Title: Evaluation of convective cloud microphysics in numerical weather prediction model with dual-wavelength polarimetric radar observations: methods and examples

Authors: Gregor Köcher, Tobias Zinner, Christoph Knote, Eleni Tetoni, Florian Ewald, and Martin Hagen

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

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).
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).
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?

Typos/Grammar/Errata:

1. Line 52, 54: "super cell" → "supercell"
2. Line 121: "dual-polarimetric" → "polarimetric"
3. Line 257: "Maxwell-Garnet" → "Maxwell Garnett"
4. Line 435: Missing)
5. Line 617: "interesting" → "relevant" or "pertinent"