

Reviewer 2

We thank the reviewer for taking the time to give feedback on our manuscript. Point-by-point responses to your comments are given below, with our responses in blue.

In this manuscript, the authors analyse radar Doppler spectra from a vertically pointing Ka-band cloud radar located at Chilbolton, UK. 23 cases of deep stratiform clouds are analysed with a peak finding algorithm in order to analyse the distribution of mono- and multi-modal spectra with respect to in-cloud temperature.

I appreciate the effort shown in this work to quantitatively analyse radar Doppler spectra including the derivation of statistical properties. However, I see some weaknesses in the interpretation of the results, which I elaborate on in the specific comments below. I think many conclusions drawn are too strong given that only reflectivity spectra are analysed. From the analysis the typical two temperature regimes (-18...-12°C and -8...-5°C) emerge where many previous studies found spectral multi-modalities to appear.

Considering the author's statement in the abstract that "we can provide the first quantitative estimate for the distribution of multimodal spectra with temperature" I am wondering whether the author's are aware of this recent publication:

Wugofski, S. and Kumjian, M. R.: Detection of Multi-Modal Doppler Spectra – Part 2: Evaluation of the Detection Algorithm and Exploring Characteristics of Multi-modal Spectra Using a Long-term Dataset, Atmos. Meas. Tech., 18, 6569–6590, <https://doi.org/10.5194/amt-18-6569-2025>, 2025.

I think the manuscripts needs major revisions before it can be published. Several technical details need to be clarified, the review and discussion of recent literature related to this topic should be improved, and the conclusions and interpretations should be made more carefully.

General comments:

I miss in this manuscript the discussion about the fact that sometimes two distinct particle populations (for examples small ice from SIP and aggregates, or aggregates and slightly rimed particles) might exist but they might not be discernible as two distinct peaks in the Doppler spectra. This might be caused by broadening effects, longer averaging times, or simply an overlapping terminal fall velocity regime. This effect is well known from previous Doppler spectra studies related to drizzle development. In order to still capture the existence of a second particle population, skewness was used as a very sensitive measure for the non-symmetry of the spectrum. I would like the authors to comment why they didn't analyse skewness at least in addition to the peak number.

Further discussion regarding the fact the distinct particle populations do not always appear as individual peaks in the Doppler spectrum has been added in Section 1. Whilst skewness has been studied in the context of drizzle formation in liquid phase clouds, we are not aware of published work where it is applied to multimodal ice population formation (a current work-in-progress by the authors is investigating this). However, we now show plots of skewness in the discussion in Sections 4.1 and 4.2, and we find that the regions of multimodality identified by the peak finding algorithm are often collocated with regions of high skewness.

In this manuscript (and in many others) it is commonly assumed that SIP always produce small ice particles (hence, they can form a slow secondary mode in the spectra). But is this necessarily always the case? What, for example, if a graupel collides with a snowflake? Wouldn't this produce much larger fragments? I understand that the authors cannot answer this question but I suggest that we discuss this aspect. Right now, it is implicitly assumed that SIP always produce small ice particles.

The reviewer raises an interesting point, and is in fact a topic we explored in our previous work (Mammatt et al 2026, cited in the text), where we found that secondary particles evolved into large

crystals – however even these large crystals had distinct physical characteristics, and were still distinguishable from the primary population in terms of their size, shape, or both. We believe this means that the secondary particles will often occupy a distinct region of velocity space, and indeed this is evident in our examples where the modes are typically well-separated in coherent structures spanning vertical distances of 1km or more.

We agree that fragments are not necessarily small – the key is that they have distinct velocities to the primary mode. To reflect this, reference to ‘small’ secondary particles in the context of Doppler spectra observations throughout the manuscript has been removed and has been replaced by reference to ‘slow-falling’ secondary particles.

