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

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doi: 10.5194/amt-2021-299

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. The study is well-motivated and described with clear, informative figures and has the potential to be very well-received as it is a very topical study. The study also benefits from its large sample size (versus individual case studies done in the past). However, in addition to a few changes for clarity and requested further exploration, I have concerns about some of the analysis and conclusions drawn, contributed to by both vagueness of the details of the radar operator and the understood assumptions about the microphysical schemes employed. In addition, some of the analysis seems to rely on simplifying assumptions/conjecture that could potentially be resolved by including additional info from the simulations (e.g., PSDs) besides the bulk polarimetric quantities. Because of the fundamental nature of these concerns, I recommend major revisions before publication in AMT.

Thank you for your detailed explanation of your concerns. You were right about our partial misunderstanding on assumptions on model and forward simulator side in our conclusions. We have re-examined our analysis and updated many parts with the new found understanding. Please see our point-by-point response as below.

Main comments:

1. I really think much more information is needed about the forward polarimetric radar operator applied. Even though a citation is given, the realism of the assumptions made about the treatment of 1) particle shapes, 2) particle orientations, and 3) dielectric constants (especially during multi-phase environments, such as melting), etc. could really strongly influence the resultant simulated polarimetric radar variables and thus deserves to be fleshed out here. None of this uncertainty (or the inevitable reduction in variability inherent in applying fixed relations within an operator like this) is currently acknowledged or taken into account in the subsequent analysis. Finally, the lack of details about things like aspect ratio relations provided complicates the understanding of other parts of the discussion, such as

raised in the following comment.

This is a topic that the first reviewer also commented on. We have extended the CR-SIM section with information about the assumptions concerning particle shapes, orientations and dielectric constants in section 2.4. We reevaluated our subsequent analysis to acknowledge the uncertainties as a result of the forward simulator, as well as relate our findings to details of the forward simulator. This includes multiple parts in section 3.2, 3.3, 3.4 and 4. Find below our answer to the first reviewer:

We have not been clear on the assumptions of the radar forward model (CR-SIM). The section of CR-SIM (2.4) has been appended with the assumptions that CR-SIM is making regarding particle shapes, particle orientation and dielectric constants for each microphysics schemes:

> *The dielectric constant of water is 0.92. Solid phase hydrometeors are assumed to be dielectric dry oblate spheriods and are represented as a mixture of air and solid ice. The refractive index hence depends on the hydrometeor density and is computed using the Maxwell-Garnet (1904) mixing formula. There are no mixed phased particles simulated. This means mixed phase radar signatures (for example the "bright band", Austin and Bemis, 1950) will not be reproduced by the simulation. In order to simulate polarimetric radar observables, a radar forward simulator must assume particle shapes and particle orientation. The particle orientation assumptions are the same for all schemes. It is assumed that the particle orientations are 2D Gaussian distributed with zero mean canting angle as in Ryzhkov et al. (2011). The width of the angle distributions is specified for each hydrometeor class: 10° for cloud, rain, and ice and 40° for snow, unrimed ice, partially rimed ice, and graupel. Regarding the shape assumptions, cloud droplets are simulated as spherical (aspect ratio of 1) and raindrops are simulated as oblate spheriods with a changing axis ratio dependent on the drop size according to Brandes et al. (2002) in all schemes. For ice hydrometeor classes, the same aspect ratio assumptions are applied for all schemes except the P3 scheme: cloud ice is assumed as oblate with a fixed aspect ratio of 0.2. Snow is assumed as oblate with a fixed aspect ratio of 0.6. Graupel is assumed to be oblate with an aspect ratio that is changing from 0.8 to 1, dependent on the diameter and according to Ryzhkov et al. (2011):*
>
> $$ar = 1.0 - 0.02 \qquad \text{if } D < 10\ \text{mm}\,,$$
> $$ar = 0.8 \qquad \text{if } D > 10\ \text{mm}\,.$$
>
> *The P3 scheme does not provide the standard ice hydrometeor classes. Instead, the aspect ratio of small ice (spherical, fixed aspect ratio of 1), unrimed ice (oblate, fixed aspect ratio of 0.6), partially rimed ice (oblate, fixed aspect ratio of 0.6) and graupel (spherical, fixed aspect ratio of 1) is assumed by CR-SIM. This means in comparison to the other schemes that the P3 simulation deviates for small ice (aspect ratio of 1 in P3, while cloud ice in other schemes is assumed to have an aspect ratio of 0.2) and graupel (0.8 - 1 in other schemes, while graupel particles in P3 are assumed to have an aspect ratio of 1). Resulting differences in the radar signal are discussed in the result section 3 whenever it might influence the simulated radar signal.*

We further added a small abstract in the conclusions acknowledging uncertainties produced by the radar forward operator:

> *Furthermore, there are uncertainties connected to the radar forward simulator applied. To calculate scattering characteristics, assumptions have to be made including the particles aspect ratio, orientations, shape and more. The variability of the simulated signals is reduced by applying fixed relations compared to the potential variability of shapes, orientations and aspect ratios in nature. In addition, the radar forward simulator applied in our study does not consider mixed phase particles. This means that, e.g., effects such as the bright band where particles melt cannot be reproduced by the simulations. To circumvent some ambiguities introduced this way, the comparison could be extended from radar signal space to cloud hydrometeor space. I.e., retrieved hydrometeor classes can be compared to simulated ones.*

Finally, we added multiple parts in the discussion and conclusions that relate forward simulator details to the results:

Section 3.2

> *Given that the forward simulator applied in this study does not consider wet particles, we find the high bias in Z exists even without considering wet graupel and comes mostly from rain, suggesting PSDs that contain too many large rain drops compared to the observations.*

Section 3.3:

> *Furthermore, 3) the observed variability of ZDR is possibly not correctly captured by the radar forward simulator which has to assume fixed distributions of particle orientations as well as a fixed aspect ratio of the particles.*

Section 3.3:

> *All schemes assuming spherical cloud ice or with other dominating spherical hydrometeor classes at these heights show small ZDR. This is true for the P3 small ice fraction for which the forward simulator assumes spherical aspect ratio of 1. In the Thompson schemes, the assumed aspect ratio by the forward simulator is 0.2, suggesting that other hydrometeor classes with lower ZDR like snow or graupel dominate the signal. Only for FSBM and Morrison (aspect ratio 0.2) cloud ice dominates the signal. The stronger signal in FSBM and Morrison is not a result of different density assumptions, because both, the FSBM and Morrison scheme assume lower density of cloud ice compared to Thompson. The observations do not show increased ZDR at these heights. This could either mean that 1) there are no large cloud ice particles observed, 2) that the signal is dominated by other more spherical particles in the observations, or 3) that the assumed aspect ratio of 0.2 by the radar forward operator is unrealistic and the observed particles are more spherical in nature.*

Section 4:

> *This could either be a result of simulated cloud ice particles being too large or too many, but this could also be a result of the assumed flat cloud ice shape with an aspect ratio of 0.2.*

2. There seems to be some confusion about the nature of the microphysics schemes employed that influences some of the manuscript's analysis and main conclusions. The primary issue is with regard to the Thompson microphysics scheme, although similar language/conclusions permeate the paper. The authors state on line 186 that snow is "not considered to be spherical" in Thompson in contrast with other schemes, which treat particles as spherical. An examination of the Thompson et al. (2008) manuscript indeed sees similar language employed to describe the scheme, which has a mass-size exponent that differs from the "spherical" value of 3. This value, of course, comes from the volume of spherical particles (D^3) multiplied by a density that varies inversely with diameter (D^{-1}), as stated in the abstract of Thompson et al. (2008), leading to an ultimate dependence of mass on D^2. However, despite the language used concerning this exponent, which upon reflection I now consider a misnomer, this does not actually ensure that the treatment of the particles is non-spherical in the physical shape sense. In fact, I am not aware of any operational microphysics scheme that actually explicitly predicts the shape of the snow with the exception of the FSBM (and possibly the P3?), that uses fixed aspect ratio-size relations to evolve particle shape as mass is gained/lost (see A1 in Shpund et al. 2019). However, other schemes may implicitly incorporate some shape information through things like the capacitance term in the deposition/sublimation rate equations, etc (for example, this is done in Thompson; see Deposition/sublimation section in the Appendix in Thompson 2008), but it isn't clear that this implicit information is actually being used by the radar operator. Hence, similar language about other schemes (e.g., Line 221 about "non-spherical" snow in the P3) is also potentially misleading.

   In fact, the shape assumptions are applied in the forward simulator and this has to be made clear. We have added this information as in our answer to major point 1. We then carefully reevaluated our discussion and conclusions to remove the misleading language.

This confusion in framing/treatment leads to incorrect assumptions further on, such as line 360 where it is stated that the Thompson scheme actually treats snow as "oblate" particles in a way that would actually affect scattering amplitudes at different polarizations. That is, to my understanding, only something that would be specified within the forward polarimetric radar operator, which is why it is important to include details of how shapes, etc. are being handled as per Main Comment 1. One could envision two alternative scenarios in conflict with these ideas: a model scheme that considered snow to be "spherical" (in the Thompson parlance) for microphysical purposes that has a constant density and an m-D relation with an exponent of 3 but that in the radar operator is assigned an aspect ratio < 1 that results a ZDR > 0 dB. Alternatively, one could have an m-D relation with an exponent of 2 that was "nonspherical" (in the Thompson parlance) but that in the radar operator treated all snow as spheres regardless of the density varying across the size spectrum, resulting in a ZDR of 0 dB regardless.

As a result, the assertion on line 359 that the particles are being treated as “spherical” in the FSBM scheme is the reason for its poor ZDR observational agreement is (to my understanding) necessarily incorrect, as 1) shape *is* predicted in the FSBM via size-shape equations, 2) the inverse-dependence of density on particle diameter for snow is also taken into account in the FSBM, so it is “non-spherical” even in the D^2/Thompson parlance, but also 3) this also leads to the incorrect conclusion that that is related to why the simulated ZDR is 0 with no mention of the radar operator. If snow and ice particles were in fact treated as spheres in the radar operator, where the ZDR calculations are actually being performed, every single ZDR value aloft would be 0 dB, but we do see spread apparent in the CFADs even in the FSBM and Morrison schemes, which can’t be explained by this theory of spherical treatment in the microphysics scheme. All of this also obfuscates the role that density and the assumed PSD form in each scheme are likely playing in the spread of ZDR values aloft, which are hardly discussed at all in the manuscript’s results section. I believe much of the analysis needs to be re-examined in light of these understandings.

The concerns connected to our conclusions, especially regarding the ZDR signals are clearly justified and we understand that we misunderstood the role of the microphysics schemes in contrast to the role of the radar forward simulator. We reevaluated our discussion and our conclusions in light of the new understanding. In general, we now attribute the differences between the schemes more clearly to differences in the underlying particle size distributions or to density assumptions of the radar forward operator. Major changes are made to the discussion and conclusion in section 3.2, 3.3, 3.4 and 4. Please see our marked-up version for the details and our answers to the corresponding specific comments.

Specific comments:

1. Line 105: Were cases chosen in any systematic way (e.g., precipitation intensity, coverage, etc) or just randomly throughout the 2019 and 2020 seasons?

   The days were not chosen systematically. We were measuring when convective precipitation was forecast.

2. Line 115: Is there a reason KDP was neglected in the analysis? It is available from CR-SIM and would provide important additional information for contextualizing the differences between observations and simulations. Otherwise please include an explanation of why the study was limited to Z/ZDR/DWR.

   The main reason is that our KDP observations are very noisy. We added this as an explanation to the manuscript in section 3.3:

   > *We found KDP to provide not much additional value, in part due to noisy observations, which is why we neglect KDP in the subsequent analysis.*

Apart from this, we could not find additional information from the KDP CFAD that was not already visible in the ZDR CFAD. See the KDP CFADs below:

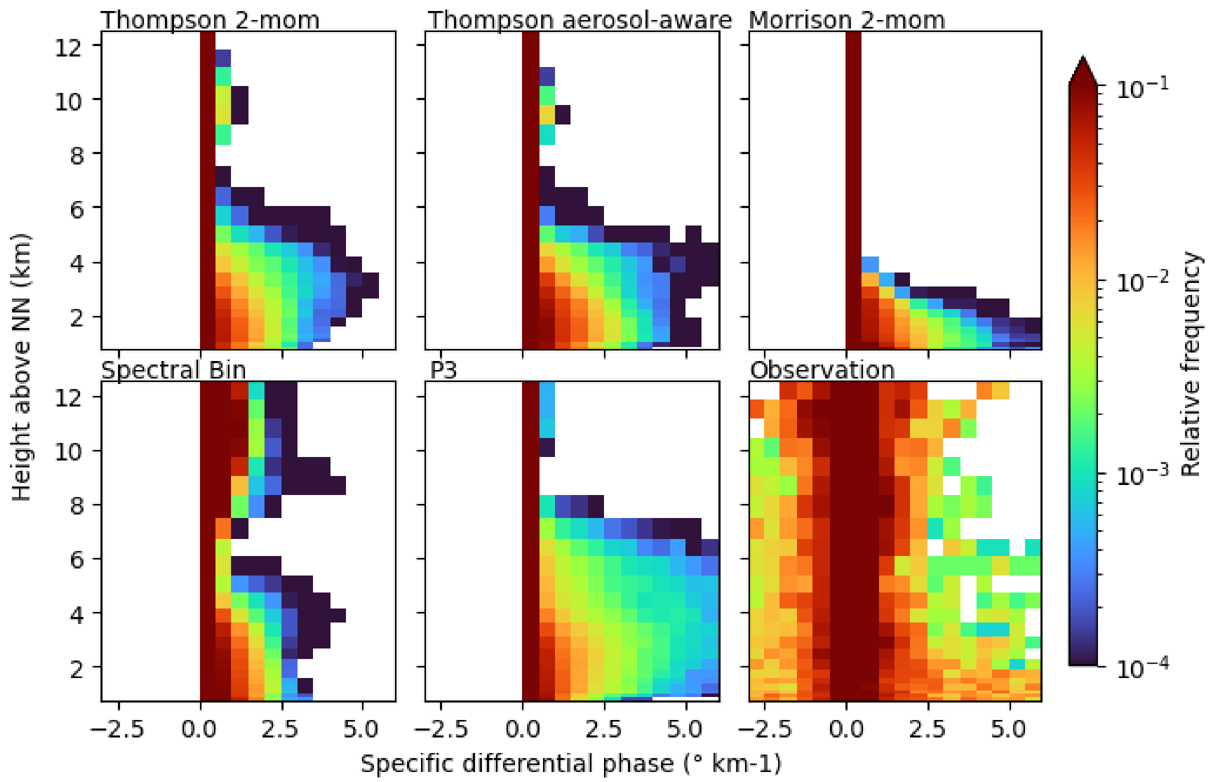


Figure 1: CFADs of simulated and measured specific differential phase over 5 convective days in 2019. Radar observations with Poldirad.

3. Section 2.1: Can information about any efforts for radar calibration be included, particularly for the non-operational research radars? Poor calibration could, in theory, affect both Z and especially ZDR.

   Yes, we added the following passage to section 2.1:

   > *The absolute calibration of reflectivity Z of Poldirad is estimated to have an error of ± 0.5 dB from calibration with an external electronic calibration device (Reimann, 2013) while the reflectivity error of Mira-35 is estimated to be ± 1.0 dB (Ewald et al., 2019). We estimate Poldirad ZDR to have an offset of about 0.15 dB from measurements in a liquid cloud layer where ZDR near 0 is to be expected. This offset is corrected before any of the subsequent analysis is done.*

   The ZDR offset was not corrected in the previous version, we corrected this for the current version of the manuscript.

4. Line 132: It isn’t clear how far away from the radar these cells typically were when being scanned, but was any effort made to correct the ZDR values to account for high-elevation scans in the RHIs?

   We assume this comment refers to the different viewing angle towards particles when doing high-elevation scans instead of low-elevation scans (The beam illuminates particles from below, instead of the side which results in different ZDR values). The CR-SIM radar simulator is able to simulate the radar position and simulates the viewing geometry, so this is accounted for in our simulations. It should be noted that in the vast majority of our cases, we looked at the convective cells from the side, i.e., the Poldirad elevation angle was never above 40°.

5. Line 138: Was the cell movement just a simple extrapolation of storm centroids, or done by visual inspection?

   Yes, it was a simple extrapolation. We added this information to section 2.1:

   > *This cell movement is projected using two previous Poldirad overview PPI scans by calculating the displacement at which the cross-correlation between the two PPI images is at maximum.*

6. Line 155: Please include UTC conversions and list in terms of LST instead of ‘am/pm’.

   We were actually referring to UTC (am/pm was misleading). We clarified this in the manuscript:

   > *The spin-up always starts at 18 UTC (20 LST) on the previous day. Thus, the 24 hour forecast exactly covers the day of interest (0 - 24 UTC).*

7. Line 156: What exactly is meant by ‘nudging’ from the GFS? Was the entire (large-domain) model background replaced with the new GFS analysis, or was it incorporated/assimilated somehow? Or did this only apply to the boundary conditions?

   By nudging, we mean the WRF option for grid analysis nudging which appends a nudging term to the prognostic equations of temperature, humidity and wind that “nudges” the WRF grid value towards the GFS grid value. This is done for all grid points of the large-domain but it is not a replacement, because the nudging term is much smaller than the physical WRF terms (Nudging coefficient of 0.0006 s^{-1} in our model runs, see namelist.wrf in supplement). Nudging in the planetary boundary layer was turned off, it was applied only above the PBL. We added more detail to the manuscript:

   > *The parent Europe domain is nudged towards the global GFS data, by appending a nudging term to the prognostic equations for humidity, temperature and wind that*

"nudges" the WRF grid value towards the closest GFS grid value for each grid point of the Europe domain above the planetary boundary layer (grid analysis nudging).

8. Line 168: It may help to specify which D is being referred to: maximum diameter, equivolume diameter, etc.

   It is referring to the maximum diameter. This is now specified.

9. Line 179: Just for clarity, I would add "fixed" before non-zero mu just to make clear it is not a free parameter.

   Added as suggested.

10. Line 238: How was this 32 dBZ threshold chosen and why? Is this the default TINT value?

    32 dBZ is a threshold at a common magnitude to identify convective storms. See for example: Dixon and Wiener (1993), 35 dBZ; Muñoz et al. (2018), 35 and 40 dBZ; Han et al. (2009), 35 and 40 dBZ; Jung and Lee (2015), 35 dBZ; Kober and Tafferner (2009), 37 dBZ; Johnson et al. (1998), 30, 35, 40, 45, 50, 55, 60 dBZ. We added the following part to section 2.5:

    > *32 dBZ is at a common magnitude to identify convective storms (e.g., Dixon and Wiener, 1993; Jung and Lee, 2015). Higher thresholds potentially miss moderate or weaker convective cells, while lower thresholds will misidentify more non-convective echos as convective cells.*

11. Line 254: When there are multiple Cartesian input grid points for a given target spherical grid, how are they "all included"? Means? Median? Weights? Etc.

    If multiple grid points fall into the same spherical grid, they are weighted by the distance to the radar volume center. We appended the sentence in the manuscript section 2.6 with this information:

    > *I.e., for the interpolation to a grid point of the target spherical grid, all Cartesian input grid points that are within the beam width are included with a weight depending on the distance to the radar volume center.*

12. Line 284: It isn't clear to me exactly why 32 dBZ is used as a threshold here – it seems much larger than most studies. I understand 32 dBZ is used for TINT (as per comment 10),

but once a cell is identified can't a lower Z threshold be chosen for cloud/echo top height? Similarly, I am not sure I like the use of 'echo top' here given such a high reflectivity threshold, as this normally applies to thresholds like -10 dBZ or 0 dBZ while 32 dBZ is solidly in the middle of many convective cells. By using "echo top height", it implies something about the depth of the simulated storms, while in actuality the trends seen seem to just reflect high biases in the simulated Z throughout the depth of the cells. Consider alternate language.

