

The manuscript entitled "Measurement Report: Aircraft-Ground Observation Study of a Spring Snowstorm Event in the North China Plain: Cloud Microphysical Characteristics and Precipitation Vertical Structure" by Song et al. details the microphysical and remote sensing measurements of a springtime convective system with occasionally intense snowfall and rainfall at the surface. The authors provide a thorough presentation of the results from the available airborne and ground-based instrumentation and attempt to relate the observations to microphysics processes occurring in the mixed-phase clouds. However, work still needs to be done to more clearly contextualize the findings to the figures and to past literature before it meets the standards for publication in the journal Atmospheric Chemistry and Physics. As such, I recommend Major Revision to the manuscript.

Major Comments

1. The habit analysis (e.g., Fig. 3 and related text) is incomplete as it does not give enough context to the distribution of particle sizes for each habit. For example, to hexagonal plates dominate for the smaller CIP size bins? As shown, holistic fractional habit contributions for the 3 representative periods aren't very revealing. Further, the authors need to discuss the limitations of the CIP as it relates to (1) the probe resolution (could small particles be misclassified as hexagonal?) and (2) maximum detectable size (if aggregates aren't identified in the Holroyd habit scheme until $D_{\max} > 1.6$ mm, how many aggregates in the cloud are actually undetected in this analysis?). Alternatively, the authors may consider analyzing each habit's fractional contribution to the bulk mass (assuming a mass-Dimension relationship) or reflectivity (simply summing the $D^{(2+bm)} \cdot n(D) \cdot dD$ bin contributions for each habit).

Response: Thank you very much for your detailed suggestions. In the revised manuscript, we have added a discussion of the shape and size distribution (Fig. 4). In addition, we have supplemented the analysis of the contribution rates of particles of each shape to number concentration and mass (Fig. 5).

Furthermore, we have also added a discussion of the limitations of CIP instrument observations. The CIP has a detection resolution of 25 μm . Cober et al. (2001b) pointed out that the potential errors in shape determination are great for particles smaller than 10 pixels, and that 5 – 40% of particles in ice clouds are misidentified as spherical particles. Korolev et al. (2000), through an analysis of spherical particle images from the OAP-2D-C, concluded that a spherical particle can be accurately identified only when it has at least 23 – 25 pixels in total. Particles smaller than 25 pixels were classified as tiny particles. As can be seen from the CIP shape – size distribution (Fig. 4), the particles contributing most to the hexagonal are those of 300 – 500 μm , and there are no hexagonal particles smaller than 150 μm . Therefore, we consider the impact of small particles being identified as hexagonal to be minor. In addition, since the maximum size detectable by the CIP is 1550 μm , hydrometeors exceeding this size range cannot be detected. However, it is difficult for us to quantify exactly how many such particles there are. The supplementary discussion of the CIP limitations is provided in Chapter of methods and data.

2. More discussion as it relates to past studies is needed. This includes but is not limited to: comparing the bimodal ice PSDs for stages 1–3 to other convective snowstorms in the literature (~L245), relating vertical motions and collision/coalescence to lightning activity (L347), and contextualizing the Z, V, W CFADs (L388–415) as has been done in other winter storm studies (e.g., Rosenow et al. 2014 doi:10.1175/JAS-D-13-0249.1).

Response: We sincerely thank you for the valuable comment regarding the need to strengthen the discussion by relating our findings to past studies. Following your suggestions, we have added the following discussions at the corresponding locations.

