This paper presents analysis of EarthCARE Doppler velocity measurements that coincide with identified tropical cyclones. It combines EarthCARE measurements and a tropical cyclone tracking dataset to evaluate dynamical and microphysical properties of tropical cyclones.

Considering the novelty of this type of data, I think this work has the potential to be impactful. The paper would benefit from reframing of the problem and clearer presentation of data to support the conclusions being made. My main suggestion is to highlight the parts of the study that are novel. For example, the abstract states that the results show a transition from a convective-like structure in the eyewall to a stratiform structure in the rain band. While it is useful to confirm that the orthodox picture of tropical cyclones holds when evaluating it against global Doppler velocity retrievals, the strength of the dataset is that new or uncertain relationships can be evaluated. Moreover, the main value of the data is that it provides global measurements of the "coupling between dynamics and cloud microphysics in TCs", but the conclusions made about the coupling are presented with either weak or unclear evidence. In the following review, I point out places where I suggest the paper should be reframed and strengthened.

We thank the reviewer for this overall assessment and helpful suggestion. In the revised manuscript, we have reframed the main observational findings to emphasize the regional differences in Doppler velocity distributions and their relationships with vertical air motion and particle sedimentation, rather than focusing primarily on the convective-to-stratiform transition. We have also strengthened the supporting analyses and clarified the interpretation of the coupling between dynamics and cloud microphysics. Detailed revisions addressing these points are described in our responses to the specific comments below.

- L47: How do you know that "the presence of supercooled liquid water has important implications for latent heat release in the upper levels."? The latent heat of freezing is an order of magnitude smaller than the latent heat of condensation/deposition, so there would need to be significantly more supercooled water freezing than other types of latent heat release for it to contribute a lot to TCs.

We agree with the reviewer that the original statement overemphasized the direct contribution of freezing latent heat. We have therefore revised as the follows: ‘The presence of supercooled liquid water affects riming, freezing, and hydrometeor growth in the upper levels’ in Line 48-49.

- Lines 46-56 summarize an open problem in the field that I think this paper may address. The summary states that there has been some prior work examining how supercooled water in the upper parts of the tropical cyclone impacts TC intensity, but there is disagreement about whether or not supercooled water exists in large quantities there. It seems to me that the EarthCARE data presented in this work can both evaluate whether the supercooled water exists in large quantities (compared to other condensate) and whether the presence of supercooled water impacts the intensity of the TC. (Whether it impacts the vertical velocity or maximum radial wind.)

We thank the reviewer for this insightful suggestion. We agree that the occurrence of supercooled and mixed-phase particles is an important open question that EarthCARE can help address. However, the AC_CLP particle classification used in this study identifies particle types rather than directly quantifying the mass of supercooled liquid water. We have therefore revised the manuscript to make this limitation explicit. In the revised Introduction and Results, we focus on the occurrence and vertical distribution of mixed-phase and supercooled particle classifications and their relationship with Doppler velocity and vertical air motion, rather than making quantitative claims about the amount of supercooled water or its direct impact on TC intensity.

- Fig. 2: I'm unclear on how these figures are generated. Do you collocate the TC, then take the average radar reflectivity given by EarthCARE over the portion of the TC that EarthCARE crosses? Then bin the resulting 1D profile by the RMW of the TC? If that is indeed the methodology, then how do you deal with the following two cases: (1) the EarthCARE satellite passes 900 km away from an RMW=10 tropical cyclone, in an area where there is just stratiform precipitation and (2) the satellite passes through the center of an RMW=10 tropical cyclone? In Fig. 2b, would these two profiles get averaged together? The method you use should be clearly presented in the paper.

We thank the reviewer for pointing out that the construction of the radial composites was not sufficiently clear in the original manuscript. We have expanded Section 2.2.1 to describe the procedure explicitly. For each EarthCARE radar profile, we calculate the distance from the interpolated TC center at the overpass time and normalize that distance by the corresponding RMW. All valid profiles are then grouped according to their normalized radial distance and composited within each radial bin.

Therefore, profiles are not averaged simply because they belong to the same TC intercept. For example, a profile sampled far from the storm center is assigned to a large normalized-radius bin, whereas a profile passing through the inner-core region is assigned to a small normalized radius bin. Only profiles falling within the same normalized radial bin contribute to the same composite value. Because sampling is limited near the TC center, all

profiles within 2 RMW are combined into a single inner-core bin, while 1-RMW bins are used from 2 to 15 RMW. We have also added supplementary sampling information showing both the number of independent intercept events and the number of valid profiles contributing to each radial bin.

- The second sentence in Section 3.1 states that sufficient number of samples exist for RMW ranging from 2 to RMW. The next sentence then draws a conclusion about RMW<2. Since the number of samples is low there, it is unreasonable to draw conclusions about those results.

We agree with the reviewer that the limited number of independent samples in the inner-core region requires caution. We have revised the manuscript to explicitly state that only two independent EarthCARE intercept events contribute to the inner-core composite. To assess the robustness of these results, we additionally compare the individual-event vertical profiles in the Supplement. Although the two cases show similar vertical structures in Doppler and air velocities, we emphasize that these results should not be interpreted as statistically representative of TCs in general.

- I don't see why this is true: "A relative minimum in reflectivity at 5 RMW suggests a separation between the primary and outer rainband regions." What other evidence do you have, or prior work, shows this?

We agree that the original interpretation of the reflectivity minimum near 5 RMW was not sufficiently supported. We have removed this statement and no longer interpret 5 RMW as a physically defined boundary between the primary and outer rainbands. Instead, 5 RMW is used as a practical threshold for the regional analysis. We also performed sensitivity tests using 4 and 6 RMW as alternative boundaries, and the resulting Doppler velocity distributions remain qualitatively similar (see Fig. S4 and S5 below).

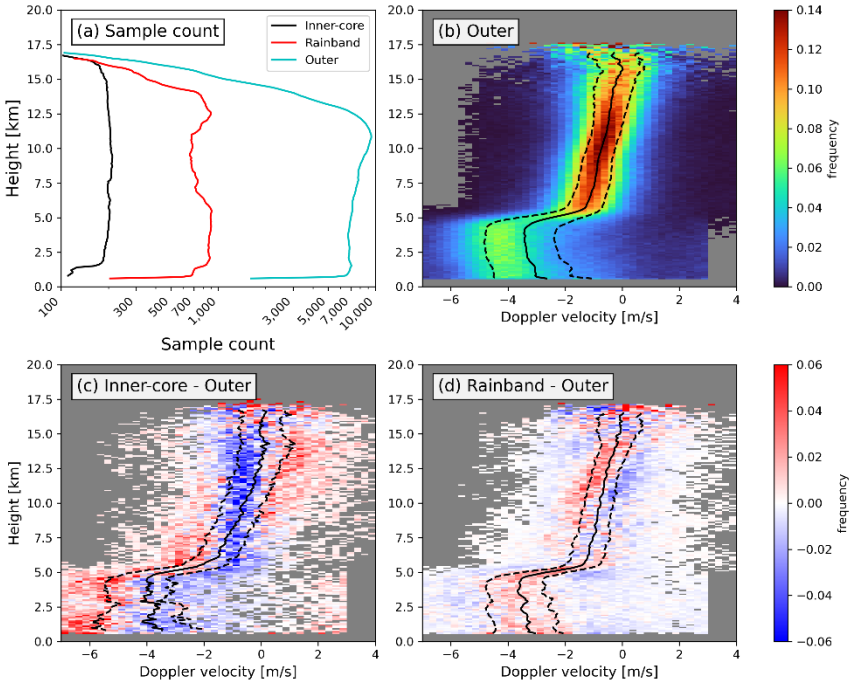


Figure S4. Same as Fig. 3, but using 4 RMW as the boundary between the rainband and outer regions. The rainband and outer regions are therefore defined as 2–4 RMW and 4–15 RMW, respectively.

