Answers to reviewer 1

We would like to thank the reviewer for his time and effort reviewing our study. We have found the comments to be constructive and helpful and think that they have helped to improve our study! The comments of the reviewer are marked in black, our answers to the reviewer in red and the new text and lines of the revised document where the adjusted text can be found in blue. In the revised document, all new text is marked in blue, and deleted text is crossed out in red.

Major comments:

1. Some areas of the text containing claims of the microphysical processes observed (e.g., aggregation, SIP) could be scaled back somewhat given the absence of in situ microphysical observations.

A: We completely agree with the reviewer, that it would be ideal to have additional proof by in-situ observations collocated with the radar observations. However, we believe that our interpretation of radar signatures in terms of underlying microphysical processes are well-grounded on previous studies where for example radars and airborne in-situ observations have been matched for case studies. In addition, a number of laboratory studies provide detailed information on which processes and particle habits can be expected for example at certain temperatures. Practically, it would be extremely expensive to collect airborne in-situ data for the many cases that we include in our statistics. We would like to emphasise that we see the strength in our analysis in the statistical approach instead of interpreting single case studies. Nevertheless, we added the following text in section 5.1 acknowledging the lack of in-situ observations for additional proof of process interpretation.

New Text in Line 572-575: *Although we are lacking in-situ observations from inside the cloud for our dataset, we base our interpretation on well-established relations between microphysical processes and distinct radar signatures. For example, aggregation can be clearly associated with an increase in DWR (e.g. Ori et al., 2020; Barrett et al., 2019; Dias Neto et al., 2019; Mason et al., 2019), whereas plate-like particle growth is strongly linked to enhanced ZDR and KDP (e.g. Moisseev et al., 2015; Schrom et al., 2015)*

Further, measurements at the ground (e.g., Fig. B1) should be regarded with caution as they are the result of complex particle growth histories subject to hydrometeor transport by the horizontal wind and vertical motions, and as such are difficult to associate with radar measurements valid several km above ground. Some of the minor comments below address areas where text should be reworded to acknowledge these limitations or investigated further to strengthen the claims made.

A: We agree that the measurements on the ground as provided in Fig. B1 should not be regarded as a proxy for the microphysical processes happening in the cloud. We

have adjusted the sentence accordingly to avoid an over-interpretation of the snowflake pictures provided.
New Text in Line 322-326: *Certainly, the particles observed on the surface are not necessarily representative for the particles sampled with the radars due to impacts of advection and further growth processes during sedimentation towards the surface. Still, the snowfall reaching the ground was mostly comprised of unrimed or only slightly rimed crystals and aggregates. Visual observation on the ground at the site between 9 and 10 UTC (Figure B1) reveal the presence of stellar and dendritic crystals reaching up to 4 mm in size mixed with unrimed aggregates with maximum sizes up to 10 mm.*

2. Discussion around DWR/polarimetric variables and cloud top temperature (CTT) can probably be flushed out more. First, DWR and KDP would probably be more affected by the depth below cloud top (e.g., better linked to residence time) than the CTT.
A: We agree that the residence time of the particles is an important factor especially for DWR as the growth time will probably most affect PSD and mean size. Still, we focussed on CTT for two main reasons: 1) Previous studies showed some evidence that the aggregate size in the DGL is correlated with CTT - a connection which we wanted to evaluate with our larger dataset. 2) There is indeed a strong temperature dependence of primary ice nucleation and temperature which should impact the initial ice particle concentration. We prefer to leave the suggested investigation on additional parameters for future studies because the current manuscript is in our opinion already quite long. In addition, such an analysis would also require to take effects such as turbulence and advection of particles into account, which is not a trivial task.

