Response to the reviewers (EGUSphere-2026-1257)

Development of the TCWA2 Bulk Cloud Microphysics Scheme and Its Integration with a Dual-Polarization Radar Operator for Forecasting Applications

by Tzu-Chin Tsai, Jen-Ping Chen, Zhiquan Liu, Siou-Ying Jiang, Rong Kong, Ying-Jhang Wu, Junmei Ban, Ling-Feng Hsiao, Yu-Shuang Tang, Pao-Liang Chang, and Jing-Shan Hong

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Reviewer 1

Overall Evaluation

This paper describes the development and evaluation of the new TCWA2 bulk microphysics scheme (BMS), which is based on the more detailed 3-moment NTU scheme. The new scheme is described, along with descriptions of how aspects of TCWA2 are "parameterized" based on NTU. The scheme also has a dual-polarization radar simulator component. The scheme and the forward model (radar operator) are described. Detailed illustrations of how the scheme works are shown through idealized 2D WRF simulations of a squall line. Finally, a real case convective system over Taiwan is performed using the MPAS model with comparisons to observations (precipitation and dual-pol radar) and to the two existing BMPs in MPAS.

Overall this is a solid paper. It is clearly written and presented and scientifically sound. The authors do a nice job in illustrating how the new scheme works in the simple idealized 2D framework before proceeding to show its behavior for a real-case 3D simulation. I do not have any fundamental problems or major comments, just a few minor comments. I will recommend minor revisions, but these should be straightforward.

We are grateful to the reviewer for carefully reading our manuscript and for the helpful and encouraging comments. Our point-by-point responses are provided below in blue, together with the corresponding revisions made in the manuscript.

SPECIFIC COMMENTS:

1. The Abstract could probably be broken up into two paragraphs, the second starting at "The intrinsic behavior…". WRF and MPAS should probably be defined in the abstract.

Thanks for this helpful suggestion. We have divided the abstract into two paragraphs, with the second paragraph beginning with the intrinsic behavior of TCWA2. The WRF and MPAS have also been defined in the abstract.

2. Line 120: Is there are reference for the MPAS model?

Thanks for pointing this out. We have added the following references for the WRF and MPAS models.

Skamarock, W. C., J. B. Klemp, J. Dudhia, D. O. Gill, Z. Liu, J. Berner, W. Wang, J. G. Powers, M. G. Duda, D. M. Barker, and X.-Y. Huang, 2019: A Description of the Advanced Research WRF Version 4. *NCAR Tech. Note NCAR/TN-556+STR*, 145 pp

Skamarock, W. C., Klemp, J. B., Duda, M. G., Fowler, L. D., Park, S. H., and Ringler, T. D., 2012: A multiscale nonhydrostatic atmospheric model using centroidal Voronoi tesselations and C-grid staggering. *Mon. Wea. Rev.*, **140**(9), 3090–3105.

Skamarock, W. C., Duda, M. G., Ha, S., and Park, S. H., 2018: Limited-area atmospheric modeling using an unstructured mesh. *Mon. Wea. Rev.*, **146**(10), 3445–3460.

3. Line 227 "predicting the second moment (M2)". This is potentially confusing since the intended meaning, I think, is that M2 would be predicted for a 3-moment scheme (i.e. "second moment" suggested 2-moment scheme).

We agree that the original wording may be confusing. In the 3-moment NTU scheme, M2 is prognosed as an additional higher-order moment. We have replaced "the second moment" with "the second-order moment" to avoid the misleading implication that it refers to a 2-moment scheme.

4. Line 234-235 "Thus, traditional BMPs usually treat ice particles as spheres with fixed, prescribed densities." This is legally true as worded (with "usually") but misleading to the point of being incorrect – since after 2005 several BMPs started using m-D relationships for "snow" with an exponent of 2, not 3; thus not treated as spheres and not with constant densities. This should be acknowledged.

We thank the reviewer for this important clarification. We have added a sentence noting that "several BMPs since 2005 (e.g., Milbrandt and Yau 2005b; Thompson et al. 2008) have incorporated specific mass-dimension relations with exponents smaller than 3 to represent nonspherical snow structures".

5. Table 1: The values of the coefficients appear to have way too many significant digits.

We agree that the coefficients in Table 1 were reported with too many significant digits. We have therefore rounded them to an appropriate number of significant digits for presentation. The full-precision coefficients are retained in the model source code used for the simulations.

6. Line 402: Which idealized case is this? Please either show the initial sounding or add a reference.

Thanks for this suggestion. We have added references to Weisman and Klemp (1982, 1984) for this idealized squall-line case.

Weisman, M. L., and J. B. Klemp, 1982: The dependence of numerically simulated convective storms on vertical wind shear and buoyancy. *Mon. Wea. Rev*., **110**, 504–520.

——, and ——, 1984: The structure and classification of numerically simulated convective storms in directionally varying wind shears. *Mon. Wea. Rev.*, **112**, 2479–2498.

7. For the vertical cross-section plots (Figs. 3-7), you could consider plotting the altitude axis labels for only the left-most panels. This would allow you to widen each of the panels. I also think panel labels [a), b), etc.] should be added and appropriately referenced in the main text when referring to a specific panel.