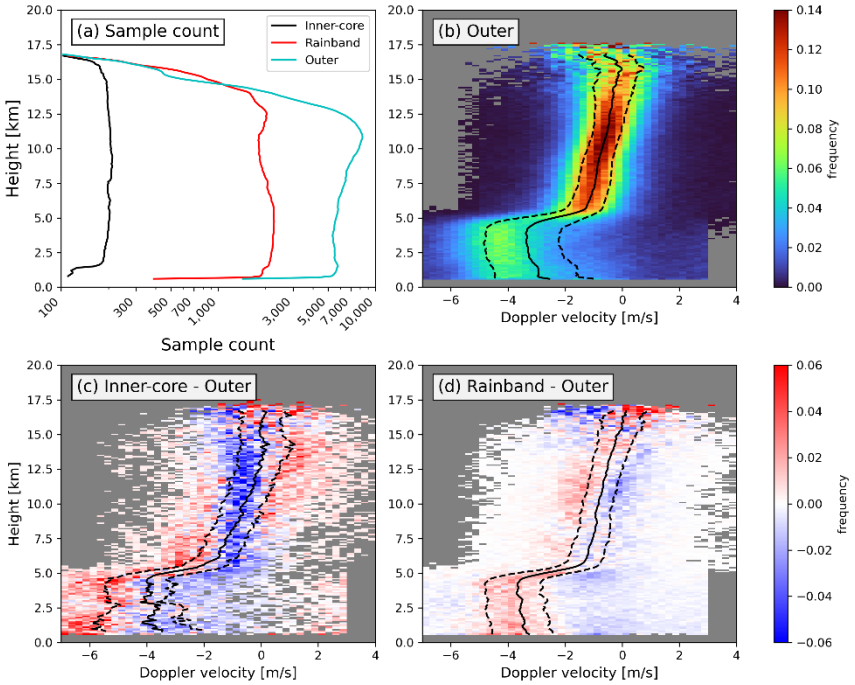


Figure S5. Same as Fig. 3, but using 6 RMW as the boundary between the rainband and outer regions. The rainband and outer regions are therefore defined as 2–6 RMW and 6–15 RMW, respectively.

- It looks like air velocity is positive everywhere in the tropical cyclones that you observe, according to Fig. 2b. Is that what you expect? Why is that?

We thank the reviewer for raising this point. The positive mean air velocity should not be interpreted as the mean vertical motion over the entire TC circulation. Our analysis is conditional on radar-detectable cloud and precipitation samples because only grid points with radar reflectivity greater than −20 dBZ are retained. Therefore, the composite preferentially samples cloudy regions, where upward motion is more frequently observed. This sampling criterion is described in Section 2.2.1.

- Fig. 3 b-d. Is the probability distribution normalized by each row, or is this a two-dimensional histogram? Should we interpret these figures as the probability density of the doppler velocity at 5 km, for example? Moreover, when you write Eyewall-Outer and rainband-Outer, do you mean that you compute the PDFs of each, then subtract the PDFs?

We thank the reviewer for pointing out that. We have clarified the procedure in Section 3.2: 'At each height, the Doppler velocity PDF is normalized independently using the valid samples at that level, so that each vertical level represents its own Doppler velocity distribution.' in Line 274-275. Thus, for example, a horizontal slice at 5 km represents the Doppler velocity probability distribution at 5 km. For Fig. 3c and 3d, we first calculate the normalized PDF separately for each radial region and then subtract the outer-region PDF from the inner-core or rainband PDF, respectively. Positive values therefore indicate Doppler velocity ranges that occur more frequently than in the outer region, while negative values indicate lower relative frequency.

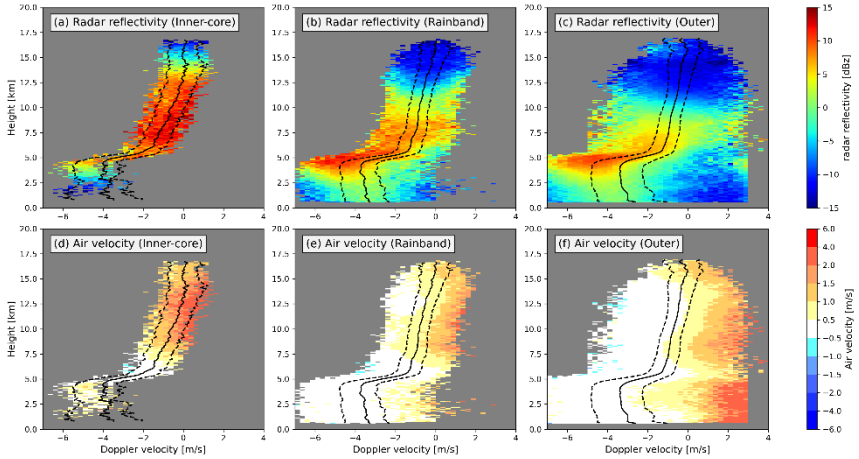
- "Above 6 km, enhanced reflectivity is generally associated with Doppler velocities below the upper quartile, suggesting that Doppler velocity in the rainband is contributed by the particle falling" Isn't Doppler velocity always impacted by falling hydrometeors? What is this statement attempting to distinguish about the rainband?

We agree with the reviewer that hydrometeor fall speed always contributes to Doppler velocity, and the original wording did not clearly describe what distinguishes the rainband region. We have therefore revised this discussion. The revised analysis shows that vertical air velocities in the rainband and outer regions are broadly similar over much of the ice-phase layer, whereas the radar reflectivity-weighted terminal velocity and retrieved particle terminal velocity are slightly more negative in the rainband. We therefore interpret the systematic negative shift of the rainband Doppler velocity distribution relative to the outer region as being more closely associated with differences in particle sedimentation rather than vertical air motion.

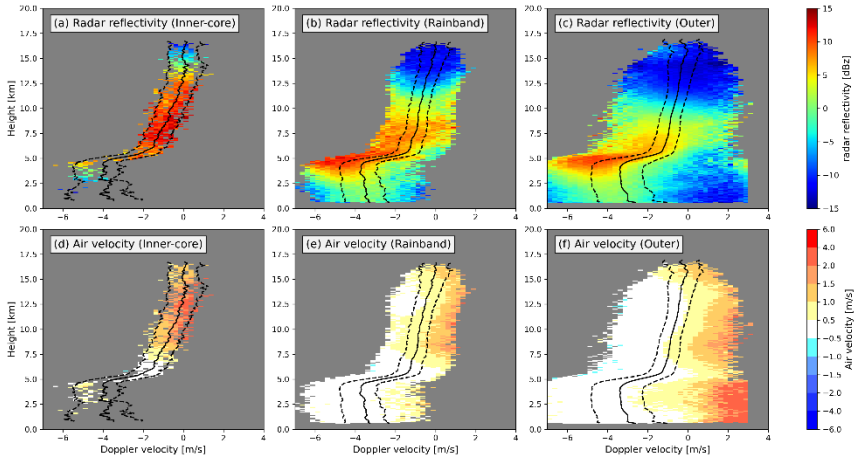
- "We eliminate the points where sample size less than 5." That's a very low threshold. How does increasing this impact the results?

We thank the reviewer for raising this concern. The minimum sample threshold in Fig. 4 is used only to mask poorly sampled Doppler velocity–height bins and does not affect the calculation of the conditional composites themselves. We repeated the masking using thresholds of 10 and 15 samples (see figures below). Although the displayed area becomes smaller as the threshold increases, the main patterns and regional differences remain qualitatively unchanged.

The same figure as Fig. 4, but the region where sample size is less than 10 is masked.



