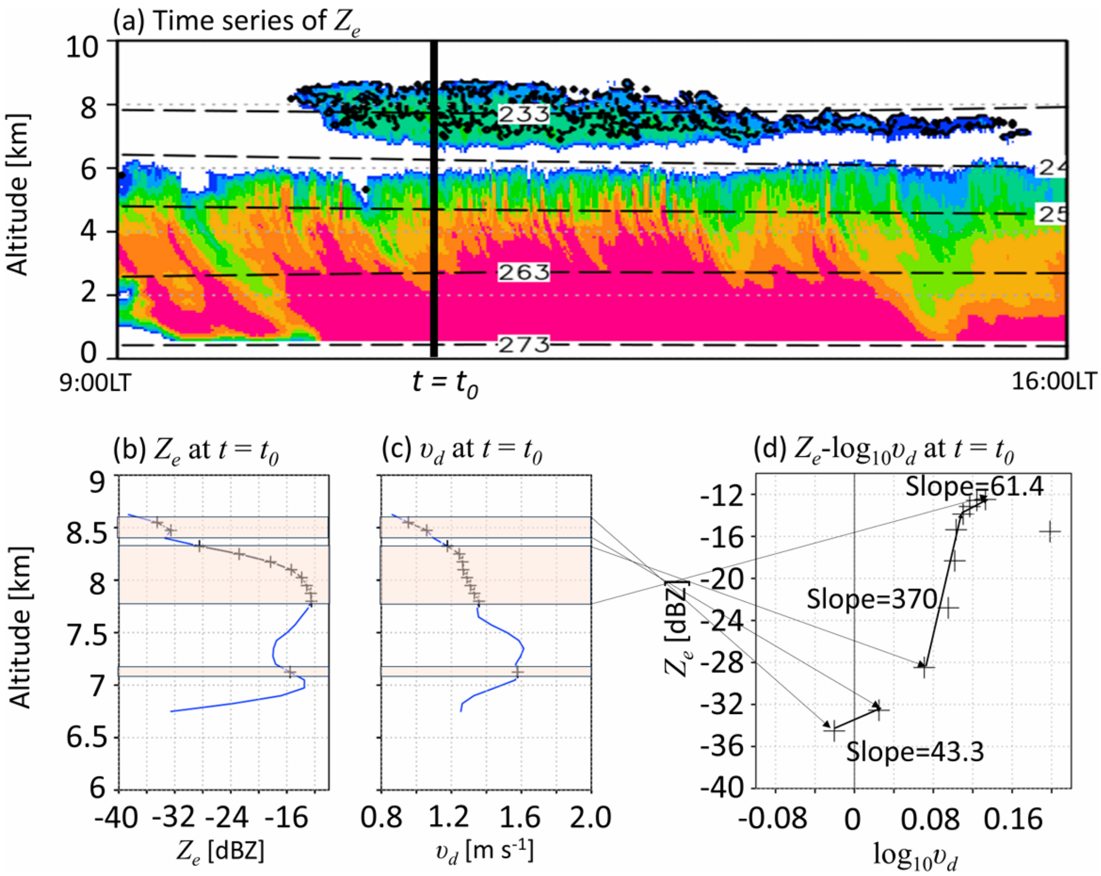

FIG. 3. (a) Vertical distribution of Z_e observed from 0900 to 1600 local time on 6 Jan 2022. Isotherms are shown as dashed lines, and cirrus layers in the growth mode are enclosed by solid black lines. (b),(c) Sample profiles of Z_e and v_d at time t_0, with cirrus layers in the growth mode highlighted by shaded areas. (d) Scatterplot of Z_e and $\log_{10}v_d$ at time t_0, with slope values superimposed.

After Figure 3 in Seiki et al. (2025), © American Meteorological Society. Used with permission.

9. Why did you choose the threshold in equations 8 to 10?

Thank you for this question. The thresholds used in Eqs. (8)–(10) were chosen to be sufficiently small so that they do not affect the results of the analysis. From the observational statistics, the median values of Ze and Doppler velocity typically change by about 20 dBZ and 0.4 m s^{-1}, respectively, over a temperature range of approximately 40 K. This corresponds to approximate gradients of

$dZ_e/dT \approx 0.5$ and $dv_d/dT \approx 0.01$.

