The authors have addressed my comments and have improved the paper substantially by clarifying the methodology and reframing the results. I recommend the publication for publication with the following minor suggestions:

- In the abstract, the acronyms K21 and T24 are used, but were not defined. The authors should clarify what these are for the reader.

We thank the reviewer for pointing this out. We have revised the Abstract to spell out the Kodama et al. (2021) and Takasuka et al. (2024) configurations directly and no longer use the K21 and T24 acronyms in the Abstract. The revised sentence reads: "Compared with the Kodama et al. (2021) configuration, the Takasuka et al. (2024) configuration more closely reproduces upper-level structure, with more gradual reflectivity and Doppler velocity changes and a broader cloud ice distribution."

- The authors should define what they mean by "regional", since the word regional is often used to refer to different regions of the globe in this field. Here, regional refers to regions of the tropical cyclone, which is not obvious until one gets mid-way into the paper.

We thank the reviewer for this suggestion. We have revised the description of the study objectives in the Introduction to explicitly define the regions considered in this study as the inner-core, rainband, and outer regions of TCs. We also changed "regional variations" to "radial variations" in the Abstract to avoid ambiguity. The revised text in the Introduction reads: "Using 16 months of observations, we examine differences among inner-core, rainband, and outer regions in radar reflectivity, Doppler velocity, retrieved air motion, and cloud particle types. We further investigate how air motion and particle sedimentation influence the Doppler velocity distributions across these regions." in Lines 76–79.

Referee Report

egusphere-2026-2530 — “EarthCARE Observations of Vertical Motion and Cloud Microphysical Structure in Tropical Cyclones” (Huang et al.)

Recommendation: minor/moderate revision

1. Assessment

This is a substantive, good-faith revision. All four of my major comments were engaged with. However, I feel that there are a few real problems left, including one that touches the paper's central new claim. I support acceptance after minor/moderate revisions.

2. Major comments

Responses to my previous comments:

1. Model improvement pathway. This comment was only partially addressed. The new closing paragraph (L573 – 582) does map observables onto constrainable quantities: reflectivity → size distributions, water/ice content → condensate mass, Doppler minus retrieved air velocity → sedimentation and terminal-velocity settings (L574–577). That is more than was there before, but it stops short of "how would one actually do this" — no mention of which coefficients in the fall-speed relation, what target metric, or what the K21→T24 contrast implies about the direction of adjustment. The more concrete deliverable is actually elsewhere: the new text at L533–535 recommends T24 as the baseline configuration for future NICAM TC simulations. I would accept that as satisfying the comment, except see the contradiction flagged below. The bin-microphysics part of my original question was not addressed.

We thank the reviewer for this helpful suggestion. We have revised the final paragraph of the Summary to provide a more concrete pathway for model improvement. The revised text now specifies that future sensitivity experiments can modify the fall-speed coefficients of individual hydrometeor species and evaluate the resulting simulations using joint PDFs of radar reflectivity and Doppler velocity. The K21–T24 comparison suggests that reducing the effective fall speeds of frozen hydrometeors may improve the simulated vertical structures of

radar reflectivity and Doppler velocity in this case. We also added double-moment and bin microphysics as future directions for examining the role of particle size distributions and additional microphysical degrees of freedom. We revised the paragraph as follows: “The comparison between K21 and T24 suggests that reducing the effective fall speeds of frozen hydrometeors may improve the vertical structures of radar reflectivity and Doppler velocity in this case. Future sensitivity experiments can therefore vary the fall-speed coefficients of individual hydrometeor species and evaluate them using joint PDFs based on radar reflectivity and Doppler velocity. Future studies will also compare NSW6 with the double-moment NDW6 scheme (Seiki and Nakajima, 2014) and explore more complex microphysics, including bin schemes, to examine the role of particle size distributions and additional microphysical degrees of freedom.” in Lines 591–598.

2. Single-moment justification. The authors' response is reasonable, but stranded in the response letter. The argument (NDW6 adds degrees of freedom that would obscure the terminal-velocity sensitivity; K21 and T24 are both in active use in the NICAM community) is sound, and Seiki and Nakajima (2014) is now cited. But the manuscript only mentions NDW6 as future work in the Summary (L580–582). A reader reaching Section 4 still has no answer. I suggest adding one sentence of justification to the NSW6 configuration paragraph in Section 2.2.2 (L206–216).

We thank the reviewer for this suggestion. We have added the justification directly to Section 2.2.2: “We focus on the single-moment NSW6 scheme to isolate the sensitivity to different settings of terminal velocity, because the double-moment NDW6 scheme introduces additional microphysical differences that would make this sensitivity more difficult to isolate.” in Lines 214–216.

3. Imprecise language — This has been addressed. The specific g m^{-3} cases are all fixed ("toward" → "beyond"/"at least"; see L513–514 and L521–525).

We thank the reviewer for confirming that this issue has been addressed.

4. Sample size. This has been well addressed, and is more revealing than expected: 101 intercepts, 28 TCs, 1,853 valid profiles (L155–157), plus Figs. S2 and S3. Two things remain unclear. First, whether the 101/28 figures refer to all intercepts or only the Category 3+ subset used for compositing (L150–151) — I assume the

former, but it should be stated. Second, the Category 3 threshold is now stated only qualitatively at L150–151, with no wind value given, while Fig. S2's title specifies ≥ 95 kt; Category 3 on the Saffir–Simpson scale is ≥ 96 kt. Please state the threshold explicitly in the text and make it consistent with the Supplement.