We have added a comparison of the bimodal ice PSDs for stages 1–3 with previous studies. Bimodal ice PSD features have been documented in prior studies (Long et al., 2025; Hu et al., 2022; Wang et al., 2023b; Feng et al., 2023). The bimodal characteristics observed in stages 1–3 of this study are morphologically similar to the Bimodal1-type PSDs reported in the Hu et al. (2022), with the breakpoint location (~100–200 μm). Within the temperature range of -15 to -5 $^{\circ}\text{C}$, the occurrence probability of bimodal spectra exceeded 50% (Hu et al., 2022). However, a unimodal distribution is observed at all altitudes of a stratus cloud with snowfall over Hebei (Li et al., 2027). Compared with the snowfall event over Hebei, the number concentrations of ice in this study are obviously higher. Recent IMPACTS field observations indicate that the occurrence frequency of the multimodal mode increases with temperature and ice water content (Hu et al., 2022). The peak positions of the spectral distributions were basically the same as those in the Qilian Mountains region (Long et al., 2025; Brechner et al., 2023). Under high ice crystal concentrations, secondary ice production is a possible cause of the bimodal distribution (Hallett and Mossop, 1974; Mason, 1998). Peak positions vary depending on the types of ice crystals involved in the collisions (Grzegoreczyk et al., 2023).

We have added a discussion linking vertical motions and collision/coalescence processes to lightning activity. The core framework of non-inductive charging is that, in the presence of supercooled liquid water, charge transfer is achieved through rebounding collisions between graupel and ice crystals; subsequently, gravitational sedimentation and updrafts act in concert to separate positive and negative charges, ultimately establishing an electric field sufficient to initiate lightning. The observed vertical hydrometeor distributions in this study—aggregates and vertically oriented ice crystals above, supercooled water in the middle, and graupel below are closely related to lightning enhancement. We have connected this mechanistic discussion with previous observational and modeling work on winter thunderstorm electrification to strengthen the physical interpretation of our results.

Rosenow et al. (2014) used the Wyoming W-band cloud radar to systematically investigate embedded convection and generating cells in the comma-head region of continental winter cyclones. The W CFADs across different regions of the cyclone comma heads were compared and analyzed. They interpret the radar statistics in the context of synoptic background, and cloud system type. In the revised manuscript, we have expanded the discussion accordingly. Specifically, we have made the following changes. We have supplemented the synoptic background of the observations. A

discussion of the physical processes and thermodynamic background underlying the CFADs of Z, V, and W. Finally, we added comparisons with previous studies that analyzed CFADs of Z,V,W in snowfall and mixed-phase clouds (Jeoung et al., 2020; Ramelli et al., 2021). In particular, our case is characterized by a pronounced warm inversion layer and repeated precipitation-phase transitions, such that the observed CFAD evolution reflects the combined effects of particle growth, vertical air motion, and melting.

3. Discussion of the results are frequently difficult to follow without explicit context from the figures. The authors need to add parenthetical figure references throughout the results section next to where claims are made so that their arguments are clear and with scientific backing. A short list of effected lines follow, but there are many more areas that can use figure citations: L306, L340, L395, L424, ...

Response: Thanks for this helpful comment. We have added parenthetical figure citations throughout the Results section, including at the specific locations noted (L306, L340, L395, L424), as well as at many other places where claims were made without explicit figure support. We believe these additions make the arguments clearer and better supported by the figures.

Minor Comments

1. Several spots throughout the paper do not have a space immediately before parenthetical citations. Please correct this.

Response: Thank you for carefully checking the formatting details. We have carefully gone through the entire manuscript and added a space before every parenthetical citation where it was missing. These corrections have been made throughout the text.

2. Discussion of results typically alternated between past and present tense within the same paragraph. The authors should choose one to remain consistent throughout.

Response: Thanks for pointing this out. We have therefore carefully revised the discussion of results to ensure that tense usage is consistent within the same paragraph. All changes have been made with Track Changes mode in the revised manuscript.

3. Many parts of the paper mention “snowstorms with thunderstorm” (e.g., L104) or similar phrasing. This should be rephrased to say “convective snowstorms” as it better describes the dynamical environment being framed for this study and is less redundant of the use of “storm”.

Response: Thank you for your suggestion. Accordingly, we have revised all instances of "snowstorms with thunderstorm" (including L104) and similar phrasing to "convective snowstorms" or "snowstorms with convection" throughout the manuscript.

