The authors continue exploring their idea of identifying the ice growth areas in the clouds using Doppler spectrum width σv which was first described in their 2023 paper in Advances in Atmospheric Sciences. In the submitted manuscript, they added analysis of additional cases and investigated the relationship between σv and differential reflectivity ZDR and cross-correlation coefficient ρhv.

Doppler spectrum width was somewhat abandoned in weather radar studies of clouds and precipitation during the last two decades compared to emerging polarimetric variables demonstrating superb power for hydrometeor classification and quantification. Nevertheless, the spectrum width may provide some additional information about the microphysics of ice beyond its traditional use for quantifying turbulent eddy dissipation rate (EDR). It is quite possible that enhanced σv may signify the areas of supercooled water inside ice clouds because micro-updrafts associated with strong turbulence may produce sufficient super-saturation with respect to water. This is likely often the case in the dendritic growth zones (DGZ) where local buoyancy can be generated due to the strong release of latent heat caused by the rapid depositional growth of very anisotropic ice particles with large capacitance. The σv enhancement in DGZ is clearly visible in Fig. 5. However, the DGZ is much better recognized by the increase of ZDR and KDP as shown in the study by Griffin et al. (JAMC, 2018, p.31) which is not cited in the manuscript. According to Griffin et al., the value of ZDR in the DGZ is mainly determined by the height of the storm above DGZ.

In the same Fig. 5, it is obvious that ZDR is maximal at the times of the lowest storm heights. Although the enhanced turbulence causes increase of σv and decrease of ZDR due to more random orientation of particles, this aerodynamic effect is not the primary one determining the magnitude of ZDR. The value of ZDR in the DGZ is primarily dependent on the relative contributions of the quasi-spherical ice generated above the DGZ and very anisotropic ice such as dendrites or hexagonal plates locally grown in the DGZ as explained in Griffin et al. (2018).

ANSWER: First of all, the authors would like to express our sincere gratitude to the referee who took the time to review the manuscript in details.

The present study analyzed σ_v and verified their microphysical characteristics using dual-pol weather radar variables. It has already been presented in Figures 10 and 11, and in addition, the results are consistent with those of Griffin et al. (2018) mentioned by the referee.

Additionally, the Z_{DR} maximum, also confirmed in Figure 5 is a phenomenon observed for around 30 minutes before the precipitation system completely passed through the field area. In other words, this phenomenon does not affect the analysis results of this study. Furthermore, to take these characteristics into account, the relationships between the dual-pol radar variables were analyzed in Figures 10 and 11.

Lastly, GZDA presents the representative boundaries of GZs for each case as a bulk scheme. It implied that zones that are not actually GZ may be included.

In addition to the regions of intense small-scale turbulence, σv can be high in areas of strong wind shear and the latter is the most common factor affecting σv in the cold-season stratiform clouds. There is no discussion on the connection of the vertical wind shear and σv in the manuscript.

ANSWER: First of all, the authors apologize for causing confusion to the referee by not using clear expressions. There is a method to extract vertical wind shear (VWS) components in polar coordinates based on σ_v (i.e., Chapter 5.3 Doppler Spectra of Weather Signals of the book Doppler Radar and Weather Observation written by Doviak and Zrnic).

However, the main point presented in the present study is that SW depends on the type of solid hydrometeors, and the radar-based wind shear component has limitations in that it depends on σ_v. In other words, it will be reasonable to calculate VWS from wind components directly measured from a radiosonde. VWS components are obtained from the Heuksando Island radiosonde observation site officially operated by KMA, located on the Yellow Sea, and are shown in the figure below.