We thank the reviewer for this useful suggestion. Figs. 3-7, 9-11, and 15 have been revised to display altitude axis labels only on the leftmost panels. Panel labels have also been added, and Sections 3.1 and 3.2 have been revised to reference the relevant panels in the discussion.

8. Section 3 gives a nice overview of how the BMP scheme and dual-pol simulator work. By definition of an idealized set-up, no "verification" is needed. However, would it be possible to provide some descriptions, and even figures, of how some of these fields look for a typical squall line? For example, I have an idea what Z_hor should look like, so my sense from Fig. 9"a" is that the general structure and values are reasonable though it appears to be missing a bright band and the values above the melting layer in the stratiform region look too high (though maybe this is just an artifact of the simple model set-up). However, I don't have a sense of what the other dual-pol "total" values should look like (e.g. Figs. 10a, 11a). A plot of these from a real squall line would be really great for highly qualitative "verification".

We agree with this thoughtful comment. The idealized experiment is intended to assess whether the TCWA2 microphysics and radar operator can produce physically interpretable polarimetric structures in a controlled convective environment, rather than to provide formal verification. To address the reviewer's concern, we provide representative cross-sectional snapshots of polarimetric structures from the real-case convective simulation in Response Fig. R1. These plots are included in the response letter as qualitative context for the simulated range of dual-polarization signatures.

All three model simulations produce intense convective cores with reflectivity exceeding 60 dBZ, while intermittent melting-layer signatures appear near 5 km altitude. Above the melting layer, WSM6 produces stronger reflectivity aloft, reaching about 45 dBZ, compared with generally weaker values of 20–30 dBZ in Thompson and TCWA2. In TCWA2, enhanced ZDR values greater than 1 dB are mainly located below 5 km, with a peak near 4 dB in the lower portion of the convective core.

Enhanced KDP values greater than 1° km−1 are concentrated primarily within the convective core, with a maximum close to 8° km−1. These spatial structures are sensitive to the selected cross-section in a complex three-dimensional case. Thus, the added figure is intended as qualitative context rather than a formal validation. A detailed comparison with radar RHI observations is left for future work.

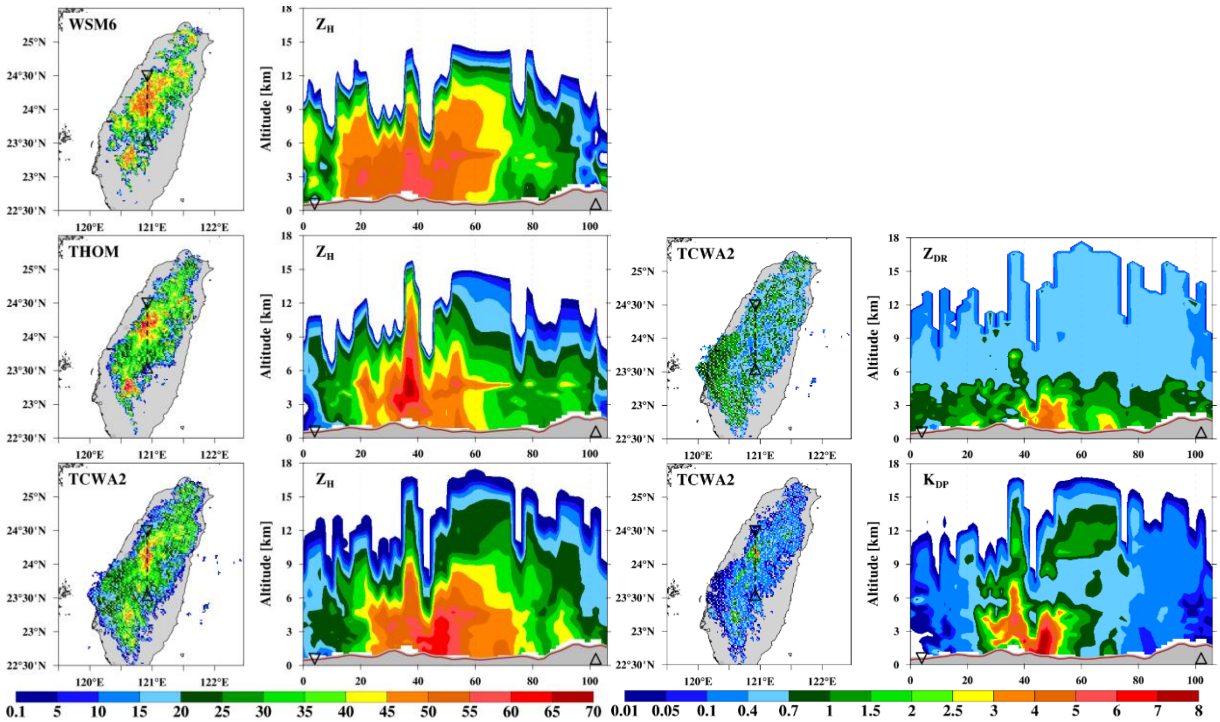


Fig. R1 Spatial distribution of horizontal radar reflectivity (dBZ) in the left six panels at 2022/6/24 08Z. Panels from top to bottom indicate the WSM6, Thompson, and TCWA2 runs. The leftmost three panels show the horizontal distribution at 5 km altitude, along with the cross-section from (24.5°N, 120.9°E) to (23.5°N, 120.9°E) shown as a black dashed line. The four right panels depict the corresponding TCWA2 simulated differential reflectivity (dB) and the specific differential phase at C-band (° km^{-1}). The brown area in the cross-section panels represents the model terrain height (km).