The thresholds used in Eqs. (8)–(10) were set to values about one order of magnitude smaller than these typical gradients, ensuring that they act only as minimal constraints and do not significantly influence the sampling.
As illustrated in Fig. 3 of Seiki et al. (2025), the variations between vertical layers within individual cirrus clouds are often considerably larger than these threshold values. Therefore, the chosen thresholds are sufficiently small relative to the natural variability of the observed profiles.

10. When discussing the integration length, you are stating that the correlation coefficient slightly decreases by temporal smoothing. However, you correlation coefficient was already really small to begin with, this is not a significant reduction.

Thank you for this comment. As discussed in our response to Major Comment 2(b), the small correlation coefficient itself does not invalidate the method used in this study. Rather, the magnitude of the correlation reflects the diversity of particle growth processes within the sampling volume.
The purpose of examining the integration length was therefore not to evaluate the absolute value of the correlation coefficient, but to assess how strongly the covariance structure related to particle growth is affected by spatial averaging. By applying spatial filtering through the integration length, we investigated the extent to which the covariance associated with cloud microphysical processes is smoothed out.
The results indicate that the characteristic spatiotemporal scale of the dominant microphysical processes does not change significantly between the 1 km and 10 km integration lengths.

11. In Section 3.2 you also state that the slope shifts by 10 to 15%. However, this is not supposed to be a problem because the difference between observations and simulations are larger than that. I don't understand the magnitude of difference between simulations and observations matter here. In the simulations all kinds of biases caused by the microphysics schemes can be expected, so just because the differences here are smaller than that is not a argument for 10% to be acceptable.

Thank you for this comment. The representative slope derived in this analysis is not intended to provide an absolute estimate of a physical parameter. Rather, it is used as a comparative diagnostic to highlight differences in the dominant cloud microphysical processes between different cloud systems, regions, or datasets (e.g., satellite observations versus model simulations).
Therefore, the key requirement is the internal consistency of the method used to estimate the representative slope. As long as the same methodology is applied, differences in the slope between datasets can be interpreted as differences in the dominant microphysical regimes.
In Seiki et al. (2025), the differences in representative slope between numerical simulations and

ground-based radar observations were found to be substantially larger than the methodological variability discussed here. Consequently, the variability of about 10–15% associated with the integration length represents a relatively small methodological uncertainty and does not prevent the use of the slope diagnostic for evaluating differences between models and observations.

12. With your method you want to distinghuish between aggregation and depositional growth. However, most previous studies have found that aggregation is mainly active at temperatures warmer than-20°C (e.g. Von Terzi et al., 2022, and references therein). This is mainly due to the increase of the quasi-liquid layer on ice particles with warmer temperature, and the mechanical interlocking of dendritic branches which are found to be growing at temperatures around-15°C. Why do you expect aggregation at temperatures colder than-20°C? In my opinion a method to distinguish between aggregation and depositional growth at these temperatures is not required because previous studies said that it is basically negligible.

Thank you for this important comment. We agree that aggregation driven by mechanical interlocking of dendritic branches is generally considered to be most active at relatively warm temperatures (around −15 °C), as discussed in previous studies such as Von Terzi et al. (2022).
However, the microphysical behavior of ice particle aggregation in very cold cirrus clouds remains highly uncertain. As discussed by Phillips et al. (2017), laboratory measurements of sticking efficiency are mainly constrained down to temperatures of about −30 °C. At much colder temperatures, such as those frequently encountered in cirrus clouds (sometimes below −60 °C), both experimental and theoretical constraints on sticking efficiency are limited.
Because of this lack of observational constraints, numerical weather prediction and climate models often rely on parameter tuning for aggregation processes under very cold conditions. Previous studies have shown that such parameterizations can lead to an overestimation of aggregation in cold cirrus environments (Seiki and Ohno, 2023; Seiki and Nagao, 2024).
Therefore, evaluating the relative importance of aggregation and depositional growth in very cold clouds should not be excluded a priori. One motivation of the present method is precisely to provide an observational constraint on microphysical growth regimes in these poorly constrained temperature ranges.