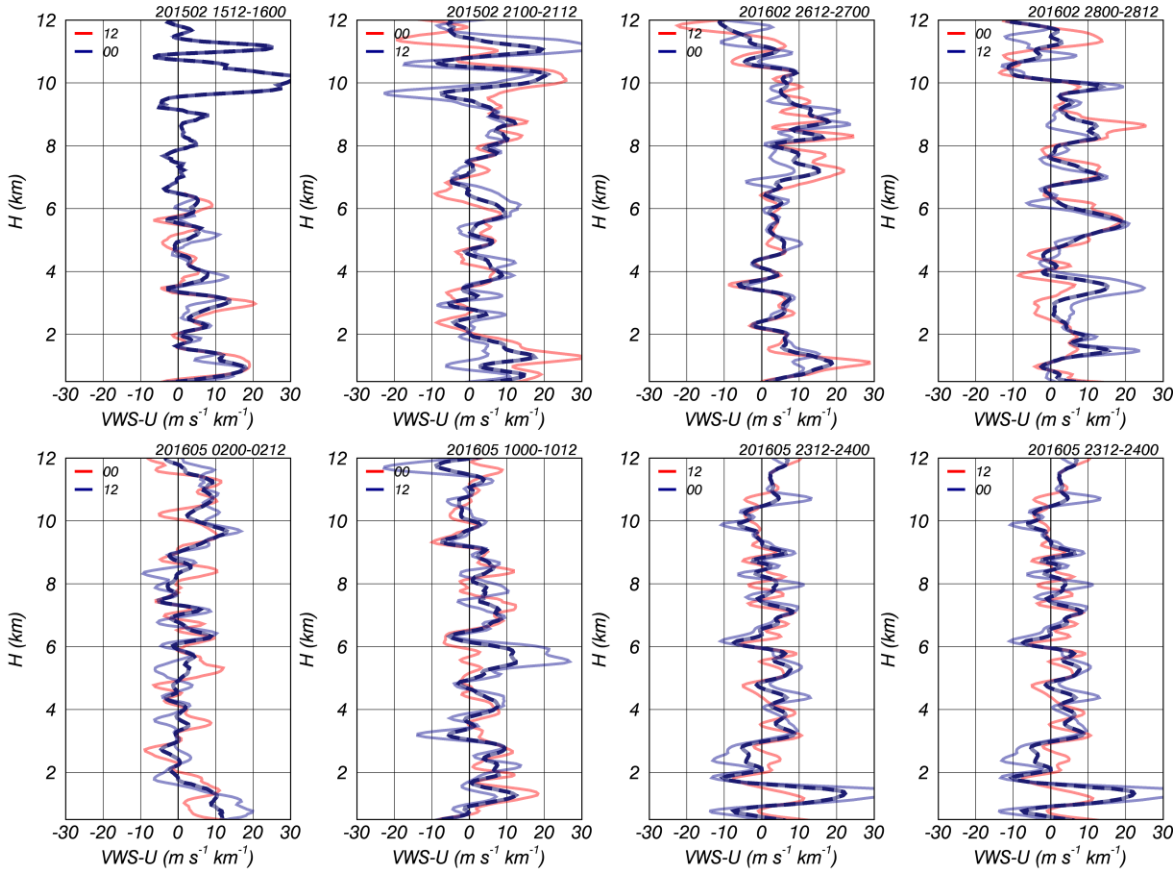

Figure A-1. Vertical Wind Shear (VWS) for zonal wind components for 8 analysis cases observed from the Heuksando radiosonde operated by the Korea Meteorological Administration (KMA)

