Reviewer2

General comments. This paper describes a study that incorporates the generalized double moment normalization (GDMN) method for rain DSDs into the WDM6 bulk microphysics scheme. This version of WDM6 is compared to the original version for a case of deep convection over the Korean peninsula. It is shown that there are substantial differences in the pattern of rainfall, reflectivity CFADs, and thermodynamic structure between GDMN and the standard WDM6. The authors attribute these differences mainly to increased evaporation (and presumably stronger cold pool) that shifts the precipitation pattern with heavier precipitation to the southwest compared to the original WDM6. This pattern is more consistent with observations.

Overall, the paper is generally well written and presents an important advance by incorporating the GDMN method into a microphysics scheme. I do have several comments and questions as detailed below. One of my main concerns is that the results are based on only a single case study over a fairly small region. While this is useful to show proof-of- concept and the basic utility of the approach, this seems too narrow for drawing broader conclusions about the performance of the new approach compared to existing schemes. I also have some questions about the how GDMN was implemented and the analysis. Several minor comments and suggestions are listed below the major comments. Overall, my recommendation is major revision before the paper should be further considered for publication.

: We appreciate the reviewer's thoughtful and constructive comments. In response, we additionally conducted a one-month-long simulation during the summer season and included more detailed technical descriptions of the DSD retrieval method in the revised manuscript. In addition, we performed a sensitivity experiment in which the latent cooling effect associated with rain evaporation was removed to better explain the linkage between process-rate differences and convective evolution in the two simulations. Detailed responses are provided under each comment below.

Major comments.

1. As mentioned above, the authors draw sweeping conclusions about the relative performance of the schemes based on a single case study over a fairly small region (e.g., in the abstract, and on lines 427-432 in the conclusions section). This is a case of deep convective precipitation, with inherently low predictability, and therefore I question the broader conclusions on scheme performance based on this single case. For a paper in GMD, I think it's reasonable to focus on only one case as a proof-of-concept of the approach, but the caveats that these results are based on only a single case study need to be emphasized in the paper. The authors briefly acknowledge this on line 439 in the conclusions section. However, I think this point should be emphasized much more strongly, and it should be made clear that readers should not draw broader conclusions based on a single case.

: We thank the reviewer for this important comment. The main objective of this study is to demonstrate the implementation of GDMN in a bulk microphysics scheme and to examine its potential impacts on simulated precipitation and convective evolution using a single convective case that is not adequately reproduced by widely used bulk microphysics schemes. Nevertheless, we agree with the reviewer that additional cases under diverse meteorological conditions are necessary to fully evaluate the robustness and general applicability of the proposed approach. In the conclusion section, we revised the sentences from "Although this work evaluates only a single convection case, the merits of the GDMN method are evident when compared simulations using more advanced microphysics schemes, such as the P3 scheme and the bin-type schemes. Future work will extend the GDMN approach to prescribe the DSDs of other hydrometeors and to evaluate its performance across a wider range of precipitation systems."" To "We would like to note that this study evaluates only a single convective case, and the advantages of the

GDMN approach may be less pronounced for other types of convection. Therefore, our study should be regarded primarily as a proof-of-concept demonstration based on one convective case over a limited domain. Additional multi-case evaluations under diverse meteorological conditions, along with the extension of the GDMN framework to other hydrometeors, are needed in future work."

Additionally, we conducted and included a one-month-long simulation over the East Asia region during the summer season, encompassing various types of summertime convection. The simulation results, shown in Figure R1, further support the improved performance of the GDMN approach compared to the original scheme. Statistical skill scores, including Pattern Correlation (PC) and BIAS, are improved in GDMN compared to WDM6. The PC (BIAS) values are 0.52 (1.02) for GDMN and 0.47 (1.04) for WDM6.

