

1) Scientific significance

Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?

Comment 1: The paper does not present any findings that are new or generalizable. There is no evidence presented that the data shown are representative of a specific type of storm in this region of China. The paper reads like a report for a class exercise. It describes what the graphs show and then relates those descriptions to well-known findings from the literature with which they are consistent. For example, when an elevated warm layer is present in a winter storm, different precipitation types occur at the surface depending on the temperature profile characteristics of the warm layer aloft and the colder layer at the surface.

Response: Thank you for the forthright comments. We acknowledge that a single event cannot be taken as representative of all storms of this type in the region. We have added an explicit statement of this limitation in the Conclusions. We respectfully note that the primary contribution of this paper is not the proposal of a new physical mechanism, but the documentation and analysis of a specific spring storm event in the North China Plain, for which in-situ observations of elevated thunderstorm are relatively scarce. The objective of this study is to analyze the cloud microphysical characteristics associated with the snowstorm event. In the revised manuscript, we have added a discussion of the contributions of ice particles with various shapes to the number concentration and mass. To our knowledge, statistical studies on in-cloud particle habits for snowfall events produced by elevated thunderstorms using airborne measurements remain relatively scarce.

Comment 2: The paper examines small samples of data from a spring storm on 16 March 2023 with varying surface precipitation types in two different regions of a larger storm. Region with airborne data--3 usable flight segments of 22 min (B-D), 55 min (E-G) and 9 min (H-I) using aircraft in situ data and airborne Ka-band radar. Segments starting at E had a clear melting layer at ~ 2km altitude. Segment A-B not usable see below. These data are put in context of S-band dual pol radar (location ZZ in Fig 1). Region with ground based observations including vertically pointing Micro Rain Radar and S-band radar data (location SQ, Figure 1b) had an elevated warm layer with colder air below. Surface precipitation at SQ for a period of ~9 hours (Fig. 10).

Response: Thanks for the careful reading of our manuscript. We agree that this study is based on limited observations from one spring storm case on 16 March 2023. The airborne in-situ measurements, ground-based S-band dual-polarization radar, and vertically pointing Micro Rain Radar together constitute our multi-sensor dataset for this case. Although limited to one case, the multi-platform coincident observations provide a unique opportunity to investigate microphysical processes and vertical structure of the cloud during an elevated thunderstorm snowfall event. To discuss the

representativeness of our results, we have added a comparative analysis between the findings of this study and observations from other snowfall events in the revised manuscript.

Comment 3: Finding of a specific Z_e -SR relationship (Kulie and Bennartz) that happens to work better for this specific storm and location within the storm is not generalizable as it well known that Z_e -SR relations vary a lot even within the same storm (Rasmussen et al. 2003).

Response: We fully agree that the Z_e - SR relationship can vary significantly even within a single storm. The Z_e - SR relationship may be related to ice crystal habits, and researchers have established Z_e - SR relationships for different particle types. In the absence of statistical results from more cases, Z_e -SR relationship (Kulie and Bennartz) that happens to work better is not generalizable.

We do not intend to claim that the Z_e - SR relationship proposed by Kulie and Bennartz is universally applicable to storms in this region. What we actually mean is that selecting a Z_e - SR relationship based on hydrometeor classification results can lead to better snowfall estimation. In this event, the dual-polarization radar identified the surface snowfall hydrometeor type in the Shangqiu area as aggregates (the corresponding dual-polarization radar observations have been added in the revised manuscript), and we found that the aggregate Z_e - SR relationship established by Kulie and Bennartz produced better precipitation estimates. Although this is only a single case, we still tend to believe that the better performance of the Z_e - SR relationship (Kulie and Bennartz) is not coincidental, but rather reflects that selecting a Z_e - SR relationship based on dual-polarization particle habit classification can improve snowfall estimation. In the future, when conditions permit, we will conduct studies on more cases, with the aim of developing a quantitative snowfall estimation method that incorporates hydrometeor classification results.

Comment 4: Not showing Doppler velocity data from airborne KPR radar is a big problem (lack of evidence). Rather vertical air motions are inferred from Z spatial pattern and in situ data. This does not work since microphysical characteristics are a time-integrated characteristic. For example, irregular particles detected by aircraft could have been formed 10s of minutes earlier and then transported to location where the aircraft found them.