The authors seem to me very much in favour to link all spectral multi-modalities which might be caused by SIP to the Hallett-Mossop process. Given the fact that

1. nobody (at least I haven’t met anybody and also recent SIP reviews like Korolev&Leisner say that the mechanism is still unclear) can really explain the physical mechanism of Hallett-Mossop,
2. a very particular combination of graupel, larger super-cooled droplets and specific temperature region is needed,
3. recent lab study could not reproduce the famous Hallett-Mossop diagram at all,

make us more cautious to interpret our radar signatures of secondary particle modes as a potential result of this SIP process?

We agree that the precise details of the Hallett-Mossop process remain uncertain, including the physical mechanism and the rate of splinter production. But whilst Seidel et al. (2024) couldn’t reproduce splinter production in the lab, many earlier laboratory studies were able to generate enormous concentrations of crystals as a target was rimed (Hallett and Mossop, 1974; Choularton et al., 1980; Heymsfield and Mossop, 1984; Saunders and Hosseini 2001) and numerous aircraft studies (e.g. Harris-Hobbs and Cooper, 1987; Rangno and Hobbs, 2001; Crosier et al., 2011, 2014; Lloyd et al., 2014; Keppas et al., 2017) have shown evidence that multiple distinct particle populations exist at temperatures around -5°C, particularly high concentrations of needles. From aircraft observations it is impossible to say precisely which microphysical mechanism is producing these high concentrations of needles, but if rime splintering is not responsible for these observations, then another (potentially unexplained) mechanism is. Throughout the discussion in Section 4, we intended to demonstrate that rime splintering could be an explanation for the Doppler signatures that we see, but further discussion into different SIP mechanisms has now been added at the relevant points. We strongly agree that this is an uncertain area, and more laboratory work is needed to understand the whether observations of high crystals near -5°C are caused by splintering of riming droplets, or via some other process which occurs in similar conditions.

In Sections 4.3 and 4.4, references we make to the ‘rime splintering regime’ are in fact references to temperatures between -8°C and -3°C. By this, we do not mean that rime splintering is definitely occurring in this regime but simply that it is the temperature regime typically associated with rime splintering. This has been clarified at the beginning of Section 4.3.

I also would like to emphasize that the radar Doppler spectra are unable to tell whether a new ice particle mode is really caused by SIP. We only see new (slow) ice particles growing. And yes, in some temperature regions it appears quite unlikely that primary nucleation is sufficient. But what we really have to do is to compare simulations of SIP or aircraft in-situ data converted into radar space with real radar observations.

We completely agree that Doppler spectra, or indeed any remote sensing measurement, alone cannot definitively reveal the processes active in clouds. This is discussed in the conclusions (Section 5).

Specific comments and questions:

L. 90: Better “maximum range”. What is the Doppler spectral resolution? How many raw spectra are averaged in the 1-minute period? What is the sensitivity and beam width of the radar? (Maybe a table would help to summarize all those technical radar details.)

These properties are all introduced when relevant in the manuscript, but we agree that including these details in one place is useful so have added a table in Section 2.1.

L. 115: “The Copernicus radar is routinely calibrated by comparing the signal to that from the Chilbolton Advanced Meteorological Radar (CAMRa)”. Can you provide more details on how this is done? For which clouds do you compare Copernicus and CAMRa? How do you account for the wavelength difference (Ka vs. S-band)? At which height? How do you account for attenuation effects at Ka-band which could be misinterpreted as calibration offset?

L. 116: “S-band and is absolutely calibrated to within ± 0.5 dB in Rayleigh scattering cirrus clouds”. I suspect that the ZDR-Z consistency method is used? Where can the reader find more on how the CAMRa is calibrated?

Response to L115 and L116:

We follow the procedure outlined in Hogan et al (2000). The 3 GHz radar is calibrated in heavy rain using the redundancy between reflectivity, differential reflectivity and differential phase shift, as described in Goddard et al (1994). 35 GHz reflectivity profiles are then matched with the calibrated 3 GHz data in high level cirrus clouds where the particles are Rayleigh scatterers at both 35 and 3 GHz. The small gaseous attenuation at 35 GHz is taken into account by using numerical forecast output for the water vapour profile of the atmosphere, and a line-by-line absorption model (Liebe et al 1989).

This explanation has been added to the manuscript at this point.

