

## **Response to Reviewers**

**“Dual-frequency profiler study of hydrometeor fall speeds in tropical deep convection”**

**Giangrande et al.,**

**Preprint, in discussion for Atmospheric Measurement Techniques (AMT)**

### **General Comments from the Authors:**

*We would like to thank the reviewers and the editor for their efforts to improve this manuscript towards it being suitable for publication in AMT. Our responses have been combined into a single document. We use blue italics font to differentiate author text from reviewer comments.*

### **Responses to Referee # 1:**

General: This is an interesting observational study that gives a better understanding of hydrometeor behavior within deep convection. It refines the reflectivity-based parameterization for estimating terminal fall speed of graupel in tropical regimes based on dual-frequency profiler observations that graupel falls faster in Darwin convection than Oklahoma for the same reflectivity. The study has no apparent major flaws, and the manuscript is well-written. However, there are some minor clarifications required.

*We thank this reviewer for their thoughtful comments and suggestions. We hope we have addressed all of their concerns in our responses that follow.*

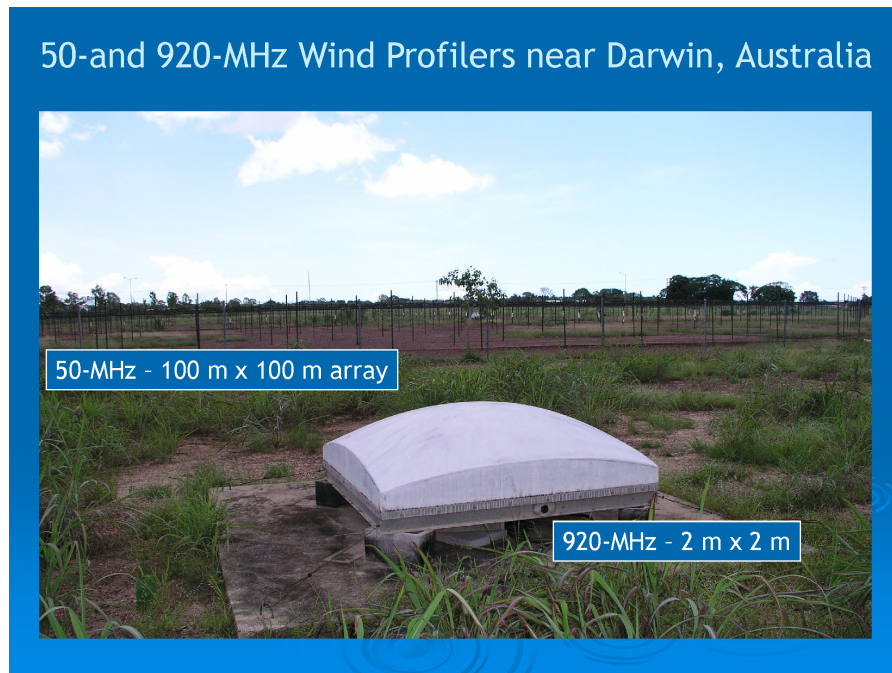
Specific comments:

1. Echo classification: Figure 1 presents 4 different classes, but it is not clear how these profiles are assigned, especially what is the difference between stratiform and bright band—aren't they one and the same? The text refers to the technique used in Giangrande et al. (2013), but the relevant section in that reference is rather vague on what conditions define each of these four classes.

*We add extra details in the text. The reviewer is correct that regions classified as “bright band” are drawn from columns that otherwise would be labelled “stratiform”, but exhibit enhanced (fuzzy-logic, confidence sense) signatures in quantities ( $Z$ , standard deviation of  $Z$ ,  $V_d$ , Spectrum width) associated with melting aggregates. As with most echo classifications, we only expected this as a means-to-and-end, as the nature of this problem (defining “convection”) is fluid/transitional and there may be columns with convective-type detrained media aloft that are transitioning to more stratiform-type behaviors below, etc.*

2. Profiler matching: This matching result needs some additional clarification, especially since NBF is mentioned. What is the combined spatial scale of the two profilers (i.e., do the beams overlap and if so how much)? An idea of how far apart the systems are located would help.

*Agree; The details may not be clear to those unfamiliar with the Darwin profiler set-up. We have added text. The photograph below shows the 920-MHz antenna (with size 2x2 m), and that it is approximately 20 m away from the 50 MHz antenna (with size 100x100 m).*



3. Disdrometer analysis: The text around line 195 states indicates that fall speed is not a direct measurement of the disdrometer. However, this study uses a 2DVD, which provide a direct measurement of fall speed because they use two cameras with 6-7 mm vertically offset fields of view (Schonhuber et al. 2008).