The 32 dBZ threshold comes from the TINT cell-tracking and is at a common or even slightly lower value than used in most studies (see answer to specific comment nr. 10). The height of the identified convective core is an output of TINT and hence very straightforward to analyze. That's why we prefer to use the 32 dBZ height instead of using a lower threshold which we would have to do manually after applying the TINT cell tracking. However, we understand that the use of 'cloud top height' is a misleading language, as we use the height of the cell core (or 32 dBZ echo top height) in reality, which is of course not the cloud top height. We changed the language to avoid the use of 'cloud top height' and instead use 'cell core height' or '32 dBZ echo top height'. This was applied throughout the manuscript at multiple occasions.

13. Line 305: This entire paragraph seemed a bit random and out of place to me. The results here are never compared to the findings of Caine et al. (2013), and the subsequent defense of the study has already been thoroughly provided earlier in the paper.

Caine et al. (2013) was using a similar method to compare convective cell characteristics in NWP model and radar observation. That's why we think it is worth to discuss. However, the paragraph is indeed a bit out of place and we never discuss their findings. We moved the paragraph to the actual comparison of the cell geometry, which was done in the two paragraphs before. Furthermore, we removed the part were we defend our study and instead compare our results against theirs (section 3.1):

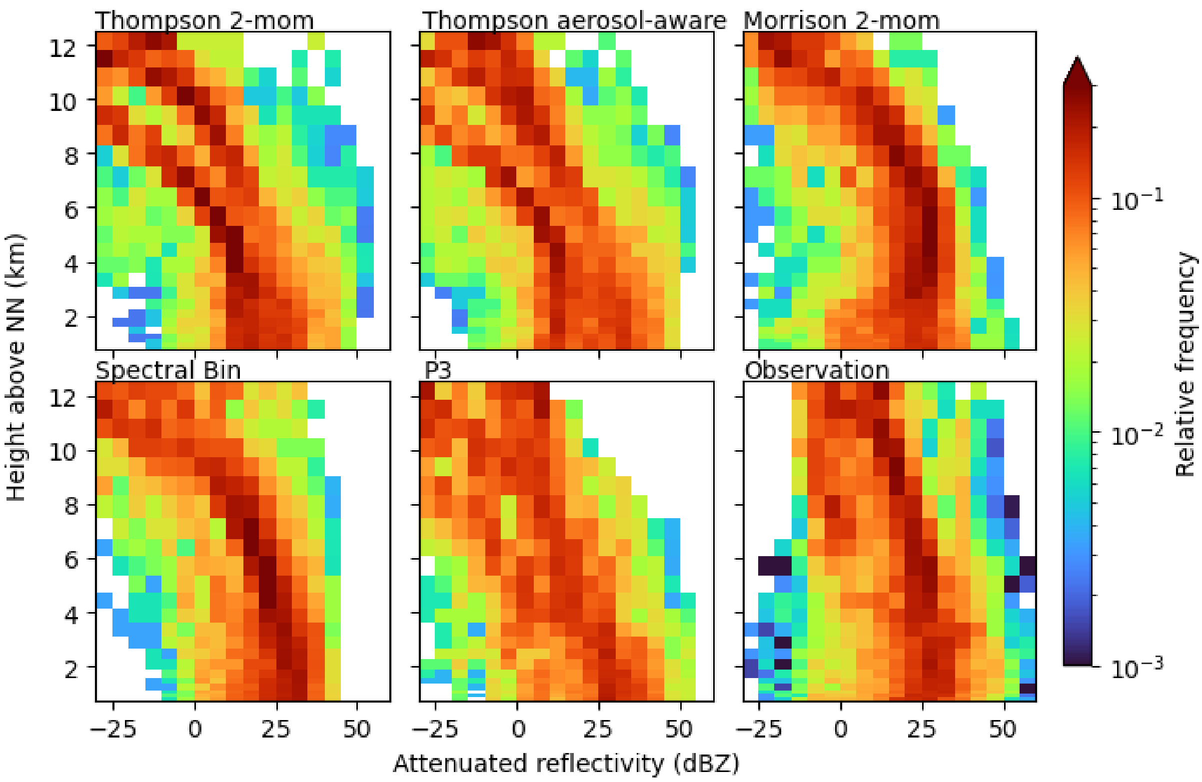
> *A similar approach to compare cloud geometry in simulation and radar observation was followed in Caine et al. (2013). They objectively compare simulated cell characteristics with observations over 4.5 days after applying a cell-tracking algorithm on their data. Among other things, they found the simulated convective cells to reach higher altitudes on average compared to their radar observations, which is also visible in our analysis. This is independent of the chosen cloud microphysics scheme and mainly a result of the missing small-scale cells in the simulations which is indicative of a resolution effect: the very small cell heights correspond to tiny cells that we might not be able to resolve even with our 400 m grid spacing.*

14. Lines 319-324: It still isn't clear to me if a bias may be being introduced here due to the RHI scanning strategy. Were the +/- 2 deg RHI scans typically still within the precipitation core or on the edges/ flanks of the cells? With all simulated columns being included in the

CFADs it almost seems inevitable that more weak precipitation regions would be captured that way?

The cells typically were not very far away (always closer than 24 km to Mira35), so even with the +/- 2 deg we were typically still within the precipitation core. However, we also analyzed the model data with the center profile only, see below. The model data is then much more noisy, due to the smaller amount of data points. This demonstrates, however, that the differences between radar and model could be in part a result of the radar noise, which is why we added a paragraph in section 4 to acknowledge this uncertainty:

> *Finally, there is more noise in our radar statistics compared to the simulation statistics (for example Figure 5) due to the lower number of data points available from the observations. This could partially explain biases between model and radar, reminiscent of the large observational effort to statistically compare convective cloud characteristics.*



15. Line 332: I know this probably varied among cases but including an approximate ML height here may be useful.

We added this information:

> *While most schemes exhibit a smooth transition from ice to liquid phase, the prominent exception is the P3 scheme for which reflectivities abruptly increase by*

about 15 dBZ at the melting layer height (approximately at 3.6 km height, varies among cases)

16. Line 335: While the distributions are certainly broader and extend to higher Z values above the ML, the medians for most schemes still appear quite close to the median in the observations to me.

At that point, we were referring to the higher Z values that most schemes extend to above the melting layer height. We rephrased the sentence for clarification (section 3.2):

> *Most other schemes directly above the melting layer height extend to higher reflectivities, showing reflectivities greater than 25 dBZ too often.*

17. Line 338: Were PSDs actually examined? It says “(not shown)”, but this may be helpful to include. While it is definitely plausible that the graupel produced is too large, could it also be that there’s just too much riming in general (so the particle density is the problem, not its size)?