The same figure as Fig. 4, but the region where sample size is less than 15 is masked.



- Fig. 6. It may be worth emphasizing that this probability density plot differs from the

other types of probability density plots you showed. It looks like the probability is normalized by column here, which is not the case for the others (like 3 or 7). Are we to interpret each column as the probability density of (in (a), for example) small ice particles occurring at each height, binned by the RMW? This plot is also missing labels on the x axis and color bar.

We thank the reviewer for pointing out that the normalization of Fig. 6 was unclear. We have clarified the calculation in the revised manuscript: 'At each radial-distance-height bin, particle frequency is calculated as the number of samples classified into each particle group divided by the total number of valid particle classifications in that bin. Therefore, the frequencies of the five groups sum to one in each bin'. This normalization is different from the Doppler velocity PDFs shown in Fig. 3 and 7. We have also revised Fig. 6 to include the missing axis and color-bar labels.

- Why not plot Fig. 7 with air velocity instead of Doppler velocity on the x axis? It seems to me that you are aiming to relate condensate and air motion in the text, so the figure might support your conclusions better if it used air velocity.

We thank the reviewer for this suggestion. We agree that relating particle type directly to air velocity would be useful for examining cloud dynamics. However, we retain Doppler velocity on the horizontal axis because it is directly observed by CPR, whereas air velocity is a retrieved quantity obtained by estimating and removing the hydrometeor terminal velocity from the Doppler measurement. The uncertainty in retrieved air velocity is therefore larger and depends on assumptions about particle properties and terminal velocity, particularly in mixed-phase regions. For this reason, we use the more directly observed Doppler velocity in Fig. 7 to examine how different particle types occupy different parts of the measured velocity distribution. The relationship with air motion is then interpreted using the retrieved air velocity analyses in Figs. 4 and 5.

- "In particular, the occurrence of supercooled water and mixed-phase particles in regions of upward motion implies that riming processes are likely enhanced within convective updrafts, where supercooled liquid water is available." This sentence is stated in reference to Fig. 7, which combines all the data, whether it was taken at the eyewall or rainband. Since that's the case, it is unreasonable to make conclusions specifically about convective updrafts based on this figure. Is it possible to subset to only the convective part of the storms to evaluate your hypothesis? Is it possible to show that riming is more important than other condensate formation processes?

We agree with the reviewer that the original interpretation was too strong. Because Fig. 7 combines samples from all radial regions, it does not provide sufficient evidence to attribute

the observed mixed-phase particles specifically to convective updrafts. In addition, the available observations do not allow us to isolate riming from other condensate growth processes. We have therefore removed these statements and revised both figure and description. The revised discussion is limited to the observed relationship between particle type and Doppler velocity. The paragraph is revised as the follow: 'We further project the frequency diagrams onto Doppler velocity PDFs to investigate the relationship between Doppler velocity and cloud particle types. Because the sample sizes in the inner-core and rainband regions are insufficient for constructing separate frequency diagrams, all three regions are combined to construct the frequency diagrams in Doppler velocity space (Fig. 7). Small ice particles dominate above 7.5 km, except in regions beyond the upper quartile of Doppler velocity (Fig. 7a). In these regions, snow particles are present, although with lower frequency (Fig. 7b). Snow particles are mainly found between approximately 5 and 12 km and extend toward more negative Doppler velocities than small ice particles (Fig. 7b). Below the freezing level, rain is the dominant particle type (Fig. 7c). However, its frequency decreases in the region of Doppler velocity close to 0 m s-1, where small liquid droplets become more evident (Fig. 7d). Mixed-phase particles are concentrated near and above the melting level and exhibit a distribution across a broad range of Doppler velocities (Fig. 7e). Enhanced frequencies occur on both the more negative and less negative (or near-zero) sides of the Doppler velocity distribution. The less negative side overlaps with Doppler velocity ranges where stronger upward air motion is identified in Fig. 4, while the more negative side is consistent with faster particle falling. These results indicate that mixed-phase particles occur under two different conditions: convective-like vertical air motion and regions with faster ice particle falling. The different cloud particle types occupy distinct parts of the Doppler velocity distribution, which provides the relationship between cloud particle properties, vertical air motion, and particle falling motion in TCs for evaluating cloud microphysics in models.'

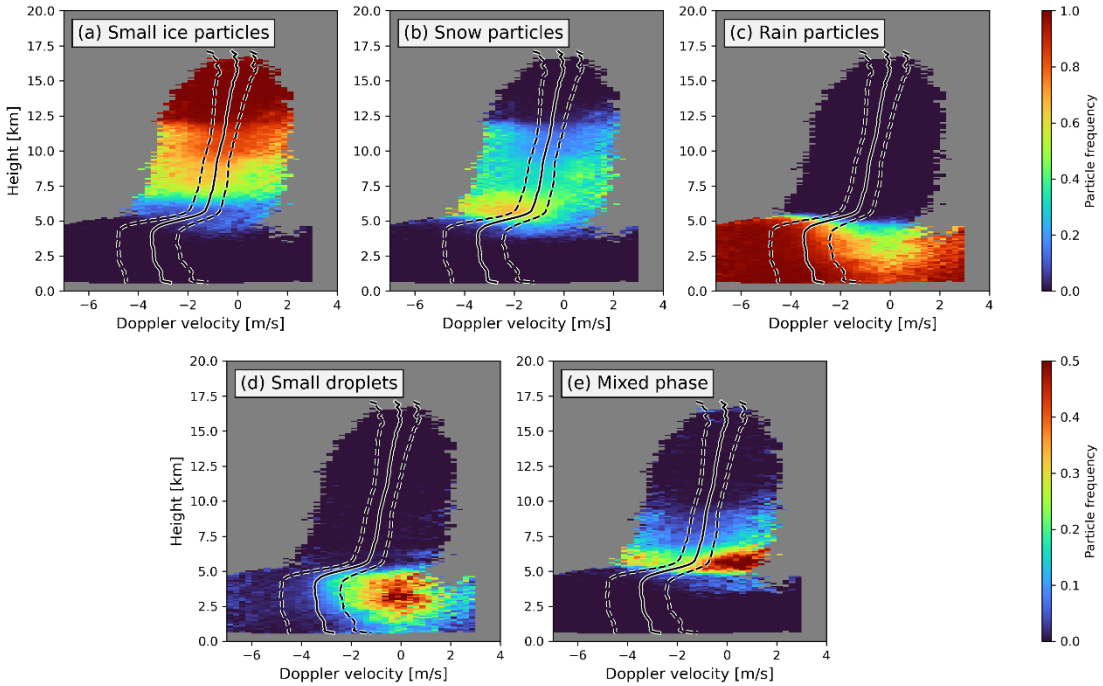


Figure 7. Frequency diagrams of (a) small ice particles, (b) snow particles, (c) rain particles, (d) small droplets, (e) mixed-phase particles on the DPF of Doppler velocity.

- Fig. 7d shows the two-dimensional histogram of supercooled water. It looks like supercooled water is most prevalent when the dopper velocity is highest. The claim that supercooled water is more prevalent than expected would be stronger if there was a clearer comparison of the relative frequencies of the different type of particles.

We thank the reviewer for this suggestion. We agree that examining a single supercooled-water distribution alone does not provide a sufficient basis for assessing whether it occurs more frequently than other particle types. We have therefore reorganized the particle-type analysis to provide a clearer comparison of relative frequencies. In the revised Fig. 6, the retrieved particle classifications are grouped into five categories: small ice particles, snow particles, rain particles, small droplets, and mixed-phase particles. This provides a direct comparison of the relative occurrence of the different particle groups. We have also revised Fig. 7 using these grouped categories and reduced the emphasis on supercooled water alone. Accordingly, we no longer make a strong quantitative claim that supercooled water is more prevalent than previously expected.