4. Specify time zone or UTC for all times listed throughout the paper.

Response: Thanks for pointing this out. We have now specified the time zone for all times listed throughout the manuscript. All times in this study are reported in Beijing Time (BJT, UTC+8) unless otherwise noted. We have added the corresponding description in the section of Methods and data to address this issue.

5. L143: Clarify what you mean by “raw data files with spectral Doppler density data”. Is this raw Doppler spectra or derived moments (skewness, kurtosis, etc.) from the Doppler spectra?

Response: Thank you for this clarifying question. By "raw data files with spectral Doppler density data", we mean the original Doppler spectra measured directly by the radar. The derived moments (such as skewness, kurtosis, etc.) were subsequently calculated from these raw spectra after data processing. We have revised "raw data files with spectral Doppler density data" to "original Doppler spectra data" to make this distinction clearer.

6. L153: Expand on “the dimensions of non-complete particles were corrected”. Does this mean a maximum dimension definition where the diameter of a circumscribing circle was used? If not, how were the particles corrected?

Response: Thanks for your questions. Non-complete particles were corrected using a method similar to the equivalent-area circle diameter. The specific method is described in detail in Heymsfield and Parrish (1978), which we have now cited in the revised manuscript.

7. L162–165: I think a sentence or two is needed for the MRR QC process and fuzzy logic algorithm as these are not straightforward and should be succinctly summarized here.

Response: Thank you for this suggestion. We have added a concise summary of the MRR QC process and fuzzy logic algorithm in the revised manuscript. We believe this addition improves the clarity and reproducibility of our methodology.

The MRR quality control process and the phase retrieval algorithm are briefly introduced as follows: First, noise removal is applied to the Doppler spectra, followed by velocity dealiasing to obtain the quality-controlled spectra. Based on the quality-controlled spectra, physical quantities including particle fall velocity V , Doppler spectral width σ , and skewness (S_k) are calculated. Then, using the empirical relationship between particle fall velocity and equivalent radar reflectivity factor (Atlas et al., 1973), combined with the characteristics of the melting-layer bright band, the precipitation phase is determined.

The dual-polarization radar fuzzy logic hydrometeor classification algorithm is described as follows: Membership beta functions are defined for each hydrometeor type and each variable: Z_h , Z_{dr} , K_{dp} , and ρ_{hv} . The hydrometeor with the highest fuzzy logic score was identified as the most probable type at each grid point. The overall score μ for a given hydrometeor i is calculated with

$$\mu_i = \left[\frac{W_{Z_{dr}} \beta_{Z_{dr},i} + W_{K_{dp}} \beta_{K_{dp},i} + W_{\rho_{hv}} \beta_{\rho_{hv},i}}{W_{Z_{dr}} + W_{K_{dp}} + W_{\rho_{hv}}} \right] \beta_{T,i} \beta_{Z_h,i}$$

where i indicates the hydrometeor category, W is the weight for each variable, and β is the score from DR09's Eq. (14) for a given hydrometeor and variable set.

Corresponding descriptions have been added in the revised manuscript.

8. L169: Quantify (degrees C) the temperature decrease over this time period.

Response: Thank you for this suggestion. The temperature decrease over this time period has been quantified as suggested.

9. L173: I don't see this echo base height in the top panel. It looks like precipitation (and echo) reaches the surface. And by “horizontal flight” do you mean “constant altitude”?

Response: Thank you for pointing this out. We agree that precipitation reaches the surface after 11:24. The echo base height we previously described refers to the period before 11:24. We have supplemented the description with a time restriction and analyzed the echo base height in segments in the revised manuscript.

Yes, "horizontal flight" mentioned in the text refers to "constant altitude". To make the expression clearer, we have modified the language of the original text.

10. Fig. 2b: Should the units for the N_CIP y-axis label be L^{-1} ? A number concentration of 10/cc is extremely high for ice. Also, it should be explained upfront why points C and F aren't mentioned in the analysis of this figure. It's hard to follow the 4 different stages when some of them skip a letter.