We were referring to the high reflectivity above the melting layer height which we analyzed by looking at reflectivity CFADs from the single hydrometeor classes. These have shown that the high reflectivity areas above the melting layer height correspond to graupel. We added an Appendix B where we added the corresponding HM-class CFADs, together with this text passage:

> *CR-SIM calculates radar signals for the single hydrometeor classes independently, next to the total signal of all hydrometeors together. Below are CFADs of the signals calculated from the most interesting hydrometeor classes. The CFADs are shown on the original WRF grid and without attenuation correction. The FSBM simulation sometimes showed spurious rain signals at the highest levels (> 10 km). Sometimes there are small numbers of rain drops are present in the largest bins, even though the mixing ratio of rain is 0 in the FSBM simulation. We consider this an error with no physical meaning.*

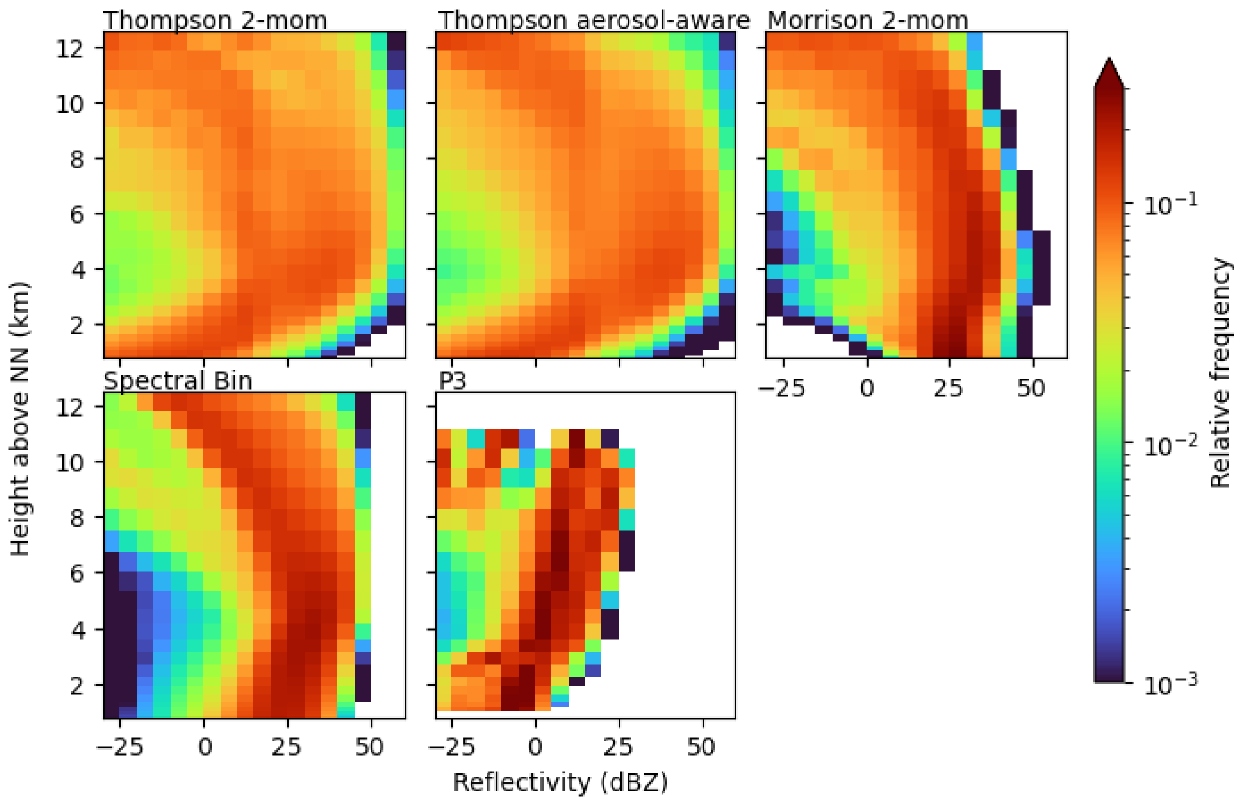


Figure 2: CFADs of simulated reflectivity of the graupel hydrometeor class over 5 convective days in 2019.

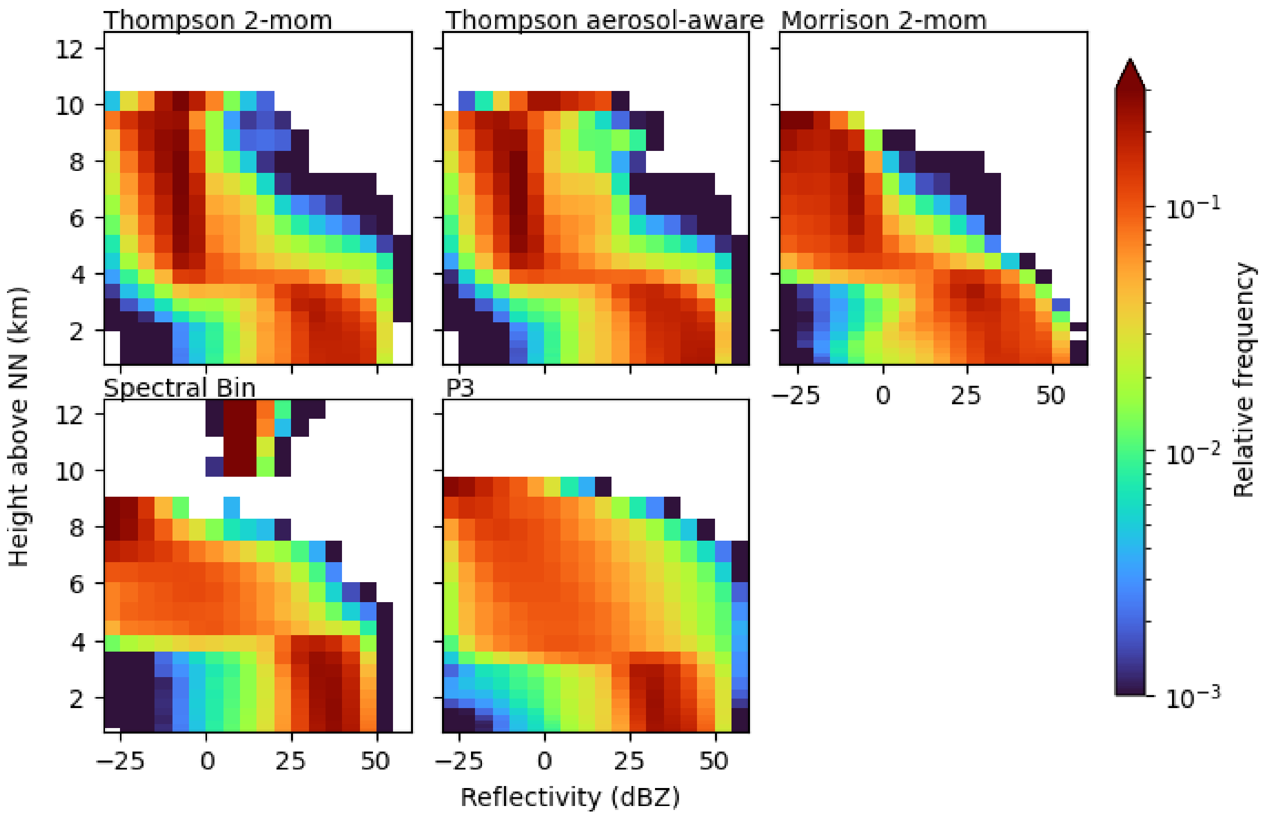


Figure 3: CFADs of simulated reflectivity of the rain hydrometeor class over 5 convective days in 2019.

