Reviewer #1

Overview

This study describes the introduction of prognostic graupel bulk volume (and thus predicted graupel density) into a double-moment bulk microphysics scheme (WDM6) and evaluates its impact on (1) a 2D idealized squall line simulation and (2) simulations of several observed cases during a field campaign over the Korean Peninsula. The latter simulations are compared with observations including a large spatial array of surface meteorological stations that measured precipitation, a 2D video disdrometer, and a multi-angle snowflake camera. The study found that introducing predicted graupel density improved the representation of surface precipitation spatially and reduced statistical errors in most of the case study simulations, and that the new scheme reasonably represented the observed relationship between graupel density and fall velocity. While this study is founded on sound science questions and a robust methodology, and provides some very interesting results, there are several aspects that need to be addressed and improved before being considered for publication. Overall, the manuscript and interpretation of results could be improved by a more thorough description of the microphysics scheme as opposed to just the description of the new implementation. Since the study is heavily focused on a microphysical evaluation, there is justification to provide a little more background on the scheme structure. This study obviously involved a significant amount of work, but I am skeptical of some of the physical interpretations of the results. Apart from that, there are important and interesting results that are presented, and I think the manuscript would be particularly improved by focusing more closely on those robust results instead of casting such a wide net on the evaluation. The implementation of the new scheme and the vast constraints used to evaluate it against the observations are a huge undertaking that was performed well, and I think focusing on the observation-model comparison more would better highlight the novelty and success of the science that was performed.

: Thank you for your valuable review and constructive feedback. Our manuscript has been revised including a more detailed description of the WDM6 microphysics scheme to offer better understanding of results. Additionally, we focused more closely on the observation-model comparisons by highlighting the verification of model results with 2DVD. We are confident that these changes make our study more accessible and informative for readers, better focusing the importance and effectiveness of the new predicted graupel density implementation in the WDM6 microphysics scheme.

General/Major Comments

1. Overall, the description of the scheme needs to be revised/revisited. While some existence of knowledge should be assumed by the reader, the paper would benefit greatly from some additional information–even just a few basic sentences on the foundation of the WDM6 scheme. In addition, an improved description of the implemented, modified graupel species is needed, in particular how this implementation affects (or doesn't affect) the other ice species in the scheme.

: In response to your comments, we have added a detailed introductory appendix that outlines the foundational information of the WDM6 scheme. This section provides the necessary background on the scheme's core principles, including the parameters that define the different categories.

Additionally, we have included the following sentences to explain how the modified graupel species affect the other ice species:

Line 150: "Therefore, the modified WDM6 incorporates varying a_G and b_G parameters in the V_G–D relationship and c_G in the M_G–D relationship by implementing predicted graupel density. The several microphysics processes in the WDM6 can be affected by the newly derived V_G–D and M_G–D relationships. The microphysical processes of Pgmlt, Pgacw, Pgdep, Pgevp, and Ngacw are affected by a_G and b_G in the V_G–D relationship and Pgmlt, Pgaci, Pgacr, Pgdep, Pgevp, Pgacw, Ngaci, Ngacr, Ngeml, and Ngacw are affected by c_G in the M_G–D relationship. Since these processes act as source/sink for both mass mixing ratio and number concentration of cloud water, rain, cloud ice, snow, and graupel (Fig. A1 in the appendix), varying parameters with predicted graupel density can affect the mass mixing ratio and number concentration of liquid-phase hydrometeors as well as solid-phase hydrometeors."

2. In general, much of the introduction was characterized by referencing past studies saying that including/neglecting certain things in the scheme changed the simulated system. By the time I got to the end, it seemed like a huge amount of information being provided to the reader but without much physical insight. I think the introduction would benefit from reducing the number of references where it is just stated that "X changes Y", and instead focus on a more limited number of studies and provide some physical pathways for how Y is changed by X. Otherwise, including all of these references isn't very informative; it just shows that changes to microphysics changed the simulations without any substance as to how or why.

: Thank you for your suggestion. In response to your suggestion, I have undertaken a substantial revision of the introduction, detailing the physical pathways for how Y is changed by X and removing the references that were published a long time ago. The following sentence has been modified in the revised manuscript:

Line 38: "Morrison and Milbrandt (2011) demonstrated that different approaches in treating graupel or hail produce distinct differences in storm structure, precipitation, and cold pools strength for idealized supercells. This is because graupel leads to more anvil condensate and weaker cold pools compared to hail. Bryan and Morrison (2012) showed that the fall velocities of graupel and hail affect the simulated reflectivity and dynamics for an idealized squall line. Simulations with graupel instead of hail produce convective regions that are too wide and have lower reflectivity, primarily due to the slower fall velocity of graupel compared to hail. Adams-Selin et al. (2013) reported that the development of a bow echo is highly sensitive to the parameters defining the fall velocities of graupel and hail. The simulations with slower-falling graupel-like particle created a wider stratiform region and stronger cold pool, allowing for more melting and evaporation, which helped generating bowing segments earlier than in the faster-falling hail-like simulations."

Line 65: "Johnson et al. (2016) evaluated the reproducibility of the polarization signatures in supercell storms for several partially or fully two-moment (2M) schemes. Realistic signatures were obtained only with those microphysics schemes that predicted graupel density. Predicted graupel density assigns high-density frozen drops to the graupel category, resulting in relatively high-density graupel that can later grow into hail. These differences in the treatment of rimed-ice processes allow hail to grow larger and produce a much more prominent hail signature.

Line 73: "Since CCN concentration affects cloud droplet number concentration and mean droplet diameter, the model's microphysical response depends on how well parameterized processes involving the ice phase account for droplet size effects. Mean droplet size impacts graupel growth directly through the collection efficiency between graupel and droplets. Additionally, predicted graupel density influences graupel growth by increasing graupel fall speeds and enhancing accretion rates. Based on their analysis, they suggested that an accurate representation of graupel in microphysics schemes is crucial for appropriately simulating the effects of changes in the concentration of cloud condensation nuclei in selected systems."