Phillips, V. T. J., M. Formenton, A. Bansemer, I. Kudzotsa, and B. Lienert, 2015: A Parameterization of Sticking Efficiency for Collisions of Snow and Graupel with Ice Crystals: Theory and Comparison with Observations. *Journal of the Atmospheric Sciences*, **72**, 4885–4902, https://doi.org/10.1175/JAS-D-14-0096.1.

Seiki, T., and T. Ohno, 2022: Improvements of the Double-Moment Bulk Cloud Microphysics Scheme in the Nonhydrostatic Icosahedral Atmospheric Model (NICAM). *Journal of the Atmospheric Sciences*, **80**, 111–127, https://doi.org/10.1175/JAS-D-22-0049.1.

——, and T. M. Nagao, 2024: Evaluation of the Aggregation Efficiency Modeling at Colder Atmospheric Temperatures in Comparison to Satellite Observations. *Journal of the Atmospheric Sciences*, **81**, 1689–1710, https://doi.org/10.1175/JAS-D-23-0208.1.

13. In your discussions you are talking about Rayleigh and non-Rayleigh scattering, which is definitely necessary at W-Band. How large are the largest observed irregular ice crystals in cirrus clouds? Are they large enough to move to the Mie regime? Otherwise, aggregation is the most likely source of non-Rayleigh scattering, as ice crystals are too small to cause that. However, I see that you are only considering temperatures colder than-20°C. At these temperatures, most previous studies have stated that aggregation (the main driver of non-Rayleigh scattering in the W-Band) can mostly be neglected (see previous comment), thus also Mie-scattering can probably be neglected.

Thank you for this comment. We agree that the wavelength of W-band radar (~3 mm) implies that the size parameter for typical ice particles in isolated cirrus clouds is generally small, and therefore Rayleigh scattering is expected to dominate in most cases.

In the present study, the discussion of non-Rayleigh scattering is included mainly in the context of potential extensions of the method to denser cloud systems. In particular, we consider the possibility of applying the analysis to deeper anvil cirrus associated with deep convection. In such environments, large ice particles and frozen droplets detrained from convective cores may be present, and deviations from Rayleigh scattering can become more relevant.

We also note that, in the Mie regime, radar backscattering does not follow a simple power-law dependence on particle size, but instead exhibits oscillatory behavior as a function of the size parameter. Therefore, the analytical framework used in this study may not be directly applicable in such conditions, and caution is required when interpreting the slope. In accordance with this, the description of the Mie regime has been revised in Section 2.1.

For this reason, the latter part of the Discussion introduces the CPR–DPR coincident dataset as a possible avenue for future studies. The combination of these sensors may enable the investigation of cloud systems with larger particle sizes, where such non-Rayleigh effects could be more significant.

14. Figure 6 is not discussed in great enough detail. Where does this positive bias come from? Why do they only see it in cirrus? What is the difference between the red and blue line? How do the authors want to adress this strong positive bias in their analysis? Please explain this Figure and its meaning in more detail.

Thank you for pointing this out. The explanation of Fig. 6 in the original caption may have been confusing. In this figure, Gaussian noise with a standard deviation of 1 m s^{-1} is added to all pixels of the ground-based Doppler velocity data, and the PDF of the resulting velocity difference is examined. The red line shows the PDF calculated using all cloudy pixels. Because the sample size is sufficiently large, the resulting distribution closely follows the reference Gaussian distribution (black line).
The blue line represents the error distribution for cirrus samples selected by the sampling procedure used in the Z_e–$\log_{10}\upsilon_d$ PCA analysis. In this analysis, the logarithmic transformation requires positive Doppler velocities. Therefore, samples where $\upsilon_d + \delta v$ becomes negative are excluded. This selection causes the error distribution for the cirrus samples to become positively skewed as was commented by the major comment 2a.
We have revised the caption of Fig. 6 to clarify this point.

