Summary:

This study compares polarimetric dual-wavelength observations of convective cells observed by three radars to simulations conducted using 5 different microphysics schemes on a large statistical basis. As before, I believe the paper is a strong one that is highly relevant to where the field is headed on evaluating NWP output with polarimetric radar observations. I am happy to report that the authors have responded thoroughly to the previously raised objections, with the manuscript being significantly improved with the newly added discussion and caveats. There are still a few minor things that I think need to be clarified, but no major scientific issues remain. As such, pending the addressing of the minor issues below, I believe the manuscript is suitable for publication in Atmospheric Measurement Techniques.

Minor Comments:

1. Line 207: I am still a bit concerned about the language here of snow not being considered to be "spherical" since I think it will imply something about the physical shape of snow (i.e., aspect ratio) within the Thompson scheme to readers which is not what is meant. However, the following sentences do make this a bit clearer. Can this be changed to something along the lines of, "Snow is not considered to have a constant density across the particle size distribution; rather, the mass is proportional to D^2…"?

Changed the sentence to the following:

> *Snow is not considered to have a constant density across the particle size distribution, the mass is proportional to D^2 (b=2) to better fit observations.*

2. Lines 241-245: The same concerns exist here as the previous comment. I understand what the authors mean, but still think invoking the word "spherical" will imply something about the shape of the particles when what is really meant is how density varies with size. Please amend these descriptions in an analogous manner to the previous comment. (Note: the discussion appears to have been appropriately modified on lines 440-450).

We changed the phrasing, following a similar language as in Morrison and Milbrandt (2015):

> *Unrimed ice, grown by vapor diffusion or aggregation, and partially rimed ice have an effective density that is generally less than that of an ice sphere (b=1.9). The parameter a follows an empirical relationship from Brown and Francis (1995) ($a = 0.0121$ kg $m^{-b}$) for unrimed ice and depends on the rime mass fraction $F_r$ for partially rimed ice ($a = 0.0121/(1 - F_r)$ kg $m^{-b}$), i.e., a increases with the rime mass fraction.*

3. Line 257: Please clarify which species is the matrix and which species is the inclusions within the Maxwell Garnett mixture.

Ice is the matrix, air the inclusion (Oue et al., 2020). Text passage adjusted as follows:

> *Solid phase hydrometeors are assumed to be dielectric dry oblate spheroids and are represented as air in an ice matrix. The refractive index hence depends on the hydrometeor density and is computed using the Maxwell Garnet (1904) mixing formula.*

4. Line 324: I am a bit confused – is there anything pertaining to horizontal area of the cores in Figure 4? It appears to just be the height of the top of the 32-dBZ core and the maximum Z within those cores?

Thank you for this catch. We analyzed the horizontal area in an early version of the manuscript but decided that the cell core height is sufficient for the publication (because it is strongly correlated to the cell area). The reference to the area is a remnant from this early version. We removed it.

5. Line 435: This is a minor point, but should Figure B5 really be described as a CFAD, since it is showing a DSD at each height rather than a 2D histogram of single values by height? In other words, isn't this really more like a mean DSD at each level more than a CFAD? Or is it actually the relative frequency of occurrence of any number of particles existing with a given size bin, which then naturally emulates a DSD because larger drops are rarer? Given the use of bulk schemes it isn't clear to me how the latter would work (i.e., what cutoff would be used in the PSD to say a drop exists in a bin).

It is actually showing the relative frequency of occurrence at a given particle size, so it is indeed a CFAD. Using the bulk-parameterization formulas for the rain PSDs, one can calculate the number of droplets for a given diameter. We calculated the number of droplets for the diameters that correspond to the FSBM bins (more specific: the diameter at the geometric center of each bin). By summing up all time steps and grid boxes, we obtain a total number of drops at each height and for each of the bin center diameters. This is visualized as a relative frequency distribution over the height which is, to our understanding, a CFAD. We clarified this passage in the manuscript:

> *The FSBM scheme provides the drop size distributions over a number of size bins, for the bulk schemes we calculated the distributions according to the schemes parameterization. The FSBM bins are approximated by calculating the number of droplets for the geometric centers of the FSBM bins. The calculated number of droplets for the given bin center diameters are then summed up over all time steps and over the grid boxes at each height and visualized as a relative frequency.*

6. Relatedly for Figure B5 (and B3), I am surprised to see any rain at all up to 8-10 km. Are these just within the most powerful updrafts, or are they supercooled water that have somehow reached raindrop sizes? While rare in most schemes and probably more a reflectance of low absolute frequencies, ZDR values for rain of 1-4 dB at 10 km is somewhat surprising (e.g., in the Thompson aerosol-aware and FSBM schemes). (Edited to add: I see now this is discussed on lines 618-620. Despite it being in the Appendix, it may not hurt to move the mention of this potential error to within the main text, although I will leave it up to the authors to decide).