3. Fig. 10 and associated discussion on the impacts of microphysics on vertical velocity: These differences in vertical velocity seem really insignificant to me. The only real shift you're talking about is in the 0.5-1 m/s bin, and the difference in the frequency of occurrence is less than 1%. Sure, it makes sense that less graupel in the profile may weaken the drag from condensate loading or perhaps have an effect like you described from Adams-Selin et al. (2013), but Fig. 10 is not convincing at all that these differences are not just noise. In fact, Fig. 10a shows very small but actually weaker vertical velocities in the PD scheme for the higher vertical velocity thresholds. I just don't think this effect is substantial enough to attribute the dynamical shift to microphysics as opposed to just perturbed system evolution. One could run a test by doing a small ensemble of PD runs with white noise added to the initial conditions to see if this very small shift is robust. But ultimately I don't think this is necessary, because I don't think this is an important result from your study and that there are more interesting things that

you've already focused on. Personally, I think the manuscript would be improved by removing the discussion of impacts on vertical velocity and focusing instead on the more certain points. After all, these cases are synoptic lows with orographic enhancement, right? I wouldn't expect to see significant impacts on this type of system anyway compared to deep convection cases where cold pools are important for system evolution and where vertical motions are driven by buoyancy instead of synoptic-scale circulations.

: We concur with your assessment that the differences in vertical velocity depicted in Fig. 10 are minimal and do not substantiate the suggested impacts of microphysical changes as robustly as required for a conclusive argument. Considering your comments and after careful consideration, Fig. 10 and its related descriptions are removed in the revised manuscript.

Specific Comments

4. Lines 72-76: This association between predicted vs. fixed particle density and the CCN concentrations is not very clear. You state that graupel density matters for appropriately simulating the impacts of varying CCN, but provide no details on the pathway for which this occurs. It would be helpful to the reader to briefly provide a clearer connection between the two rather than just saying one thing changed another–a physical explanation is prudent here.

: Thank you for your insightful feedback. To address your comment, we have provided a clearer explanation of the connection between predicted versus fixed particle density and the impacts of varying cloud condensation nuclei (CCN) concentrations as following:

Line 72: "~, particularly related to different number concentrations of cloud condensation nuclei (CCN) in a mid-latitude continental squall line."

Line 73: "Since CCN concentration affects cloud droplet number concentration and mean droplet diameter, the model's microphysical response depends on how well parameterized processes involving the ice phase account for droplet size effects. Mean droplet size impacts graupel growth directly through the collection efficiency between graupel and droplets. Additionally, predicted graupel density influences graupel growth by increasing graupel fall speeds and enhancing accretion rates. Based on their analysis, they suggested that an accurate representation of graupel in microphysics schemes is crucial for appropriately simulating the effects of changes in the concentration of cloud condensation nuclei in selected systems."

5. Lines 82-83: This sentence in particular is not very informative. Instead of saying that the simulated precipitation is simply sensitive to graupel density, tell us how it's sensitive to it. What happened to simulated precipitation when graupel density increased/decreased in Li et al. (2019)?

: In response to reviewer's comment, the following sentence has added in the revised manuscript:

Line 85: "Li et al. (2019) showed that the simulated precipitation exhibits significant sensitivity to changes in graupel density in the WDM6 scheme. Specifically, a lower-graupel density tends to contribute more to one-month precipitation amounts below 100 mm and less to those above 100 mm during the autumn season. Conversely, a higher-graupel density shows the opposite pattern."

6. At the beginning of Section 2, I'd recommend providing a brief few sentences on the background of the WDM6 scheme. For example, you don't mention what the 6 prognostic species are, but instead just start discussing the densities of the various species on Line 105. They should be cloud water, rain, cloud ice, snow, graupel, and CCN, correct? Related to this, you mention the "4 categories of ice" on Line 115. This can be very confusing because it seems you are referring to the species of ice in the WDM6 scheme, for which there are only 3. I assume you mean the 4 coefficients used to represent varying properties of the graupel species in the V-D and A-D relationships (as you state on Line 134)? This is not clear at this point in the manuscript. Recommend clearing this up where you first introduce it (Line 115). It could be helpful, though not necessary, to provide a table of the 6 species and their relevant m-D, V-D, and density parameters. This could also be a short Appendix addition. While the scheme is well-

documented in past literature, a self-contained description often seems appropriate in a paper where only one scheme is being considered in such detail.

: Thank you for your valuable feedback regarding the introduction of the WDM6 scheme in Section 2. As you suggested, we have revised the beginning of this section to include a description of the hydrometeor characteristics of the WDM6 scheme. We have also included a table in Appendix, detailing the parameters of hydrometeors characteristics as following:

Table A1. Parameters for hydrometeor (rain, ice, snow and graupel) characteristics in WDM6 scheme.

	$V_x - D_x$ relationship		$M_x - D_x$ relationship		Shape parameter (μ_x)	Density (ρ_x)
	a_x	b_x	c_x	d_x		
Rain	841.9	0.8	$\frac{\pi\rho_R}{6}$	3	1	1000
Cloud ice	2.71×10^3	1.0	$\frac{\pi\rho_I}{6}$	3.0	0	500
Snow	11.72	0.41	$\frac{\pi\rho_S}{6}$	3.0	0	100
Graupel	330.0	0.8	$\frac{\pi\rho_G}{6}$	3.0	0	500

Line 101: "In the original WDM6 scheme, ~~graupel~~ characteristics are pre-defined using the static value of density (ρ_G), constant coefficients for the mass (M_G)–diameter (D_G) and fall velocity (V_G)–D_G relationships." → "In the original WDM6 scheme, characteristics of hydrometeors are pre-defined using the static value of density (ρ_X), constant coefficients for the mass (M_X)–diameter (D) and fall velocity (V_X)–D relationships. Here, X represent the species of hydrometeors including cloud water, rain, cloud ice, snow, and graupel. The specific values of parameters are available in Table A1 of the Appendix."

Thank you for highlighting the potential confusion regarding the phrase, "four categories of ice particles" which is initially described for the study of Mitchell (1996). To make the meaning of sentence more clearly, the corresponding sentences are revised as following:

Line 158: "Meanwhile, Mitchell (1996) addressed that the Reynolds number (Re)–Best number (X) relationship produces the power-law expressions of fall velocity according to ice particle types based on the relationships of mass and projected area with the dimensions as Eq. (5)."

