Second review of "**Implementation of a double-moment scaling normalization method for raindrop size distribution in a WRF 4.3.1 bulk-type cloud microphysics scheme: A case study of the Korean peninsula**", by Jo et al., submitted to GMD.

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 authors' 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.

Note: line numbers mentioned below correspond to the track changed version.

Overall recommendation: *Minor revision*

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 μ_R 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.

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.

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 50^{th} and 75^{th} percentile reflectivities of 20-30 dBZ while the observations are less than 20 dBZ for the 75^{th} 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.

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.

Minor and editorial comments.

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

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.

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.

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.

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

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

7. Line 84. "decrease" should be "decreases".

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…"

9. Line 108. It's more standard to say "with units of" rather than "with a unit of".

10. Line 109. Suggest "which has units of meters".

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

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”.

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

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

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

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 $h_{mean}(0,3,\mu,c(x))$ across the range of x shown? Why do you need to add “for each channel”? Also, if the solid black line is $h_{mean}(0,3,\mu,c(x))$, what is the dotted black line? Please clarify that in the figure caption.

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).

18. Lines 167-172. It’s confusing to say that the original WDM6 scheme employs fixed shape parameters c = 1 and m = 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 $\mu_R = c\mu-1$.

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

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

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 μ_R parameter). It would help readers to clarify this in the text. That is, add “in GDMN” after “parameters” on this line.

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

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

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

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

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

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

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