However, it is true that our conclusion that graupel must be too large is not necessarily correct: the high reflectivity could also be a result of too dense graupel particles. With our setup, we are not able to distinguish if the density or the size is the problem, but we reevaluated our conclusions (as for main comment 2) to include the possibility that the density could also be the problem. We reworked the part concerning this comment as follows:

> *These extreme reflectivity values are produced mostly by graupel and to lesser extent by rain (see Appendix B for CFADs of radar signals separated by hydrometeor classes). Compared to our measurements these reflectivities are unrealistically large. A high bias in reflectivity could be produced in principle by three mechanics: the simulated particles are 1) too dense, 2) too many, or 3) too large. The graupel densities assumed by the schemes (and correspondingly in the forward simulator) are 500 kg m^{-3} in the Thompson schemes and 400 kg m^{-3} in the Morrison and FSBM scheme. The higher graupel density could explain the higher bias seen in the Thompson scheme compared to the moderate bias in Morrison and FSBM, but the underlying PSD could also play a role.*

18. Line 355: While I have no doubt in general that the flexibility of the FSBM is aiding its ability to reproduce realistic ZDR values, have other factors been considered, such as the different treatment of drop breakup among schemes, etc?

    We have not considered other factors and added this as an information to the manuscript (section 3.3):

    > *However, contributions by other microphysical processes, such as drop breakup or evaporation could also facilitate the ZDR signatures and were not examined in this study.*

19. Lines 357-362: In addition to the issues raised in the main comments regarding lines 359-360, how much of the differences (for example, the narrowness of the ZDR distributions) between schemes is due to differences in the calculated differential attenuation versus differences in ‘intrinsic’ variables that affect ZDR, such as shape and density? In general the differential impact on density needs to be explored. The profound differences in ZDR at high altitudes between the FSBM/Morrison scheme and the Thompson/P3 schemes also deserves to be explored.

    The calculated differential attenuation does not have a major impact. See below the CFADs with and without simulated attenuation:

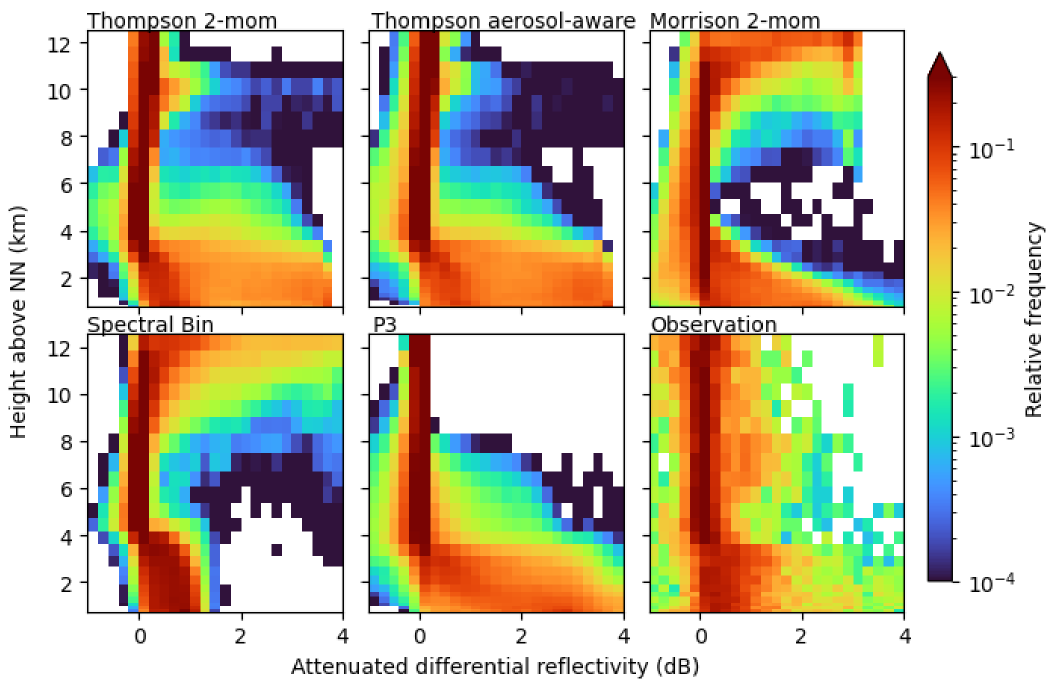


Figure 4: CFADs of simulated and measured differential reflectivity over 5 convective days in 2019. Observation with the Poldirad radar.

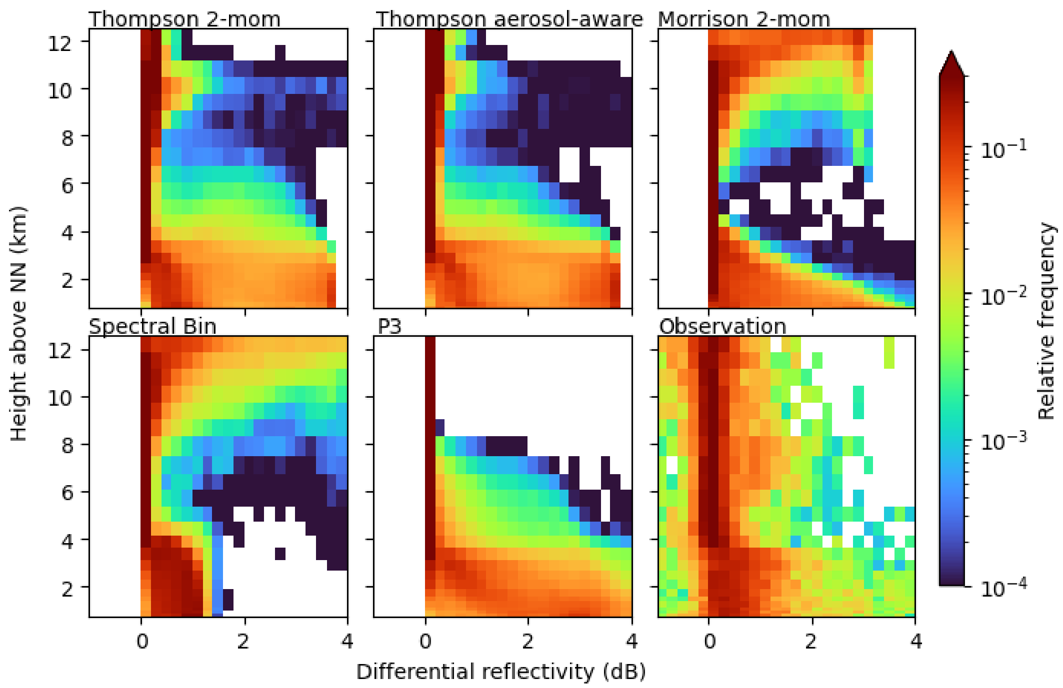


Figure 5: CFADs of simulated and measured differential reflectivity over 5 convective days in 2019 without simulated attenuation correction. Observation with the Poldirad radar.