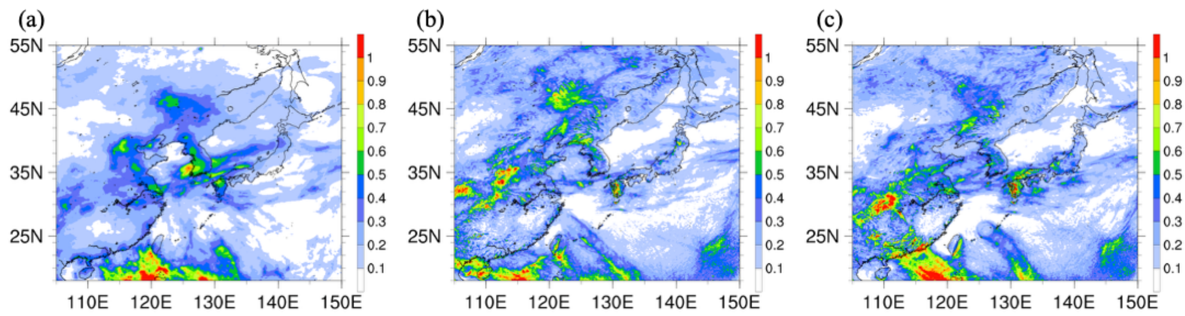

Figure R1. Spatial distribution of hourly precipitation [mm hr^{-1}] from (a) the Integrated Multi-satellite Retrievals for GPM (IMERG) observations and model simulations using (b) WDM6 and (c) GDMN for the regional climate case during July 2023.

2. I have some questions about the implementation of GDMN in WDM6, and relatedly have some questions about the comparison with the original WDM6. First, as I understand it, you use observations to constrain c and μ for the normalized DSDs in GDMN, by normalizing using the 3rd and 4th moments. The 3rd and 4th moments (which are more weighted by larger drops in the DSD than lower order moments) was presumably used because small drops are undercounted by the 2DVD. I assume you derived an equation for hGG(x) similar to eq. (7), except for normalizing by the 3rd and 4th moments rather than 0th and 3rd as shown in eq. (7). I think you then obtain c and μ from the observations valid for that equation (not eq. 7). If so, this should also be explicitly stated.

It's also not clear exactly how the best-fit c and μ were determined. Can you give more detail on how the values of c = 2.70 and μ = 0.24 were derived? Were these parameters obtained by the minimization of hmed(x) and h(x)? Similarly, how were c = 2.60 and μ = 0.29 derived for the h(x) normalized by the 0th and 3rd moments?

I have other related questions as well. On line 216 you use hmean(x) but then on line 217 you mention hmed(x). This seems like an inconsistency.

It's also not clear what the sum is over for the minimization procedure. Is this over bins in x? If so, what is the bin spacing? How many bins? Finally, it's not clear exactly what is being shown in Fig. 4. Is the spread of results shown by the normalized frequency the normalized h(x) derived from the observational points (that is, NR(D) normalized by the 0th and 3rd moments)? In the caption it says that the solid line is hmed(x). Is this obtained from NR(D) associated with the hmed(x) from the observations but normalized by the 0th and 3rd moments, following equation (12)? Is this the same, or a very close fit, as eq. (7) with the c = 2.60 and μ = 0.29? This isn't clear, because the caption mentions the solid line is hmed(x), but then c = 2.60 and μ = 0.29 are shown in the upper right part of the figure.

Finally, I suggest a small reorganization by moving the description of how c and μ are derived (lines 209-223 and Fig. 4) to earlier in section 2.3 that describes implementation of GDMN in WDM6. I think this would improve the flow of the paper

: Thank you for pointing out the lack of clarity in the manuscript. We did not directly derive the values of c and μ from the observed DSDs. Instead, the observed DSDs were first used to derive a theoretical DSD based on the third and fourth moments, and this theoretical DSD was subsequently renormalized using the zeroth and third moments. To clarify the method of normalization by than 0th and 3rd moment, we revised the original manuscript as following:

"The WDM6-GDMN allows the observed shape parameters, c and μ, to be applied to the rain DSD. To calculate c and μ from observed rain DSDs over the Korean Peninsula, data from a 2DVD are utilized. The 2DVD is widely recognized as a reference instrument for the large end of the DSD but has been shown to underestimate the concentrations of small drops (Raupach et al., 2019), which significantly affects M_0. Consequently, it is not appropriate to derive $h_{\mathrm{GG}(0,3,\mu,c)}(x)$ directly from observed DSDs by normalizing with the zeroth and third moments. Therefore, this study follows two steps to obtain $h_{\mathrm{GG}(0,3,\mu,c)}(x)$. Normalization is first performed using N_0' and D_{m}', defined with the third and fourth moments, M_3 and M_4, for the observed rainfall events (threshold: $R \geq 0.1$ mm h^{-1}) at the Boseong standard weather observatory during 2018 and 2019 (Fig. 2b). Then, $h_{\mathrm{GG}(3,4,\mu,c)}(x)$ is derived from the mean *h(x)* by minimizing $\sum_{i=1}^{i=20}(\log h_{\mathrm{mean}(3,4,\mu,c)}(x_i) - \log h_{\mathrm{GG}(3,4,\mu,c)}(x_i))^2$ (Bang et al., 2020), where $h_{\mathrm{mean}}(x)$ is the channel-wise mean of the normalized DSDs and the channel interval is 0.2. The derived parameters of $h_{\mathrm{GG}(3,4,\mu,c)}(x)$ are c=2.70 and μ=0.24. Then, the theoretical DSDs, $N_{\mathrm{R}}(D)$, are derived from $h_{GG(3,4,\mu,c)}(x)$, $N_0'(M_3, M_4)$, and $D_{\mathrm{m}}'(M_3, M_4)$:

$$N_{\mathrm{R}}(D)/(\mathrm{m}^{-4}) = N_0'(M_3, M_4)h_{\mathrm{mean}(3,4,\mu,c)}(D/D_{\mathrm{m}}'(M_3, M_4)). \tag{12}$$

These theoretical $N_{\mathrm{R}}(D)$ is then normalized by $N_0{}'(M_0, M_3)$ and $D_{\mathrm{m}}{}'(M_0, M_3)$, where M_0 and M_3 are derived from $N_{\mathrm{R}}(D)$. The normalized DSDs, $h_{(0,3,\mu,c)}(x)$, are presented as a frequency distribution in Figure 4. Finally, $h_{\mathrm{GG}(0,3,\mu,c)}(x)$ is obtained by minimizing the value of $\sum(\log h_{\mathrm{mean}(0,3,\mu,c)}(x) - \log h_{\mathrm{GG}(0,3,\mu,c)}(x))^2$. The derived shape parameters c and μ of $h_{\mathrm{GG}(0,3,\mu,c)}(x)$ are 2.60 and 0.29, respectively."

In response to reviewer's comment, we have corrected the notation "hmed(x)" to "hmean(x)" in the revised manuscript.

Additionally, the caption of Figure 4 has been changed as following:

"Figure 4: Normalized DSD derived from $N_0'(M_0, M_3)$ and $D_{\mathrm{m}}'(M_0, M_3)$, along with their $h_{\mathrm{mean}(0,3,\mu,c)}(x)$ from theoretical $N_{\mathrm{R}}(D)$ in Eq. (12) over the Boseong area. Colors indicate the normalized frequency of the normalized DSDs with M_0 and M_3 and the solid line represents $h_{\mathrm{mean}(0,3,\mu,c)}(x)$ for each channel."

We have also moved the description of the parameter derivation (previously lines 209–223) and the related Fig. 4 to the earlier part of Section 2.3, where the implementation of GDMN in WDM6 is introduced.

3. The sentence on line 209 states that GDMN allows the observed shape parameters to be incorporated into the scheme. But why couldn't the shape parameters from the standard (non-normalized) gamma DSDs in WDM6 be fit to the observed DSDs? I understand that the double moment normalization may make this easier and reduce uncertainty, but I don't see why in principle the regular (non-normalized) DSDs in WDM6 couldn't be fit to the observed data – in other words, why couldn't the shape parameters for these (non-normalized) DSDs be fit to data? My concern is that you are comparing results using GDMN with parameters fit to observations but are using tWDM6 with the standard values of c = 1 and μ = 2. This doesn't seem like a fair comparison GDMN is improved with observations but WDM6 is not. In other words, is the main impact from using GDMN itself, or the fact that you're obtaining the shape parameters from observations but for WDM6 are simply using the standard shape parameters?

: We thank the reviewer for this important and insightful comment. In the original WDM6 scheme, the rain DSD is represented using a conventional gamma distribution, in which the DSD shape is controlled by a single parameter, μ_R, defined as $\mu_R = c\mu - 1$ ($c = 1$ and $\mu = 2$). Because c is implicitly assumed to be unity in original WDM6, $\mu_R = \mu - 1$. Therefore, the observed values of c and μ cannot be directly prescribed separately within the original WDM6 formulation. As the reviewer pointed out, μ_R can be adjusted using observational data. However, the use of a single shape parameter inherently limits the flexibility of the DSD representation, while observed DSDs exhibit substantial variability that is difficult to capture without the GDMN framework.

