

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

The study applies an ice-particle growth identification algorithm that the authors developed in a previous 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 Puigdomènech 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 Puigdomènech Treserras, the study is not suitable for publication.

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.

2. Several aspects of the Ze-log₁₀(vd) method are not clear to me and need revision/clarification.
 - (a) why do you use log₁₀(v_d) 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?
 - (b) looking at your distributions, the correlation between Ze and log₁₀(v_d) 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 $\log_{10}(v_d)$. Can you comment on why that is the case and why you still chose the linear fit?

- (c) I do not understand the step from equation 1 to 2. What is the change in Z_e ? Do you mean temporal change in Z_e at the same height? Or change of Z_e with height? This is an important aspect. Also, what is e in equation 2? Where is it coming from? Same for equation 3.
 - (d) how do you do the fitting of the line to your Z_e - v_d data? Is this a least-squares method? In the description of your method you are saying that the slope of $\Delta Z_e / \Delta(\log_{10}(v_d))$ depends on $\Delta M / \Delta D$. Why do you know fit a line into the Z_e - $\log_{10}(v_d)$ space? Also, you are then talking about variation in slope, which variation do you mean? Between different heights? This is not clear to me.
 - (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?
 - (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 $(60-10b)/\beta$ considered to be fixed? And $60/\beta$ is not?
 - (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.
3. There are no error bars or other statistical analysis regarding the goodness of your fit to Z_e - $\log_{10}(v_d)$. 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.
 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 Z_e values are strongly biased and your slope is not correct.
 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?
 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 v_d accordingly? This is not described in a well enough manner.
 7. Figure 1 and all similar figures: the readability of the Z_e - $\log_{10}(v_d)$ relation is really bad. Also, what are the colored lines?
 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 Z_e and v_d time-height plot of a representative case study to give the reader some context.
 9. Why did you choose the threshold in equations 8 to 10?
 10. When discussing the integration length, you are stating that the correlation coefficient slightly decreases by temporal smoothing. However, your correlation coefficient was already really small to begin with, this is not a significant reduction.
 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 an argument for 10% to be acceptable.

12. With your method you want to distinguish 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.
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.
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 address this strong positive bias in their analysis? Please explain this Figure and its meaning in more detail
15. As I find the comment by Bernat Puigdomènech 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.

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?
- 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 v_d and therefore, considering v_d is useful to find regions of enhanced riming. Also, the early stages of aggregation and depositional growth influence the Doppler velocity.
- Line 46: is the ATBD available somewhere? Please add a citation here!
- Line 87: where do you get the information that quasi-stationary downdraft exist associated with snowfall drag? Please cite the relevant literature!
- Line 95: The beginning of the sentence could be improved by removing "in" (Prior to the analysis...)
- 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.
- Line 108: where do you get the temperature information from?
- Line 111: what do you mean by "this indicates the transition of microphysical sensitivity from cloud top toward cloud base"?
- Line 131: you state that in the upper troposphere, ice nucleation, aggregation, deposition, sublimation and sedimentation dominate. Please cite sources for this statement!
- Line 138: which year are the January to March data taken from?
- Line 226: please cite your sources for the random error statement!

- 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?
- Line 249: Where do I see the greater spread toward the left of the diagram? I did not find that in your Figure 6.
- Figure 4, 6 and 11: why do you choose these x-axis limits? Your data is clearly going to smaller and larger vd
- Line 410: I dont understand this. So what criterion do you know apply to your data? Please state that explicitly here.
- Line 441: What is this NCEP-FNL product? Please provide the full name and a citation here.
- 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.
- 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.

Technical corrections:

Line 35: change "has" to "have"

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

References

- Heike Kalesse and Pavlos Kollias. Climatology of high cloud dynamics using profiling arm doppler radar observations. *Journal of Climate*, 26(17):6340–6359, 2013.
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- Sergey Y Matrosov. Frozen hydrometeor terminal fall velocity dependence on particle habit and riming as observed by vertically pointing radars. *Journal of Applied Meteorology and Climatology*, 62(8):1023–1038, 2023.
- Heike Kalesse, Pavlos Kollias, and Wanda Szyrmer. On using the relationship between doppler velocity and radar reflectivity to identify microphysical processes in midlatitudinal ice clouds. *Journal of Geophysical Research: Atmospheres*, 118(21):12–168, 2013.
- Leonie Von Terzi, José Dias Neto, Davide Ori, Alexander Myagkov, and Stefan Kneifel. Ice microphysical processes in the dendritic growth layer: a statistical analysis combining multi-frequency and polarimetric doppler cloud radar observations. *Atmospheric Chemistry and Physics*, 22(17):11795–11821, 2022.