We recognize that the use of the four coefficients to represent varying properties of the graupel species in the M-D and A-D relationships in equation (10) and (11) was not clearly explained in the original manuscript. To address this, we have revised the sentence as follows:

Line 126: "In our modified WDM6, c_G varies with the predicted ρ_G (Eq. (3)). The coefficients of the area (A_G)–D relationship ($A_G = \gamma D^{\sigma}$), γ and σ, are set to $\frac{\pi}{4}$ and 2.0, respectively due to the sphere-shaped graupel in the WDM6 scheme.

Line 146: "Further, c_G and d_G represent the coefficients of the M_G–D relationship, while γ and σ are the coefficients of the A_G–D relationship~~, $A_G = \gamma D_G^{\sigma}$, and they are set as $\frac{\pi}{4}$ and 2.0,~~ respectively. ~~These four coefficients vary depending on ice particle types~~."

7. I'm wondering how variable graupel density (e.g., as low as 100 kg/m3 as displayed in Fig. 1) impacts snow and the transition between these categories? Does the snow species rime? If so, how much before it is considered graupel? Related to this, Eq. 2, which is the source/sink processes for graupel, includes terms listed in Table 1 that have nothing to do with graupel, such as accretion of rain by snow, or snow by rain. So, is accretion of rain by snow a source term in that the mass from the snow + rain accretion is transferred to the graupel species?

: Thank you for your comment. As we mentioned in comment #1, we have included the following sentences to explain how the modified graupel species affect the other ice species:

Line 155: "The several microphysics processes in the WDM6 can be affected by the newly derived V_G–D and M_G–D relationships. The microphysical processes of Pgmlt, Pgacw, Pgdep, Pgevp, and Ngacw are affected by a_G and b_G in the V_G–D relationship and Pgmlt, Pgaci, Pgacr, Pgdep, Pgevp, Pgacw, Ngaci, Ngacr, Ngeml, and Ngacw are affected by c_G in the M_G–D relationship. Since these processes act as source/sink for both mass mixing ratio and number concentration of cloud water, rain, cloud ice, snow, and graupel (Fig. A1 in the appendix), varying parameters with predicted graupel density can affect the mass mixing ratio and number concentration of liquid-phase hydrometeors as well as solid-phase hydrometeors."

To provide a clearer description of snow as suggested, we have added the following sentence to the revised manuscript:

Line 104: "In the WDM6 scheme, 'snow' is defined as an unrimed ice phase (large crystals-aggregates) with a standard density of 100 kg m^{-3}, indicating that it does not undergo riming. Conversely, 'graupel' is characterized as heavily rimed crystal particles that have not undergone wet growth. In nature, graupel has a wide range of densities according to the degree of riming. However, the original WDM6 scheme is unable to simulate this variability in graupel density as it undergoes riming because it uses a predefined constant value for graupel density."

Regarding the accretion processes mentioned in Eq. 2 and Table 1, these processes, including the accretion of rain by snow, indeed contribute to the formation and growth of graupel (or snow). To help in understanding the source/sink flow chart of these microphysical processes, we have added the following sentences with Figure A1 to Appendix:

Line 557: "One of the accretion processes, Psacr, represents the accretion between snow and rain particles, which primarily contributes to the formation of graupel or snow. When the mass mixing ratios of both rain and snow are greater (smaller) than 1.e-4 kg kg^{-1}, it contributes to the formation of graupel (snow). This process acts as a source process for the graupel or snow mass mixing ratio and as a sink process for the rain mass mixing ratio (Fig. A1a)"

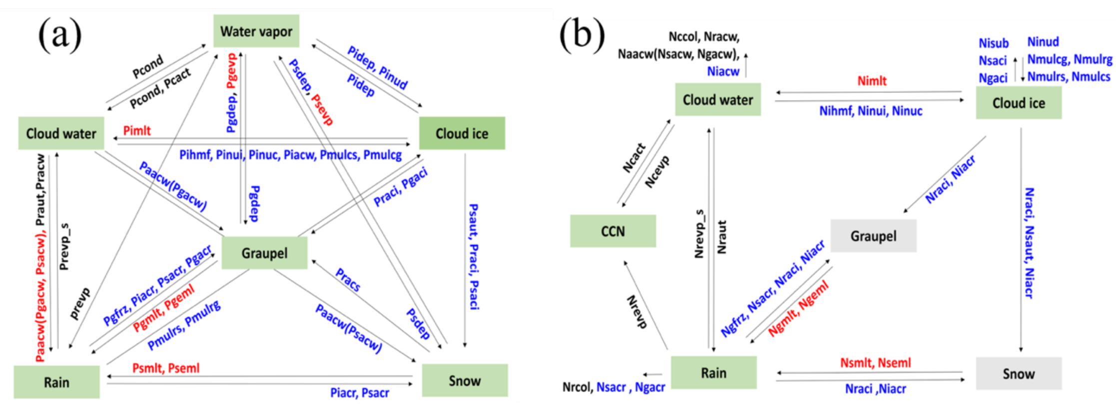

Figure A1. Flowcharts of microphysical processes for predicting (a) mass mixing ratio (S_{q_x}) and (b) number concentration (S_{N_x}) of hydrometeors in WDM6 scheme. The number concentrations of hydrometeors in the green boxes are predicted only (e.g., cloud water, cloud ice, rain, and cloud condensation nuclei (CCN)). Microphysical terms drawn with red (blue) are activated when the temperature is above (below) 0°C. Terms drawn in black are activated regardless of temperature.