Regarding the profound differences at higher altitudes, we added a text passage in section 3.3 together with the cloud ice CFAD in the appendix:

> *At upper levels clear differences between Morrison/FSBM and Thompson/P3 can be seen. Morrison and the FSBM scheme show ZDR values of up to 4 dB at these heights while the Thompsons and the P3 schemes are close to 0 dB. Here, the high ZDRare caused by cloud ice (see Appendix B for CFADs of radar signals separated by hydrometeor classes). All schemes assuming spherical cloud ice or with other dominating spherical hydrometeor classes at these heights show small ZDR . This is true for the P3 small ice fraction for which the forward simulator assumes spherical aspect ratio of 1. In the Thompson schemes, the assumed aspect ratio by the forward simulator is 0.2, suggesting that other hydrometeor classes with lower ZDR like snow or graupel dominate the signal. Only for FSBM and Morrison (aspect ratio 0.2) cloud ice dominates the signal. The stronger signal in FSBM and Morrison is not a result of different density assumptions, because both, the FSBM and Morrison scheme assume lower density of cloud ice compared to Thompson. The observations do not show increased Z DR at these heights. This could either mean that 1) there are no large cloud ice particles observed, 2) that the signal is dominated by other more spherical particles in the observations, or 3) that the assumed aspect ratio of 0.2 by the radar forward operator is unrealistic and the observed particles are more spherical in nature.*

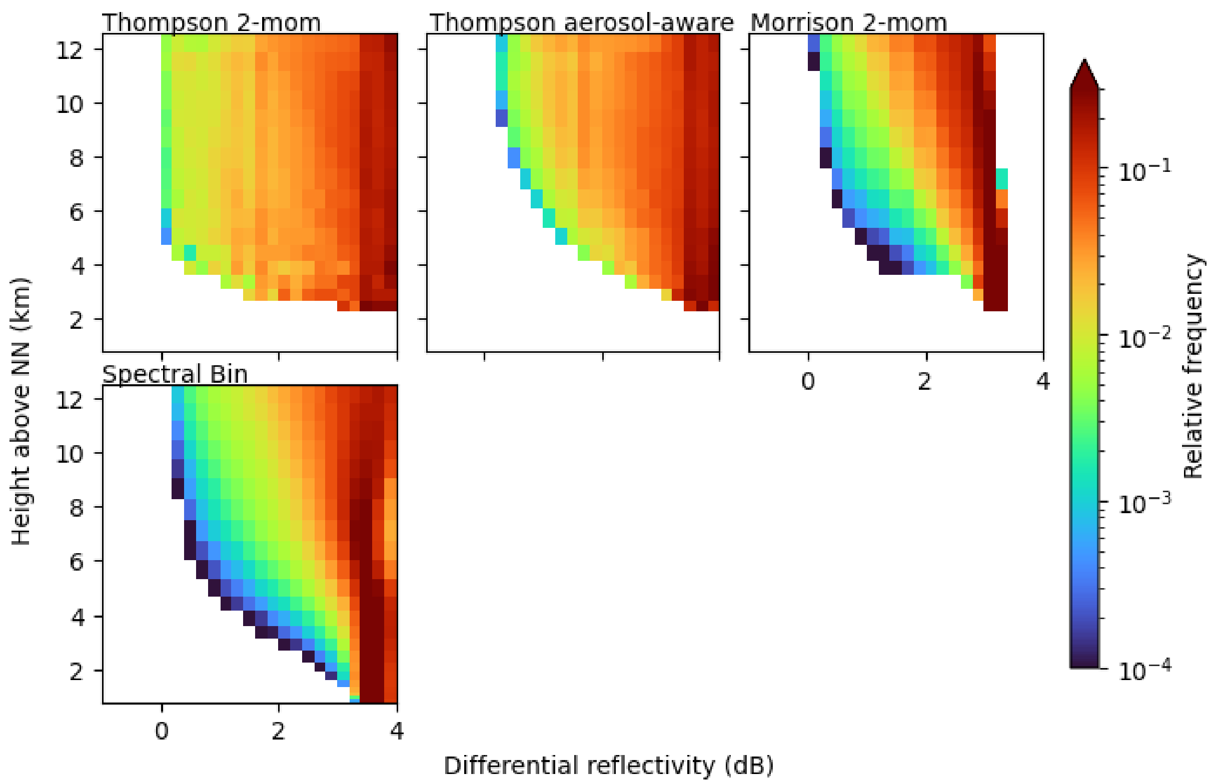


Figure 6: CFADs of simulated differential reflectivity of the cloud ice hydrometeor class over 5 convective days in 2019. The P3 scheme does not provide the classical cloud ice category.

20. Line 366: While size is definitely the main factor, I would not diminish the role that density plays in determining the resonance parameter and thus whether non-Rayleigh scattering is occurring. (Although, of course, in these simplified schemes density is at best a simple function of size, so it isn't a free parameter…)

We have appended the corresponding sentence in section 3.4:

> *In contrast, DWR is rather sensitive to the particles size. In principle, it is also sensitive to the particle density, but the simulated density is assumed to be constant or a function of particle size.*