Hogan, R. J., A. J. Illingworth, and H. Sauvageot, 2000: Measuring Crystal Size in Cirrus Using 35- and 94-GHz Radars. *Journal of Atmospheric and Oceanic Technology*, 17(1), 27-37.
[https://doi.org/10.1175/1520-0426\(2000\)017<0027:MCSICU>2.0.CO;2](https://doi.org/10.1175/1520-0426(2000)017<0027:MCSICU>2.0.CO;2)

Goddard, J. W. F., J. Tan and M. Thurai, 1994: Technique for calibration of meteorological radars using differential phase. *Electronics Letters*, 30(2), 166–167.

Liebe, H.J. (1989) MPM—An Atmospheric Millimeter-Wave Propagation Model. *International Journal of Infrared and Millimeter Waves*, 10, 631-650. <http://dx.doi.org/10.1007/BF01009565>

L. 123: “we see that there are two ‘strands’ to the spectrogram, which is indicative of a particle population which is evolving independently to the primary population.” I would be careful which such a conclusion. The fact that the two modes are not merging quickly does not necessarily imply that their particles are not interacting. In fact, the radar observations can’t really tell how many particles in the new mode are interacting (for example, aggregating) with the old mode. Certainly, we cannot conclude that from Ze-spectra alone.

By ‘evolving independently’, we are referring to the fact that these populations likely form at different heights/temperatures and may have different particle habits. We have changed in the language here so that we now refer to two ‘distinct’ populations instead of two ‘independent’ populations to hopefully make this clearer.

L. 130 ff: If I understand the method correctly, you assume that the standard deviation of MDV over 30s is solely driven by turbulence and hence it is a “measure” of the turbulence intensity. Can you be sure that fluctuations in the terminal fall velocities during this time period are not misinterpreted as turbulent motions? I think for a complete discussion of broadening effects, you should at least mention that other broadening terms (e.g., shear broadening) exist but that they can probably be assumed to be small for your radar.

That is correct, following the methodology in O'Connor et al (2004) we assume microphysical variations are small over our 30s window, and that the fluctuations in mean Doppler velocity from one second to the next are dominated by turbulent fluctuations. This tends to work well in ice clouds where the fall speeds are small in magnitude, and hence *variations* in the fall speeds are even smaller in magnitude (in rain, the technique is less reliable because the velocities are much faster, but we are only analysing ice).

The reviewer is correct that in principle there could be a few pixels where some strong microphysical change occurs on small scales, which “looks like” turbulence.

However, recall that the purpose of this analysis is to identify pixels in which it is possible that turbulence is so strong that the spectra are being blurred, and where modes may become harder to distinguish. This happens infrequently, even with our conservative threshold which corresponds to blurring over a single velocity bin. So the worst case scenario is that we throw out a little more data than we really need to. But we feel that is acceptable: what we want to avoid is including spectra that could be ambiguous because of turbulent broadening, and our method achieves this.

Shear broadening is indeed likely to be small: using equation 6 in O'Connor et al (2004) we estimate this term to be 0.015m/s for a 10m/s wind speed.

L. 150 ff: Why do you only consider rain attenuation but not attenuation from the melting layer? If you have the rain rate or Z_e (attenuation corrected) below the melting layer, you can estimate the melting layer attenuation as well (for example using relations in Li and Moisseev, JGR, 2019).

Thank you for this suggestion. We acted on this and have now estimated attenuation caused by the melting layer using the relationship with rain rate described by Li and Moisseev, and removed regions of cloud where the combined attenuation from rain and the melting layer exceeds 10dB. The description for this step has been added to Section 2.1.3, and Figure 3 has been expanded to show attenuation caused by rain and the melting layer individually, as well as the total attenuation.

L. 175: Recently, similar Doppler spectral peak identification and tracking tools have been developed (Peako and peakTree as described in Vogl et al., AMT, 2024. <https://doi.org/10.5194/amt-17-6547-2024>). Can you comment on why did you decide to develop your own method or in which way is your method advantageous?

