

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

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.

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.*

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 $h_{GG}(x)$ 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 $\mu = 0.24$ were derived? Were these parameters obtained by the minimization of $h_{med}(x)$ and $h(x)$? Similarly, how were $c = 2.60$ and $\mu = 0.29$ derived for the $h(x)$ normalized by the 0th and 3rd moments?

I have other related questions as well. On line 216 you use $h_{mean}(x)$ but then on line 217 you mention $h_{med}(x)$. 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, $N_R(D)$ normalized by the 0th and 3rd moments)? In the caption it says that the solid line is $h_{med}(x)$. Is this obtained from $N_R(D)$ associated with the $h_{med}(x)$ 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 $\mu = 0.29$? This isn't clear, because the caption mentions the solid line is $h_{med}(x)$, but then $c = 2.60$ and $\mu = 0.29$ 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.

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 $\mu = 2$. 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?

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 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?

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?

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.

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 $\text{g kg}^{-1} \text{ s}^{-1}$. It's correct in panel (c). Finally, the quality of some figures is poor, e.g., Figs. 8 and 10 are blurry.

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.

7. Please carefully go through the referenced list and check the in-text citations. There are many inconsistencies and mismatches.

Minor comments.

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

Line 31. What is “P3, 2015” in the reference list?

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).

Line 33. I think you need to add “weather” after “numerical”?

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

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

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?

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”.

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

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...”

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 $1/q_R$ which would simplify the equation and make it easier to read.

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

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 $d_R = 3$.

Line 104. I wouldn't say that the units of N_{OR} are “unphysical”, but you can say that they vary with c and μ .

Line 137. Replace “to” with “on”.

Line 185. Suggest replacing “defined as” with “from”.

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

Line 217. There should be a comma not a period before “Where”, so it should read “...(Bang et al., 2020), where $h_{med}(x)$...”

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”.

Line 389. “represent” should be “represented”.

Line 390. “is” should be added before “more”.

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

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

Line 409. Typo: “us” should be “is”.

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?

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.