Reviewer1

accepted subject to technical corrections

Suggestions for revision or reasons for rejection

The revised manuscript has adequately addressed my main comments from the previous review. In particular, the authors have refined the novelty statement regarding the implementation of GDMN in WRF WDM6, added useful discussion on the robustness of the observation-derived normalized DSD function, and provided additional sensitivity experiments to assess the dependence of the results on the prescribed shape parameters. The added FSS metric and the additional latent-cooling sensitivity experiment also help to strengthen the interpretation of the convective propagation mechanism.
Overall, I find that the revised manuscript is substantially improved and that the authors have responded appropriately to my concerns. I recommend publication in Geoscientific Model Development after minor editorial checks.
I suggest that the authors continue to phrase the broader implications cautiously, emphasizing that the results demonstrate the potential of the GDMN approach primarily for the examined case and supporting simulations, rather than establishing a general superiority of the scheme under all convective conditions.

: We greatly appreciate the reviewer's encouraging assessment that the manuscript has been substantially improved.

Following the reviewer's suggestion, we have carefully revised the manuscript to ensure that the broader implications of our results are presented more cautiously by adding following sentences in the revised manuscript.

“Our findings highlight the potential of the GDMN approach for the examined case under the specific model configuration used in this study, while recognizing that further evaluation is required before its superiority can be generalized to a broader range of convective conditions.”

Reviewer2

General comments. The revised paper is improved from the earlier version. The authors have clarified several parts of the text that were not clear before, and generally did a good job addressing the previous reviews in my opinion. They also made it clear that the purpose of this paper is "proof of concept" of the new approach to demonstrate initial results for the GDMN approach implemented in a 3D model (WRF). The authors also performed 1 month simulations and showed marginal improvement in skill scores using GDMN compared to the original WDM6 (it's not clear if these simulations were short simulations re-initialized every few days, or a single month-long simulation). The results from these simulations were shown in the author's reply to my previous review. I think they should consider moving this into the main paper. A few other major comments are given below – nothing critical, but some additional points that I feel the authors should address before publication. Several minor and editorial comments and suggestions are listed after.

: We sincerely thank the reviewer for the positive assessment of our revised manuscript and for the additional constructive comments. Following the reviewer's suggestion, we have incorporated the results from the one-month simulation into the revised manuscript and recalculated CFAD presented in Figure 7. Our detailed responses to the remaining major and minor comments are provided below.

Major comments.

1. In my previous review, I asked if the original WDM6 scheme could be modified to use the same shape parameters as GDMN. This raised the issue of whether the impact of GDMN was primarily because of how it represents the shape parameters, versus other factors. The authors replied that WDM6 applies only a one-parameter gamma and therefore is not compatible with separate fitting of two parameters. They also stated that if a combination of c and μ parameters in GDMN analogous to the single parameter μR in WDM6 were used, it would give results identical to GDMN.

In that case, one could still design a scheme based on a generalized gamma DSD with two shape parameters c and μ, without using GDMN. This may not be how WDM6 was designed, but in principle it could be done. GMDN was used because it allowed for systematic constraint of c and μ from observations. More broadly, it seems there are two possible approaches for the GDMN approach: 1) the ability to constrain two parameters of the generalized gamma function, 2) the ability to move away from assuming a gamma shape to the DSD and instead use a generalized (non-gamma) function h(x). Here the approach is to use #1, since a generalized gamma DSD is assumed. I think that the reason this was done instead of approach #2 is because disdrometers cannot accurately measure small drops, so it is difficult to obtain the generalized (non-gamma) DSD function h(x).

More discussion of these points would be useful, and I think it would be good to clarify these uses/applications of GDMN using a generalized function versus a generalized gamma function.

: We sincerely thank the reviewer for this insightful comment. Following the reviewer's suggestion, we have expanded the discussion in Section 5 (Summary and Discussion) of the revised manuscript to address the points raised, including a more detailed discussion of the uniqueness of the GDMN approach, as shown below.