15. As I find the comment by Bernat Puigdom`enech Treserras to be sufficiently detailed on the problems concerning the pointing correction for CPR, I will not further state points here. I suggest the authors to address his comments during the review.
Thank you for this comment. We appreciate the constructive suggestions provided by Dr. Treserras. Following his detailed comments regarding the pointing correction of CPR, we have implemented an improved data screening procedure to selectively remove pixels with low reliability.
The main changes include excluding sea-ice-covered regions and introducing a quantitative reliability assessment performed for each 1° × 1° latitude–longitude grid. These reliability criteria are described in detail in Appendix B. These modifications were introduced to address the issues raised in the reviewer's comments.

Minor points:

- ***First paragraph of introduction (Line 30 to 32) could use some improvement. Sure, observations of vertical air motion are important, but this is not what you are doing in your study is it? And was that the main reason why Doppler was employed on EarthCARE?***

Thank you for this comment. We agree that the original wording could give the impression that the present study focuses on retrieving vertical air motion. In this study, Doppler velocity is used primarily to investigate cloud microphysical processes rather than vertical velocity itself. To clarify this point, we revised the sentence in the Introduction to emphasize that Doppler radar observations contain information on both vertical air motion and hydrometeor fall velocity, which together provide insight into cloud microphysical processes.

- ***Line 40: you write Doppler velocity observations have been shown to be sensitive to distinguish between different cloud microphysics schemes. I don't understand what you want to say here. What***

are the Doppler observations sensitive to? In my opinion Doppler information is important because some ice microphysical processes are linked to an increased Doppler velocity. Especially riming is known to increase vd and therefore, considering vd is useful to find regions of enhanced riming. Also, the early stages of aggregation and depositional growth influence the Doppler velocity.

Thank you for this comment. We have modified the sentence as follows:

” Through early evaluations of atmospheric models using in-situ observations (e.g., Roh et al., 2024), Doppler velocity observations have been shown to be *sensitive to hydrometeor terminal velocity and growth processes, and therefore provide a useful constraint for distinguishing different cloud microphysics schemes.*”

- ***Line 46: is the ATBD available somewhere? Please add a citation here!***

Thank you for the suggestion. The EarthCARE Algorithm Theoretical Basis Document (ATBD) is publicly available from the JAXA EarthCARE document repository, although it does not have a DOI. We have therefore added a citation with the corresponding URL and access date in the revised manuscript.

- ***Line 87: where do you get the information that quasi-stationary downdraft exist associated with snowfall drag? Please cite the relevant literature!***

Thank you for this comment. We agree that the original wording required clarification and a supporting reference. We have therefore revised the sentence and added a citation to Houze (1997), which describes the occurrence of broad and relatively weak downdrafts in stratiform snowfall regions. The sentence has been modified accordingly in the revised manuscript.

- ***Line 95: The beginning of the sentence could be improved by removing ”in” (Prior to the analysis...)***

Thank you for the suggestion. We have removed “in” and revised the sentence accordingly.

- ***Title of Section 2.2 and all other mentions of in-situ observations: radar observations are per definition remote sensing observations, in-situ means that you are investigating something within its current location (i.e. aircraft probes that measure the properties of the particles that are within the same volume). What you likely mean is ground based. Please correct all instances of in-situ.***

Thank you for this comment. We agree that the term “in-situ” is more appropriately used for measurements made directly within the sampled air volume (e.g., aircraft probe observations). Therefore, we have replaced all instances of “in-situ radar observations” with “ground-based radar observations” throughout the manuscript.

- ***Line 108: where do you get the temperature information from?***

Thank you for pointing this out. The atmospheric variables used in this analysis were obtained from the NCEP Final (FNL) operational global analysis dataset. We have added this information and the corresponding citation in the revised manuscript.