Meanwhile, the GDMN framework reformulates the DSD in a way that explicitly retains c and μ as independent parameters, allowing observationally constrained values to be incorporated into the scheme. In addition, the normalized formulation helps reduce uncertainty associated with parameter estimation.

Importantly, when the same parameter values ($c = 1$, $\mu = 2$) are applied in both formulations, the resulting DSD representation and simulation outcomes are identical. Therefore, the differences reported in this study arise not only from the mathematical reformulation, but also from the fact that the GDMN framework enables the direct use of observation-derived shape parameters and provides a more robust representation of their variability.

4. p. 12. Are these differences between WDM6 and GDMN robust? Since this is only at two grid points (KWK and GDK), this isn't clear. Also, it's not clear what period the CFAD analysis covers. What times are shown in Fig. 7?

: The CFAD analysis covers the full analysis period from 02:00 to 17:00 UTC on 6 August 2013, using hourly model output during this period. These descriptions are already provided in Section 3.2 (201-208 lines) and in the figure caption. To improve clarity for the analysis region, we revised the sentence from "Reflectivity is examined within a 100 km radius of each radar site." to "Reflectivity is examined using all model grid points located within a 100km radius of each radar site, where observational data available."

The bright band is mentioned here – how is this represented in the model? Is there any explicit bright band parameterization for the simulated radar reflectivity? More generally, how is reflectivity calculated in the model?

: In the WRF model, the radar reflectivity (Z) for precipitating hydrometeors is calculated using the equivalent reflectivity factor, as shown in Eq. (R1), which is derived from the prognostic mixing ratios and number concentrations of grid-resolved hydrometeor species.

$$Z\,[\mathrm{m}^6\mathrm{m}^{-3}] = \int_0^{\infty} D_x^6 N_X(D_X)d(D_x) = N_{0x}\Gamma(1 + 2d_x + \mu_x)\left(\frac{1}{\lambda_x}\right)^{1+2d_X+\mu_x} \quad \text{(R1)}$$

The total equivalent reflectivity factor(Z_T) is then obtained by summing the reflectivity contributions from each hydrometeor species, as shown in Eq. (R2).

$$Z_T[dBZ] = 10\log_{10}[(Z_{Rain} + Z_{graupel} + Z_{snow}) \times 10^{18}] \quad \text{(R2)}$$

In this calculation, radar reflectivity is computed for rain and solid-phase hydrometeors (snow and graupel), which are assumed to be non-water-coated at temperatures below 0 °C. Meanwhile, at temperatures above 0 °C, the model applies a simple water coating scheme for melting solid-phase hydrometeors. In regions where the temperature exceeds 0 °C, melting particles develop a water coating on their surfaces. The formation of this water coating increases the dielectric factor and allows the effect of melting particles to be represented in the reflectivity calculation. More details are explained in Park et al. (2025) and we added the following sentences in the revised manuscript:

"The simulated radar reflectivity is calculated following the methodologies of Koch et al. (2005), Stoelinga (2005), and Park et al. (2025). The model incorporates a simple water coating method for melting solid-phase particles to represent bright band."

Also, Figure 7 does not look correct, especially the 25th, 50th and 75th percentiles for the observed radar in (a) and somewhat for (b) as well. For example, the color contours showing frequency in Fig. 7 (a) below ~4 km seem to suggest much smaller values of the 25th and 50th percentiles than show by the solid black lines (that is, the highest frequencies occur at much smaller reflectivities than suggested by these percentiles). This is important because the solid lines indicate the 25th, 50th, and 75th percentiles are nearly constant with height in (a), which is closer to WDM6 in (b) rather than GDMN which shows a decrease with decreasing height in (c). Is there a bug in the CFAD code or some other inconsistency?

: We re-examined the CFAD calculation and confirm that there is no coding error in the percentile analysis. The percentile curves were computed consistently from the full reflectivity frequency distribution at each height level. For the observed radar CFAD in Fig. 7(a), although the highest frequencies below approximately 4 km occur at relatively lower reflectivity values, a substantial number of stronger reflectivity samples (greater than ~30 dBZ) are still present. As a result, the background shading is dominated by lower reflectivity frequencies, while the upper portion of the distribution remains influenced by these stronger echoes. This causes the 25th, 50th, and 75th percentile curves to remain at relatively high reflectivity values with only weak vertical variation.