*Fixed. Yes, video/laser disdrometers report a fall speed estimate for drops. The lead author did not use these values (in ARM raw files). It is common to substitute a fit to Gunn and Kinzer, e.g., we do not expect this to result in a significant change in  $V_d$ . Disdrometer processing also typically filters (i.e., in Tokay-style) drops with fall speeds outside (~50%) of such standard fits.*

4. Stratiform processes and resultant DSDs: Around line 240, a relevant citation is warranted but missing. Here are some to consider: Waldvogel 1974, Huggel et al. 1996, Gatlin et al. 2018.

*Thank you for the suggestion. We have added a reference to Gatlin et al. (2018).*

5. Line 281, grammatical mistake: “...increasingly rapidly...”

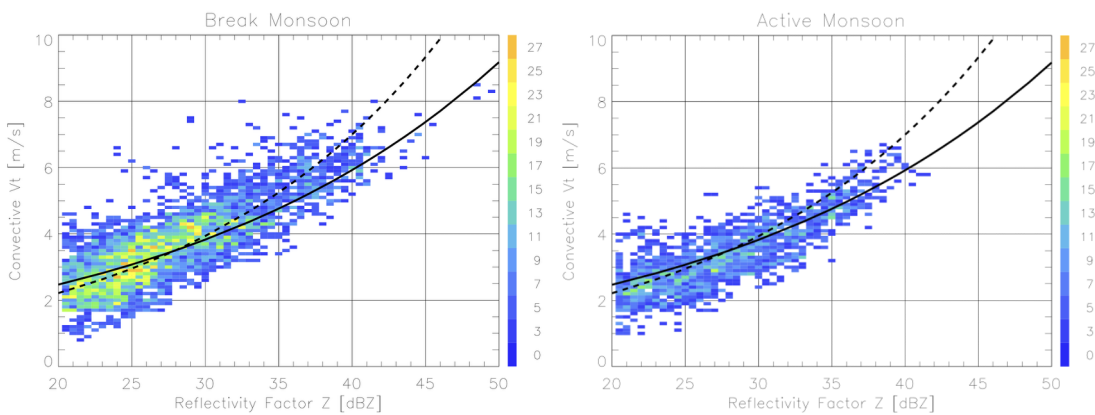
*Thank you. Fixed.*

6. Other relevant studies to consider: Matrosov (2023) also examined the fall speed of frozen hydrometeors using a vertically pointing radar. Albeit that was for snowfall regimes, it included cases with rimed particles and thus should be considered for comparison with the observations from Darwin.

*Thank you. We have added some mention of this reference and its relationships.*

7. Figure 5: Suggest using different types of lines to delineate the two fits in each panel

*Thank you for the comment. Will address this with a revised Figure 5 as below.*



## Responses to Referee #2

General comments: The study quantifies relationships between fall speeds and reflectivity above the freezing level using two profilers in Darwin. The dual-frequency approach allows for more robust separation between air motions and fall speeds so that the reflectivity-weighted fall speed can be retrieved. The authors use a large sample size of data to quantify the Z-vt relationships for rain, snow, and mixed-phase media. The analysis is particularly helpful for quantifying and reducing the fall speed uncertainty of mixed-phase media.

The manuscript is very well written, and the analyses use existing techniques that are well-established. I have a few minor comments for the authors to consider.

*We thank this reviewer for their thoughtful comments on this manuscript. We hope we have addressed their concerns in our responses below. For select answers, some detail we provide may not be directly communicated in the revised manuscript, but the reviewer called our attention to several details we may need to pursue in the future.*

Minor comments:

1. Line 12: Place  $V_t$  in parentheses

*Thank you. Fixed.*

2. Line 83: Did the authors mean deep instead of deeper?

*Agree that "deep" is more appropriate. Fixed.*

3. Line 115: Raleigh should be Rayleigh

*Fixed.*

4. Line 150: Suggest cross-beam distance or other wording rather than beamwidth since most readers will relate beamwidth to its measure in degrees

*Agree; We want to emphasize the arclength of the pulse volume, which is the product of the beamwidth (in radians) and range. As written, the text focuses on the beamwidth, not the arclength. A few sentences have been re-written in this paragraph.*

5. Section 2: While past research supports the idea that break events have more intense updrafts, did the authors consider using profiler or other data to confirm that the updrafts were actually more intense or other storm characteristics were different (e.g., particle size implied by maximum Z, echo top, etc.)?