- ***Line 111: what do you mean by "this indicates the transition of microphysical sensitivity from cloud top toward cloud base"?***

Thank you for the comment. In this sentence we intended to describe the systematic change in the representative slope with increasing temperature, which reflects the transition of dominant cloud microphysical regimes from colder cloud-top regions to relatively warmer and deeper parts of the cloud. To clarify this point, we have slightly revised the sentence in the manuscript as follows:

"*This indicates that the microphysical regimes contributing to the* Z_e*–*$\log_{10}\upsilon_d$ *relationship change systematically with temperature, reflecting the transition from cloud-top to deeper cloud layers.*"

- ***Line 131: you state that in the upper troposphere, ice nucleation, aggregation, deposition, sublimation and sedimentation dominate. Please cite sources for this statement!***

Thank you for the comment. We agree that a reference should be provided for this general statement. We have therefore added a citation to a review of ice-cloud microphysical processes (e.g., Khvorostyanov and Curry, 2014) and modified the sentence as follows:

"In the upper troposphere, *where liquid-phase hydrometeors are absent*, cloud microphysical processes *such as* ice nucleation, aggregation, vapor deposition, sublimation, and gravitational sedimentation dominate *(e.g., Khvorostyanov and Curry, 2014).*"

- ***Line 138: which year are the January to March data taken from?***

Thank you for the comment. The data used in this analysis correspond to the HG-SPIDER observations from January to December 2022, which is described at the beginning of Section 2.2. To avoid ambiguity, we have clarified that the January–March data refer to the year 2022.

We also note that the analysis period of the EarthCARE CPR data differs from that of the HG-SPIDER observations, as the Level 2 CPR data provided by JAXA were only available for January to March 2025 at the time this study was initiated (see Section 3 for details).

- ***Line 226: please cite your sources for the random error statement!***

Thank you for the comment. Although Hagihara et al. (2022) was already cited in the preceding sentence, we agree that the source of the random error statement should be explicitly indicated here. Therefore, we have added the citation to Hagihara et al. (2022) at the beginning of the sentence to improve clarity.

- ***Line 246: you write "to assess the influence of this noise" do you mean the positive bias? Or the increase of noise due to different PRF?***

Thank you for the comment. Here, "the influence of this noise" refers to the positive bias introduced in the Z_e–$\log_{10}\upsilon_d$ analysis when random errors are added to the Doppler velocity. As explained in our response to Major Comment 16, the sampling condition requiring positive values of $\upsilon_{d,20km} = \upsilon_d + \delta_{\upsilon,window}$ results in a positive bias in the Doppler velocity used in the analysis. This issue is addressed in Section 4 of the manuscript.

- ***Line 249: Where do I see the greater spread toward the left of the diagram? I did not find that in your Figure 6.***

Thank you for pointing this out. Our wording was not sufficiently clear. In this part of the manuscript, the discussion was intended as an interpretation of the error sources in the Z_e–$\log_{10}\upsilon_d$ diagram derived from the EarthCARE satellite observations shown in Fig. 4. Therefore, we have revised the text to explicitly state that "the diagram" refers to the Z_e–$\log_{10}\upsilon_d$ diagram in Fig. 4.

- ***Figure 4, 6 and 11: why do you choose these x-axis limits? Your data is clearly going to smaller and larger vd***

Thank you for this comment. The x-axis limits were chosen to focus on the physically meaningful Doppler velocity range relevant to the present analysis.

For the EarthCARE satellite observations, large absolute Doppler velocities are affected by Nyquist folding. In the JAXA Level-2 product, a practical threshold of ±3 m s^{-1} is adopted based on empirical assessments (Hagihara et al., 2022). Therefore, in Fig. 6 the x-axis range is limited to this meaningful range of Doppler velocity.

For Figs. 4 and 11, the x-axis limits were chosen to match the reliable Doppler velocity range observed by the ground-based HG-SPIDER radar, which has much smaller noise. Extending the axis to larger Doppler velocities mainly visualizes the spread caused by noise in the satellite observations rather than additional physical signals.

Therefore, the figures intentionally focus on the Doppler velocity range where the microphysical interpretation of the Ze–$\log_{10}\upsilon d$ relationship is most meaningful.

- ***Line 410: I dont understand this. So what criterion do you know apply to your data? Please state that explicitly here.***

Thank you for the comment. To improve readability, we now explicitly refer to the thresholds defined in Eqs. (8; 10) at this point in the manuscript. In the original sampling criteria, three thresholds were used. In the revised analysis, the temperature gradient criterion for radar reflectivity (Eq. 8) is retained because it is considered reliable, while the temperature gradient criterion for Doppler velocity (Eq. 9) is removed due to its sensitivity to observational noise. The non-negativity condition required for the logarithmic analysis (Eq. 10) is naturally retained.