Second, using Ka-band echo top heights (prone to some attenuation especially with rain below a bright band) and temperature from Cloudnet to get the CTT seem like compounding error sources. Was there satellite data to deduce the CTT? If not, it could be good to quantify or estimate the uncertainty with these CTT values, or add depth below cloud top to the discussion.
In order to adress possible uncertainties in the temperature information, we performed a comparison of the cloudnet temperature product to 27 radiosondes that were launched during the campaign. In general with a correlation of 0.9, a bias of 0.2°C and standard deviation of the bias of 1.1°C, the temperature information from cloudnet showed good agreement with the measured temperature (see Figure 1 below). We have added a new subsection describing this comparison of cloudnet and radiosonde temperature (see new section 2.4). We expect the impact of random uncertainties in the temperature information to have a negligible effect on our statistical analysis. We also agree that cloud top height might be biased low due to attenuation effects. However, we are quite confident that our CTH estimates are reasonable. Attenuation at Ka band is less severe then at W band and fortunately the rain rates during the campaign were moderate enough so that we don't expect

large attenuation effects. This is also confirmed by comparing the DWRs between X and Ka at cloud top which would be enhanced in situation of strong Ka band attenuation.

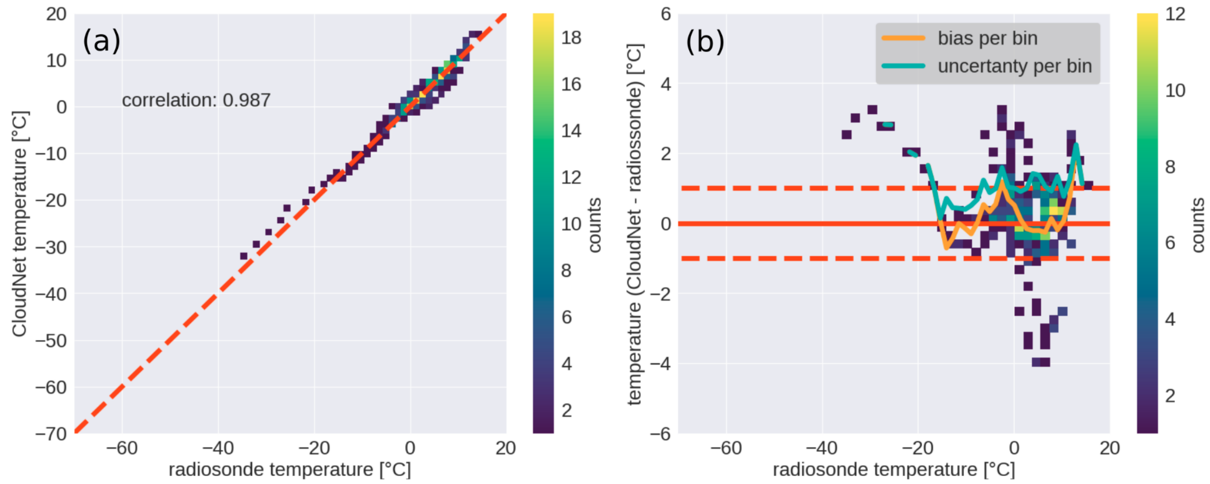

Figure 1: comparison of cloudnet and radiosonde temperatures during the TRIPEx-pol campaign. (a) scatterplot between cloudnet and radiosonde temperatures, (b) radiosonde temperature against the difference in cloudnet and radiosonde temperature

Third, while the Ka-band radar has a high sensitivity, are you able to comment on any discrepancies between echo top and cloud top height? In other studies where airborne lidar measurements exist, the cloud top and echo top height (e.g., Ka-band) has been known to differ by a couple to a few hundred meters (e.g., generating cells).

Unfortunately, we do not have airborne lidar measurements. We agree that with our method we likely underestimate the cloud-top height (CTH). For the analysis of the CTT, we chose relatively wide CTT bins (10°C bin width). We therefore do not think that a potential underestimation of the CTH by a few hundred metres or the over/underestimation of the temperature at cloud top would change the results significantly. We have tested the sensitivity of the statistical analysis of the CTT classes by perturbing the estimated CTH by several hundred metres. The Figure 2 and 3 below show the identical statistical analysis when adding an offset of 100 or 500m to the estimated CTH. Even with such a big perturbation, the medians of the different CTT classes still show similar features as described in the manuscript. We therefore think that a potential underestimation of the CTH does not change the interpretation of the CTT classes as included in the original manuscript. We have also tested the impact of possible under-and overestimation of the temperature at cloud top. We have added a bias of upto +-2°C to the CTT. Similar to changes in CTH, a change in CTT has only minor effects on our results of the statistical analysis (Figures are not shown here).