8. Lines 277-278: I'm not sure Fig. 6c actually shows that particles grow mostly via vapor deposition. The color-scale for deposition/sublimation uses the same contour thresholding as freezing and accretion, both of which appear to reach maximum values of 10 g/kg/s surrounding the core, and values of 0.01 enclosing most of the graupel mass. Ultimately this figure does not really show a closed budget. Although you refer to the DelaFrance et al. (2023) paper, I'm rather skeptical that vapor deposition is the primary growth process for graupel in a deep convection simulation--you'd have to prove to me that that isn't the case using a closed budget to accept this statement as true–for example by summing the individual process rates along horizontal levels and showing a profile. You state again on Line 288 that DEP is the main process producing graupel. Again, the color-scale/contouring doesn't really support this besides at the far reaches of the anvil. And even then, I wouldn't say that DEP is producing graupel in the anvil, but rather is just the most active process growing graupel in the anvil region, which would make sense. Deposition is certainly active, but (1) I doubt it's the primary production mechanism of graupel and (2) I'm skeptical it is the primary growth mechanism besides in the anvil region. To address (2), you'd have to show me and the readers

: Thank you for your comment. In response to the reviewer's comment, we have included Figure R1 to show vertical profiles for the time-domain averaged source/sinks of the graupel mass mixing ratio. Our analysis of the process rate in Figure R1 indicates that ACC is the most effective sources of graupel, while the magnitude of DEP is relatively smaller than that of ACC at 1, 2, and 4 hour. To clarify the microphysical budget analysis, we have deleted the following sentences in the revised manuscript:

"The microphysical budget analysis shows that particles mostly grow by vapor deposition in the initial stage (Fig. 6c). The sensitivities in ice-phase particle growth and transport due to variabilities in the riming processes over an orographic barrier were examined by using a unique Lagrangian particle-based precipitation model in the study by DeLaFrance et al. (2023). This study revealed that particles initially grow by deposition and have a lower effective density. Very dense graupel (ρ_G values of 900 kg m^{-3}) are located in the marginal regions of updraft cores (Fig. 6b)."

Also, we have added Figure R1 to the supplementary figures, the following sentence has added in the revised manuscript:

Line 323: "<u>The vertical profiles for the domain-averaged major source/sink microphysics processes are presented in Figure S1 of the Supplement. ACC and MLT are analyzed as the most active source and sink processes in both WDM6_PD and WDM6_FD</u>"

Additionally following sentence has been revised from "Over the corresponding region, DEP <u>is the main process producing</u> graupel." to "Over the corresponding region, DEP <u>and ACC are the primary active processes for growing</u> graupel."

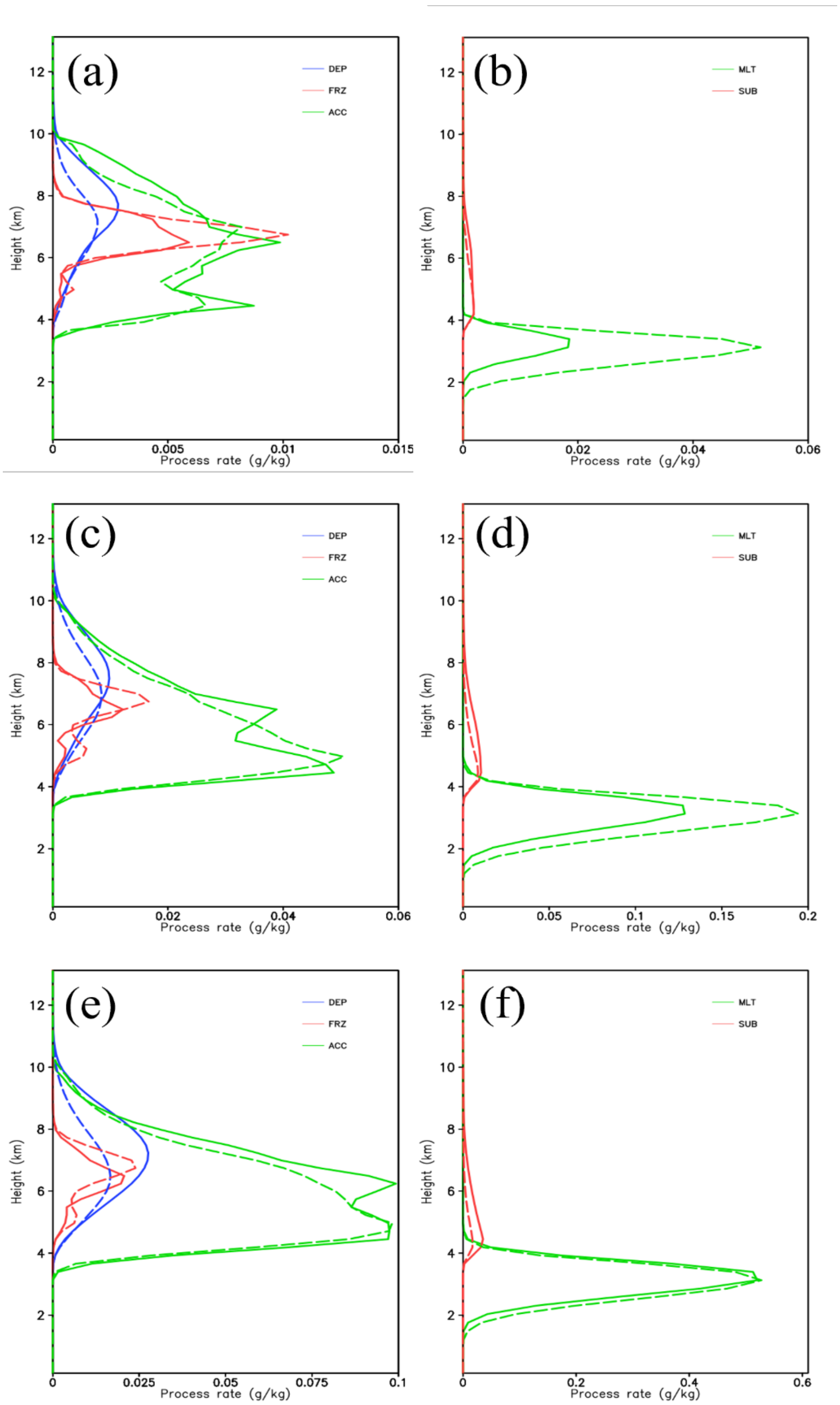

Figure R1. Vertical profiles for the domain-averaged (a) sources and (b) sinks of graupel mass mixing ratio in WDM6_FD (Solid line) and WDM6_PD (dashed line) at 1 hour (a and b), 2 hour (c and d), and 4 hour (e and f). The main source processes, namely, deposition (Pgdep; DEP), accretion (mean of Paacw, Psacr and Pgacr; ACC) and freezing (Pgfrz; FRZ) are plotted with the major sink processes, namely, sublimation (Pgsub; SUB) and melting (Pgmlt; MLT).