Response: Thank you for your careful review and thoughtful comments. We have rechecked the unit of CIP in the figure, and it is indeed /cc. We agree that such a high concentration up to 10/cc is unusually high for ice particles. We carefully examined the particle images during this period when the CIP number concentration exceeding 10/cc (after 13:36) and consider that the CIP measurements were affected by supercooled water freezing, rendering the results less reliable.

Thank you for your reminder. Points C and F in the figure were actually labeled to correspond to the positions in the radar cross-section (Figure 6 in original manuscript). We also realized that this might make it difficult for readers to follow, so we have supplemented the analysis with an explanation of the basis for the segmentation and the purpose of labeling C and F before analysis.

11. L190: Quantify how heavy snowfall was (e.g., snowfall rate).

Response: Thank you for your suggestion. In the revised manuscript, we have now added quantitative snowfall rate data to better characterize the intensity of the snowfall event.

12. L193: Quantify the number concentration and size range you're referring to for the large particles.

Response: Thank you for your suggestion. Large particles actually refer to the particles observed by CIP ($D > 50 \mu\text{m}$), while small-sized particles refer to the particles observed by CDP. In the revised manuscript, to make the expression clearer and more explicit, we have now revised the language in our analysis.

13. L194: "higher content" is a far stretch for an LWC of $< 0.01 \text{ g/m}^3$. In fact, I'd hedge to say it's below the measurement sensitivity of the CDP.

Response: Thank you for this critical and valuable comment. We acknowledge that a value of $< 0.01 \text{ g/m}^3$ is indeed not indicative of "higher content" — in fact, it is below the typical measurement sensitivity of the CDP instrument. We have therefore revised the wording throughout the manuscript to avoid any misleading characterization. Specifically, we have replaced "higher content" with "low supercooled water content".

14. L196: State the instrument(s) that observed mixed precipitation and, if applicable, what in the data indicated this.

Response: Thank you for your comment. We would like to clarify that the mixed precipitation was observed by ground-based automatic weather stations (AWS).

15. L200: Quantify the echo top height reduction

Response: Thank you for your suggestion. The reduction in echo top height has been quantitatively assessed, with the results presented in the main text.

16. L202: Rather than simply state the decrease, quantify it and provide physical context such as why it's lower in this part of the storm vs stage 2.

Response: Thank you for your suggestion. We have quantified the magnitude of the decrease in CDP number concentration and supplemented the reasons for the decrease in concentration.

16. L203: It should be stated the aircraft descended prior to this stage

Response: Thank you for your suggestion. We have now explicitly stated that the aircraft descended prior to this stage in the revised manuscript.

18. Table 2 should list mean $\pm$ standard deviation in the microphysical properties

Response: Thanks a lot for this constructive suggestion. We have revised Table 2 to now include the mean $\pm$ standard deviation for all microphysical properties listed.

19. L209: How can this be? The CIP PSDs for sub-200 micron particles are very similar across the 4 regions and since there are fewer large particles in Stage 4 I'd think the CIP number concentrations ($D > 50 \mu\text{m}$) should be lower, not higher.

Response: Thank you for your critical question. We have carefully re-examined our data processing and plotting procedures and regret to find an error in the calculation of the average PSDs for Stage 4. This has been corrected in the revised manuscript, with the PSD plots and corresponding analysis updated accordingly. With this correction, the sub-200 μm particle concentrations in Stage 4 are now clearly higher than those in Stages 1 and 2, explaining the higher total number concentration observed in this stage.

20. L219: These are relative/general terms that need to be quantified. Riming is best defined in particle imagery, so I'd think the visual CIP images would strengthen the argument here.

Response: Thank you for your suggestion. We have quantified the supercooled liquid water content. I agree with the point that CIP particle images can help confirm the existence of the riming process. Unfortunately, the CIP particle image data has been anomalous during this period as stated in the response to comment 26, so we cannot provide usable high-quality particle images to support this part of the argument. As a result, we currently have to rely on LWC to infer the riming process. We have removed the analysis in the original text that lacked experimental data support.