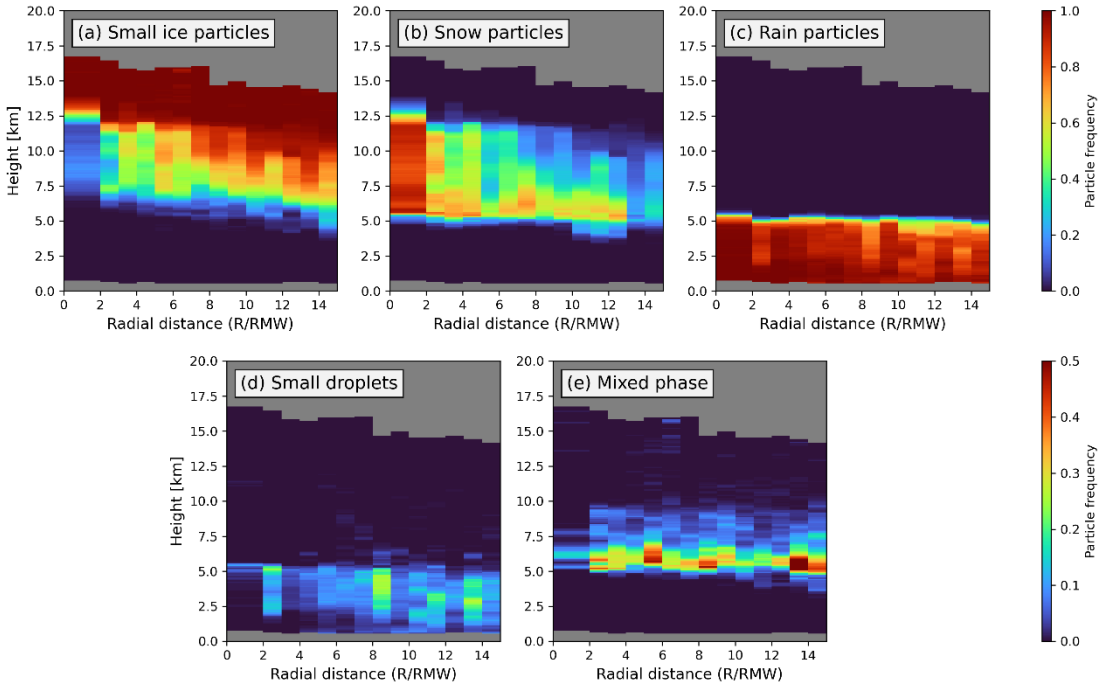


Figure 6. RMW-height frequency diagram of (a) small ice particles, (b) snow particles, (c) rain particles, (d) small droplets, and (e) mixed-phase particles.

Minor comments

- Typo in affiliation 1: Atmoshpere -> Atmosphere

We thank the reviewer for pointing out this typo. It has been corrected.

- Typo in correspondence name: Hunag -> Huang

We thank the reviewer for pointing out this typo. It has been corrected.

- L42: This sentence is unclear, should rephrase: "The above studies demonstrated a breakthrough in TC understanding using CloudSat observations despite only radar reflectivity"

We agree that the original sentence was unclear. We have revised it to: 'These studies demonstrate the value of CloudSat radar observations for understanding TCs, although CloudSat provides radar reflectivity without Doppler velocity.'

- L48: Should be past tense, and phrasing is unclear: "Therefore, supercooled water is regarded as the target to weaken TC structure and intensity by seeding during STORMFURY (Willoughby et al., 1985)." Is this saying that the goal of the campaign was to reduce the amount of supercooled water in storms?

We thank the reviewer for pointing out the unclear wording. We have revised the sentence to use the past tense and to describe the purpose of Project STORMFURY. The revised text now states: 'Project STORMFURY therefore attempted to modify TC structure and intensity by seeding supercooled water (Willoughby et al., 1985).'

- I suggest editing the text for clarity.

We thank the reviewer for this suggestion. We have carefully revised the manuscript throughout to improve clarity

- Fig 2a: The color bar chosen here makes it hard to distinguish small and large values.

We thank the reviewer for this suggestion. We have revised Fig. 2a to improve the visibility of the sample-size distribution. The figure now displays the actual sample counts to make it easy to be distinguished.

- Fig 7: hard to see the black line on dark blue background

We thank the reviewer for pointing this out. We have revised Fig. 7 to improve the contrast and visibility of the overlaid lines against the shaded frequency distribution.

- Fig. 10, 11: hard to see the tick labels

We thank the reviewer for this comment. We have revised Figs. 10 and 11 by increasing the size and improving the visibility of the axis tick labels and other figure annotations.

- How many samples are there? How many TCs did EarthCARE intercept over the 14 month period?

We thank the reviewer for pointing out the need to report the sampling information more clearly. The revised manuscript now states ‘Radial composites are constructed by collecting all valid EarthCARE profiles within each normalized radial bin. The dataset includes 101 EarthCARE intercept events from 28 TCs in the Northern Hemisphere. These intercepts provide 1,853 valid profiles within 15 RMW.’ in Line 155-157.

Reviewer's comments on "EarthCARE Observations of Vertical Motion and Cloud Microphysical Structure in Tropical Cyclones" by Huang et al.

General Comments

This paper examines EarthCARE observations of tropical cyclones (TCs), focusing on the vertical structure of radar reflectivity, Doppler velocity, vertical air motion, and cloud particle types. Based on 14 months of observations, the authors compiled a few hundred to a few thousand snapshots of TC-related clouds and precipitation, depending on the distance from the TC center. Radial composites of the above variables were then constructed. In addition, the authors performed a NICAM simulation of Hurricane Humberto, which was sampled by an EarthCARE overpass through the storm center, and compared the simulation with the corresponding EarthCARE observations.

Overall, this is a useful study that will make a valuable contribution to the literature, as it represents the first spaceborne statistical characterization of TCs from the perspective of a cloud profiling radar (CPR) with Doppler capability. The observations provide a unique opportunity to jointly examine the cloud microphysics and dynamics of tropical cyclones.

The paper consists of two main components: (1) an EarthCARE-based characterization of tropical cyclones and (2) a comparison between EarthCARE observations and NICAM simulations. I find the first component to be strong, comprehensive, and well developed. In contrast, the NICAM–EarthCARE comparison is much more tentative. My primary concern is that the model evaluation is based on only a single case study (Hurricane Humberto), and the overall simulation of storm intensity for this case is rather poor (e.g., minimum sea-level pressure and maximum near-surface wind; see Fig. 8), with differences in storm intensity approaching 80%. It is entirely reasonable that a global storm-resolving model (GSRM) cannot faithfully reproduce every individual tropical cyclone. Such models are generally expected to perform better in a statistical sense than for individual storms. However, evaluating the model using only one case substantially limits the significance of the comparison. After all, it could

simply be an unfortunate case in which NICAM failed to reproduce Hurricane Humberto particularly well. At the same time, asking the authors to perform NICAM simulations for a large sample of TCs comparable to the EarthCARE observational dataset would likely be impractical. Consequently, the second half of the paper is considerably less compelling than the first.

We thank the reviewer for this thoughtful assessment. We agree that the comparison between NICAM and EarthCARE is more limited than the observational analysis because it is based on a single TC case and the simulated intensity of Hurricane Humberto is not fully reproduced.