Response: Thank you very much for your valuable comments. We completely agree with your point that microphysical characteristics are a time-integrated property. Considering that the velocity and spectral width from KPR are affected by aircraft attitude, airspeed, and other factors, their accuracy remains questionable, and accurate correction of the velocity is extremely difficult (more details see as response for Comment 4 of Scientific Quality). After careful analysis, we have decided to remove the analyses related to velocity and spectral width.

2) Scientific Quality

Comment 1: Key missing reference is Rasmussen et al. (2003, JAMC) which makes the point that snowfall rate can vary up to two orders of the magnitude for the same radar reflectivity.

Response: Thank you for your comment. We have added the relevant references in the revised manuscript.

Comment 2: Aircraft in situ data especially from particle probes are not reliable in ascents and descents. Any discussion of data and results from flight segment A-B (shown in Fig. 2) needs to be removed from the paper (text, Table 2, several figures).

Response: Thanks for raising this important question regarding the reliability of aircraft in situ data during the aircraft ascent segment. We agree that particle probe measurements during aircraft ascents and descents are more susceptible to airflow distortion, particle shattering. The generation of spurious particles (e.g., streak artefacts and shattered fragments), which can artificially enhance measured number concentrations. For our dataset, data collected in the ascent leg were subjected to rigorous quality control. Meanwhile, the use of aircraft in situ data from vertical profiling segments, including ascents and descents, is well established in the cloud microphysics literature (Gayet et al., 2009; Xiong et al., 2023; Barrett et al., 2020; Giangrande et al., 2016; Stechman et al., 2020; Lloyd et al., 2015). Based on the above, we respectfully maintain that the A–B segment data, after quality control, remain suitable for the analysis presented in this paper.

Comment 3: “vertical ice crystals” are mentioned several times (including in Abstract) but this term is not a standard term of ice crystal shape and it is not defined.

Response: “vertical ice crystals” actually means “vertically oriented/aligned ice crystals”, which can be a useful category for diagnosing regions of possible strong electric fields. We have replaced the term "vertical ice crystals" with the more precise and standard phrase "vertically oriented ice crystals", which is commonly used in the polarimetric radar literature to describe ice particles whose major axes are aligned along the vertical direction.

Comment 4: Figure 5b shows mean Doppler velocity values close to 3 m/s which appear to be unrealistically high. There are also several spikes in the profiles between 4.5 and 7 km that are unexplained. The corresponding KPR Doppler velocity and spectral width data are needed explain this. My concern is that the airborne KPR is not pointing exactly vertically and aircraft motions and/or horizontal winds are incorporated into the Doppler velocity values. There is a big discrepancy between Figure 5b mean values and distribution of velocity values from MRR (which are dominated by near zero and negative values) shown in Figure 8 middle row.

Response: Thank you very much for your comment. We sincerely apologize that, in our earlier processing of the KPR data profiles, we averaged the parameters over

individual range bins. However, because the aircraft flight altitude varied, each range bin corresponded to a different actual altitude. Upon re-examination, we realized that this approach would average echoes that actually originated at different heights, which is not entirely appropriate. Therefore, we performed height interpolation and recalculated the physical quantity profiles.

In addition, when the KPR scans upward and downward, at approximately 300 m above and below the aircraft, the noise and gain generated by the KPR during pulse transmission and reception cause the detected values near the aircraft track to be relatively large. The sharp peaks at around 4.5 km and 7 km in the previous figures were both spurious values near the aircraft flight altitude. To eliminate the influence of these spurious values, we applied quadratic spline interpolation to the original profiles and updated and reanalyzed the profiles in the figures.

Regarding the issue you mentioned about the unrealistically high KPR Doppler velocity, we agree that the velocity values may indeed have been affected by aircraft motion and other factors. The observed results for velocity and spectral width are shown below (Fig.S1 a), where anomalously large values can be seen during certain periods (regions outlined by black boxes). These periods mostly correspond to areas where the aircraft was turning (Fig.S1 b), indicating that the data during these stages were clearly affected by flight attitude, with the aircraft velocity component severely contaminating the plane speed.

