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.
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).
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).
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.

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.

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.