To improve the representation of the storm, we repeated the simulations using the ECMWF IFS operational analysis at approximately 9-km resolution as the atmospheric initial condition instead of the coarser ERA5 reanalysis used previously. In addition, we changed the initialization time to 26 September so that the simulation begins from an earlier and weaker stage of the hurricane, which helps reduce the intensity bias. These changes improve the simulated storm intensity and bring it closer to the IBTrACS data (see Fig. 8 below), although the maximum near-surface wind remains underestimated

We have also reduced the emphasis placed on the model comparison throughout the revised manuscript. The NICAM analysis is now explicitly described as a preliminary single-case comparison rather than as a general evaluation of NICAM performance. The purpose of the comparison has been reframed to examine how two commonly used NSW6 microphysical configurations affect EarthCARE observable cloud structures under comparable simulated storm states, rather than to assess the overall ability of NICAM to reproduce tropical cyclones.

This limitation is now emphasized in the Abstract, Introduction, Section 4, and Summary. We also state that the differences between K21 and T24 demonstrate sensitivity to the microphysical configurations in this particular case and should not be generalized to NICAM performance for TCs as a whole.

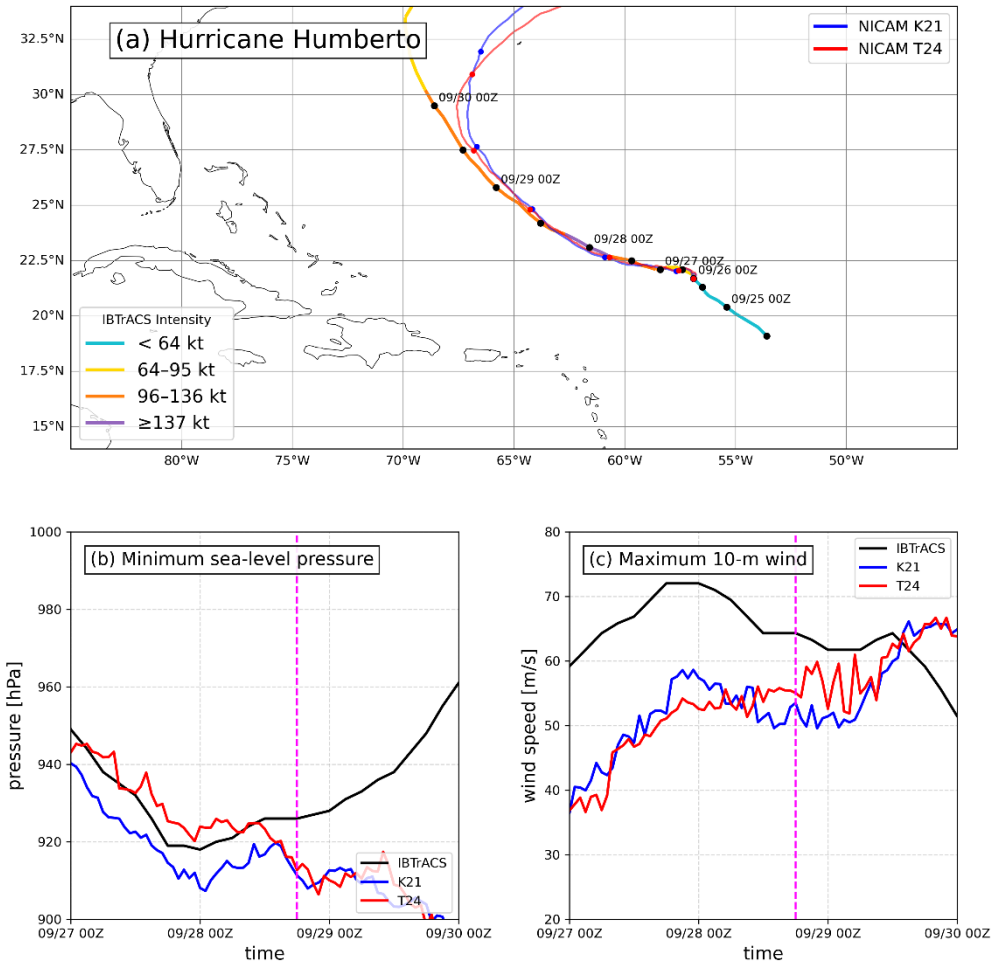


Figure 8. (a) Tracks of IBTrACS, K21, and T24. The colored segments along the IBTrACS track indicate the observed storm-intensity categories. Time evolutions of (b) minimum sea-level pressure and (c) maximum near-surface wind. The vertical dashed pink lines indicate the timing that EarthCARE intercept with hurricane Humberto.

Another major point concerns the interpretation of EarthCARE's Doppler velocity measurements and vertical air motion. The paper should clearly state, early in the introduction or data section, that the EarthCARE CPR has a Doppler Nyquist velocity of approximately ±5 m s ^{1}. Vertical velocities exceeding this range are therefore aliased $^{-}$ (folded). Without this explanation, readers, particularly those familiar with ground-based or airborne radar observations of tropical cyclones,

may be surprised that the vertical motions within the hurricane eyewall rarely exceed 5 m s ^{1}. To be fair, this is not a flaw, $^{-}$ but rather a consequence of the instrument's design. EarthCARE's Doppler capability was developed primarily to observe the relatively weak vertical motions associated with clouds such as cirrus and stratocumulus, rather than the intense convective updrafts found in tropical cyclones.

We thank the reviewer for pointing out this important issue. We have added a clear description of the CPR Doppler Nyquist velocity in Section 2.1.1. We also clarify that all vertical velocities in this study are defined as positive upward, so particle fall velocities are negative. The added part can be found 'The CPR Doppler Nyquist velocity is approximately ±4.8 m s^{-1}, so velocities exceeding this range are aliased in the raw measurements and subsequently corrected through Doppler unfolding in the Level-2 processing. In this study, all vertical velocities are defined as positive upward, so particle fall velocities are negative.' in Line 103-106. These additions are intended to help readers interpret the relatively limited range of Doppler velocities observed in the TC inner-core region.

On balance, I recommend major revision. One option would be to remove the model comparison section and focus exclusively on the EarthCARE observational results. Alternatively, the authors could substantially reduce the emphasis placed on the NICAM comparison. Presenting a single-case comparison in which the simulated storm intensity differs so substantially from observations ultimately weakens confidence in the model evaluation, especially since NICAM is not intended to function as a numerical weather prediction (NWP) model optimized for reproducing individual storms.

Specific Comments

- L29: Roh et al. (2026) is missing from the reference list.

We thank the reviewer for pointing this out. The publication year has been corrected to Roh et al. (2025).

- L43–44: The synergy between CPR and ATLID is not, by itself, what makes

EarthCARE unique. CloudSat/CALIPSO already provided radar-lidar synergy. EarthCARE's distinguishing feature is that its CPR has Doppler capability, whereas the CloudSat CPR does not.

We agree with the reviewer. We have revised the Introduction to clarify that the distinguishing capability of EarthCARE for this study is the Doppler measurement provided by CPR, rather than radar–lidar synergy itself. We now explicitly note that CloudSat provided radar reflectivity without Doppler velocity and emphasize that EarthCARE CPR adds new information on cloud particle motion and vertical air motion.

● L95–96: This sentence unnecessarily repeats the information already presented in L88–90.

We thank the reviewer for the suggestion. We have reorganized this part of Section 2.1.1 and removed the repetitive description.

● Fig. 1d: It should be explicitly stated that positive velocity denotes upward motion in this figure. By convention in radar meteorology, positive Doppler velocity usually indicates motion away from the radar, which would correspond to downward motion for a spaceborne radar.

We thank the reviewer for pointing out this convention. We have clarified that 'The CPR Doppler Nyquist velocity is approximately ±4.8 m s^{-1}, so velocities exceeding this range are aliased in the raw measurements and subsequently corrected through Doppler unfolding in the Level-2 processing. In this study, all vertical velocities are defined as positive upward, so particle fall velocities are negative.' in Line 103-106.

