Review of "Ice microphysical processes in the dendritic growth layer: A statistical analysis combining multi-frequency and polarimeteric Doppler cloud radar observations"
by von Terzi et al.

The study by von Terzi et al. examines various microphysical processes that may contribute to the radar reflectivity, dual-wavelength ratio, mean doppler velocity (MDV) and polarimetric (ZDR and KDP) signals within the dendritic growth layer between -20 and -10℃. This work expands upon the TRIPEx campaign observations with Doppler spectra and polarimetric measurements that are known to be influenced by the presence of habits such as dendrites, aggregates, and needles. The manuscript is exceptionally written, well-referenced, and detailed in the methodology used. My main criticism revolves around some of the claims of the microphysical processes "observed" without actual in situ microphysical measurements. Nonetheless, I believe that the manuscript should be accepted for publication in ACP once the following Major and Minor comments are addressed.

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. 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.
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. 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. 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).

Minor Comments

1. L2: “likely also” → “perhaps”
2. L92: “provide also” → “provide”
3. L302–304: Hydrometeor transport (e.g., horizontal wind) should be noted here.
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)?
5. L325: Are you able to cite a previous study that corroborates your claim/postulation?
6. L335: I’m not sure parentheses are needed around sDWR. Maybe turn this phrase into a list?
7. L343: Seems more like -7 or -8°C.
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).
9. L380: “temperature,” → “temperature”
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?
11. L398: Specify “more negative” in addition to slightly larger for clarity.
12. L400–401: Add negative signs to your MDV values for consistency.
13. L405: I’m unsure what you mean by “upwind” as it relates to large scale lifting. Can you clarify?
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?
15. L456–457: True, but is the case study representative of the entirety of the project as it relates to ZDR?
16. L471: “confirmed by” → “consistent with”
17. L472–473: Remove “apparently”, “DGL is correlated” → “DGL appears to be correlated”
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.
19. L498: I think a word was left out for “until the -5°C is reached”. Possibly rephrase?
20. L506: Parentheses should be like “Takahashi (2014)”.
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.
22. L516: “then” → “than”
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.
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.

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