9. Line 651 "…with a peak of 242 mm, indicating it overestimates precipitation efficiency." In my opinion, a single point value should not be used to make this claim. My suggestion would be to take an average value over the highest 20 (?) points – or something like that – and the same for the QPESUMS observations.

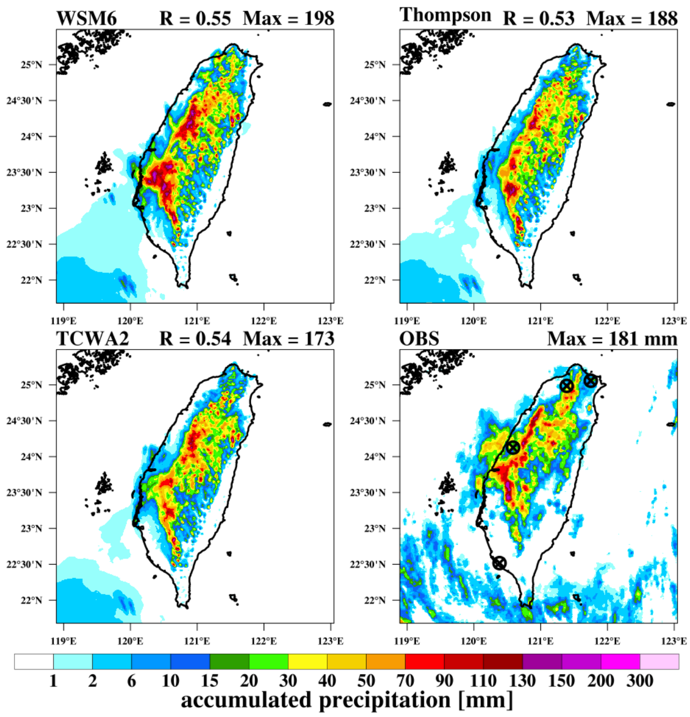
We agree that using a single grid-point maximum is insufficient to support a conclusion about precipitation efficiency. We have revised the analysis to use the mean of the highest 10 grid-point precipitation values for QPESUMS and each model simulation. The corresponding text has been updated to avoid overinterpreting a single-point maximum. Specifically, the mean of the highest 10 precipitation values is 198 mm for WSM6, 188 mm for Thompson, and 173 mm for TCWA2, compared with 181 mm for QPESUMS. The WSM6 run produces a larger area of intense rainfall exceeding 90 mm over central Taiwan, indicating it overestimates the most intense precipitation amounts. Thompson and TCWA2 generate a narrower distribution of heavy rainfall, closer to the observed value, for this metric.

10. Line 751 “Figure 18 further examines…”. This type of phrase appears is several places and should be improved. A figure does not examine anything, it *illustrates* or depicts something.

Thanks for pointing this out. The phrase “Figure X examines” has been replaced by “Figure 17 illustrates” and “Figure 18 depicts” as appropriate.

11. Figure 14: I would recommend making this a larger 2 x 2 panel figure.

Figure 14 has been revised to a larger 2 x 2 panel layout, as shown below.



12. Given that there is an operational NWP direction of this work, it would be useful in section 4 to include some information about computational time of, e.g., Thompson vs. TCWA2 in MPAS. If TCWA2 is significantly more expense, I don’t think you need to worry about it at this point – the results are impressive and the proof-of-concept is clear. But people are always interested in computational cost.

We agree that computational cost is an important consideration for the potential operational application of TCWA2. Accordingly, we have added a brief description of the wall-clock time of the WSM6, Thompson, and TCWA2 simulations in MPAS at the end of Section 4.

"All simulations were run with MPI parallelization across 32 CPU cores. The total wall-clock times, with microphysics-component times in parentheses, were 402 (22), 511 (34), and 550 (118) s for the WSM6, Thompson, and TCWA2 simulations, respectively. Compared with Thompson, the TCWA2 microphysics component required about 3.5 times as much computational time, primarily due to revised parameterizations and additional calculations of dual-polarimetric radar variables. However, the total wall-clock time increased by only 39 seconds, or about 7.6%, in this model configuration. Thus, TCWA2 remains computationally practical because it retains a double-moment prognostic framework while diagnosing additional microphysical and radar-related properties."