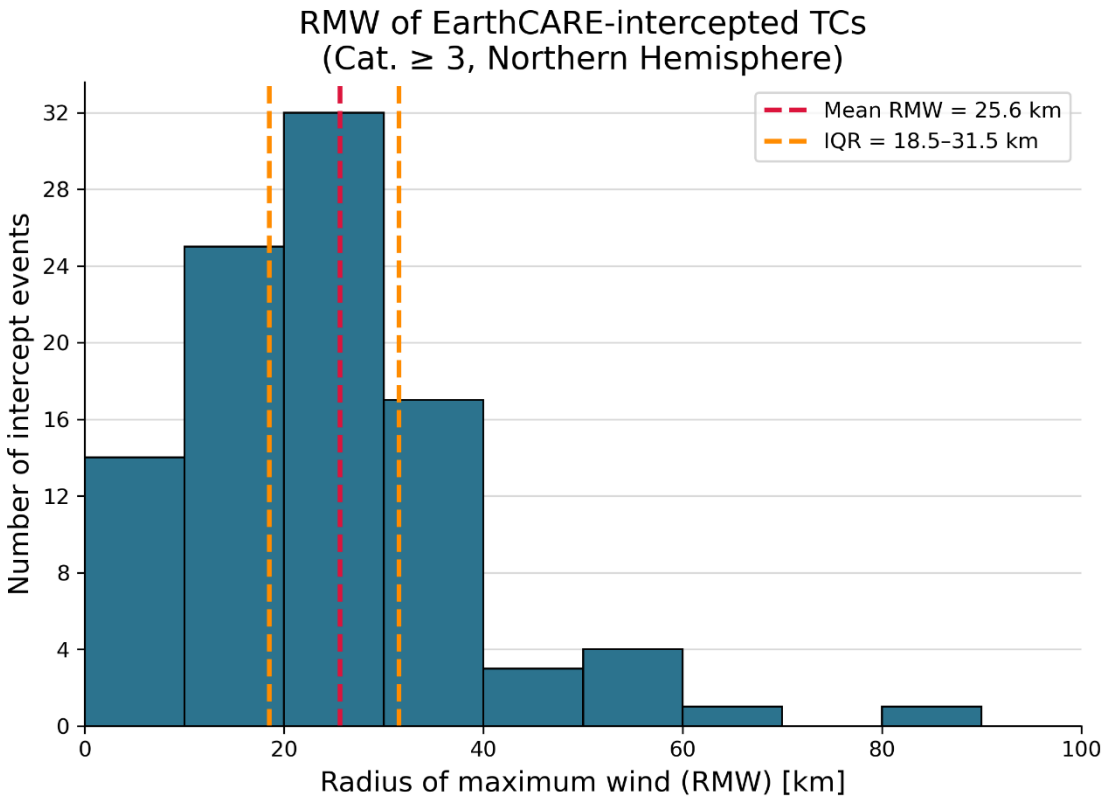
● Fig. 1d: Readers familiar with ground-based or airborne radar observations of TCs may be surprised that Doppler velocities within the eyewall only reach about 6 m s ^{1}. It should therefore be explained that the EarthCARE CPR has a Doppler Nyquist velocity of approximately ±5 m s ^{1}, so vertical velocities exceeding this range are $^{-}$ $^{-}$ aliased (folded). A similar discussion should also be included for Figs. 2c and 2d to facilitate proper interpretation.

We thank the reviewer for this important suggestion. We have added a description

'The CPR Doppler Nyquist velocity is approximately ±4.8 m s^{-1}, so velocities exceeding this range are aliased in the raw measurements and subsequently corrected through Doppler unfolding in the Level-2 processing. In this study, all vertical velocities are defined as positive upward, so particle fall velocities are negative.' in Line 103-106.

● Fig. 2 (and L127): Some statistics of the radius of maximum wind (RMW) should be provided for the general reader. For example, if a typical RMW is on the order of 50 100 km, this normalization implies that one unit of normalized distance corresponds to roughly 50–100 km from the storm center.

We thank the reviewer for this suggestion. We have added statistics of the RMW for the sampled TCs to provide a physical scale for the normalized radial distance



and desccription: 'The sampled TCs have a mean RMW of 25.6 km and an interquartile range of 18.5–31.5 km (Fig. S1).' in Line 149-151.

Figure S1. Distribution of the radius of maximum wind (RMW) for EarthCARE intercept events of Category 3 or stronger TCs. The dashed line indicates the mean RMW (25.6 km) and interquartile range (18.5-31.5 km).

● Fig. 2a: I suggest plotting the actual counts rather than their logarithms. Since the counts span roughly 50–3000, the color scale can still be displayed on a logarithmic scale to preserve dynamic range. However, requiring readers to mentally convert values of 2.0–3.4 into powers of ten interrupts the flow of reading and interpretation. The same comment applies to Fig. 3a.

We thank the reviewer for this suggestion. We have revised Figs. 2a and 3a to display the actual sample counts rather than their logarithms. This allows the sampling density to be read directly.

● L186: Is there a previous reference supporting the use of RMW = 5 as the boundary between the primary rainband and the outer rainband? If so, it should be cited.

We thank the reviewer for raising this point. We do not intend to interpret 5 RMW as a strict physical boundary between the rainband and outer regions. We have revised the manuscript to clarify that 5 RMW is used as a practical threshold for the regional analysis. We also performed sensitivity tests using 4 and 6 RMW as alternative boundaries. The resulting Doppler velocity distributions are qualitatively similar, indicating that the main conclusions are not sensitive to the exact choice of this boundary. These sensitivity results are provided in the Supplement.

● L195–205: The differing air-motion characteristics among the eyewall, rainbands, and outer region are arguably among the most important findings of this study. This section deserves substantially more discussion. In particular, how do these EarthCARE CPR results compare with previous studies based on ground-based or airborne radar observations? Relevant references should be included.

We thank the reviewer for this important suggestion. We added two paragraph to discuss that: ‘Airborne and ground-based Doppler radar observations provide a useful context for the vertical air motions observed by EarthCARE. Black et al. (1996) found that more than 70% of vertical velocities in eyewall, rainband, and stratiform regions of seven hurricanes were within -2 to 2 m s-1, while the eyewall exhibited the broadest distribution and approximately 5% of the vertical velocities exceeded 5 m s-1. In our EarthCARE observations, vertical air velocities in the

sampled inner-core region are mostly around 1-2 m s-1 above the freezing level, with stronger upward motions exceeding 2 m s-1 occurring on the more positive side of the Doppler velocity distribution. Thus, although the strongest updrafts reported by airborne radar are not well represented in the present EarthCARE composite, the broader distribution and stronger upward air motion in the inner-core region are qualitatively consistent with previous observations. Ground-based dual-Doppler observations of Hurricane Ike also identified localized eyewall updrafts with a mean peak upward velocity of approximately 8.3 m s-1 associated with mesovortices (Wingo and Knupp, 2016), further illustrating that localized eyewall updrafts can be substantially stronger than the composite values observed here.

For the rainband region, the EarthCARE composite is dominated by weaker upward motion of approximately 0.5 m s-1 and a pronounced bright band, indicating mainly stratiform-like characteristics in the sampled profiles. Airborne Doppler radar studies, however, have shown that localized convective updrafts can be embedded within TC rainbands (Hence and Houze, 2008; Barron et al., 2022). Barron et al. (2022) reported that the modal maximum velocity of identified rainband convective updrafts was approximately 1-2 m s-1, with 2-3 m s-1 updrafts also frequently observed at larger normalized radii. The weaker motions in our composite therefore should not be interpreted as evidence that convective elements are absent from rainbands. Instead, these elements are intermittent and occupy only part of the rainband area, while the limited number of independent EarthCARE intercepts and the narrow nadir sampling make them difficult to isolate in the present analysis. The EarthCARE composite therefore mainly represents the more frequently sampled stratiform component of the rainband, and additional observations will be needed to better characterize the embedded convective elements. Overall, the EarthCARE results are qualitatively consistent with previous Doppler radar observations, while differences in sampling should be considered when comparing the magnitude of vertical motion.' in Line 357-380.