"The WDM6 and GDMN method employ the same generalized gamma DSD, and therefore they produce identical simulation results when the same shape parameters are used. The current version of WDM6 adopts fixed shape parameters (c=1 and μ=2) following the previous study (Lim and Hong, 2010). The key advantage of GDMN method is that it constrains the shape parameters using the normalized DSD, which is considerably more stable than the raw DSD. As a result, GDMN method provides a robust and observationally constrained normalized function, h(x), whereas direct estimation of the generalized gamma shape parameters from raw DSDs, as in WDM6, is subject to substantial variability and often leads to unreliable parameter estimates. This study utilizes the fitted generalized gamma function, $h_{GG}(x)$, rather than the observed mean normalized function, $h_{mean}(x)$, because disdrometer measurements are subject to significant uncertainty for small drops."

2. I raised a similar point in my previous review: In Figure 7a, the cumulative reflectivity does not look correct (black lines) relative to the CFAD in color contours for the observations at heights below ~4 km. The authors mentioned there is no bug, but I don't understand how the color contours and the cumulative distributions can be mismatched. Below 4 km, the black lines (cumulative distributions) don't even align with the CFADs, the black lines are relatively constant with height while the CFAD colors show angled increase with height. Are the cumulative distributions not being calculated over the full distributions? That is, is some lower cutoff being used for the cumulative distribution function? That's the only thing I could see that might potentially explain the discrepancy.

: We sincerely thank the reviewer for pointing out this issue. In the original analysis, reflectivity values between 5 and 60 dBZ were included in the CFAD calculation, while values outside this range were excluded using cutoff thresholds. Following the reviewer's suggestion, we have revised Figure 7 to include reflectivity values between 0 and 60 dBZ, excluding only values below 0 dBZ (Figure R1). The revised figure shows more improved agreement between the cumulative reflectivity (black contours) and the CFAD (colored shading), compared to the previous version. Accordingly, the corresponding description in the manuscript has been revised from "The reflectivity values range from 5 to 60 dBZ." to "The reflectivity values range from 0 to 60 dBZ."

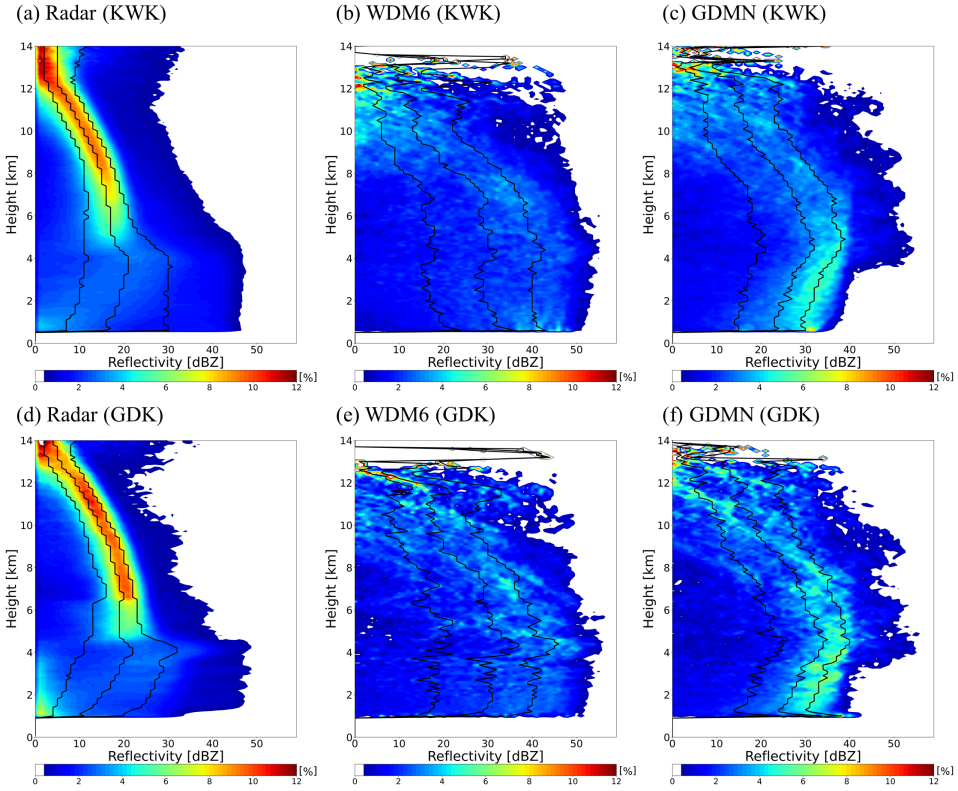

Figure R1. Same as Figure 7 in the revised manuscript.