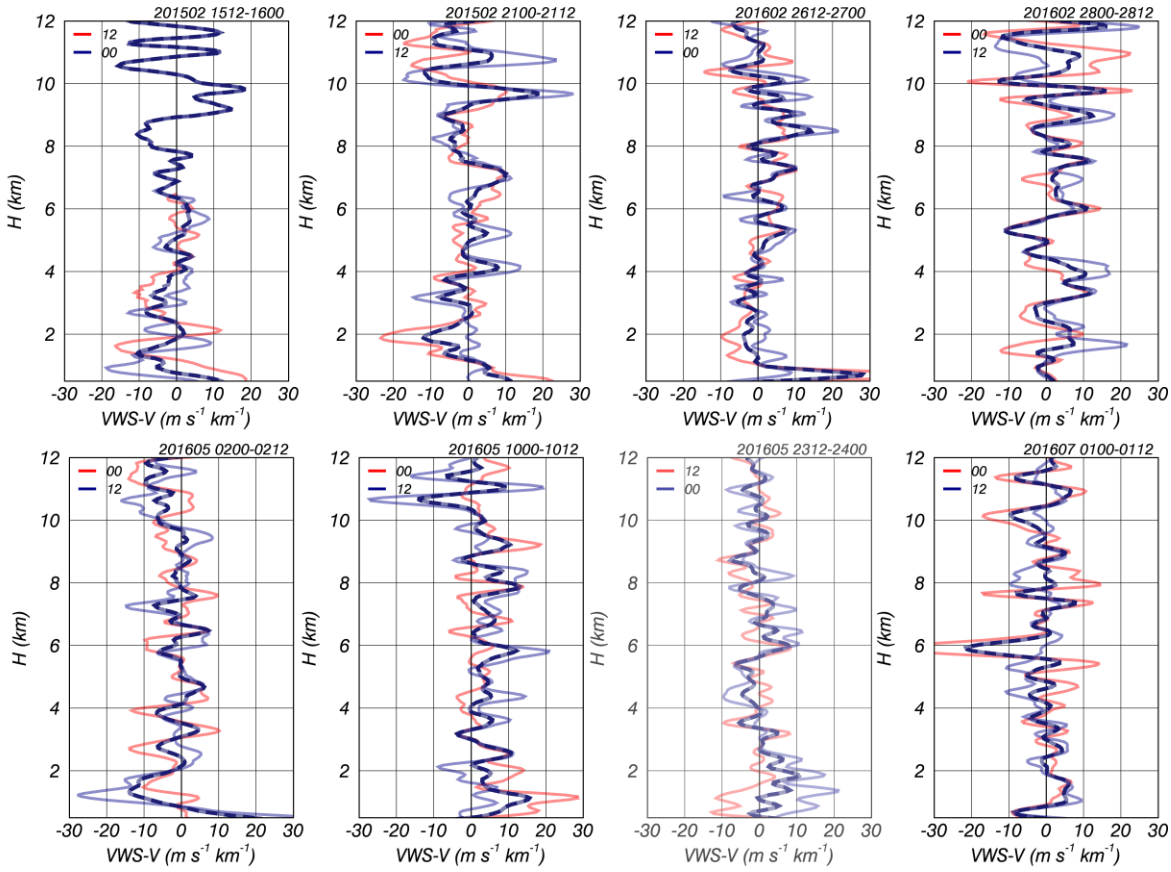

Figure A-2. Vertical Wind Shear (VWS) for meridional wind components for 8 analysis cases observed from the Heuksando radiosonde operated by the Korea Meteorological Administration (KMA)

From these results, we can see that the correlation between σ_v and VWS is not confirmed. In this study, the background conditions for the analysis cases were not sufficiently explained in the draft what the referee's concerns, so the following sentence has been added.

Line 117:

Lastly, it was confirmed for the selected cases that there was no correlation between σ_v obtained from YIT and the vertical wind shear obtained from the Heuksando radiosonde site (125.4508°E, 34.6872°N) officially operated by KMA.

I have too many concerns about this manuscript, some of which follow:

The authors apparently experience big problems with English usage to the extent that it is very difficult to understand the meaning of many sentences due to awkward formulations.

ANSWER: The manuscript was written with a review and proofreading process by native experts, and the other referee informed us that the draft was well written. Nevertheless, out of respect for the referee's opinion, the English expressions have been reviewed in their entirety. We hope that the referee will be satisfied through this review process.

As an example, in lines 238 – 239 it is said that "The potential range of the GZs were defined by the average range of the GZs identified in this study". What does it mean?

ANSWER: We apologize for the confusion. Since GZ is not determined in the convective cell, this means that the average vertical thickness of the DGZ determined as GZDA for the case of stratiform precipitation was set as the potential range for the DGZ in the convective cell. This sentence has been revised as follows.

Line 238-239:

The potential vertical depth of the GZs for the convective cases was defined by the average range of the GZs identified in stratiform cases,

One of the major conclusions of the article formulated in "Summary and Conclusions" is "…the variation range of ZDR in the DGZ was narrower than that of σv". ZDR is measured in dB and σv – in m/s. This is a classical comparison of apples and oranges.

ANSWER: The authors agree with the referee's opinion. The original sentence was written to express a relative comparison of the two GZs, and has been revised as follows.

Line 373:

Firstly, the range of σ_v in the DGZ was wider than that in the NGZ.

The authors make a strong statement that high σv is an indication of growth zones (GZ) of ice. Has this been confirmed by in situ observations? What is meant by GZ? In line 312, they use the term “maturity of GZs”. How is this quantified?

ANSWER: Although GZ was not determined using in situ measurement (e.g., aircraft, video sonde) other than weather radar, the altitude of DGZ confirmed by GZDA was confirmed to be centered around -15~-10°C and -5~0°C (Figure 13), which can be sufficiently explained as DGZ and NGZ, respectively.

The meaning of GZ is already defined in the manuscript (growth zones (GZs), line 54), and the maturity of GZs is an expression used to qualitatively describe the degree of growth or distribution of σ_vs in each GZ (DGZ, NGZ) based on the intensity of σ_v.

“Freezing level” means a particular altitude but the authors use this term for a deep layer with negative temperature.

ANSWER: We apologize for the confusion. Problems with two sentences in Line 208 and Line 220 were confirmed, and these were replaced with sub-zero level.

There is a completely weird statement: “Water vapor pressure depends on atmospheric pressure, meaning that the development of GZs can be determined by altitude”.