9. Line 307: Is the total amount of surface snow actually reduced from WDM6_PD to WDM6_FD in Fig. 7c? This isn't clear from the map, where it looks like positive and negative values could offset each other in total. Recommend being more quantitative with this statement. While it is obvious that graupel

is reduced in Fig. 7d, it could also be helpful to be more quantitative, perhaps by using a domain-accumulated snow/graupel relative difference and mentioning it in the text.

: To provide a clearer and more quantitative comparison as suggested, we have added the following sentence associated with Figure 8 in the revised manuscript:

Line 346: "Specifically, the total surface snow is reduced by 93% (domain-averaged snow amount is 0.75 mm in WDM6_FD and 0.80 mm in WDM6_PD), and surface graupel shows an increase of 124% (domain-averaged graupel amount is 0.64 mm in WDM6_FD and 0.51 mm in WDM6_PD) in WDM6_PD compared to WDM6_FD."

Line 353: "Surface snow decreases significantly by 92% in WDM6_PD (domain-averaged snow amount is 0.77 mm in WDM6_FD and 0.84 mm in WDM6_PD), compared to WDM6_FD, while the surface graupel increases by 121 % (domain-averaged graupel amount is 0.21 mm in WDM6_FD and 0.18 mm in. WDM6_PD) (Figs. 8g and h)."

10. Fig. 8: This isn't necessary, but the interpretation of this (and other) figure(s) would probably benefit by showing the 0 deg C level with a horizontal line.

: We agree that the inclusion of the 0°C level as a horizontal line would significantly enhance the interpretability of this and other figures. As such, we have redrawn Figure 9 to include a horizontal line indicating the 0°C level and modified its caption.

Line 394: "The sum of snow and graupel mass mixing ratios (g kg^{-1}) is indicated by red lines, and the 0°C level by the grey dashed horizontal line."

11. Line 343: "resulting in an increase in the amount of surface graupel deposited"—this statement may be a little confusing. You go on to explain why this is the case (basically, smaller graupel → faster fallspeeds (relative to FD) → faster sedimentation → greater surface graupel accumulation but lower graupel mass in the profile), and this is an interesting result, but when this statement is presented, the reader doesn't yet know the association/reasoning. This could be a good opportunity to state something along the lines of: "if graupel mass is reduced on average in the profile when using predicted density (Fig. 8c,f), why does it lead to greater surface graupel accumulation (Fig. 7c)?"

: In response to the reviewer's comment, we have added the following sentence to include a more explicit discussion of the underlying processes before presenting the association/reasoning of increased surface graupel accumulation.

Line 387: "~, resulting in an increase in the amount of surface graupel deposited (Figs. 7d and h)." → "~, resulting in an increase in the amount of surface graupel deposited (Figs. 8d and h). The reason for the lower graupel mass (Figs. 9c and f), despite the greater surface graupel accumulation (Figs. 8d and h) in WDM6_PD, will be analyzed in the subsequent Figures 10 and 11."

12. Lines 354-358: Related to the prior point, I think the reader would really benefit from a figure showing, perhaps, profiles with percentiles of the mass-weighted mean diameter rather than just giving domain-horizontal-and-vertical averages. This point really seems to be getting to the crux of the interpretation, which is pretty interesting and deserves to be highlighted. For example, Fig. 1 shows clearly that for all densities, the graupel terminal fall velocity with predicted graupel density is faster than with fixed density–but this is only unanimously true for particles smaller than ~ 1-2 mm, where you say your mean Dm lies. So I think this point is deserving of a little more attention. Of course this isn't necessary, but I think it would improve the manuscript.

:. In response to the reviewer's suggestion, we have added Figures 10b and d, which presents profiles of D_m with percentiles. Additionally, we have added the following sentences in the revised manuscript:

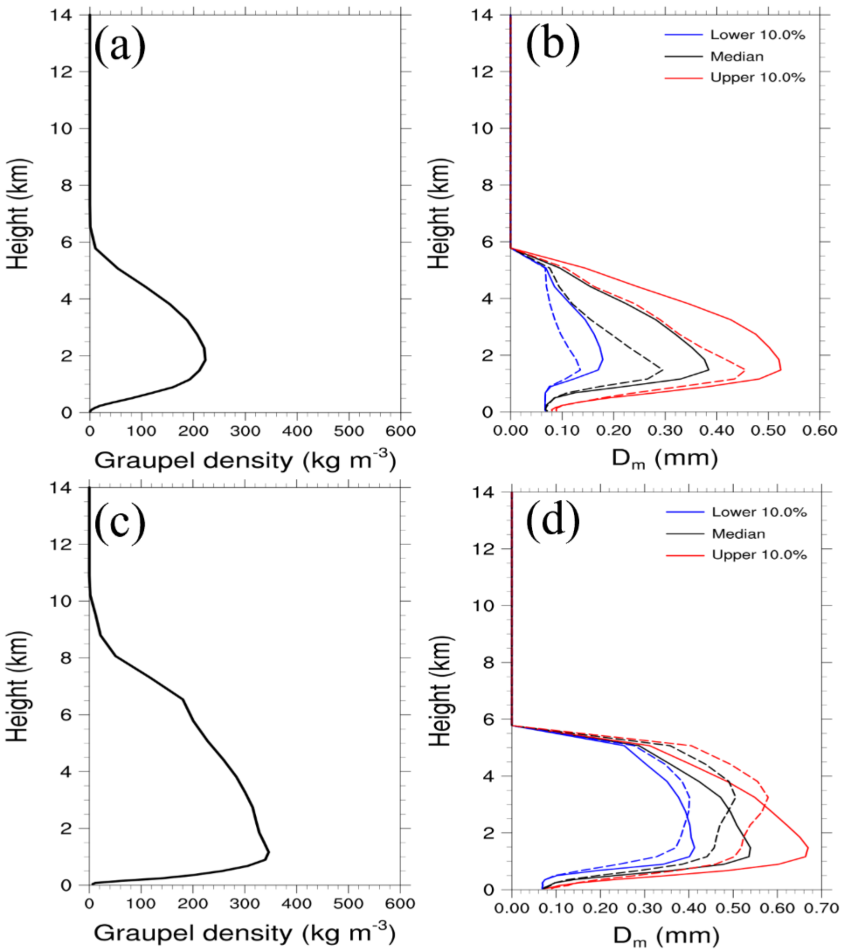

Figure 20. Vertical profiles for the time-domain-averaged ρ_G (kg m^{-3}) for (a) CL and (c) WL cases with WDM6_PD. Time-domain-averaged mass-weighted mean diameter (D_m) (mm) with WDM6_PD and WDM6_FD are drawn in (b) and (d) for CL and WL cases. The solid and dashed lines represent WDM6_FD and WDM6_PD, respectively.

Line 401: "The time-domain-averaged mass-weighted mean diameter (D_m) in WDM6_PD is greatly reduced compared to WDM6_FD (Figs. 10b and d). In the CL case, the range of D_m is quite wider below the 4-km level, indicating more variability in graupel sizes than the WL case. In both cases, WDM6_PD presents smaller graupel than WDM6_FD, especially over the lower level."

Line 404: "In WDM6_PD, the time-domain-averaged mass-weighted mean diameter (D_m) is simulated as 0.110 and 0.191 mm for the CL and WL cases, respectively, whereas in WDM6_FD, it is simulated as 0.133 (CL) and 0.199 (WL) mm, indicating that WDM6_PD simulates smaller graupel diameters." → In WDM6_PD, the time-domain and vertical-averaged D_m is simulated as 0.110 mm and 0.191 mm for the CL and WL cases, respectively, whereas in WDM6_FD, it is simulated as 0.133 mm (CL) and 0.199 mm (WL)."

Line 410: "In the CL case, WDM6_PD simulates ρ_G with a maximum value of 220 kg m^{-3} and D_m with a maximum value of 0.44 mm at around the 2 km level (Figs. 10a and b)."

Line 419: "In the WL case, graupel, which exists up to the 10 km level, ρ_G increases significantly up to a value of 350 kg m^{-3} at 1 km level (Fig. 10c). Even though D_m of WDM6_PD is larger than that of WDM6_FD above the 3-km level, graupel particles in WDM6_PD have a greater falling velocity (Figs.10d and 1) and fall from a relatively higher level of 8 km compared to WDM6_FD (Fig. 11c)."

13. Fig. 9b and Line 360: I'm not really sure what you mean by "falling graupel mixing ratios depending on the mass-weighted terminal velocity" or by "the maximum level of falling graupel"; and I'm not really sure what's being shown in Fig. 9b,e. The units on the x-axis imply it is a mixing ratio, but there are negative values in the profile. Please revise the description of what you're showing here, because it makes the discussion around Line 360 rather hard to follow.

: To make the meaning of the x-axis label of Figures 11a and c clearly, it has been revised to "q_z-q_{z-1} (g kg^{-1})" in the revised manuscript. Additionally, to clarify the meaning of figures, we have revised the caption from "Time-domain averaged falling graupel mass mixing ratios depending on the mass-weighted terminal velocity" to "Time-domain averaged difference in graupel mass mixing ratio between the levels 'z' (q_z) and 'z-1' (q_{z-1}) due to sedimentation" in the revised manuscript.

14. Lines 362-364: While Fig. 8 clearly shows more snow mass in the profile on average, Fig. 9c,f doesn't show convincing evidence of greater snow deposition between the two simulations. I mean I see what you're talking about, but those differences seem remarkably small and insignificant relative to noise. Furthermore, for Fig. 9c,f, I would label these as "process rates" and not "production rates". Deposition is likely not producing graupel–and sublimation can't produce anything since it's a sink process.

: In response to reviewer's comment, we have revised the terminology as "process rates".

To illustrate the greater snow mass of WDM6_PD for the CL case (Fig. 9c), we have analyzed snow advection in Figure R2. It is evident that the inland area, receiving abundant precipitation, presents more snow advection at the 850 hPa level in WDM6_PD compared to WDM6_FD (Fig. R1b). The snow advection towards the inland area by the strengthened northeasterly wind can certainly enhance the snow mass mixing ratio in WDM6_PD.

Additionally, more efficient accretion between cloud water and snow/graupel (Paacw), due to the increased snow advection, further contributes to the increased snow mass mixing ratio in WDM6_PD (Fig. R1c). To provide a clearer reason for the increased snow mass in WDM6_PD, we have added Figure R2 to the supplementary figures, the following sentence has added in the revised manuscript:

Line 416: "Furthermore, the northeastern inland area, receiving abundant precipitation, exhibits more positive snow advection at the 850 hPa level in WDM6_PD compared to WDM6_FD (Fig. S6 in the Supplement). Increased snow advection towards the inland area enhances the snow mass mixing ratio in WDM6_PD. Additionally, efficient Paacw with more available snow mass can contribute to the increased snow mass mixing ratio in WDM6_PD."

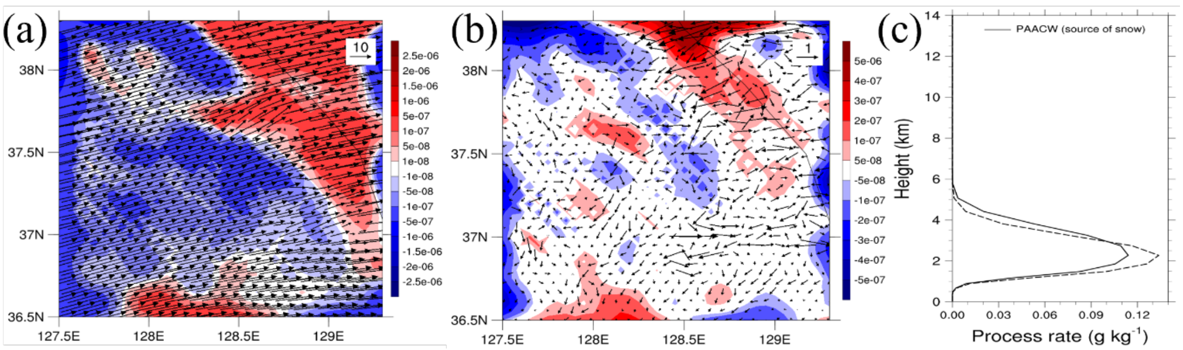

Figure R3. Snow advection (g kg^{-1} s^{-1}) and wind vector (m s^{-1}) at 850hPa for CL case from (a) WDM6_FD, and (b) the difference between WDM6_PD and WDM6_FD (WDM6_PD minus WDM6_FD). The vertical profiles of the time-domain-averaged Paacw process for C1 case are shown in (c). The solid (dashed) line represents WDM6_FD (WDM6_PD).