21. L222: Tiny particles, as defined by the habit algorithm or tiny as in some undefined size threshold?

Response: Thank you for your suggestion. Tiny particles are those with fewer than 25 shadowed pixels, as defined by the habit algorithm (Holroyd, 1987). We have added a clear definition related to an area threshold based on the probe resolution as suggested (see response to comment 25).

22. Fig. 3a–c: The y-axis should be relabeled as it's fractional contributions that are plotted rather than percentages. The figure caption should indicate the height of the image strips (units mm).

Response: Thank you for pointing this out. The y-axis label has been changed to "Fractional contribution" instead of "Percentage". The figure caption has also been

updated to include the height of the image strips in mm.

23. L227: Chain aggregates are commonly comprised of planar crystals, so it's a stretch to say that plates don't contribute to aggregation.

Response: Thanks for your critical comments. After careful consideration, we agree that our previous description was indeed a stretch. Plate crystals have relatively large, flat basal faces, which make them prone to contact with other crystals during fall, and thus readily aggregate to form aggregates. We have deleted the sentence in the original text.

24. L231: Be careful when conflating supercooled liquid droplets and the spherical habit category. One can assume liquid if the CIP shows circular particles with a clear diffraction signature, but it's better to pair this discussion with CDP concentrations or LWC as the CDP is better at detecting/differentiating liquid.

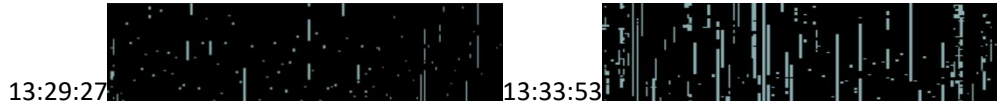
Response: Thank you very much for your comment. We agree that discussing the CIP and CDP results together is more reliable. Liquid cloud particles are mostly spherical when not affected by aerodynamic drag, but not all spherical particles are liquid cloud droplets. Therefore, using spherical morphology to identify supercooled droplets carries a certain risk of misjudgment. The CDP observes small cloud particles in the size range of 2 – 50 μm , but in terms of its measurement principle, it also cannot effectively distinguish the phase of particles. Although some studies have shown that LWC from CDP correlates well with hot-wire LWC measurements, using CDP observations as evidence for the presence of liquid water or supercooled liquid water is often questioned. Following your suggestion, we have removed the statements in the original text that directly associated supercooled water droplets with the spherical shape category and added a discussion incorporating the CDP results.

25. L234 (or earlier in paper): Discussion of the “tiny” category should be better defined. Holroyd defined tiny as having < 25 pixels shadowed, so this distinction can be related to an area (units mm^2) threshold based on the probe resolution.

Response: Thank you for your suggestion. As defined by Holroyd (1987), “tiny” particles are those with fewer than 25 shadowed pixels. Given that the CIP used in this study has a resolution of 25 μm , this corresponds to particles with an area smaller than 0.0156 mm^2 . We have added a clear definition of this criterion in the revised manuscript.

26. L233–237: I would tend to believe the high habit fraction of tiny particles observed by the CIP in stage 4, actually. Since we know from the CDP that supercooled droplets are abundant in this period and their concentrations are orders of magnitude greater than ice particles, the CIP is likely being inundated (by number) with these droplets.

Response: Thank you for your comment. The CDP did observe high particle number concentrations during this stage, and we fully agree that supercooled droplets were abundant in stage 4. However, the CIP images from this stage (see the supplementary figure) do not appear to be normal particle images; instead, they look more like artifacts caused by a stuck bit. Therefore, we are more inclined to believe that the CIP probe was affected by the freezing of supercooled droplets during this stage, which reduces the credibility of the detection results.