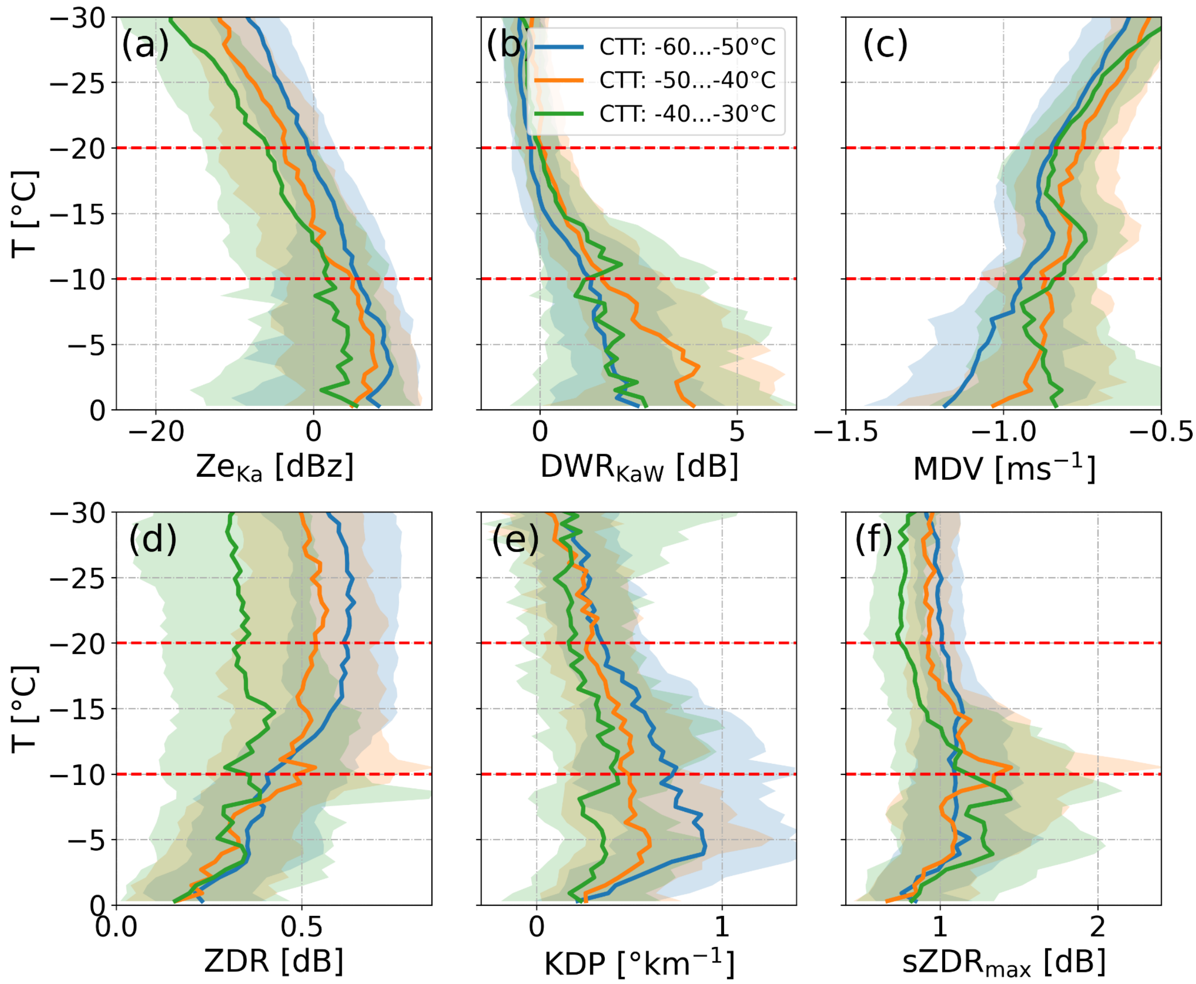

Figure 2: Cloud top temperature analysis as is provided in Figure 8 in the manuscript. However, we added a height offset of 100m to the estimated cloud top height, in order to visualize possible impact of an underestimation of the cloud top height.