3. Lines 333-343. Overall, the GDMN improvements relative to the original WDM6 still seem overstated. I disagree with some of the discussion based on Fig. 7. Where is the bright band evident in GDMN (as stated on line 337)? The narrow bright band is clearly seen in the observations for both cases, with a sharp shift in the CFAD to larger reflectivities (moving downward with altitude) right around 4 km. No such trend is seen in either WDM6 or GDMN. GDMN has a very broad region of higher reflectivity between 4 and 7 km, but this is not a bright band (e.g., it extends well above the melting level). Thus, please modify the statement about the bright band in GDMN to better reflect the results shown in Fig. 7.

Second, both GDMN and WDM6 seem about equally biased with reflectivities that are too large above 6 km. Particularly above 9-10 km, both schemes produce cumulative 50th and 75th percentile reflectivities of 20-30 dBZ while the observations are less than 20 dBZ for the 75th percentile. The GDMN reflectivities are perhaps slightly lower and thus closer to observations, but the difference is small. In other words, the differences between GDMN and WDM6 are much smaller than differences between these schemes and the observations at these altitudes. Thus, I think that the discussion on lines 340-342 needs to be modified. Overall, the main difference is that GDMN reduces the reflecitivity high bias in WDM6 below 4 km. That is a nice result, and I feel this should be emphasized by the authors rather than trying to claim substantial improvements for ice-phase regions aloft that are much less clearly seen in the figure.

: We thank the reviewer for this valuable comment. Accordingly, we have revised the manuscript to describe this reflectivity enhancement more cautiously.

The description of the KWK case has been revised from "GDMN shows a bright-band signature at 5 km and a decrease in reflectivity near the surface, indicating possible substantial evaporation in the lower layers." to "Although GDMN does not reproduce the narrow, distinct bright band observed near the melting layer likely due to the forward operator within WRF, it well reproduces reflectivity intensity below 4 km, bringing it into better agreement with the observations. GDMN also shows a decrease in reflectivity toward the surface, indicating possible substantial evaporation in the lower layers."

For The description of the GDK case, we have deleted following sentence "In contrast, GDMN simulates a clear bright band at around 4 km, with a median reflectivity of 35 dBZ."
We have moderated the discussion regarding the upper-level reflectivity structure from "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." to "Although discrepancies with the observations still remain after applying the GDMN approach, particularly above approximately 6 km, GDMN overall reduces the overestimation of reflectivity compared with WDM6."

4. Why not include results from the 1 month simulations that you mentioned in your reply to my previous review? This provides useful context and there should be enough room to include it.

: Following the reviewer's recommendation, we have included the results of the one-month simulations in the revised manuscript as Figure 14 (Figure R2 in this responeses), with the corresponding discussion in the "Summary and Discussion" section to provide additional context for the performance of the GDMN approach.

"To address the limitations of the single-case analysis, we additionally conduct a single month-long simulation over East Asia for July 2023, encompassing a wide range of summertime convective systems. The model configuration, including the physical parameterizations and initial and lateral boundary conditions, is identical to that used in the single-case simulation, except that a horizontal grid spacing of 10 km and a model time step of 60 s are employed. Sea surface temperature (SST) is prescribed using the Optimum Interpolation SST (OISST) dataset (Huang et al., 2021). The performance of the original WDM6 and the modified WDM6 is evaluated against hourly precipitation from the Integrated Multi-satellitE Retrievals for GPM (IMERG) product (Huffman et al., 2015) (Fig. 14). Compared with the original WDM6, the modified WDM6 scheme reduces the precipitation over Northeastern China and increases the precipitation over the South Sea of Korea, resulting in better agreement with the observations. The statistical skill scores for surface precipitation also demonstrate the improved performance of the modified WDM6, with the pattern correlation (PC) increasing from 0.47 to 0.52 and the bias decreasing from 1.04 to 1.02. These results indicate that the performance improvements achieved by the GDMN approach are maintained in a one-month simulation encompassing a broad range of summertime convective systems, suggesting that its benefits extend beyond the single convective case examined in this study."