27. Figure 5 caption and L261 should indicate these are mean vertical profiles. To this end, were the reflectivity profiles (Fig. 5a) averaged using the reflectivity factor in linear units ($\text{mm}^6 \text{m}^{-3}$) for each altitude level? If not, this should be recalculated to ensure the discussion/conclusions don't change.

Response: Thank you very much for your comments. The previous reflectivity profiles were not averaged using the reflectivity factor in linear units ($\text{mm}^6 \text{m}^{-3}$). Following your suggestion, we have recalculated the reflectivity factor profiles. In addition, we have indicated in both the figure captions and the text that these are mean vertical profiles.

28. L274–279: I would think that the drops are getting larger via collision/coalescence and should actually contribute to a greater radar reflectivity given the D^6 dependence. Couldn't attenuation or multiple scattering at this wavelength be causing the signature you're noticing?

Response: Thank you very much for your comment. To rule out attenuation as the cause of the decrease in reflectivity with decreasing height, we extracted ground-based S-band weather radar echoes along the aircraft track (Fig. S1). The ground-based radar observations also show that radar echoes below the freezing level exhibit a similar decreasing trend with decreasing height. The ground-based radar operates at a longer wavelength and it observes from below upward. The attenuation effects in the low levels are weak. Therefore, we conclude that the decrease in reflectivity with decreasing height is not caused solely by attenuation. Multiple scattering may induce near-surface ghost echoes, generally result in enhanced reflectivity (Battaglia et al., 2011; Battaglia et al., 2016). To a large extent, multiple scattering offsets the impact of attenuation. Precipitation did reach the ground in the observation area, and the weather radar echoes also extended down to the surface. We consider that the echo is not the ghost echoes caused by multiple scattering and multiple scattering is not responsible for the observed decrease in reflectivity with decreasing height below the 0°C level.

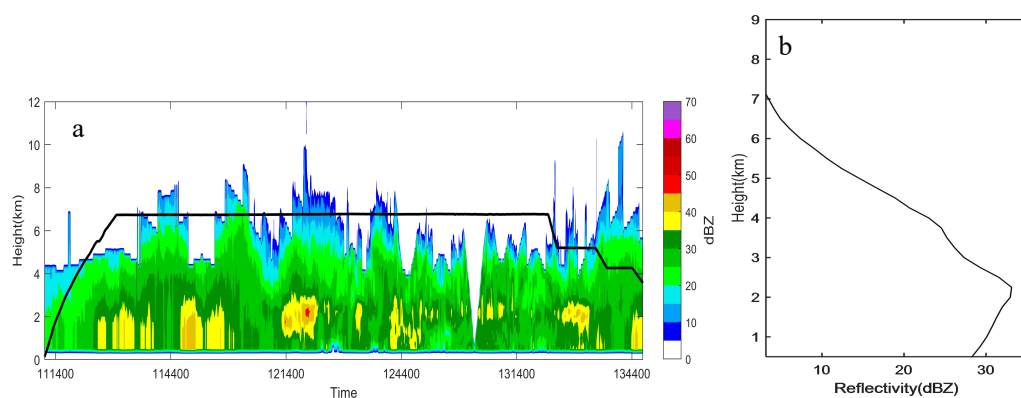


Figure S1. Ground-based radar echoes along the flight track (a) and mean vertical profile of reflectivity during Stage 3 (b).

29. L292 is difficult to follow. Do you mean that the in-situ measurements only apply to the aircraft altitude?

Response: Thank you for pointing this out. What we originally intended to convey is that the aircraft observations have certain limitations: the aircraft flies at around 6700 m for most of the detection period, and the CIP measurements only apply to the aircraft altitude. So, it's hard to know the vertical distribution of hydrometeors due to the lack of more vertical gradient measurements. We have revised the sentence to make this clearer.

30. L296: Signify these are ground-based radars and mention they are CAPPI views

Response : Thank you for your suggestion. Sorry for the incorrect figure caption earlier — the figure in the original text was actually a PPI image at 0.48° elevation. In our revised manuscript, we have replaced it with a composite reflectivity (CR) figure. Meanwhile, we have also specified that these are ground-based radars and clarified that the displayed views are CR.