Reviewer 2

Summary

The manuscript describes the development of the new TCWA2 bulk microphysics scheme, which introduces several microphysical parameterizations designed to better reproduce polarimetric radar signatures without substantially increasing the computational burden for operational weather forecasting in Taiwan. Several aspects of the development are innovative, detailed and impressive, especially the treatment of spectral shape parameters, particle shapes, density, fall speeds, and their coupling to the polarimetric radar operator. These improvements allow the model to generate realistic distributions of dual-polarization radar variables. My main concern is that the MPAS real-case simulation section is simultaneously too ambitious and too limited. It attempts to demonstrate the operational applicability of TCWA2, but the manuscript does not provide sufficient space to describe the case setup, verification methodology, physical interpretation, and comparison with other schemes in adequate detail. As a result, this section feels underdeveloped relative to the more carefully presented microphysics and idealized-simulation sections.

I therefore suggest that the authors consider removing the MPAS real-case simulation from the present manuscript and instead focus this paper on the development, formulation, and idealized evaluation of TCWA2 and its internally consistent radar operator. This should be enough to provide a very informative paper for community. A separate follow-up paper could then be devoted to real-case simulations and detailed operational evaluation. Thus, my recommendation is "major revision". Please see my detailed comments below.

We are grateful to Dr. Toshi Matsui for the careful reading of our manuscript and for the constructive and insightful comments. Our point-by-point responses are provided below in blue, along with the corresponding revisions in the manuscript.

Major Comments

1. Section 2.3: I could not find a clear description of how elevation-angle dependence is treated in the radar operator. In both radar simulations and observations, the radar elevation angle affects the apparent particle shape relative to the radar beam direction, except in circular-polarization measurements. This effect can influence simulated Zdr and Kdp. The authors should clarify whether the fitted formulas already incorporate an assumed mean elevation angle when calculating Zdr and Kdp., or whether the radar variables are computed under a fixed viewing geometry. If a fixed or simplified geometry is assumed, the authors should discuss its potential impact on comparisons with PPI radar observations at different elevation angles.

We thank the reviewer for this important comment. In the revised manuscript, we have clarified in Section 2.3 that the simulated polarimetric variables are computed using a fixed horizontal-viewing geometry at a zero-elevation angle. Therefore, elevation-angle-dependent projection effects are not represented. We also added a note that this simplification may introduce uncertainty for nonspherical ice particles when comparing simulated radar variables with observations at nonzero elevation angles. In Section 4.2, we revised the interpretation of the occasional K_{DP} overestimation near 10 km altitude. These discrepancies may partly reflect limitations in the RI parameterization, the fixed horizontal-viewing geometry, and the broadside-falling assumption for particle orientation without explicit tumbling or electric-field-induced alignment.

2. Fig. 6 (RI): The bulk densities of rimed ice (RI) do not appear to be very large near the convective core, around X = 350–380 km, where riming should be most active based on the cloud droplet distribution shown in Fig. 3. Could the authors explain why the RI bulk density remains relatively low in this region? This point is important because active riming would generally be expected to produce denser particles, and later plot (Fig. 15) TCWA2 scheme underestimates 40dBZ elevation in comparison with observation. The authors should clarify whether this behavior results from the RI density parameterization, the diagnosed riming rate, particle size effects, melting fraction, or other assumptions in the TCWA2 scheme.

We thank the reviewer for this insightful comment. The relatively low RI bulk density near the convective core in TCWA2 is likely related to the current diagnostic treatment of RI properties. Unlike the NTU scheme, which retains particle growth history through its prognostic moment, TCWA2 diagnoses graupel-like and hail-like RI properties using the same parameterization of the Schumann–Ludlam limit (SLL; Ziegler 1985). This approach helps maintain a computationally practical double-moment framework, but it does not explicitly track accumulated riming history. We have added a brief clarification to the revised manuscript and noted that the RI density treatment requires further refinement. The issue of the lower 40-dBZ echo-top height is addressed in our response to Major Comment 6.

"The relatively low ρ_g near the convective core can be partly attributed to the diagnostic treatment of RI density in TCWA2. The SLL parameterization provides a practical approach, but it may require further refinement as more observational constraints become available."

(If you consider removing Section 4: disregard below major comments)

3. Section 4. MPAS real-case simulations: This section requires substantially more detail to meet the standard expected for a case-study manuscript. Additional information is needed on 1) the synoptic/thermodynamic environment, 2) the temporal evolution of the storm system, such as horizontal composite reflectivity and echo-top height, 3) convective–stratiform separation for detailed analyses, 4) the relationship between simulated hydrometeor distributions and the corresponding polarimetric radar signatures upon additional case study (organized versus isolated deep convection). However, the manuscript is already quite long, and incorporating these details would likely make it even longer and less focused. In my view, the model formulation, parameterization updates, and idealized simulations are sufficient to support publication if presented as the central contribution of the paper. Otherwise, the real-case simulation section would require substantial expansion and improvement.

We appreciate the reviewer's careful assessment of Section 4's scope. We agree that the original version of the real-case section could be interpreted as attempting to provide full operational or case-study validation, which would require additional analysis. We have clarified this in the revised manuscript by revising the title of Section 4 and adding a statement as shown below. We respectfully prefer to retain the real-case section because it demonstrates that the TCWA2 scheme and radar operator can be applied in a realistic three-dimensional MPAS simulation and can produce polarimetric structures comparable to observations. However, we agree with the reviewer that a full operational evaluation requires a separate study, which we leave for future work.