The radar signals from rain at the very upper heights in the FSBM scheme have pretty clearly no physical meaning, because the actual rain mixing ratio at these grid boxes is 0. That's why we don't think it is worth to discuss in the main text which would, in our opinion, distract from the more important aspects. This is why we think the appendix is better suited for this discussion and we left it as it is.

7. Lines 443-445: It appears to me like graupel is dominating the reflectivity at those heights (i.e., Figure B1 vs. B2) rather than rain as stated, since the overall ZDR will be weighted by each species' reflectivity. From the CFADs, the graupel Z is typically on the order of 10-40 dBZ while the rain Z is -15 to 10 dBZ around 4 km AGL. I do think it's true that the sparse appearance of higher ZDR above the melting layer is due to rain rather than graupel, but overall I still think graupel dominates the ZDR signature above the ML. (Edited to add: when this was summarized on line 567, it was clearer that the authors were referring to rain dominating the sparse but large values of ZDR above the ML while graupel dominates the bulk of the signal overall. Perhaps the earlier discussion could be rephrased to make it clearer that the anomalously high ZDR at these levels are predominantly rain rather than overall).

Yes, we were referring to the sparse but large ZDR values. These values are of course not dominating the whole signal which is dominated by graupel as correctly stated in this comment. We rephrased this part to remove the misleading phrasing:

> *The signal directly above the melting layer height is generally dominated by graupel, which has the highest reflectivity signal (see Appendix B for separation by hydrometeor class) and is associated with ZDR values of 0, due to the assumed aspect ratio of 1 in the forward simulation. The sparse but large values of ZDR in the two Thompson and the P3 scheme are predominantly caused by rain, likely lifted by strong updrafts in the convective situations.*

8. Line 539-540: It is interesting that the overall Z is too high in the simulations below the ML which suggests drops that are too large (in agreement with the observed ZDR biases in this region), but the DWR below the melting layer are too low suggesting that raindrops exiting the ML are too small in the simulations. How do the authors reconcile those two results?

Comparison of the simulated and observed DWR CFADs must be done cautiously because the first DWR CFAD does not include simulated attenuation while the observed radar signal is naturally attenuated. Including the simulated attenuation increases the simulated DWR by a lot (as shown in the second DWR CFAD). We therefore think the conclusion that simulated rain drops exiting the ML are too small based on the uncorrected DWR CFAD should not be drawn. We changed the corresponding sentence:

> *The DWR directly below the melting layer height is very different between the models and the observations. However, including attenuation increases the simulated DWR and its variability, making it difficult to quantify DWR deviations between model and observations as discussed below.*

Typos/Grammar/Errata:

1. Line 52, 54: “super cell” → “supercell”
Changed as suggested.

2. Line 121: “dual-polarimetric” → “polarimetric”
Changed as suggested.

3. Line 257: “Maxwell-Garnet” → “Maxwell Garnett”
Changed as suggested.

4. Line 435: Missing )
Added.

5. Line 617: “interesting” → “relevant” or “pertinent”
Replaced “interesting” with “relevant”.

**References**

Brown, P. R. and Francis, P. N.: Improved measurements of the ice water content in cirrus using a total-water probe, Journal of Atmospheric and Oceanic Technology, 12, 410–414, 1995.

Maxwell-Garnet, J. C.: Colours in metal glasses and in metallic films, Phil. Trans. R. Soc. Lond, A, 203, 385–420, 1904.

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Oue, M., Tatarevic, A., Kollias, P., Wang, D., Yu, K., and Vogelmann, A.: The Cloud-resolving model Radar SIMulator (CR-SIM) Version 3.3: description and applications of a virtual observatory, Geoscientific Model Development (Print), 13, 2020.