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.

General/Major Comments

- 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 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.
- 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.

<u>Specific Comments</u>

- 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.
- 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)?
- 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.

- 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?
- 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 a closed budget.
- 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.
- 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.
- 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)?"
- 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.

- 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.
- 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.
- Lines 379-382: This is an important and interesting point! I'd love to see this highlighted more.
- Lines 397-406: This is an interesting result! Really shows the utility of what you've done here, which is great.
- Lines 450-451: Again, I don't think there is convincing evidence of a reduction in the strength of upward motion.
  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)?
- 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.
- 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.

<u>Technical Comments</u>

- Line 37 and others: Recommend changing the use of "convections" to the singular "convection"
- Line 42: change "cold pools" to "cold pool"
- 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
- Line 49: "modelling" should be "modeling"

- Line 56: using “predicted” as second time in front of “rime density” is redundant. Consider removing second usage of the word.
- 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
- Line 103: “S_{BG} comprise” should be “S_{BG} comprises”
- Line 103: q_G 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.
- 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.
- Line 107: You say ρ_G can be prognosed, but it’s actually q_G and B_G being prognosed. Recommend changing this to “ρ_G can be predicted”.
- 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.
- 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, c_G is treated as a constant in the original scheme since…”
- Line 118: Again, recommend listing Equation 5 right after it is introduced, and then introduce Equations 6 and 7 with the explanations provided after.
- Line 127: Again recommending providing Eq. 9 directly after it is introduced.
- 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.
- Caption of Figure 1. You reference “Table 1” in regard to the “a” and “b” values, but these are in Table 2.
- Caption of Fig. 6: You say values are in units of mm. I think this should be m/s.
- 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.
- Line 219: “to 5 km grid” should be “to a 5 km grid”
- Line 287: “relative lower” should be “relatively lower”
- Line 288: “transported into anvil cloud region” should be “transported into the anvil cloud region”
- Caption of Fig. 10: Need to include “wind” after “positive vertical component”
- 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.
- Line 316: “between two experiments” should be “between the two experiments”
- Line 330: I would use *mass* mixing ratios–mixing ratio alone doesn’t tell us much

- Line 351: I would say that the cells develop more extensively *in the vertical* here.
- Line 361: “As graupel fall quickly” should be “As graupel falls quickly”
- 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.
- Line 365: “fall from a” should be “falls from a”
- Line 399: “rage” should be “range”
- LIne 431: “evolutions” should be “evolution”
- 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”