"**4. Proof-of-concept real-case demonstration using MPAS**

The purpose of this section is not to provide a comprehensive operational validation or scheme intercomparison, but rather to demonstrate that the TCWA2 framework can be stably integrated into a three-dimensional model and produce physically interpretable polarimetric signatures."

Without this section, the manuscript would not demonstrate that the internally consistent microphysics–radar framework can be applied in a realistic 3D forecasting environment.

4. Also, is the use of MPAS with this variable-resolution mesh configuration intended to support future operational applications? If not, the rationale for this configuration should be clarified, because the setup appears to allocate substantial computational resources to a broad domain while reducing the finest model resolution from 1 km in the idealized WRF simulation to 2 km in the MPAS real-case simulation. This resolution difference may affect the representation of convective dynamics and, consequently, the simulated microphysical behavior and polarimetric radar signatures. The authors should explain why this mesh configuration was chosen and whether the coarser 2-km grid spacing is sufficient for evaluating TCWA2 in convective cores.

We thank the reviewer for raising this point. This MPAS variable-resolution mesh configuration is closely related to the CWA operational forecasting application and covers the operational radar network over Taiwan, as introduced in Section 4. Therefore, it was selected to test TCWA2 in a realistic three-dimensional MPAS framework relevant to future operational applications. This setup allows TCWA2 to be evaluated in a realistic mesoscale environment while maintaining enhanced resolution over Taiwan. We agree that the 2-km grid spacing is coarser than the idealized 1-km WRF simulation and may affect convective-core updrafts, hydrometeor lofting, and the simulated polarimetric signatures. We have added the following limitation to the revised manuscript.

"The 2-km grid spacing is coarser than the 1-km idealized WRF simulation and may underresolve convective-core updrafts and hydrometeor lofting. Thus, this real-case convection-permitting simulation is suitable for evaluating overall precipitation organization and qualitative polarimetric structures, but it should not be interpreted as a complete assessment of TCWA2 behavior within fully resolved convective cores."

5. Figs 15, 16, 17: A CFAD is defined as a normalized frequency distribution at each height level; that is, the integral or sum of the normalized histogram should be unity at each altitude. Please revisit the original definition in Yuter and Houze. Under this definition, the CFADs of Z, Zdr, and Kdp should be presented in that form, as in Matsui et al. (2023, Fig. 7c), for example.The figures currently shown in this manuscript appear to be joint height–variable histograms rather than CFADs. I therefore suggest either revising the terminology to describe them as "joint height–Z," "joint height–ZDR," and "joint height–KDP" histograms, or modifying all related plots so that they correctly represent CFADs. This applies especially to Figs. 15, 16, and 17.

We thank the reviewer for this important correction. The frequency distributions in the original plots were normalized over the full height–variable domain, such that the sum over all height–variable bins equals 100%. We agree that the original figures are therefore more accurately described as joint height–variable probability density distributions rather than strict CFADs. To provide a direct reference using the standard CFAD definition, we have added height-normalized CFADs in Response Fig. R2. These CFADs support the same qualitative conclusions. The lower vertical extent of echoes exceeding 30 dBZ above 9 km is further discussed in our response to Major Comment 6. To maintain a consistent statistical approach throughout Section 4, we have revised the terminology in the manuscript and now refer to these diagnostics as joint height–D_{BZ}, joint height–Z_{DR}, and joint height–K_{DP} probability density distributions, as shown in Figs. 15–19 and the associated discussion.

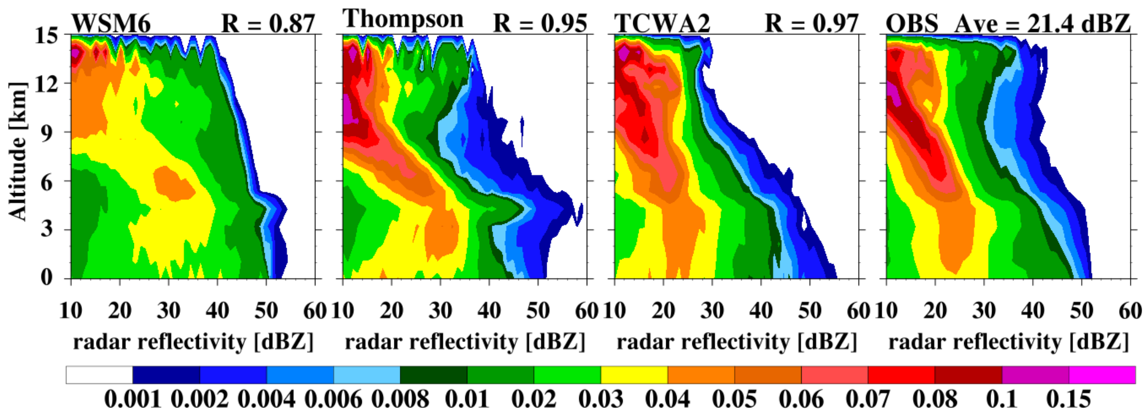


Fig. R2 Contoured frequency by altitude diagrams (CFADs) of horizontal radar reflectivity (dBZ) for simulations using the WSM6, Thompson, and TCWA2 microphysics schemes, compared with observations.