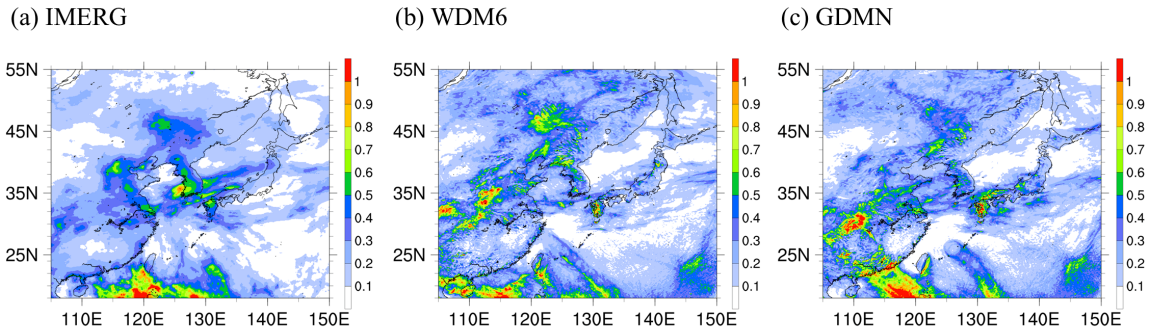

Figure R2. Spatial distribution of hourly precipitation [mm hr^{-1}] from (a) the IMERG observations and model simulations using (b) WDM6 and (c) GDMN for the one-month simulation conducted during July 2023.

References

Huang, B., Liu, C., Banzon, V., Freeman, E., Graham, G., Hankins, B., et al. (2021). Improvements of the daily optimum interpolation sea surface temperature (DOISST) version 2.1. Journal of Climate, 34(8), 2923–2939. https://doi.org/10.1175/jcli-d-20-0166.1

Huffman, G. J., Bolvin, D. T., Braithwaite, D., Hsu, K., Joyce, R., Xie, P., & Yoo, S. H. (2015). NASA global precipitation measurement (GPM) integrated multi-satellite retrievals for GPM (IMERG). Algorithm Theoretical Basis Document (ATBD) Version, 4, 26.

Minor Comments

1. Abstract, line 25. Suggest replacing “observation-constrained” with “observationally-constrained”.

: Revised accordingly.

2. Abstract, line 25. You should double check with GMD policy, but I believe the acronyms used here should be spelled out. Particularly GDMN, as this is not a standard terminology or widely used in the literature.

: Revised to spell this out more explicitly.

3. Abstract, line 26. It seems awkward to mention in the abstract that other scaling normalization bulk formulations exist and cite the Szyrmer et al. paper. Move this to the introduction.

: The corresponding sentence in the abstract has been changed to “To our knowledge, this study presents the first implementation of an observation-constrained Generalized Double-Moment scaling Normalization (GDMN)-based rain Drop Size Distribution (DSD) representation within Weather Research and Forecasting (WRF) Double-Moment 6-class (WDM6) schme and evaluates its impacts in a convection-permitting real-case simulation”

Addtionally, the corresponding reference, Szyrmer et al., 2005, was moved to the introduction as following: “Scaling normalization is commonly employed to represent the hydrometeor DSD and has been extensively studied since the 1990s (Sempere-Torres et al., 1994; Testud et al., 2001; Lee et al., 2004; Szyrmer et al., 2005; Berne et al., 2012; Morrison et al., 2019; Lee et al., 2023;).

4. Lines 40-41. The citations are not correct here. Morrison 2005 should be Morrison et al. 2005, Morrison 2015 should be Morrison and Milbrandt 2015, Thompson should be Thompson et al. 2008.

: Revised accordingly.