15. Lines 379-382: This is an important and interesting point! I'd love to see this highlighted more.

: Thank you for recognizing the significance of verifying our simulations with the 2DVD data. To highlight our verification results further, we have added the following sentence in the revised manuscript:

Line 450: "…Although WDM6_PD simulates larger ranges of fall velocity and lower ranges of ρ_G, it is closer to the observations than WDM6_FD. Our analysis hights that WDM6_PD with varying graupel density results in faster fall velocities, leading to more efficient sedimentation processes, which affect the spatial distribution and amount of graupel mass mixing ratio both in the atmosphere and on the surface. By predicting graupel density, WDM6_PD can produce more realistic characteristics of graupel particles, including their density and fall velocity."

16. Lines 397-406: This is an interesting result! Really shows the utility of what you've done here, which is great. I'd love to see this highlighted more.

: Please refer to the response for comment #15 above.

17. Lines 450-451: Again, I don't think there is convincing evidence of a reduction in the strength of upward motion.

: As previously mentioned in response to comment #3, we have removed Figure 10 and its related analysis in the revised manuscript.

18. Line 456: "but also predicts a wider range of fall velocity compared to the observed values"--isn't this the opposite of what is stated on Line 401, where you state "shows a much lower range of graupel fall velocity than the observed value"? And since Fig. 11's y-axis is logarithmic, isn't the range of fall velocities for the FD scheme smaller than observed (as stated on Line 401)?

: To address the reviewer's comment, we have modified the following sentence in the revised manuscript:

Line 501: "but also predicts a wider range of fall velocity compared to the observed values." → "but also predicts a lower range of fall velocity compared to the observed values."

19. Lines 459-460: This sentence seems a little abrupt and out-of-place, but I think it deserves a little more attention and discussion. These last two sentences truly are a unique and interesting part of this study, but it's just mentioned in passing at the very end. It's not necessary, but expanding on this a little bit, and perhaps providing suggestions for a path forward to refine the simulated fall velocity, would be a worthy addition to guide future projects.

: In response to the reviewer's feedback, we have expanded this discussion to provide potential avenues for further refinement of the simulated fall velocities. To this end, we have added the following sentences in the revised manuscript:

Line 504: "The V_G–D relationship in the modified WDM6 is derived using the least-squares method in a log–log space at the given graupel density~~; therefore, there is room to further refine the simulated fall velocity~~. The derived V_G–D relationship in our research could be refined by incorporating a broader range of graupel observational data, including hexagonal, conical, lump graupel, or graupel-like snow. Improvements in the representation of V_G–D relationship can lead to better simulation of precipitation and microphysical processes in environments where various types of graupel are generated. Additionally, the potential benefits of the predicted graupel density could be further evaluated in future works through comparison with additional observational data such as sonde and satellite.

20. This is picky semantics, but perhaps consider changing uses of "prognostic graupel density" to "predicted graupel density". The density is being derived from two prognostic variables, but the density itself is not prognostic.

: Thank you for your suggestion to change the term "prognostic graupel density" to "predicted graupel density" to better reflect the derivation process. We agree that this terminology more accurately describes the density as being derived from prognostic variables rather than being prognostic itself. In response to your feedback, we have updated the manuscript to replace all instances of "prognostic graupel density" with "predicted graupel density." throughout in revised manuscript.

Additionally, the title of this paper has been revised as "Introducing Graupel Density Prediction in Weather Research and Forecasting (WRF) Double-Moment 6-Class (WDM6) Microphysics and Evaluation of the Modified Scheme During the ICE-POP Field Campaign"

Technical Comments

21. Line 37 and others: Recommend changing the use of "convections" to the singular "convection"

: Revised accordingly.

22. Line 42: change "cold pools" to "cold pool"

: Revised accordingly.

23. Lines 42-45: You could probably combine these two sentences to just say that bow echoes and squall lines are sensitive to graupel fall speed parameters

: We have explained each of the two references in detail in response to comment 2. Please refer to the response for comment #2 above.

24. Line 49: "modelling" should be "modeling"

: Revised accordingly.

25. Line 56: using "predicted" as second time in front of "rime density" is redundant. Consider removing second usage of the word.

: In response to your comment, we have modified the following sentence:

Line 57: "Morrison and Milbrandt (2015) later developed the Predicted Particle Properties (P3) bulk microphysics scheme that predicts the rime mass fraction, rime volume, and ~~predicted~~ rime density for a single generic ice-phase category."

26. Lines 58 & 60: "ice-one" and "ice-two" doesn't really mean anything to the reader here, and don't appear to be necessary since you're just providing an example. Consider removing these names in parentheses

: Revised accordingly.

27. Line 103: "SBG comprise" should be "SBG comprises"

: Revised accordingly.

28. Line 103: qG seems arbitrarily placed here. One would assume it's the mass mixing ratio but this is not defined and comes after you talk about source/sink processes and before density. Please edit to make this sentence more clear.

: In response to your comment, we have modified the following sentence:

Line 114: " S_{B_G} comprise several microphysical source/sink processes q_G and density of specific hydrometeors (ρ_X) according to Eq. (2)." → "S_{B_G} comprise several microphysical source/sink processes for mass mixing ratio of graupel (q_G) and density of specific hydrometeors (ρ_X), as defined in Eq. (2)."