6. In addition, separating convective-core and stratiform regimes would clarify the underlying microphysical processes much more effectively than using lumped CFADs or joint height–variable histograms (again see Fig. 7 of Matsui et al. 2023). For example, in line 658, the authors refer to "a clear reflectivity core centered around 2–10 km altitude." However, this signal is likely dominated by the much larger spatial coverage of stratiform precipitation, rather than by the convective core itself. In convective cores, the maximum reflectivity typically appears near the high-reflectivity envelope at each altitude. In the manuscript, 40 dBZ echoes appear to extend above 9 km in the WSM6 and Thompson simulations, whereas they remain below about 7 km in the TCWA2 simulation. This may indicate that TCWA2 underestimates the size, density, or concentration of rimed particles in convective cores. The authors should examine whether this behavior is related to the PSD parameterization, rimed-ice density treatment, or fall-speed formulation.

Matsui, T., D. B. Wolff, S. Lang, K. Mohr, M. Zhang, S. Xie, S. Tang, S. M. Saleeby, D. J. Posselt, S. A. Braun, J.-D. Chern, B. Dolan, J. L. Pippitt, and A. M. Loftus, (2023), Systematic validation of ensemble cloud-process simulations using polarimetric radar observations and simulator over the NASA Wallops Flight Facility. *Journal of Geophysical Research: Atmospheres*, 128, e2022JD038134. https://doi.org/10.1029/2022JD038134

Thanks for this helpful comment. We find that the term "reflectivity core" in the original manuscript was misleading. Our intention was to describe the frequent occurrence of reflectivity values in the joint height–reflectivity distribution, rather than a statistically defined convective core. We have therefore revised the wording in the manuscript to avoid this ambiguity. To address the reviewer's concern, we conducted an additional convective–stratiform separation analysis using the method of Steiner et al. (1995), as shown in Response Fig. R3. The convective-region CFADs indicate that TCWA2 produces fewer reflectivity values exceeding 30 dBZ above 9 km than the other two simulations. This suggests that TCWA2 may underrepresent moderate-to-high reflectivity in the upper convective region. This behavior may be partly related to the current rimed-ice initiation treatment, in which newly formed RI is immediately converted from the riming mass of PI and SA using a mean-size approximation. Although this approach avoids introducing an arbitrary size threshold, it may underrepresent the contribution of the large-particle tail of the PSD. We have added a statement in the revised manuscript noting that the lower occurrence of moderate reflectivity at upper levels may be related to the current RI initiation and warrants further examination in future work.

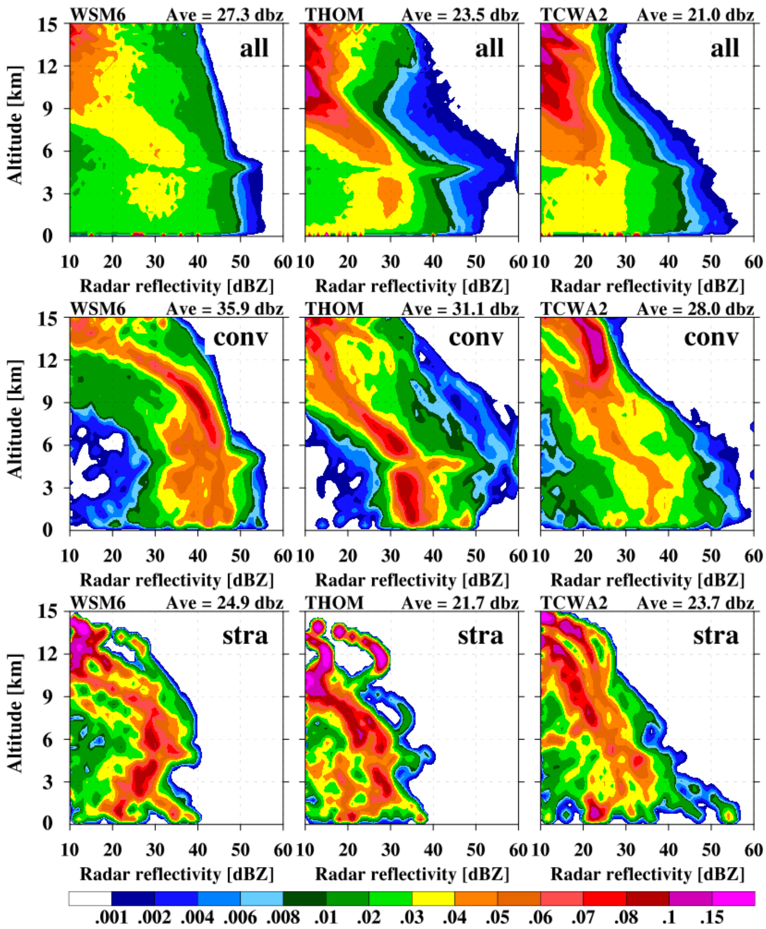


Fig. R3 Contoured frequency by altitude diagrams (CFADs) of horizontal radar reflectivity (dBZ) in terms of whole (upper), convection (middle), and stratiform (bottom) grid points for simulations using the WSM6, Thompson, and TCWA2 microphysics schemes.