5. Line 55. This looks like a typo or some grammar error. Start a new sentence here “This suggests unrealistic precipitation growth…”

: Revised accordingly.

6. Line 80. I think this reads more smoothly as “…normalization in Lee et al. (2004) using the third and fourth moments.”

: Revised accordingly.

7. Line 84. “decrease” should be “decreases”.

: Revised accordingly.

8. Line 92. This doesn’t seem correct to call the Morrison et al. (2019) approach “triple-moment” but then note it uses three or more moments. Suggest rewording this to “…extended the GDMN analysis by developing a generalized n-th moment normalization using three or more moments with single- and double-moment normalization using…”

: Revised accordingly.

9. Line 108. It’s more standard to say “with units of” rather than “with a unit of”.

: Revised accordingly.

10. Line 109. Suggest “which has units of meters”.

: Revised accordingly.

11. Line 115. Suggest replacing “unit” with “units” and “depends” with “depend” for noun-verb agreement.

: Revised accordingly.

12. Lines 144-145. Use of the word “channel” here seems confusing and non-standard. I think these are the bins over which the normalized DSD are defined and calculated. If so, I’d replace “channel-wise” with “bin-wise” and “channel interval” with “bin interval”. To further clarify, you could use the term “normalized bin”.

: Revised accordingly.

13. Line 147. “These” should be “The”.

: Revised accordingly.

14. Line 153. Suggest replacing “stable” with “general”.

: Revised accordingly.

15. Figure 1 caption, first line. I think “DSD” should be “DSDs”. Also, add “the” before “theoretical” and replace “their” with “the”.

: Revised accordingly.

16. In the Figure 1 caption, it’s also not clear what you mean by “for each channel”. Isn’t the black line simply the mean hmean(0,3,μ,c(x)) across the range of x shown? Why do you need to add “for each channel”? Also, if the solid black line is hmean(0,3,μ,c(x)), what is the dotted black line? Please clarify that in the figure caption.

: In response to reviewer’s comment, we added the following sentence in the caption of Figure 1.

“…and the solid line represents $h_{\mathrm{mean}(0,3,\mu,c)}(x)$ while the dotted line represents its best fit obtained using with $h_{\mathrm{GG}(0,3,\mu,c)}(x)$ with μ= 0.29 and c = 2.60.”

17. Equation 8. On the left hand side, I would remove “/(m-4)” as that is confusing. It’s unnecessary anyway as units of m-4 for NR were already noted just below eq. (1).

: Removed accordingly.

18. Lines 167-172. It’s confusing to say that the original WDM6 scheme employs fixed shape parameters c = 1 and mu = 2, and then say in the next sentence that it treats this using only a single parameter. I’d start this paragraph by saying that the original WDM6 scheme employs only a single parameter, and then say this is equivalent to assuming c = 1 and m = 2 using the relation μR = cμ-1.

: Revised accordingly.

19. Line 220. Replace “as horizontal” with “using a horizontal”.

: Revised accordingly.

20. Table 1, left column. Replace “horizontal resolution” with “horizontal grid spacing”.

: Revised accordingly.

21. Line 267. I think you mean the microphysical processes are not sensitive to the prescribed c and μ parameters in GDMN (since WDM6 original only prescribes a single μR parameter). It would help readers to clarify this in the text. That is, add “in GDMN” after “parameters” on this line.

: Revised accordingly.

22. Line 301. Typo. A space is missing between “Score” and “(FSS)”.

: Revised accordingly.

23. Line 303. You don’t need to spell out Fractions Skill Score again here since that is already done two lines above.

: Revised accordingly.

24. Line 322. Add “the” before “bright band”.

: Revised accordingly.

25. Line 394. I’d replace “is observed” with “occurs”, since this is discussing model results not observations.

: Revised accordingly.

26. Line 398. Replace “distribution” with “distributions”.

: Revised accordingly.

27. Line 482. Replace “hydrometeors” with “hydrometeor types”.

: Revised accordingly.

28. Figure 13 is poor quality and very blurry. This should be replaced by a higher resolution rendering of the figure

: Revised accordingly.