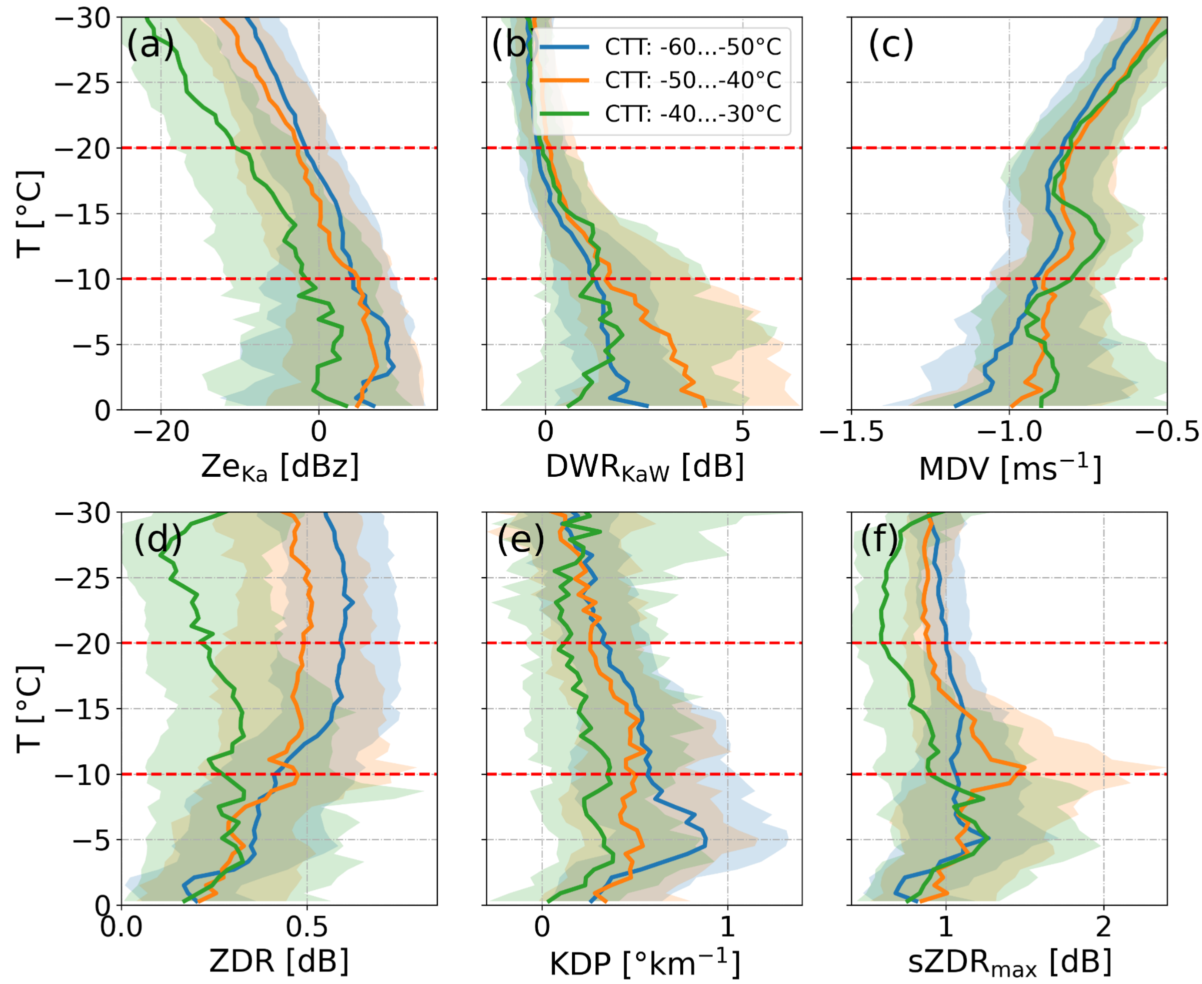

Figure 3: Same as Figure 2 but for a height offset of 500m.

Minor comments:

1. L2: “likely also” → “perhaps”

A: we have changed likely also to potentially

2. L92: “provide also” → “provide”

A: Changed in the text

3. L302–304: Hydrometeor transport (e.g., horizontal wind) should be noted here.

A: We have added possible advection of hydrometeors to this paragraph. The new text is written below the answer to the major comment 1 and can be found in line 322-326 in the revised document.

4. Fig. 2: Why were -30 and -15°C the contour levels chosen? It seems like -20 and -10°C would better fit the DGL narrative and be consistent with the subsequent profile figures. Can you also comment on the cause for the gap in DWR measurements around 0700 UTC (panel c)?

A: We agree with the suggested contour levels and we changed the figure accordingly. However, we kept the -15°C contour level because it allows to see that below -15°C the DWR and sZDRmax are enhanced. The gap in the DWR measurements is due to a high variance flag obtained by the data processing routine which causes the zenith W-Band radar data to be excluded. This flag indicates that during the relative DWR calibration described in Section 2.2.3, the variance in time of the calculated DWR between Ka and W-band were larger than 2dB^2. (See also Dias Neto et al., 2019 https://essd.copernicus.org/articles/11/845/2019/)

5. L325: Are you able to cite a previous study that corroborates your claim/postulation?
A: There are in general only very few studies that looked into spectral DWR. Our reasoning here is simply based on the fact that the maximum sDWR (independent on concentration) is not coinciding with maximum sZe (driven mainly by size, density, and concentration).

6. L335: I'm not sure parentheses are needed around sDWR. Maybe turn this phrase into a list?
A: Changed in the manuscript
New Text in line 355 in revised document: *For temperatures warmer than -15°C, the fall velocities, sZe and sDWR-KaW of the secondary mode increase…*

7. L343: Seems more like -7 or -8°C.