7. Finally, Fig. 15 shows clear melting-layer signatures in both the WSM6 and Thompson simulations, even though these schemes do not explicitly predict mixed-phase particles. The authors should clarify the assumptions used in the polarimetric radar simulator for these schemes. Were the PSD, particle density, and hydrometeor phase treated consistently with each microphysics scheme, or were additional melting assumptions applied within the radar operator?

Thanks for raising this point. The PSD assumptions used in the radar calculation are applied consistently with the available hydrometeor categories and prognostic variables of each scheme. However, for WSM6 and Thompson, ice particle densities are fixed, and the treatment of dielectric properties during melting is simplified in the radar calculation. In contrast, TCWA2 provides diagnostically varying density for partially melted ice, together with the corresponding dielectric constants. Therefore, the more pronounced bright band signatures in the Thompson run may partly result from the combined effects of the fixed ice-particle density assumptions in the microphysics schemes and the simplified treatment of dielectric properties during melting in the radar operator. We have added a short note in the revised manuscript to clarify this point. However, a detailed intercomparison of radar-operator assumptions across schemes is beyond the scope of this study, and the WSM6 and Thompson results are interpreted primarily as reference simulations.

"The pronounced bright band signatures may partly result from the combined effects of the fixed ice-particle density assumptions in the microphysics schemes and the simplified treatment of dielectric properties during melting in the radar operator."

8. Fig 16: In convective cores, strong turbulence generally promotes particle tumbling and random orientation, which tends to reduce Zdr and Kdp toward near-zero values, particularly for ice particles within convective cores. Exceptions may occur under strong electric fields associated with lightning activity, which can preferentially orient ice crystals and produce anomalous polarimetric signatures, including negative Zdr. Therefore, the interpretation in lines 697–699 should be reconsidered. The reduced or near-zero Zdr and Kdp signals in the convective core may not necessarily indicate improved microphysical representation; they may simply reflect assumptions about particle orientation or tumbling in the radar simulator. The authors should clarify how particle canting, tumbling, and possible electric-field effects are treated before attributing these signatures to improvements in the microphysics scheme.

We thank the reviewer for this insightful comment. We agree that Z_{DR} and K_{DP} in convective cores are controlled not only by microphysical properties but also by assumptions regarding particle orientation, canting-angle distribution, tumbling, and possibly electric-field effects. In the revised manuscript, we have added a description of the particle-orientation assumptions used in the TCWA2 radar operator in Section 2.3, and revised the interpretation of occasional K_{DP} overestimation near 10 km altitude in Section 4.2 to avoid attributing this signal solely to microphysical processes.

"In TCWA2, hydrometeors are assumed to have a fixed equilibrium orientation, with no explicit canting-angle variability, tumbling, or electric-field-induced alignment. Nonspherical particles fall with their maximum dimensions aligned horizontally, corresponding to an orientation angle of 90° measured from the vertical to the particle's major axis. This broadside-falling assumption is consistent with microphysical calculations of collision cross section and fall speed, but it may enhance the simulated polarimetric response because orientation randomization is not represented."

“TCWA2 occasionally overestimates K_{DP} near 10 km altitude, which might partly reflect limitations in the microphysical parameterizations of RI, the fixed horizontal-viewing geometry, and the broadside-falling assumption for particle orientation without explicit tumbling or electric-field-induced alignment, highlighting the need for further refinement.”

Minor Comments

1. Line 41: Define WRF.

We have defined WRF (Weather Research and Forecasting).

2. Line 44: Define MPAS.

We have defined MPAS (Model for Prediction Across Scales).

3. Line 44: “radar data” should be “radar measurements (or observation)”

We have reworded "radar data" to "radar measurements" as appropriate.

4. Line 48: “computationally efficient” Is it possible to provide any quantitative information?

Yes, we have added a brief description of the computational cost of the WSM6, Thompson, and TCWA2 MPAS simulations at the end of Section 4. We also provide additional timing information for the WRF serial idealized simulations: TCWA2 required 3352 s, compared with 8905 s for the NTU scheme under the same configuration. Thus, TCWA2 used approximately 38% of the NTU computational time, corresponding to a reduction of about 62%.

5. Line 66-67: Probably you should add “Rutledge and Hobbs 198X” paper here.

We have added the paper of Rutledge and Hobbs (1983) as suggested.

6. Line 69: “like” should be “such as”

Thanks, we modified the word as suggested.

7. Line 76: “microphysical properties (Fan et al. 2017)” Please discuss/cite uncertainties of cloud dynamics/turbulence, too. For example, most of operational km-scale storm-resolving NWP still cannot resolve cumulus thermals, which are the main building block of convective microphysics (e.g., Hernandez-Deckers et al. 2021).