We are aware of these tools and find them very interesting. However, they are not necessary for our application. We want to emphasise our focus in this paper is not the novelty of the peak finding technique we applied (which is well established), but the application of peak finding to a relatively large number of clouds, allowing us to gather statistics which give insight into the rate at which multimodal ice populations occur. We suspect Peako would have been a viable alternative, which indeed we did consider using at the outset of the study. However, `find_peaks` is a simple, easy to explain methodology, with well-defined parameters controlling its behaviour, and it is sufficiently computationally efficient to handle our dataset with ease.

Section 3.1.1 Peak Finding: I agree that it is difficult to find the right averaging window in order to remove noise but to keep the microphysical information. I was wondering whether you also considered temporal consistency of your peaks at one particular height as a criterion for “true microphysical information”? In fact, turbulence might not only broaden the spectra but also might cause spurious multi-modalities in the spectra. However, if the spectral features remain constant over a certain time one can assume it is more likely microphysically caused.

We agree that temporal consistency is an important feature to consider when identifying ‘true’ microphysical information. After identifying the number of peaks in each pixel, we calculate the modal number of peaks across four range gates and 10 individual spectra (10 minutes), and use this modal

number in the analysis presented later. This process is explained in more detail in Section 3.1.2; a reference to that section has been added at this point.

Also, the choice of your averaging window depends on your general radar settings: If you average only a small number of raw spectra you will need to apply a bigger averaging window. For drizzle spectral features, Acquistapace et al., AMT, 2017 (<https://doi.org/10.5194/amt-10-1783-2017>) systematically investigated this trade-off between averaging time and keeping the relevant microphysical information by recording IQ-time series. I think it is just important to point out that these settings need to be figured out individually for each radar and corresponding settings.

Thank you for pointing this out; a sentence clarifying this has been added to the manuscript in Section 3.1.1.

Section 3.1.3: Again, I have the feeling that the more generic approach in PeakTree (Radenz et al., AMT, 2019) avoids this often subjective definition of primary, secondary peak or slow or fast peak. I would like to see a discussion in the paper why you decided not to follow the PeakTree approach. I think following such a more generic approach could help to make different Doppler spectral analyses more comparable.

We believe it is fair to assume that what we are terming the ‘primary’ population (the population which sediments from higher up in the cloud) has the highest spectral reflectivity. This is based on visual inspection of many spectrograms, where the sedimenting population is discernible by high spectral reflectivity compared to those of the secondary modes, which form at lower levels in the cloud. Wugofski and Kumjian (2025) follow the same approach when differentiating between primary and secondary populations. In nearly all cases, if there is a secondary population present (secondary in the sense that it is the second population, not that it was necessarily formed by SIP) then it is falling slower than the primary population, hence the chosen terminology of ‘slow mode’.

L. 265: How do you derive MDV and Ze of the different spectral peaks/modes? Do you describe this somewhere? It should be added.

MDV and Ze are not derived for the different spectral modes. The peak finding algorithm identifies the velocity bin at which the highest local spectral reflectivity (sZ) is located, which is what we are referring to here. It is these velocities which correspond to the highest reflectivity which go into the analysis presented in Section 4.4.

L. 304: “we assume this mode is associated with ice particles instead of a drizzle mode, which would likely have very low fall speeds”. I would argue this depends a lot on the size of the drizzle drops! I find it difficult to assume this to be true in general. Especially in the relatively warm temperature region between -5 and 0°C larger super-cooled drizzle is not unlikely. In fact, one would need spectral LDR to really distinguish them (as done for example in Luke et al., PNAS, 2021).

LDR would certainly be useful for identifying drizzle, but it is unfortunately not available from this radar. We agree that the fall speeds of drizzle drops are dependent on their size, so the best evidence that they are liquid is looking at their evolution across the melting layer. We can see from our observations that the drizzle mode is identifiable because it doesn’t melt, and that examination of many spectrograms reveals that in this case, this drizzle population occurs at lower velocities than the ice modes. Figure 10 (now Figure 12) shows one example of this (updated with one-dimensional Doppler spectra), but many such spectrograms were visible on this day.