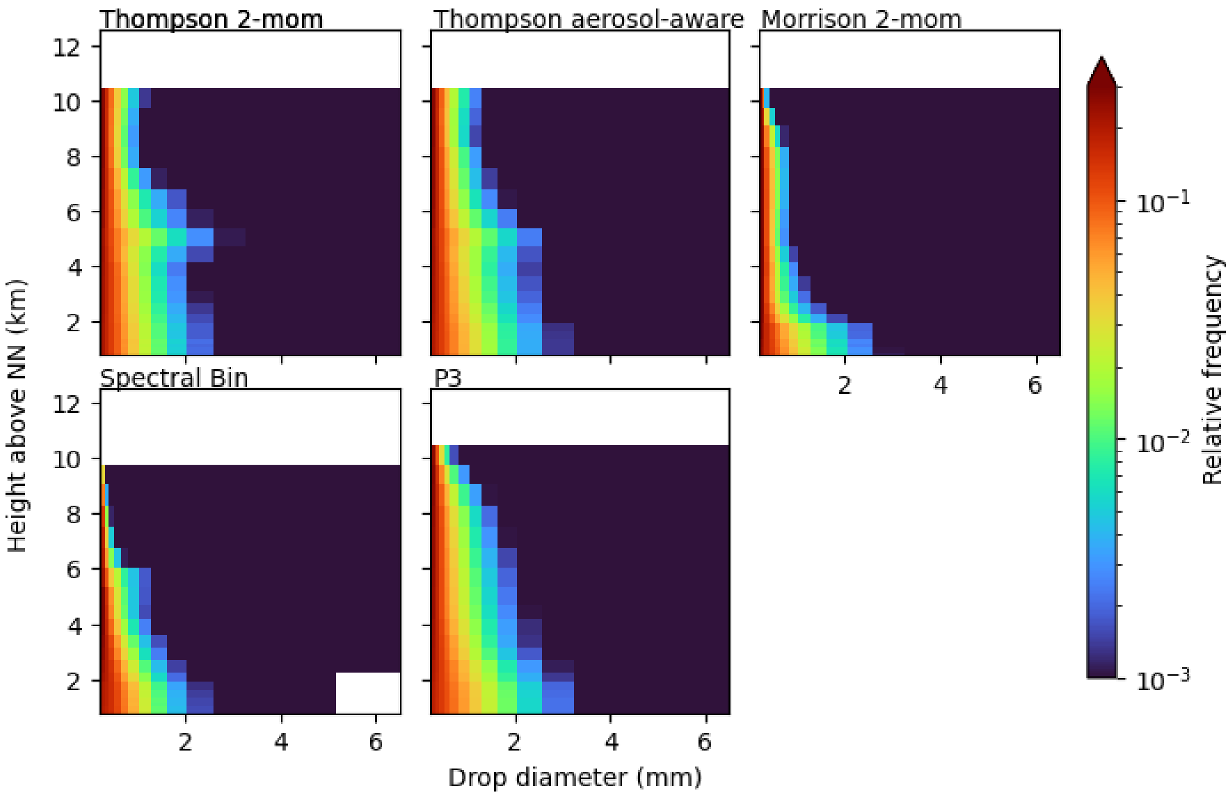
21. Line 380: This is incorrect. While the P3 is indeed more flexible, Thompson et al. (2008) says in its abstract, "[this scheme employs] a bulk density that varies inversely with diameter as found in observations and in contrast to nearly all other BMPs."

This was indeed incorrect. Snow is not of constant density in Thompson. We removed this statement. The following argument about P3 being more flexible still holds, as the P3 scheme uses multiple varying mass-size relations opposed to the one in the Thompson scheme.

22. Line 390: I appreciate the discussion about the potential erroneous growth by collection that is influencing the DWR below the melting layer. However, it is also interesting how different the DWR already is immediately below the ML. It seems to me that the particles leaving the melting layer may be very different in size between the observations and simulations. In the observations, perhaps stochastic breakup is occurring toward the surface that is reducing the DWR of large droplets in the obs while the simulated drops are too small and grow by collection instead? I think this is worthy of further exploration since it is one of the most pronounced differences between observations and simulations.

The DWR is indeed very different already directly below the ML height. Because the second reviewer was also interested in the profound differences directly below the ML height, we explored this further and calculated rain PSDs.
Below is our answer to the first reviewers major comment nr. 3):

We provided a CFAD of rain drop size distributions for convective cells (Appendix B):



he following passage was added to section 3.3:

> *In order to separate the analysis into reasons due to differences in the underlying modeled microphysics and due to different processing in the forward simulator, we examined rain particle size distributions directly produced by the NWP model (Rain PSD CFAD in Appendix B.) The FSBM scheme provides the drop size distributions over a number of size bins, for the other schemes we calculated the distributions according to the schemes parameterization. Only model grid boxes that were flagged as a convective cell by the TINT cell tracking are considered. The rain PSD CFAD confirms the findings of the ZDR CFAD: the two Thompson schemes simulate large rain drops from the surface up to the melting layer height and even above, while the Morrison scheme produces large rain drops only at the surface and the FSBM produce the highest frequency of small drops.*

Apart from the paragraph to the rain PSDs, we adjusted the DWR discussion corresponding to this comment as follows:

> *Below the melting layer the observed DWR steadily decreases towards the ground. The models do not reproduce this very well: Even though the DWR decreases in all models, this decrease happens abruptly at the melting layer. The DWR directly below the melting layer height is very different between the models and the observations, suggesting that particles falling out of the melting layer are larger in the observations compared to the simulated particles. Below this height the simulated DWR stays more or less constant while the observed decreases towards the surface.*

In the P3 simulations (and weaker in the Morrison scheme) the DWR even increases again towards the ground. At these heights rain and graupel are the dominant species. The simulated increase of DWR towards the ground is likely a result of the simulated collection process: Rain droplets grow while falling by collecting smaller droplets. This is visible also directly in the rain PSD (see appendix B) and was discussed in the previous section 3.3. Opposed to this, the large particles precipitating from the melting layer seem to shrink towards the ground, perhaps by drop breakup or evaporation. The general magnitude of simulated DWR near the surface is close to the observed again at around -3 to 10 dB.

23. Line 392: I am confused by the sudden discussion about vertically pointing radars, which were not used in this study?

    If anything, the discussion about our scanning setup versus typically vertically pointing radars belongs to the introduction. However, after reflecting, we think this is of minor importance, that's why we removed this part completely.

24. I don't think the acronyms need to be redefined in the summary (e.g., NWP, FSBM, PSD, etc).

    We removed the definitions of acronyms in the summary.

25. Line 445: Again, I am not sure this is a correct conclusion to draw as it depends more on the details of the radar forward operator.

    We removed this conclusion, as part of our answer to main comment nr 2.

Typos, etc.

1. Line 115: “differential phase” should be “specific differential phase”.
   Changed.
2. Line 117: For clarity, “single-polarimetric” should be “single-polarization” (some readers automatically infer multiple polarizations from the term ‘polarimetric’).
   Changed.
3. Line 147 and elsewhere: "times" should be "x" or "by"
   Changed.
4. Line 218: “Mass size” should be “Mass-size”
   Changed.
5. Line 260: db should be dB
   Changed.
6. Line 256: “cumulated” should be “accumulated”
   Changed.
7. Line 280: "extend" should be "extent"
   Changed.

8. Line 324: “image 5” should be “Figure 5”
   Changed.

Other author comments:
We repeated the CR-SIM simulations with P3, because we used a different aspect ratio relation for rain for the P3 simulations in the first version. This did not have a strong effect: the number of convective cells changed slightly (from 4768 to 4758 for all cells and 778 to 780 for cells > 7 km) and there are very slight differences in the P3 CFADs as a result of this compared to the first version.

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