Hernandez-Deckers, D., T. Matsui, and A. M. Fridlind (2021), Updraft dynamics and microphysics: on the added value of the cumulus thermal reference frame in simulations of aerosol-deep convection interactions, Atmos. Chem. Phys., 22, 711–724, https://doi.org/10.5194/acp-22-711-2022, 2022.

Thanks for this useful suggestion. We have added a sentence noting that uncertainties also arise from cloud dynamics, turbulence, and unresolved cumulus thermals in km-scale models (Hernandez-Deckers et al. 2022).

8. Line 96-98: What are "external assumptions" or "empirical lookup tables to fill in missing variables"?

We agree that the original wording was unclear. We have added a sentence to clarify that "external assumptions" refers to those not directly constrained by the microphysics scheme itself.

9. Line 146: "dual-pol" should be "dual-polarimetric"

Thanks for the correction. We have revised all instances of "dual-pol" to "dual-polarimetric" or "dual-polarization" in the manuscript.

10. Line 315: After the sentence, also discuss that most (nearly all) dual-polarimetric radar simulators must parameterize particle shape and tumbling rates, which significantly changes the simulated polarimetric signals." Please check the results from Matsui et al. (2019, Table1) that compare three major assumptions.

Matsui, T., Dolan, B., Rutledge, S. A., Tao, W.-K., Iguchi, T., Barnum, J., & Lang, S. E. (2019). POLARRIS: A POLArimetric Radar Retrieval and Instrument Simulator. *Journal of Geophysical Research: Atmospheres*, 124. https://doi.org/10.1029/2018JD028317

Thanks for this useful comment. We have cited Matsui et al. (2019) in Section 2.3 to illustrate the strong sensitivity of simulated polarimetric observables to assumptions of particle shape, orientation, and tumbling. We have also clarified the particle orientation and tumbling assumed in the TCWA2 radar operator.

11. Line 333: Need reference for this sentence.

We have added the reference of Jung et al. (2008a).

12. Line 382-384: I do not see a clear description of how these formulations account for the mean particle orientation angle or the degree of tumbling. Are these parameters assumed to be constant regardless of precipitation regime, such as convective core versus stratiform regions? If so, this assumption should be clearly stated and its potential impact on simulated Zdr and Kdp should be discussed.

We have added a paragraph in Section 2.3 to illustrate the particle orientation and tumbling assumed in the TCWA2 radar operator, and their potential influence on the simulated polarimetric response.

"In TCWA2, hydrometeors are assumed to have a fixed equilibrium orientation, with no explicit canting-angle variability, tumbling, or electric-field-induced alignment. Nonspherical particles fall with their maximum dimensions aligned horizontally, corresponding to an orientation angle of 90° measured from the vertical to the particle's major axis. This broadside-falling assumption is consistent with microphysical calculations of collision cross-section and fall speed, but it may enhance the simulated polarimetric response because orientation randomization is not represented."

13. Fig. 10, 11, 12: What is the elevation angle of radar beam to calculate Zdr and Kdp?

The current radar operator does not use radar elevation angle as an input. The simulated polarimetric variables are computed under a fixed horizontal-viewing geometry at a zero-elevation angle. Therefore, elevation-angle-dependent projection effects are not represented.

14. Line 623: Does MPAS require lateral boundary conditions in this configuration? Since MPAS is a global model, only initial conditions should be required.

It depends. MPAS is primarily a global model, and only initial conditions are required for a global simulation. Yet, MPAS-Atmosphere has supported limited-area regional simulations for dynamical downscaling since version 7.0. In this regional framework, lateral boundary conditions must be generated from an external driving model, such as GFS.

15. Line 629: What are the elevation angles for PPI?

The PPI scans include 15 elevation angles at the RCWF radar site: 0.5°, 0.9°, 1.3°, 1.8°, 2.4°, 3.1°, 4.0°, 5.1°, 6.4°, 8.0°, 10.0°, 12.0°, 14.0°, 16.7°, and 19.5°. The RCSL radar site includes 11 elevation angles: 1.0°, 2.0°, 3.0°, 4.0°, 5.0°, 6.0°, 9.9°, 14.6°, 19.5°, 24.5°, and 29.9°. The RCNT and RCLY radar sites include 11 elevation angles: 0.5°, 1.4°, 2.4°, 3.4°, 4.3°, 6.0°, 9.9°, 14.6°, 19.5°, 24.5°, and 29.9°.

16. Line 726-727: Again, these signatures are not controlled by microphysics alone. They are also strongly influenced by assumptions about particle tumbling and mean orientation angle, which can substantially alter simulated polarimetric variables such as Zdr and Kdp. These assumptions should be clearly described and considered before attributing differences solely to microphysical processes.

We thank the reviewer again for this comment. We have added a description of the particle-orientation assumptions used in the TCWA2 radar operator in Section 2.3 and revised the interpretation of occasional KDP overestimation near 10 km altitude in Section 4.2 to avoid attributing this signal solely to microphysical processes. In addition, we have softened the related claims and added a statement in the Summary clarifying that the remaining discrepancies in the polarimetric variables may partly reflect uncertainties associated with ice-phase parameterizations, particle-orientation assumptions, and the fixed viewing geometry.

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