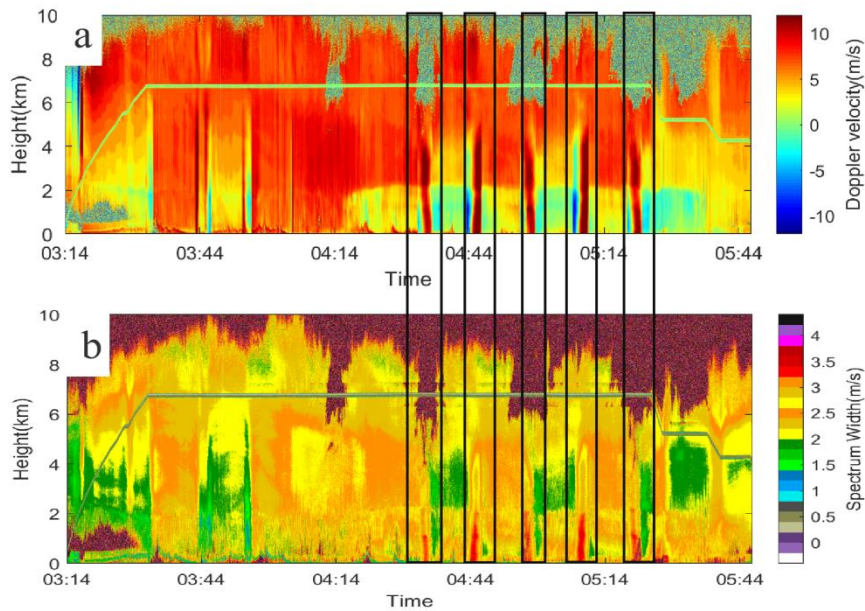


Figure S1. Doppler velocity (a) and Spectrum Width (b) observed by KPR

The KPR Doppler velocity observations are not the main focus of this study, and velocity correction is a separate, complex issue that deserves dedicated investigation. If the correction is not performed properly, the credibility of the data cannot be guaranteed. Taking all these considerations into account, we have decided to remove

the analyses related to KPR velocity and spectral width from the manuscript.

3) Presentation Quality

Comment 1: In Figure 6 showing ground-based S-band data, locally higher Z values up to 45 dBZ look more like melting layer or rain than convective cells in snow.

Response: Thank you very much for your suggestion. Pure dry-snow echoes are indeed generally difficult to reach an intensity of 45 dBZ. The radar vertical cross-section for this region shows that there was no obvious bright band at the 0°C level, and the strong echo with an intensity of 45 dBZ reached the ground (Fig. S2). So, we consider that these strong echoes are not caused by the melting layer in stratiform precipitation. These regions were also accompanied by obvious lightning activity (Fig. S3), so we believe that convective cells did indeed exist in this area. Meanwhile, the dual-polarization radar hydrometeor classification results indicate that the particles at the height of the strong echoes were graupel or wet snow, and aggregates (Figure 8), while the surface station in this region recorded snowfall as the precipitation type. We tend to believe that the locally higher Z values up to 45 dBZ are precipitation echoes from wet snow or graupel.