29. Line 104: It is customary to place the equation directly after mentioning it–otherwise the reader has to look ahead and then go back to read whatever description you've provided. Recommend putting Eq. 2 directly after its first reference and then explaining terms/variables after the equation has been introduced. Same thing for Equation 3.

: Revised accordingly.

30. Line 107: You say ρG can be prognosed, but it's actually qG and BG being prognosed. Recommend changing this to "ρG can be predicted".

: Revised accordingly.

31. Line 114: I don't really see a point to put a "G" subscript on the diameter (D) variable, since diameter is independent of species and these use gamma distributions anyway.

: Revised accordingly.

32. Lines 114-115: The way this is stated is a bit confusing without specifically stating that you are referring to the original scheme. Recommend saying that "Further, cG is treated as a constant in the original scheme since…"

: In response to your comment, we have modified the following sentence:

Line 125: "Further, c_G is treated as a constant since ρ_G is set as a constant (500 kg m^{-3})" → "Further, c_G is treated as a constant since ρ_G in the original WDM6 scheme is set as a constant (500 kg m^{-3})."

33. Line 118: Again, recommend listing Equation 5 right after it is introduced, and then introduce Equations 6 and 7 with the explanations provided after.

: Revised accordingly.

34. Line 127: Again recommending providing Eq. 9 directly after it is introduced.

: Revised accordingly.

35. Line 135: This sentence is a bit confusing because you are saying the density of graupel is "assigned" in ranges in the modified scheme rather than being predicted via Eq. 3. Do you mean that the coefficients in the V-D relationship are derived for a given graupel density range, with the ranges given in Table 2? Please clear this up.

: In response to your comment, we have modified the following sentence:

Line 148: "~~The density of graupel in the modified WDM6 scheme is assigned in the range of 100–900 kg m^{-3} at intervals of 100 kg m^{-3} to facilitate transition between aggregate and rime particles (Straka and Mansell, 2005). Further,~~ a_G and b_G in the V_G–D relationship are derived at the predicted ρ_G, which is in the range of 100–900 kg m^{-3}, at intervals of 100 kg m^{-3} to facilitate the transition between aggregate and rime particles (Straka and Mansell, 2005), using the least-squares method in a log–log space over a range of D_G of 0.3–20 mm (Table 2)."

36. Caption of Figure 1. You reference "Table 1" in regard to the "a" and "b" values, but these are in Table 2.

: Revised accordingly.

37. Caption of Fig. 6: You say values are in units of mm. I think this should be m/s.

: Revised accordingly.

38. Line 174: You say no case was selected for the air-sea interaction category, but you do list this as Case 7 in Table 3, so I don't understand what this sentence means.

: In response to your comment, we have modified the following sentence:

Line 196: "However, no case was selected for the air–sea interaction category because only one event from this category was identified during the ICE-POP field campaign." → "Although Case7 is listed in Table 3 as an air-sea interaction event, it is not selected for detailed analysis because only one event from this category was identified during the ICE-POP field campaign."

39. Line 219: "to 5 km grid" should be "to a 5 km grid"

: Revised accordingly.

40. Line 287: “relative lower” should be “relatively lower”

: Revised accordingly.

41. Line 288: “transported into anvil cloud region” should be “transported into the anvil cloud region”

: Revised accordingly.

42. Caption of Fig. 10: Need to include “wind” after “positive vertical component”

: As previously mentioned in response to comment #3, we have removed Figure 10 in the revised manuscript.

43. Line 304: Do you mean simulated mass mixing ratios? I would also refer to which panel of Fig. 7 you are talking about here, because it’s not clear to me that the two schemes produce similar snow (c,g) and graupel (d,h) for the CL case (c,d). In fact, it seems that the differences between WDM6_FD and WDM6_PD in general are larger for the CL case compared to the WL case.

: To make the meaning of sentence clearly, we have modified the following sentence:

Line 341: “WDM6_FD and WDM6_PD provide similar simulated ratios of surface snow and graupel for the CL case.”→ “The surface snow amount is similar to the surface graupel one in both WDM6_FD and WDM6_PD for CL case.”

44. Line 316: “between two experiments” should be “between the two experiments”

: Revised accordingly.

45. Line 330: I would use mass mixing ratios–mixing ratio alone doesn’t tell us much

: In response to the reviewer’s comment, we have modified ‘mixing ratio’ to ‘mass mixing ratio’ throughout the revised manuscript.

46. Line 351: I would say that the cells develop more extensively in the vertical here.

: In response to your comment, we have modified the following sentence:

Line 398: “As shown in Fig. 8, convective cells develop more extensively in the WL case than in the CL case.”→ “As shown in Fig. 9, convective cells develop more extensively in the vertical direction in the WL case than in the CL case.”

47. Line 361: “As graupel fall quickly” should be “As graupel falls quickly”

: Revised accordingly.

48. Line 362: Again, I’d be careful here to say it’s suppression of graupel generation. Sure, less graupel mass in general throughout the profile would lead to less deposition and sublimation, but I’m not sure it’s fair to say that weaker deposition suppresses graupel generation, but rather that it suppresses graupel growth.

: In response to your comment, we have modified the following sentence:

Line 412: “As graupel fall quickly in WDM6_PD, graupel deposition (Pgdep) decreases, leading to the suppression of graupel generation and sublimation (Pgsub) (Fig. 9c).” → “As graupel falls quickly in WDM6_PD, graupel deposition (Pgdep) decreases, leading to the suppression of graupel growth and sublimation (Pgsub) (Fig. 11b).”

49. Line 365: “fall from a” should be “falls from a”

: Revised accordingly.

50. Line 399: “rage” should be “range”

: Revised accordingly.

51. Line 431: “evolutions” should be “evolution”

: Revised accordingly.

52. Lines 446-447: I think it would be more appropriate to say that “the change in surface precipitation is mainly attributed to the changes in surface snow”

In response to your comment, we have modified the following sentence:

Line 493: “Therefore, the change in surface snow is mainly attributed to changes in the surface precipitation.”→ “Therefore, the change in surface precipitation is mainly attributed to changes in surface snow.”