In contrast, for WDM6 in Fig. 7(b), the nearly constant percentile structure is mainly caused by an overall overestimation of strong reflectivity below 4 km, rather than by a mixed distribution similar to the observations. Therefore, the similar vertical behavior of the percentile lines does not necessarily indicate better agreement with observations. Meanwhile, for GDMN in Fig. 7(c), the excessive strong reflectivity below the melting layer is reduced, leading to a more realistic decrease of reflectivity toward lower levels. In this respect, GDMN provides a more physically consistent representation of the observed vertical reflectivity structure than the original WDM6.

Furthermore, it's difficult to see the differences between WDM6 and GDMN because the results for these schemes are shown in separate plots. Could you add new plots showing the 25th, 50th, and 75th percentiles from both schemes in the same plot (perhaps together with observed values)? This would be useful to see, because overall to my eye the differences between WDM6 and GDMN are fairly small for the CFADs – in particular, the differences between the simulated values from the two schemes appear to be much smaller than the differences between the model and observations. Thus, overall, I think that the improvements with GDMN compared to WDM6 may be a bit overstated by the authors.

: We thank the reviewer for this helpful suggestion. The main purpose of the CFAD analysis was to show that the simulated reflectivity characteristics become more consistent with the observations, particularly in terms of the upper-level reflectivity slope and the lower-level reflectivity magnitude at the two sites. We agree with the reviewer that the effect may have been overstated in the original manuscript. Therefore, we revised the sentence from "Overall, GDMN produces a more realistic bright-band signature at the GDK site and a more accurate representation of solid-particle growth than WDM6 at both sites." to "Although discrepancies with the observations still remain after applying the GDMN approach, GDMN reduces the overestimation of reflectivity and provides a more realistic representation of solid-particle growth at upper layers, compared to WDM6.".

5. The figure quality is generally good but there are some corrections needed. Specifically, the Figure 8 caption says accumulated 1 hr precipitation, but the units in the x axis labels show a precipitation rate not accumulation. Is this a 1 hr avg rate? Also, Figure 12a,b,c shows process rates, but the units are given as g kg-1 . I think it should be g kg-1 s-1 . It's correct in panel (c). Finally, the quality of some figures is poor, e.g., Figs. 8 and 10 are blurry.

: In response to reviewer's comment, we re-generated Figures 8 and 10 in the revised manuscript. We also correct unit as process rates, which is is $\mathrm{g\,kg^{-1}\,s^{-1}}$, in Fig. 12. For Fig. 8, the plotted values represent precipitation occurring within each 1-hour interval, therefore we have revised the caption and y-axis as precipitation rate.

6. The connection to explain the process rate differences between GDMN and WDM6 on p. 17 and Fig. 12 back to changes in representation of the DSD could be stronger. In other words, what are the changes from GDMN in terms of the how the DSD is represented that might explain these process rate changes? There is also some speculation when explaining some key results, when it is stated as being more definitive. For example, on lines 413-416 it is stated that increased evaporation leads to greater near surface cooling that drives south eastward propagation of the storm. But this is not clearly demonstrated by the analysis. Sensitivity tests could help to confirm this mechanism, e.g., by artificially decreasing or increasing latent heating associated with evaporation in a sensitivity test

: We thank the reviewer for this insightful comment. We agree that the linkage between the modified DSD representation in GDMN, the resulting microphysical process-rate changes, and the dynamical response should be explained more clearly. To further examine the proposed mechanism, we added an additional sensitivity experiment (LH_TEST), in which the latent cooling effect associated with rain evaporation was removed from the GDMN simulation (Figure R2). We then analyzed the vertical cross section of temperature difference (GDMN − LH_TEST) along the red line shown in Fig. 2b.