31. L355: Do you mean reflectivity echoes?

Response: Yes, we mean reflectivity echoes. We have revised the wording in the manuscript.

32. Fig. 8: Similar to Comment 27, ensure the mean profiles were averaged using linear units before converting back to dBZe. Also, the solid black lines should be mentioned in the caption.

Response: Thank you for your suggestion. We have updated the reflectivity profiles by averaging the linear units ($\text{mm}^6 \text{m}^{-3}$) before converting back to dBZe. Compared with the previous version, the averaged Z_e values increased, but the overall Z_e profiles remain similar to those before. Also, the solid black lines have been mentioned in the caption.

33. L412: I suggest you put the ERA-5 vertical velocity in more relatable terms (m/s) to confirm it's in the ballpark of the MRR-estimated vertical motion.

Response: Thank you for the suggestion. We have converted the ERA-5 vertical velocity from Pa/s to m/s for a more intuitive comparison with the MRR-estimated vertical motion. The conversion was performed using the relation $w = -\omega/(\rho g)$, with air density at the relevant pressure level. At the level closer to 850 hPa, the value is about 0.07 m s^{-1} . It is indeed much smaller than the MRR-estimated motion. This discrepancy is expected because the two represent different scales. ERA5 ω is a grid-scale, time-averaged reanalysis vertical velocity, whereas MRR provides local, high-frequency estimates. ERA5 also tends to smooth and underestimate local vertical motion in convective cells. We would treat ERA5 as large-scale background ascent rather than interpreted as evidence for pronounced local vertical motion. After careful consideration, we have removed the relevant statement in the original manuscript that used ERA5 vertical velocity observations to demonstrate the presence of significant updrafts.

34. Table 3: The temperature inversion layer row contains the altitude and temperature ranges, but the temperature range isn't explicitly stated with units (I assume degC?)

Response: Thank you for the reminder. Yes, the unit is $^\circ\text{C}$. We apologize for the previous omission and have now added the unit in the revised manuscript.

35. L520: Again, an LWC of 0.003 g m^{-3} is within the detectible threshold of the CDP and may be instrument noise or drift if not calibrated correctly

Response: Thank you for this critical and valuable comment. We have replaced "higher content" with "low supercooled water content". The liquid water content in the first three stages was all relatively low and may have been affected by instrument noise, so we removed the description comparing the liquid water content of the first three stages from the conclusions.

Technical Corrections

1. L21: Do you mean oriented ice crystals?

Response: Yes, "vertical ice crystals" actually means "vertically oriented/aligned ice crystals". We have replaced the term "vertical ice crystals" with the more precise and standard phrase "vertically oriented ice crystals" in the revised manuscript.

2. L23–26: Change to past tense to match the surrounding text

Response: Thank you for pointing out issues with the tenses in the article. Following your suggestion, the tenses in L23–26 have been revised to the past tense.

3. L61: "were" -> "can be"

Response: We are grateful to the reviewer for pointing this out. We have revised the wording as suggested, changing "were" to "can be" in the revised manuscript.

4. L63: Define the MRR acronym

Response: Thank you for your kind reminder. We have added the full spelling of "MRR" (Micro Rain Radar) in the revised manuscript.

5. L72: "habits" -> "habit"

Response: Thank you for pointing this out. We have changed "habits" to "habit" in L72.

6. L76: "Snow crystal spectrum distribution and"...is there supposed to be another item listed after this phrase?

Response: Actually, there is no additional phrase following "Snow crystal spectrum distribution". We apologize for the typo in the previous version, where an extra "and" was inadvertently added. In the revised manuscript, we have removed the redundant conjunction "and" after "snow crystal spectrum distribution".

7. L85: "snowband" -> "snowbands"

Response: Thank you for pointing this out. We have changed "snowband" to "snowbands" in L85.