A: We agree that the weak secondary mode in sZe is closer to -7 or -8°C, however the increase of sZDR seems to start closer to -10°C. We adjusted the text accordingly.
New Text line 365-367: *At temperatures around -10°C, the sZDRmax values increase again. The maximum in sZDRmax at around -8°C roughly coincides with the appearance of a weak secondary mode in sZDR and an increase in KDP*

8. Table 2 and relevant discussion: Perhaps you can acknowledge that the D0 estimates are probably underestimated, particularly for the largest DWR class, as studies such as Mason et al. (2019; https://doi.org/10.5194/amt-12-4993-2019) find PSD shape is an important factor in the triple-frequency radar signature (and by extension in the DWR measurements).
A: We agree with the reviewer that our assumption of an inverse exponential PSD might lead to an underestimation of the D0 mentioned in table 2. We have added a discussion of the possible underestimation and dependency of D0 on the PSD shape to the manuscript.
New Text Line 400-403: *As is shown in Mason et al. (2019), the shape of the PSD influences the shape of the triple-frequency signatures, and by extension also the DWR measurements. A narrow PSD with a large D0 might account for the same DWRKaW as a more wide PSD with a smaller D0.*

9. L380: “temperature,” → “temperature”
A: Adjusted in the manuscript

10. Fig. 4b: Can you comment on the negative DWR values above the DGL? Can calibration uncertainty (i.e., Section 2.2.3) attribute to this?
A: Yes, calibration uncertainties are the main reason for those slightly negative DWRs. In order to obtain a reliable estimate of the time varying DWR offset due to attenuation and other effects (wet antenna/radome) we need to use a relatively large part of the cloud and accept also Ze values up to -10dBz. This might not perfectly restrict the regions to pure Rayleigh particles. Some remaining differential scattering could produce an overestimation of the DWR which leads to an over-compensation of the W band. The effect is less visible in the stronger DWR classes where the main signal comes from larger ice and snow particles.