● Fig. 4: The rationale for compositing reflectivity and vertical air velocity on a Doppler velocity–height diagram should be explained.

We thank the reviewer for this comment. We have clarified the motivation for Fig. 4 in the revised manuscript. Doppler velocity reflects the combined effects of vertical air motion and hydrometeor fall speed. Therefore, compositing radar reflectivity and retrieved vertical air velocity in Doppler velocity–height space allows us to examine which cloud and dynamical structures are associated with different parts of the Doppler velocity distribution. This analysis helps determine whether the regional differences identified in the Doppler velocity PDFs are more closely associated with differences in air motion or particle sedimentation.

● L281–283: It is not clear how terminal velocity is retrieved from particle size, shape, orientation, and temperature. Some explanation or an appropriate reference is needed. Figure 9 also suggests that the retrieved terminal velocity differs substantially from (V_{tz}), which warrants clarification.

We thank the reviewer for pointing out the need for clarification. We have substantially revised the description of terminal velocity in Section 2.1.1 and the discussion associated with Fig. 5. Following Sato et al. (2009, 2025), vertical air velocity is retrieved by estimating the hydrometeor terminal velocity from inferred particle properties and removing its effect from the observed Doppler velocity. The estimated particle terminal velocity depends on particle properties such as size, shape, orientation, and atmospheric conditions.

We now also distinguish explicitly between the retrieved particle terminal velocity and the radar reflectivity-weighted terminal velocity. The former represents the estimated falling speed of particles based on the retrieved particle properties, whereas the latter represents the sedimentation component of the measured Doppler velocity after radar-reflectivity weighting. Because the radar product does not directly provide the latter quantity, we estimate it as the difference between Doppler velocity and retrieved air velocity. This distinction explains why the two quantities can differ substantially.

● Fig. 8a: The four storm-intensity curves should be explicitly identified as IBTrACS

observations.

We thank the reviewer for this suggestion. We have revised Fig. 8 and its caption to clearly distinguish the IBTrACS observations

● L354–355: I am concerned about the substantial discrepancy between the simulated and observed intensity of Hurricane Humberto. The maximum near-surface wind speeds differ by nearly 80%. Given such a large difference in the overall storm intensity, it is not surprising that the structural differences shown in Figs. 10 and 11 are also significant.

We agree that the large intensity discrepancy in the original simulation, which leads to difficulties in interpreting the structural differences. In the revised manuscript, the simulation setup has been improved by using ECMWF IFS operational analysis and by initializing the model earlier on 26 September. The simulated intensity is consequently closer to the IBTrACS observations, although the near-surface wind remains underestimated. Therefore, these differences are interpreted cautiously, and the comparison is used mainly to examine the relative response of the simulated cloud structure to the K21 and T24 microphysical configurations rather than to attribute all discrepancies between model and EarthCARE to microphysics alone.

Review of "EarthCare Observations of Vertical Motion and Cloud Microphysical Structure in Tropical Cyclones"

Overall Assessment

This is a good and interesting manuscript that makes use of a novel observational dataset to characterize vertical motion and cloud microphysical structure in tropical cyclones. That said, I feel the analysis stops somewhat short of its potential, and a deeper treatment of several points below would substantially strengthen the paper's contribution.

Major Comments

1. **Model improvement pathway needs to be made concrete.** The most compelling potential contribution of EarthCARE observations is their ability to improve model representations of TC processes, but this is not developed. For instance, on Line 433 the authors state that "the observational constraints offer guidance for improving the representation of cloud microphysical processes in models," but no specifics are given. I would strongly encourage the authors to elaborate on this point — what specific microphysical parameterizations or processes could be constrained, and how would one go about using these observations to do so? Even a focused discussion of one or two concrete examples would considerably strengthen the paper.

   We thank the reviewer for this helpful suggestion. We agree that the model-improvement pathway should be described more concretely. We have therefore added a discussion at the end of the manuscript describing how different retrieved quantities can be used to constrain specific aspects of cloud microphysics. The added discussion can be found in Line 574-582: 'EarthCARE provides complementary constraints for model development. Radar reflectivity is sensitive to particle size distributions, while cloud water and ice contents provide information on condensate mass. Comparing Doppler velocity with retrieved air velocity can further help evaluate particle sedimentation and terminal-velocity settings. Although these retrieved quantities are not fully independent because they share radar inputs and retrieval assumptions, their combined evaluation can help identify microphysical settings that improve the representation of both particle distributions and sedimentation. This study compares two commonly used configurations within the single-moment NSW6 scheme to isolate the sensitivity to their different falling speed assumptions. Future studies will examine the terminal velocities of individual hydrometeor species, compare NSW6 with the double-

moment NDW6 scheme, and evaluate the sensitivity of simulated vertical motions to model resolution'.

2. **Justification for single-moment microphysics.** Why was a double-moment scheme not tested? NICAM does have a double-moment microphysics option available (see Seiki and Nakajima, 2014, J. Atmos. Sci., 71, 833–853, who developed the NDW6 double-moment scheme). The authors should clarify why they chose to test only a single-moment scheme — even if they also ran a sensitivity test with modified parameters — or, if feasible, discuss how the results might be expected to differ under a double-moment or even bin microphysics representation.

   We thank the reviewer for this important comment. In this study, we intentionally focus on the single-moment NSW6 scheme because our primary objective is to evaluate the scheme with two common configurations. The K21 and T24 configurations are both commonly used NSW6 settings in recent NICAM applications, providing a practical basis for comparison within the NICAM community. We agree that the double-moment NDW6 scheme is an important option for future evaluation. However, double-moment NDW6 scheme would add additional degrees of freedom, which would make it difficult to discuss effects of terminal velocity. We therefore leave a direct comparison between NSW6 and NDW6 to future work and have added this point to the revised manuscript in Line 580-582: 'Future studies will examine the terminal velocities of individual hydrometeor species, compare NSW6 with the double-moment NDW6 scheme, and evaluate the sensitivity of simulated vertical motions to model resolution.'.

3. **Imprecise language in places.** For example, on Line 418 the text states "with a long tail extending toward 1 g/m³ (Fig. 11a)," but the figure appears to show the distribution extending beyond 1 g/m³. Please review the manuscript for similar instances where the wording does not accurately match what is shown in the figures.

   We thank the reviewer for pointing this out. We have revised the description of Fig. 11 so that the stated ranges more accurately match the distributions shown in the figure. We also reviewed and revised imprecise descriptions where necessary throughout the manuscript.

4. **Sample size should be reported.** How many individual tropical cyclones were sampled in this study? This is arguably more important for assessing the robustness and generality of the results than the time period of the dataset, which is currently the only sampling information provided.

We thank the reviewer for this suggestion. We have added detailed sampling information to the revised manuscript in Line 155-162: 'The dataset includes 101 EarthCARE intercept events from 28 TCs in the Northern Hemisphere. These intercepts provide 1,853 valid profiles within 15 RMW. Because the number of satellite intercepts differs substantially with radius, we examine both the number of independent intercept events and the number of valid profiles contributing to each radial bin (Fig. S2). Because the number of EarthCARE samples decreases substantially toward the TC center, all profiles within 2 RMW are combined into one radial bin, whereas 1-RMW bins are used from 2 to 15 RMW in the subsequent analyses. Sampling is particularly limited within 2 RMW, so individual-event mean vertical profiles are compared to assess the robustness of the inner-core statistics (Fig. S3)'. We have also added a supplementary figure showing both the number of independent intercept events and the number of valid profiles contributing to each radial bin (see Fig. S2 below). Because sampling is particularly limited in the inner-core region, we further compare the individual-event vertical profiles in the Supplement to assess the robustness of the composite results (see Fig. S3 below).