8. L88: "base" -> "based"

Response: Thank you for pointing this out. We have changed "base" to "based" in L88.

9. L103: Remove "in"

Response: Thank you for pointing this out. As suggested, we have removed "in" in L103.

10. L106: "thunderstorm" -> "convection"

Response: Thank you for pointing this out. We have changed "thunderstorm" to "convection" in L106.

11. L112: "observation" -> "observational"

Response: Thank you for pointing this out. We have changed "observation" to

"observational" in L112.

11. L115: Clarify what you mean by “widespread stratiform–convective mixed clouds”

Response: Thank you for your question. By "widespread stratiform – convective mixed clouds," I mean extensive stratiform cloud systems (with horizontal scales of ~1000 km) embedded with intense convective cells. We have also revised the description in the original text.

13. L118: Put a space before “(Fig. 1)”

Response: Thank you for your kind reminder. We have put a space as suggested.

14. L128: Put a space before NY

Response: Thank you for your kind reminder. We have put a space as suggested.

15. L152: McFarquhar citation should not be parenthetical

Response: Thank you for pointing out this formatting error. We have corrected this throughout the manuscript as follows: Following the methodology of Mcfarquhar et al. (2018).

16. L154: “following” -> “follow”

Response: Thank you for pointing this out. We have changed "following" to "follow" in L154.

17. L157: “observation” -> “observations”

Response: Thank you for pointing this out. We have changed "observation" to "observations" in L157.

18. L172: The echo top altitude ranges between 6 and 10 km

Response: Thank you for your kind reminder. We have revised the cloud-top height range.

19. L181–182: The first two sentences need to be past tense for consistency.

Response: Thank you for pointing out the issue with the tenses. We have revised the tenses to the past tense. We have also thoroughly reviewed the entire manuscript and made consistent revisions to similar tense-related issues throughout the text.

20. L184: Remove period before Table 2 reference.

Response: Thank you for pointing this out. We have deleted the period before "(Table 2)".

21. L216: What is “ofin”?

Response: Thank you for pointing this out. Sorry for the typo. We have removed the "of " from "ofin".

22. L306: Remove the period

Response: Thank you for pointing this out. We have deleted the period.

22. L343: “is” -> “are”

Response: Thank you for pointing this out. We have changed "is" to "are" in L343.

23. L352: Remove “was”

Response: Thank you for pointing this out. We have removed "was" in L352.

25. L362: Define the ILW and IWV acronyms

Response: Thank you for your kind reminder. We have added the full spelling of "ILW" (Integrated Liquid Water) and "IWV" (Integrated Water Vapor) in the revised manuscript. Meanwhile, we have also unified the abbreviations of the corresponding

physical quantities in Figure 9.

26. L383: I'm not sure what you mean by "1, 2, 3"

Response: We sincerely apologize for not updating the information in the figure captions accordingly. The correct labels should be initial, middle, and latest. We have made the appropriate corrections in the revised manuscript.

27. L392: Remove "a showed", "a increasing" -> "an increasing"

Response: Thank you for pointing this out. We have revised the language in Line 392 according to your comments.

28. L388: Explicitly call out the panels being discussed in this paragraph as it's currently hard to follow the discussion

Response: Thank you for this helpful suggestion. We agree that explicitly identifying the panels will significantly improve the clarity of the discussion. Accordingly, we have revised the paragraph to clearly specify the relevant panels at each point of discussion.

29. References: It's difficult to distinguish from one reference to the next without proper indentation.

Response: Thanks for pointing this out. We agree that the lack of proper indentation made the references difficult to distinguish. In the revised manuscript, we have applied a hanging indent of 0.5 inches to all entries, ensuring each reference is now clearly separated and visually distinct.

Reference

- Battaglia, A., Augustynek, T., Tanelli, S., and Kollias, P.: Multiple scattering identification in spaceborne W-band radar measurements of deep convective cores, *Journal of Geophysical Research: Atmospheres*, 116, <https://doi.org/10.1029/2011JD016142>, 2011.
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