11. L398: Specify “more negative” in addition to slightly larger for clarity.
A: Changed in the manuscript
New Text Line 433: *Unlike Ze and DWR-KaW, the MDV are only slightly more negative for the larger DWR-KaW classes*

12. L400–401: Add negative signs to your MDV values for consistency.
A: Added

13. L405: I’m unsure what you mean by “upwind” as it relates to large scale lifting. Can you clarify?
A: We intended to say updraft, not upwind. We have changed that in the manuscript

14. L429: Is this difference between the slow-down on the slow and fast edges in Fig. 5a statistically significant, or at least greater than potential uncertainties in the MDV measurements?
A: In order to address the reviewer’s concern, we performed a Kolmogorov-Smirnoff 2-sample test, testing for the null-hypothesis “The velocity distributions at the temperature level where we find the first onset of a slow-down (-18°C for the slow and -16°C for the fast edge) and the temperature level with the largest observed slow down (-14°C for both edges) are equal.” At the slow edge, we found that, a p-value of $6.125*10^{-11}$. This p-value is small enough to savely reject the null-hypothesis in favour of the alternative hypothesis that both distributions are significantly different.
For the fast edge we find a p-value of 0.5, suggesting that the null-hypothesis can not be rejected. We believe that the non-significance of the fast-edge slow-down is a result of imperfect filtering of the data. For example, the updraft caused by latent heat released by depositional growth depends on the thermodynamic profile of the atmosphere. Still, we think that the fact that the fast edge velocity decreases consistently between -16 and -14°C is a strong indication for a dynamical feature. It is hard to imagine any microphysical process which could cause such a

phenomenon. Continuous particle growth should increase MDV or at least cause a constant MDV profile.
We have added a paragraph discussing the significance level of the slow down on both edges to section 4.2.
New Text line 480-485: *A Kolmogorov-Smirnoff two-sample test revealed that the slow-down on the slow falling edge is statistically significant, while it is not significant for the fast edge. In case of no updraft, we would expect the fast edge velocity to continuously increase, similarly as for temperatures colder than –16 ° C. So even if the Kolmogorov-Smirnoff test indicates that the slow-down on the fast edge is not statistically significant, we argue that the persistent stagnation of the fall velocities over the temperature range from between –16 ° C and –14 ° C strongly points towards the presence of an updraft.*

15. L456–457: True, but is the case study representative of the entirety of the project as it relates to ZDR?
A: The reference to the case study is only meant as an illustration of the fact that high-ZDR particles and low-ZDR particles are well seperated in the spectrum. This effect can be found in almost all cases analyzed.

16. L471: “confirmed by” → “consistent with”
A: Adjusted in the manuscript (see line 519 in revised document)

17. L472–473: Remove “apparently”, “DGL is correlated” → “DGL appears to be correlated”
A: Manuscript adjusted accordingly (see line 520-521 in revised document)

18. L483: The study of Moisseev et al. (2015) is briefly mentioned in Appendix C, but it could be good to relate their findings to what’s discussed in this paragraph.
A: In our opinion, it is difficult to relate our statistical findings to the results of single case studies, because usually only large KDP and ZDR cases are selected, while in our case we do not cherry-pick specific case studies were we see large KDP and ZDR but focus on all cases that we observed during the three month campaign. The case study that we showed in Section 3 was meant to illustrate the similarities and differences we see in a case with large KDP and ZDR compared to other studies. In the statistics, our KDP and ZDR (sZDRmax) are smaller than in the case study and also in case studies from previous studies because we also include cases where we do not see large KDP or ZDR occurring at the same time as enhanced DWR. This reduces the magnitude of the median.