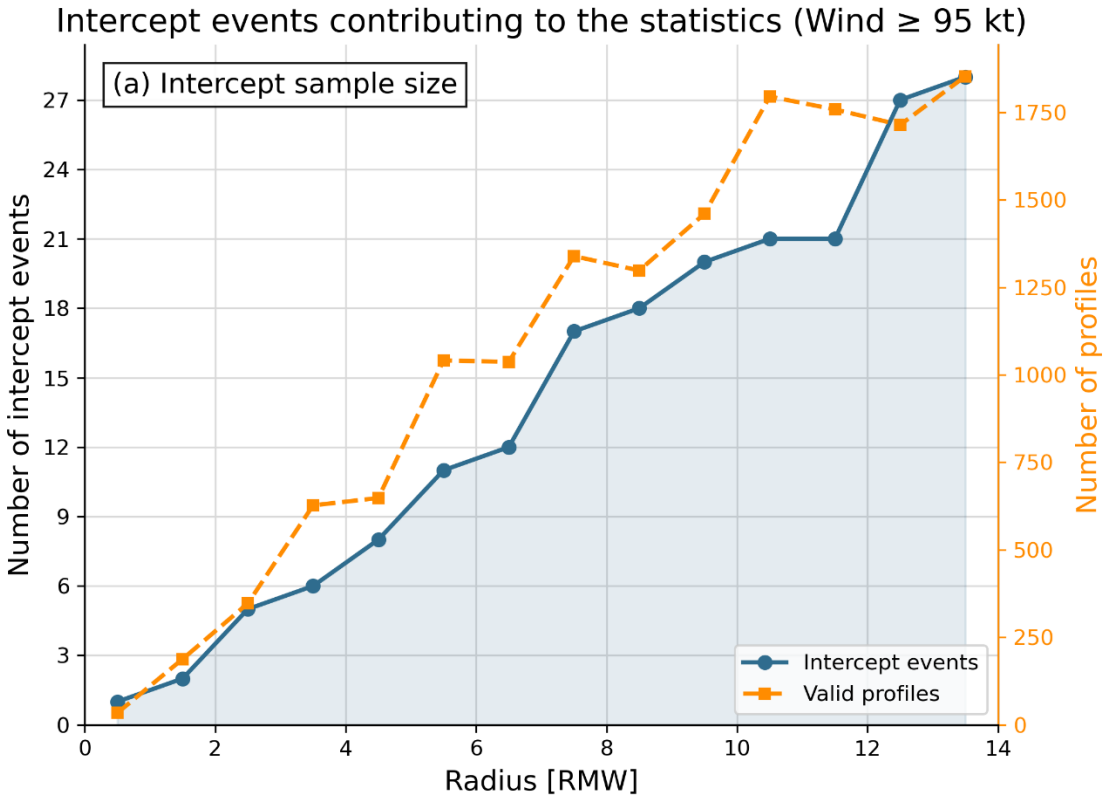


Figure S2. Number of independent EarthCARE intercept events (blue) and valid profiles (orange) contributing to each normalized radial bin for Category 3 and stronger TCs.

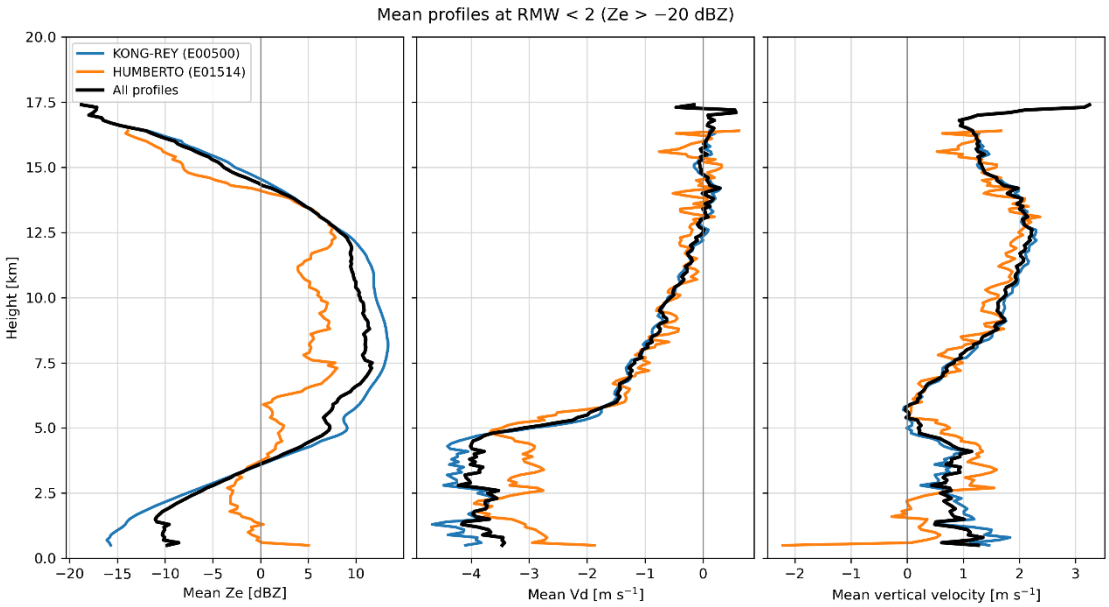


Figure S3. Mean vertical profiles of radar reflectivity (left), Doppler velocity (middle), and vertical air velocity (right) for the two independent EarthCARE intercept events contributing to the inner-core region (R < 2 RMW). The black lines show the profiles calculated using all inner-core samples.

Specific/Editorial Comments

- **L40:** "suggesting that rapid intensification may be a precursor to TC intensification" — this phrasing is circular/redundant and should be rephrased.

  We thank the reviewer for pointing this out. We have revised the sentence to: 'Wu et al. (2020) then showed that the intensification rate is correlated with IWC in rapidly intensifying TCs, suggesting that enhanced IWC may be a precursor to rapid intensification.' in Line 41-42.

- **L63:** Is TC rapid intensification really absent in coarser-resolution models? Please provide a citation to support this statement.

  We agree that the original statement was too strong. We have revised the sentence to avoid implying that rapid intensification is entirely absent in coarser-resolution models: 'Baker et al. (2024) further demonstrated that GSRMs can simulate rapid intensification, which is hard to be represented in low-resolution conventional global climate models.' in Line 65-66.

- **L68:** "in Roh et al. (2026) they argued" should be revised to "Row et al. (2026) also argued."

  We thank the reviewer for the correction. We have revised the citation as Roh et al. (2025).

- **L210:** "Between 5–10 km, a mixture of ice and liquid phases exists"

  We thank the reviewer for this suggestion. During revision, we reconsidered this interpretation and removed the statement from this section.

- **L254:** "by particles falling" should replace "the particle falling."

  We have revised this part of the manuscript and no longer use the original wording.

- **L455:** "although uncertainties remain in the retrieval" — can the authors quantify these uncertainties, even approximately?

  We thank the reviewer for raising this point. Quantifying the retrieval uncertainty with a single representative value is difficult because the uncertainty depends strongly on cloud regime, particle properties, attenuation, and the coexistence of multiple hydrometeor populations. In particular, the retrieval of terminal velocity and vertical air motion is more uncertain in mixed-phase and melting regions and under strong precipitation, where attenuation and multiple scattering can also affect the radar measurements. We have therefore expanded the discussion of these uncertainty sources in Section 2.1.1 and interpret the corresponding results cautiously. We also cite the JAXA retrieval studies (Sato et al., 2009, 2025) for details of the retrieval assumptions and limitations.