*We did not perform this check initially. The reviewer is correct if there is concern that regime separation may not guarantee we sample stronger Break storms, representative hits. However, we might weigh our simpler approach against possible concerns of cherry-picking and limited datasets. We have revised our statements to better address limitations (i.e., selection bias) in what we present. For example, we have added text such as,*

*“However, this was not an attempt to isolate the strongest Break afternoon continental storms, higher-quality overpasses, or identify cells having matched lifecycle stages; Our displays likely will not represent the most pronounced separations between these regimes, and such conditional sampling considerations may be suitable to future studies as longer-term Darwin profiler records become available.”*

*We did enter this expecting that Break events were associated with stronger rainfall rates, higher total accumulation over Darwin. Yet, a key detail is the most pronounced differences may be focused on afternoon, continental storms. In this way, the low-level Z properties in our dataset (“convection”) both have Z overpasses at similar 40 dBZ level, though our Break conditions saw additional higher Z, but also a wider spread to lower Z. That may suggest Break saw stronger convective hits, but the totality of our sampling is also important. For example, Darwin profilers were used by Schumacher et al., (2015) to contrast “Active” and “Break” velocity profiles (11 Nov 2005 to 10 Feb 2006). They found mean velocity profiles (deep convection) were slightly stronger for “Active”. Their result was followed by statements that suggest Break may not have appeared as strong (e.g., velocity profile shapes were also different) because the local overpass time was more associated with developing lifecycle stages. Both their and our study include 10 Break events prior to 10 February. The 10 events we included prior to that date had afternoon overpasses, but also some hits we might consider of lower quality (“chording” cells), as well as several outside of the local afternoon. Thus, Schumacher et al.’s result may be consistent and reasonable for that event sampling, since Break cells that may be the strongest still require (at minimum) factoring: (1) day/afternoon, (2) captured (good hit), and (3) overpass at similar “lifecycle” stages as “Active” storms they are judged against, etc. An important detail with our current study is that we included an additional 13 Break events from 11 February onward. These events were among the more impressive ones captured this season (strong afternoon events, solid hits). For the current study, we do not want sampling to turn too much into a chicken-and-egg situation, but we agree these ideas are something a longer dataset should help address (a multi-year Darwin reprocessing is underway).*

6. Line 346: I presume that the peripheral regions also have large reflectivity gradients that are smoothed by the profiler’s larger beamwidth. Is it possible that a portion of the volume contains higher Z and media with higher vt, but the volume-averaged Z is lower due to the gradient? Thus, this could result in a higher vt than expected for a given Z.

*Yes. We noted this possibility as well (line 170).*

7. Line 352: Should influence be influencing?

*Yes. Agree.*

8. Line 364: The end of this sentence is awkward. Please rephrase

*Agree. Modified the line.*

9. Line 472: Suggest tropical thunderstorms to avoid any potential confusion with tropical storms/cyclones

*Agree. Fixed.*

10. 2–4 captions: Please add histogram after cumulative to convey the type of plot

*Agree. Fixed.*

11. The focus of the study is on profiling radars. However, can the authors comment on the utility of these fall speed relationships for scanning weather radars operating at S-X bands (e.g., for multi-Doppler wind retrieval)?

*Difficult to address in our text, but we provide thoughts for the reviewer. One is on the role of attenuation in rain and how Z is handled by shorter wavelength radar (networked). There may be some scattering considerations for Vt-Z behaviors if moving from S- to X-, but the first order problem is attenuation. For X- networks, our experience has been that shorter-wavelength studies we've participated in use only the Vd (i.e., partial attenuation in rain) and a longer wavelength radar (S-) provides unattenuated Z. Multi-Doppler retrieval systems may be sensitive to fall speed relationship modification, depending on the fall speed base assumptions those retrieval start from when compared to other tunable factors (e.g., gridding choices, outlier handling, temporal offset). Until recently, many retrievals applied simple fall speed behaviors, e.g., a rain fall speed assumed everywhere. Our Stony Brook University partners now use a variety of "convective"/"stratiform"/"rain"/"snow"-like concepts that may follow relationships similar to Giangrande et al. (2013), as an example. Their previous studies (e.g., Oue et al., 2019, <https://amt.copernicus.org/articles/12/1999/2019/>) have suggested fall speeds are an important factor, but at this time, we are not aware if those changes by SBU has been documented for their potential impact.*

12. Table 1: To make the table easier to read, please include the height range as a separate column entry.

*Ok. Fixed.*