ANSWER: We apologize for the confusion caused by a mistake in words. What the author wanted to talk about was water vapor contents, that is, the mixing ratio (g kg^{-1}). This is a function of atmospheric pressure ($r_s = e_s\varepsilon/Pe_s$, ε=0.622) and is related to supersaturation (g m^{-3}), which is one of the main factors determining hydrometeor growth (Nakaya and Terada, 1935; Barbir, 2012).

The saturated mixing ratio is inversely proportional to atmospheric pressure for the same temperature. In other words, the lower the atmospheric pressure (as the altitude increases), the higher the saturated mixing ratio. Additionally, the amount of water vapor in the stable

condition is inversely proportional to altitude (e.g., Fan et al., 2010). It can be explained that as the altitude of DGZ increases, the supersaturation to grow DN sufficiently may not be satisfied.

Therefore, the original sentence has been modified as follows.

Line 332-334:

Supersaturation, which has a crucial influence on the growth of solid hydrometeors, can be determined by the amount of water vapor, in other words, a mixing ratio (i.e., Nakaya and Terada, 1935; Barbir, 2012), it is determined by the atmospheric pressure. Additionally, the amount of water vapor in the stable condition is inversely proportional to altitude (e.g., Fan et al., 2010). In short, the growth rate of the representative solid hydrometeor in each GZ can be affected by its altitude.

It is written in a discussion session as it is the author's inference. If you have any further discussions about this, please let us know any time.

References:

Barbir, F. (2012). PEM fuel cells: theory and practice. Chpt. 4 Reactant Humidity, Academic press.

Nakaya, U., & Terada Jr, T. (1935). Simultaneous observations of the mass, falling velocity and form of individual snow crystals. 北海道帝國大學理學部紀要, 1(7), 191-200.

Fan, J., Comstock, J. M., & Ovchinnikov, M. (2010). The cloud condensation nuclei and ice nuclei effects on tropical anvil characteristics and water vapor of the tropical tropopause layer. Environmental Research Letters, 5(4), 044005.

Lines 340 -341. Another awkward and strange statement: “it suggests that radar-based sub-zero T estimation from GZDA might be possible”. What is this? The QVP methodology provides clear detection and delineation of the melting layer and GZDA has nothing to do with this.

ANSWER: The authors think the referee misunderstood the sentence that you pointed. It does refer to "sub-zero level" not "the melting layer", and the sentence explains that the

temperature altitude close to 0°C < T < -5°C (NGZ), -5°C < T < -10°C (transition, secondary growth zone), -10°C < T < -15°C (DGZ) on GZ might be roughly determined by GZDA. This is well explained in the previous (Line 338-340) and the next sentence (Line 341-343) of the sentence pointed out by the referee. Furthermore, Figure 13 was also suggested to prove this sentence.

The authors believed that the referee misunderstood simply because of a problem of expression, so we specified the sentence as follows.

Line 346-347:

it suggests that radar-based T estimation above the freezing level (-5 < T (°C) < -15) from GZDA might be possible.

Temperature contours in Figs. 4 and 5 should be labeled.

ANSWER: A temperature scale will be added. Thank you.

What is the reason for negative Zdr in Fig. 10? Calibration errors of Zdr?

ANSWER: Firstly, GZDA is a bulk scheme that analyzes all radar measurements included within representative boundaries identified from QVP data over 3 hours. In other words, not all data classified as DGZ means DN.

Additionally, Z_{DR} calibration applied in this study cannot convert positive value to negative value (Fig. 12 in Ryzhkov et al., 2005).

References:

Ryzhkov, A. V., Giangrande, S. E., Melnikov, V. M., & Schuur, T. J. (2005). Calibration issues of dual-polarization radar measurements. Journal of Atmospheric and Oceanic Technology, 22(8), 1138-1155.

In Figs. 6 and 7, it would be better to use temperture instead of altitude at Y axis. Why not to label each panel in these plots as SW1, SW2, etc.?

ANSWER: Appreciate the comments and the authors agree with that. However, the MSM reanalysis data used in the present study support the weather information with altitude-dependence spatial resolution. Therefore, it is practically hard to change the Y-axis to for the temperature from for the altitude. Furthermore, the temperature information for each GZ is can be found in Fig. 13, so we believed that there will be no problems in interpreting the results in Fig. 6 and 7.

In addition, the abbreviations of cases have been added in Figure 6 and 7.

The manuscript is extremely confusing and controversial, and I strongly oppose its publishing in its current form.

ANSWER: The authors tried to sincerely resolve any confusion the referee may have in this revision stage. The authors are confident that all of the referee's questions can be resolved through the author's response, and we sincerely appreciate the referee's valuable comments, which could improve the value of the manuscript.

Citation: https://doi.org/10.5194/egusphere-2023-947-RC1