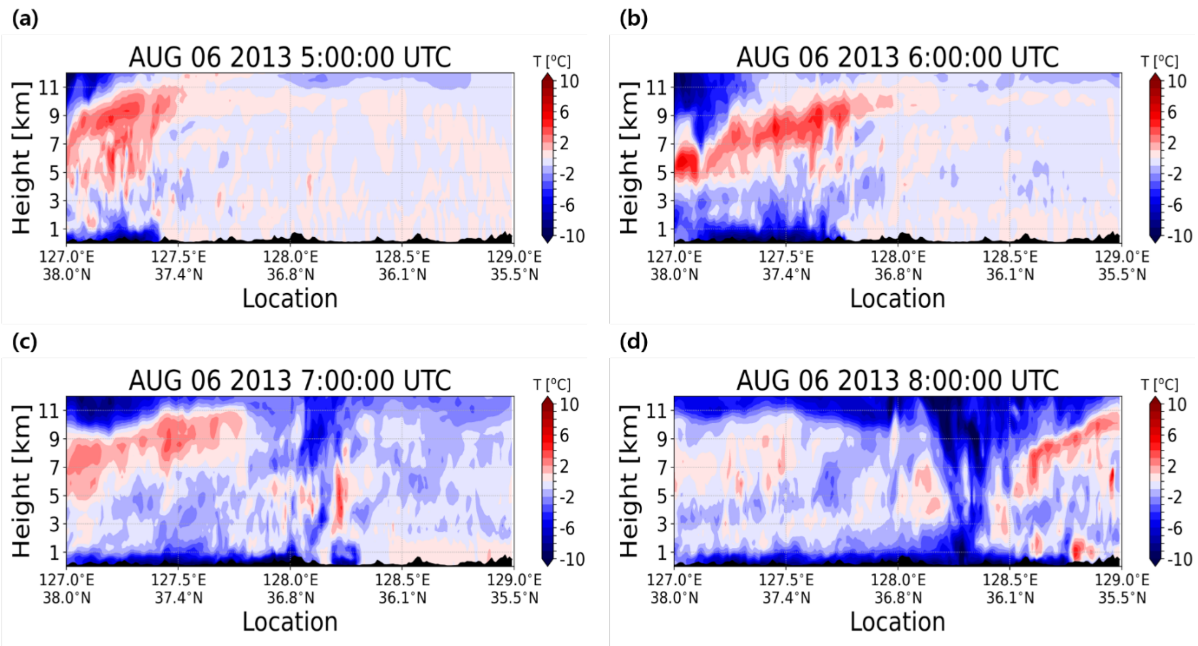

Fig. R2. Differences (GDMN – LH_TEST) in temperature (°c), shown with terrain (black color) along the cross-section indicated by the red line in Fig. 2b at 6 August 2013. (a) 05UTC, (b) 06UTC, (c) 07 UTC and (d) 08UTC.

The simulation results show that stronger low-level cooling develops as the convective cell approaches the Korean Peninsula in GDMN. The temperature differences between the two experiments (GDMN minus LH_TEST) indicate that evaporative cooling substantially contributes to the enhanced near-surface cold pool in GDMN. This supports the interpretation that increased evaporation promotes stronger low-level cooling, which in turn contributes to the southeastward propagation of the convective system in GDMN.

Minor Comments

1. Line 26, abstract. Contoured frequency by altitude diagrams of what quantity? I think you mean reflectivity? Or something else?

: Added "of radar reflectivity" accordingly.

2. Line 31. What is "P3, 2015" in the reference list?

: We Revised "P3, 2015" to "Morrison, 2015".

3. Line 32. Milbrandt and Yau is 2005 not 2011. This paper is also missing from the reference list. Again, there are many inconsistencies with the reference list (see major comment #7 above).

: We Revised "Milbrandt and Yau, 2011" to "Milbrandt and Yau, 2005". Additionally, we rechecked the remaining references.

4. Line 33. I think you need to add "weather" after "numerical"?

: Revised accordingly.

5. Line 46. Typo. It should be a comma before “suggesting”, not a period.

: Revised accordingly.

6. Line 55. Maybe “type of cloud” is better than “type of convection”?

: Revised accordingly.

7. Lines 68-69. Not exactly clear what you mean by “Lee et al. (2004) further demonstrated that the normalization approach of Testud et al. (2001) can only represent a specific case that uses the third and fourth moments.” But isn’t the Testud et al. normalization approach built on normalizing by the 3rd and 4th moments? So how can this be “further demonstrated”, rather than just being an inherent feature of that approach?

: The corresponding sentence was revised as following: “Lee et al. (2004) demonstrated that the normalization approach of Testud et al. (2001) was for the particular cases of the normalization in Lee et al. (2004), with the third and fourth moments.”

8. Line 74. It might be confusing to refer to “nth moment estimation”. Perhaps instead you could say “fractional error in estimations of the other moments not used in the normalization”, or something like that? Or at least, define what is meant by “nth moment”.