- ***Line 441: What is this NCEP-FNL product? Please provide the full name and a citation here.***

Thank you for the comment. The full name and citation of the NCEP-FNL dataset have now been provided at its first occurrence in Section 2.2 as "the National Centers for Environmental Prediction Final (NCEP-FNL) operational global analysis dataset (National Centers for Environmental Prediction et al., 2000)". Subsequent mentions use the abbreviation NCEP-FNL for brevity.

- ***Line 491 to 494: do you mean for all latitudes? Or just the one that Japan lies within? Are the two possible causes all possible causes? Or could a wrong pointing correction of earthcare, or different microphysics account for that? Because how do you know that Japan is representative for that latitude? Japan is a large island, with potentially great moisture supply, that could be missing in other regions, thus different microphysical processes might prevail in different regions of the same latitude.***

Thank you for the comment. In this part of the manuscript, we refer to the **altitude dependence**, not latitude. The statement describes that the difference between the median Doppler velocities derived from HG-SPIDER and EarthCARE becomes larger at lower temperatures (i.e., higher altitudes) and smaller at higher temperatures (i.e., lower altitudes). To avoid this misunderstanding, we have revised the text to explicitly clarify this explanation.

The possible cause of the median Doppler velocity difference is discussed in the following paragraph. As explained in our response to Major Comment 6, the HG-SPIDER radar was found to have a small misalignment, which causes contamination of the Doppler velocity by horizontal winds in the upper troposphere. This misalignment has been confirmed by direct measurements using a high-precision level instrument.

We agree that comparisons using multiple ground-based radars would be desirable to further evaluate the representativeness of the results. In the present study, we confirmed that the regional dependence of the representative slope is relatively small (Fig. 18). In addition, Seiki et al. (2025) compared the vertical distribution of radar reflectivity between CloudSat and HG-SPIDER and confirmed the general representativeness of the observations. Future analyses using additional ground-based radar datasets would further strengthen this evaluation.

We agree that the slope could potentially vary in regions with particularly moist environments, such as the Japanese summer monsoon region. Investigating the relationship between water vapor amount and the representative slope is indeed an interesting direction, and we plan to examine this in future work.

However, evaluating seasonal variability requires at least one year of accumulated observations, and analyses of regional variability also require a careful balance between sufficient sample size and spatial localization of the data.

In addition, the EarthCARE CPR observation configuration changed twice during 2025. For example, the observation window mode in low latitudes was changed from 20 km to 18 km, and the latitude separating the 18 km and 16 km modes was changed from 60° to 40°. Because a sufficiently homogeneous dataset has not yet accumulated, we consider that a robust discussion of regional variability will require a longer observation period.

- ***Line 530-533: this finding needs to be discussed with respect to other studies that have looked into microphysical processes within ice clouds at these latitudes.***

Thank you for this comment. We agree that the original discussion was not sufficiently detailed. We have therefore substantially revised and expanded this part of the manuscript as follows:

"Seiki et al. (2025) investigated the sensitivity of the slope to cloud microphysical modeling using numerical simulations. In their analysis, the slope tended to decrease when aggregation became stronger or when vapor deposition was suppressed, consistent with the theoretical relationships described by Eqs. (11–13). In addition, Fig. 12 shows that the slope also decreases as observational noise increases.

Considering these factors, the relatively large slopes observed in the Antarctic–Southern Ocean region (90°S–60°S) may reflect either the lower observational noise associated with the 16 km observation window mode or microphysical conditions characterized by weaker aggregation or stronger vapor deposition. For aggregation, a possible explanation is a reduced efficiency of interlocking mechanisms due to differences in ice particle habits (e.g., Phillips et al., 2015). For vapor deposition, differences in water vapor availability may modify particle habits and increase the deposition coefficient (Nelson and Baker, 1996)."

Technical corrections:

Line 35: change "has" to "have"

Thank you. We have fixed it.

Line 195: change "smooth outs" to "smooths out"

Thank you. We have fixed it.