We thank the reviewer for pointing out these inconsistencies. We have clarified that the reported sample numbers refer to the Category 3 or stronger TCs used in the radial composite analysis, and we now explicitly define the threshold as a maximum sustained wind speed of at least 96 kt. During this correction, we found that the previous analysis code had incorrectly used 95 kt as the Category 3 threshold. After correcting the threshold to 96 kt, the revised dataset includes 96 EarthCARE intercept events from 27 TCs in the Northern Hemisphere.

We also identified an error in the reported total number of valid profiles. The previous value of 1,853 corresponded only to the final radial bin because the profile counts were not accumulated across radial bins when the total number was printed. This error affected only the reported total sample count and did not affect the radial composites, PDFs, or other subsequent analyses, which used the correct profiles within each radial bin. The corrected total is 15,413 valid profiles within 15 RMW. We updated Figs. 2–7 and the Supplement using the corrected 96-kt threshold. The removed cases are mainly located in the outer region ($R > 5$ RMW), while the numbers of valid profiles and independent intercept events in the inner-core and rainband regions remain unchanged. The revised figures show no noticeable changes in the distributions, and the main conclusions remain unchanged.

The revised methodology now states: “The statistical composites in this study are restricted to strong TCs of Category 3 intensity or higher (maximum sustained wind ≥ 96 kt).” in Lines 156-157 and: “After restricting the dataset to Category 3 or stronger TCs, the radial composite analysis includes 96 EarthCARE intercept events from 27 TCs in the Northern Hemisphere, providing 15,413 valid profiles within 15 RMW.” in Lines 161-162. The corresponding sample counts and

threshold have also been updated in the Supplement.

Remaining concerns

1. The central inference of the paper may be partly circular, and the uncertainty question now matters more than it did. The new headline result is that the rainband's negative Doppler shift relative to the outer region reflects sedimentation rather than air motion (L324–326, L343–346, L352–354). The support is that V_tz is slightly more negative in the rainband, "supported" by the retrieved particle terminal velocity also being more negative (L344–346). But V_air is retrieved by estimating and removing terminal velocity from V_D (L110–111), and V_tz is then obtained as V_D − V_air (L118–119), so V_tz is not independent of the retrieval's own terminal-velocity estimate. Using one to corroborate the other is close to circular. The authors acknowledge this non-independence in the Summary (L577–579) but do not apply the caveat at L343–346, where it does the work. I ask that the caveat be repeated at the point of inference.

Related: the differences being interpreted are described as "slight" three times in the same paragraph, and the interquartile bands in Fig. 5b and 5c overlap substantially. With no stated uncertainty magnitude anywhere in Section 2.1.1 (L124–128) — even an order-of-magnitude figure from Sato et al. (2009) — a reader cannot judge whether these differences exceed retrieval error. This is now the strongest argument for my earlier comment asking that the retrieval uncertainty be quantified, and I press it again.

We thank the reviewer for highlighting this important issue. We agree that the retrieved particle terminal velocity and the radar reflectivity-weighted terminal velocity are not fully independent because both depend on the same Doppler observations and retrieval assumptions. We have therefore removed the previous wording in which the retrieved terminal velocity was used as independent support for the interpretation of V_tz, and we now explicitly state this non-independence at the point of inference in the discussion of Fig. 5: “Because the retrieved and radar reflectivity-weighted terminal velocities are not fully independent, their differences should be interpreted as being consistent with slightly faster particle

sedimentation rather than as independent evidence. The differences between rainband and outer regions are small and are comparable to the magnitude of retrieval differences reported in previous studies, so these differences should be interpreted qualitatively." in Lines 354-358.

We have also added quantitative context for the retrieval uncertainty in Section 2.1.1. Sato et al. (2009) reported vertical-air-velocity differences of approximately 0.1 m s^{-1} for a similar 95-GHz Doppler retrieval under ice-cloud conditions. More recently, Aoki et al. (2026) found differences of approximately 0.3 m s^{-1} between the peaks of EarthCARE CPR_CLP and CPR-DPR-based air-velocity distributions in snow layers, while the two methods showed comparable distributions in rain layers, including convective precipitation. We emphasize in the revised manuscript that these values provide an approximate scale of retrieval differences rather than a formal uncertainty estimate, because the errors depend strongly on particle properties and precipitation conditions. Accordingly, we now interpret the small rainband–outer differences qualitatively rather than as quantitatively significant differences.

2. Section 4 (L443–444) and the Summary (L570–571) both insist the comparison is a preliminary single case that "should not be interpreted as a general evaluation of NICAM performance," yet L533–535 recommends T24 as the baseline configuration for all future NICAM simulations of TCs. A community-wide baseline recommendation drawn from one storm is exactly the generalization the surrounding text cautions against. Please rectify this contradiction — softening L533–535 to something like "T24 appears the more promising configuration for further testing" would suffice.

We agree with the reviewer that the previous recommendation was too strong for a single-case comparison. We have therefore revised the conclusion in Section 4 to avoid making a general recommendation for all NICAM TC simulations: "Based on this single-case comparison, T24 appears to be a more promising configuration for further testing in NICAM TC simulations, although T24 still overestimates radar reflectivity near the freezing level and cloud ice content." in Lines 544-546. This wording is now consistent with our statement that the Humberto comparison

should be interpreted as a preliminary case study rather than a general evaluation of NICAM performance.