19. L498: I think a word was left out for “until the -5°C is reached”. Possibly rephrase?
A: We removed the “the” in front of -5°C

New Text line 564-565: *Most notably, signatures related to crystal growth or aggregation that evolved in the DGL appear to persist to lower layers until -5°C is reached.*

20. L506: Parentheses should be like “Takahashi (2014)”.
A: Adjusted in the manuscript

21. L510, 642, and possibly elsewhere: You should also acknowledge ice supersaturation as an important factor for the ice crystal habit, as is mentioned in their study.
A: We have added that to the manuscript
New Text (line 581-584): *As mentioned in (Bailey and Hallett, 2009, among others), the shape of the particles does not only depend on temperature, but also on the supersaturation that the particle experiences during growth. During the TRIPEx-pol campaign we do not have sufficient relative humidity information. In the following we therefore only focus our interpretation on the observed temperature-dependent growth of ice particles.*

22. L516: “then” → “than”
A: Adjusted in the manuscript

23. L535–543: Have you looked at or considered other (i.e., mesoscale) effects? You mentioned earlier that many of these events were frontal driven, so it’s plausible that frontogenesis or weak/elevated instability can contribute to regions of vertical motion in addition to latent heating by depositional growth.
A: We have not looked at other effects. Since the campaign took place in early to mid-winter, the height of the -15°C isotherm above the ground and also in respect to the height within the cloud changed significantly from case to case. We therefore excluded mesoscale effects in our discussion because we could not think of a reason why such a large-scale driven updraft or weak instabilities should have such a strong temperature dependency. Of course mesoscale effects can contribute to the updraft found at -15°C, but we think that statistically speaking this should be distributed over a larger temperature regime and thus the effect on the median profiles should be small.

24. L548: I agree that most bulk schemes lack the ability to resolve these growth rates and latent heating properly. Maybe add that bin schemes e.g., Lee & Baik 2018; https://doi.org/10.3390/atmos9120475) produce greater latent heat via deposition that leads to stronger updrafts.
A: Thanks for noting this study, we added the reference to the manuscript
New Text (line 624-627): *However, when simulating a heavy rainfall case, Lee and Baik (2018) found that simulations with a bin microphysics schemes reveal intense latent heat release due to depositional growth. This latent heat release is sufficient to*

cause an updraft and a positive feedback mechanism. The latent heat release in bulk schemes was found to be substantially weaker.

25. L560 and Appendix C: Have you examined the per-particle KDP for other habits in the scattering database? Since mid-latitude winter systems typically consist of many habits in a radar gate, the sensitivity of KDP by habit as shown in Fig. C1c may mean that the 1/3 contribution of aggregates to the KDP signal may have large uncertainties.

A: So far we have not examined other crystal habits in the KDP estimation. We agree that this estimation has large uncertainty. Our main goal of this ad hoc calculation was to demonstrate that the contribution of aggregates to KDP cannot be neglected, which makes the interpretation of KDP more difficult. In the near future we want to investigate the observed KDP and sZDRmax values further using Monte-Carlo particle modelling with implemented habit prediction.

26. L587: DeMott et al. (2010; https://doi.org/10.1073/pnas.0910818107) also has comprehensive (global) INP measurements.

A: Reference was added (see line 670 in revised document)

27. Conclusions: I also think you should acknowledge that future studies employing in situ microphysical measurements could be a unique opportunity to validate the findings or confirm the speculated processes presented in this study.

A: Agreed, also further laboratory or modelling studies might be useful to validate the findings and hypothesised processes. We have added that to the conclusions.

New Text line 770-771: *Such laboratory studies in addition to in-situ measurements or Monte-Carlo modelling studies could also provide unique opportunities to validate our findings and the hypothesized ice microphysical processes of this study.*