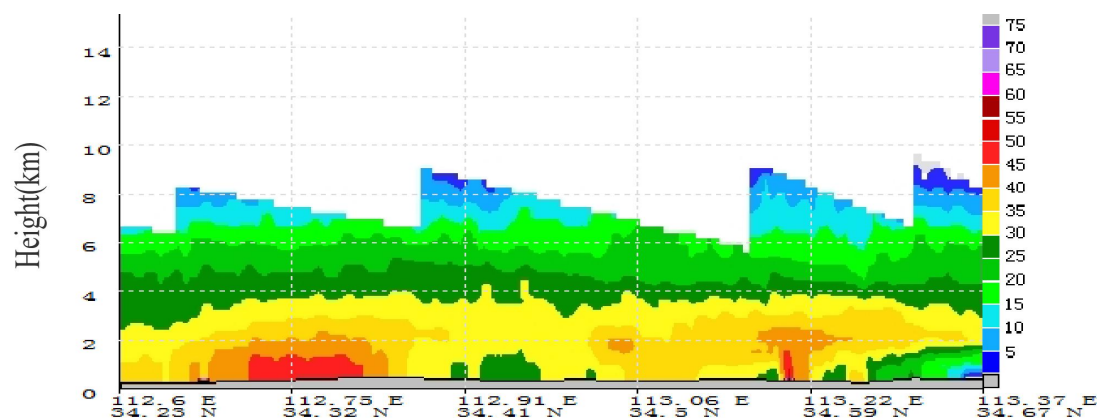


Figure S2. Cross-section of strong radar echo region at 11:24

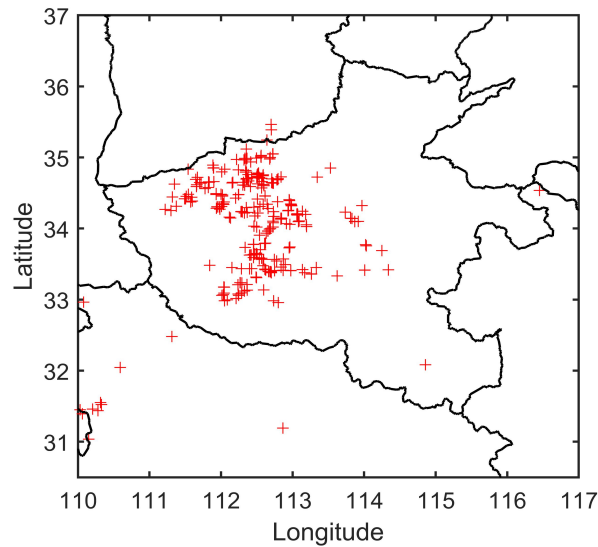


Figure S3. Lightning distribution map of the snowfall event on March 16, 2023

Comment 2: Lack of details would prevent reproducibility. For example line 422 mentions precipitation phase retrieval method but does not specific what the input data are.

Response: Thanks for pointing this out. We have now specified the input data in the revised manuscript. Specifically, the input data for the precipitation phase retrieval are the raw power spectra from the Micro Rain Radar (MRR). We have clarified the processing of the input data in the section of Data and Methods.

Comment 3: Coarse vertical resolution ERA5 data will not be able to resolve shallow warm layers. Soundings mentioned (paragraph starting on line 431) are not shown. Fuyang and Xuzhou stations (LY and NY in Figure 1) where soundings are available are quite far to the west (~112.5 E longitude) compared to SQ (~115 E longitude) from study area SQ where the ground-based radars including MRR are. Radar data shown in paper demonstrates that temperature profile conditions are not uniform across the storm.

Response: ERA5 data can reveal the general evolution of the temperature profile, but due to its coarse vertical resolution, it does not capture the shallow warm layer. We agree that the temperature profile conditions within the storm area were non-uniform. To analyze the vertical temperature distribution, we selected the three sounding stations (ZZ, FY, and XZ) closest to station SQ for a comprehensive analysis. The sounding data presented in the original manuscript starting from line 431 are from the ZZ, FY, and XZ stations. Although these stations are still some distance from Shangqiu, this represents the finest resolution achievable with the current observational network.

In the original manuscript, the FY and XZ stations were not shown in Figure 1; Figure 1 displayed the locations of the Nanyang (NY) and Zhengzhou (ZZ) sounding stations. For better presentation, the locations of FY and XZ have been added to Figure 1.

Comment 4: Transitions from sleet to rain and then sleet to snow are mentioned on lines 527-528. But their specific time periods are not shown. Hence not possible to see evidence for findings in paragraph starting on line 527. Figure 9 shows classification of "mixture" and "rain" at surface not sleet.

Response: In the revised manuscript, we have now specified the time periods of these transitions. Specifically, the transition from mixed – precipitation to rain occurred around 16:00, while from rain back to mixed – precipitation occurred around 18:00, and then the transition from mixed-precipitation to snow occurred around 22:00.

Regarding Figure 9, we acknowledge that it classifies the surface precipitation type as "mix" and "rain". To clarify, the "mix" category actually encompasses the presence of sleet at the surface, as it indicates a coexistence of liquid and solid hydrometeors near the ground. However, we realize that this may not be sufficiently clear. Therefore, we have changed the word “sleet” in the text to “mixed-precipitation”.

Comment 5: There are numerous errors in tense and word choice throughout the paper. Paper needs to a comprehensive edit to bring it up to journal standard English.

Response: Thank you very much for your comments. To improve the writing quality, the manuscript has now undergone a thorough and comprehensive language review by a native English speaker with expertise in our field. We have carefully revised the entire text to correct errors in tense, word choice, and overall clarity.

Reference

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