: Added “in estimations of the other moments not used in the normalization” accordingly.

9. Line 78. Suggest using “showed” or “demonstrated” instead of “figured out”.

: Revised accordingly.

10. Line 81. I think the Morrison et al. study used more than three moments for normalization? If so, perhaps reword to “by comparing normalization using three or more moments with single- and double-moment normalization…”

: Revised accordingly.

11. Line 98. Suggest replacing “a unit” with “units”. Equation 3. Can you write this as a single fraction rather than a multiplication 4 fractions? Then, the “1” would be removed in 1/qR which would simplify the equation and make it easier to read.

: Revised accordingly.

12. Line 103. Suggest replacing “density” with “densities”

: Revised accordingly.

13. Line 103. I don’t think it’s correct to say that raindrops are assumed to be spherical in WDM6. For example, the terminal fallspeed-diameter relationship does not assume spherical raindrops, it would very unrealistic at larger drop sizes to assume spheres. Instead, you can say that the diameter D is the diameter of an equivalent sphere which implies dR = 3.

: According to the reviewer’s comment, the sentence, “Since raindrops are assumed to be spherical in WDM6, d_{R} has a value of 3.”, has been changed to “The diameter of rain, D, is an equivalent sphere diameter, which implies d_{R} equals 3.”

14. Line 104. I wouldn’t say that the units of N0R are “unphysical”, but you can say that they vary with c and μ.

: Revised accordingly.

15. Line 137. Replace “to” with “on”.
: Revised accordingly.

16. Line 185. Suggest replacing “defined as” with “from”.
: Revised accordingly.

17. Line 186. Replace “resolutions” with “grid spacings”. In models like WRF, the resolution is not the same as the grid spacing.
: Revised accordingly.

18. Line 217. There should be a comma not a period before “Where”, so it should read “…(Bang et al., 2020), where hmed(x)…”
: Revised accordingly.

19. Line 217. “train” to be implies some type of more advanced learning or ML. Suggest replacing “train” with “fitted”. Figure 5 caption. “observation” should be “observations”.
: Revised accordingly.

20. Line 389. “represent” should be “represented”.
: Revised accordingly.

21. Line 390. “is” should be added before “more”.
: Revised accordingly.

22. Line 396. GDMN address some but not all limitations of WDM6. Thus, suggest rewording this to “alleviates some limitations of the original WDM6 scheme”
: Revised accordingly.

23. Line 398. Add “for this case” after “observations” to clarify that these are not necessarily general behaviors, but rather results for this particular case.
: Revised accordingly.

24. Line 409. Typo: “us” should be “is”.
: Revised accordingly.

25. Line 423. It’s not clear exactly what is being discussed here. Do you mean “total surface area and scattering characteristics” of the drop population?
: Added “of drops” accordingly.

26. Lines 421-423. I disagree with this sentence -- clearly it’s not the fixed DSD shape that leads to this precipitation pattern in most of the bulk schemes (except GDMN), because SBM produces a very similar pattern
: In response to reviewer’s concern, the corresponding sentence, “The prescribed DSD function and fixed shape parameters in these schemes constrain their ability to realistically simulate precipita tion.” has been changed to “The prescribed DSD function and fixed shape parameters in bulk s chemes and other parameterization assumption or introduced uncertainties in microphysics sche mes constrain their ability to realistically simulate precipitation.”

References:

Koch, S. E., Ferrier, B., Stoelinga, M. T., Szoke, E., Weiss, S. J., and Kain, J. S.: The use of simulated radar reflectivity fields in the diagnosis of mesoscale phenomena from high-resolution WRF model forecasts, Preprints, 11th Conf. on Mesoscale Processes, Albuquerque, NM, Amer. Meteor. Soc., J4J, 1–9, 2005.

Park, S.-Y., Lim, K.-S. S., and Jo, J.: Assessing the relative importance of the prognostic hail mixing ratio and predicted graupel density on the vertical reflectivity structure in bulk cloud microphysics schemes, Atmospheric Research, 108440, 2025.

Stoelinga, M. T.: Simulated equivalent reflectivity factor as currently formulated in RIP: Description and possible improvements, University of Washington Tech. Rep., 5 pp., available online at http://www.atmos.washington.edu/~stoeling/RIP_sim_ref.pdf, 2005.