Figure 10b: Where do the new slow modes in the rain part (1.5km) come from? Is this a feature advected from the side? Did you generally consider analysing the spectra along fallstreaks as done for example in Kalesse et al., ACP, 2016? If not, why?

It is likely that the additional rain modes are features advected in from the side. Our focus in the study is the statistical behaviour in many events. We do appreciate that for process understanding within a

heterogeneous cloud sampling along fallstreaks could be more informative, but this is not in scope for our study.

L. 334: So you averaged together all spectrograms of an entire day? I would expect that microphysical features will be smoothed out as you averaging over spectra with are also shifted by vertical air motion (even if you remove strong turbulence). I think you need to discuss this aspect.

The idea here was simply to highlight how persistent these features were over the scene. However we understand the reviewer's criticism, and these average spectrograms have been removed, and we now show sample snapshot spectrograms.

L. 337: The velocity decrease at -17°C is a very common feature and discussed in detail in von Terzi et al., ACP, 2022, <https://doi.org/10.5194/acp-22-11795-2022>.

This feature is discussed in much greater detail later in the manuscript (Section 4.4), including reference to von Terzi et al. A forward reference to that discussion has been added here.

L. 341: "The presence of this secondary population and the increase in particle velocities seen throughout this layer reinforces the suggestion that riming is the dominant source of secondary particles at these temperatures via the rime splintering mechanism." I suggest to be cautious with such interpretations in light that rime splintering (or Hallett-Mossop process) could not be reproduced in recent lab experiments (Seidel et al., ACP, 2024). If it is a real process at all, it might be only one possible scenario. In fact, needles generally grow very efficiently by vapour deposition in this temperature regime especially if humidity is close to water saturation. They also show relatively high terminal velocities even if not rimed. I am not convinced that riming is needed to explain this spectral features although it might be one possible explanation. We also don't really know whether the new mode was initiated by rime-splintering, ice collisional fragmentation, or even primary nucleation. Again, my point is: It could be rime splintering, but we cannot reliably conclude that from the radar spectra. Especially, if we only consider reflectivity spectra.

The language here has been softened here to make to clear that riming/rime splintering is one possible explanation for the features seen in the observations. Discussion has also been added concerning other SIP mechanisms.

L. 395: Maybe this recent study should be mentioned in the context of multi-model spectra statistics: Wugofski, S. and Kumjian, M. R.: Detection of Multi-Modal Doppler Spectra – Part 2: Evaluation of the Detection Algorithm and Exploring Characteristics of Multi-modal Spectra Using a Long-term Dataset, Atmos. Meas. Tech., 18, 6569–6590, <https://doi.org/10.5194/amt-18-6569-2025>, 2025

Thank you – we were aware of this paper, which we have been exploring in a separate strand of work. It is distinct from ours in the sense that they do not directly sample statistics of multiple modes, but use elevated spectrum width as a proxy for multimodality. But we agree a reference to this study is needed, and has now been included.

L. 402: "The percentage of multimodal spectra continues to increase at temperatures higher than this ice multiplication regime, which suggests that secondary particle populations continue to grow and evolve throughout this relatively narrow temperature band before reaching the melting layer." This is one explanation. But it is also more likely to find super-cooled drizzle when approaching the melting layer and those drizzle particles would also cause a second mode (see first spectrograms in Zawadzki et al., AR, 2001).

It is true that supercooled drizzle would be identifiable as a secondary mode, like those seen in Zawadzki et al. But examining individual spectra shows evidence that the secondary populations are traceable with altitude. The spectrogram shown in Figure 1 is a good example of this; the secondary population forms at $\sim 3.8\text{km}$ and is visible all the way down to the melting layer at $\sim 3\text{km}$ where the particles accelerate as they melt into raindrops.

L. 406: I suggest considering the discussion and statistical analysis done in von Terzi et al., ACP, 2022 (<https://acp.copernicus.org/articles/22/11795/2022/>) in this context. They discuss a potential link between increasing aggregation at these temperatures (collisional process) with potential ice fragmentation. I think it deserves also to be mentioned in the discussion.

The work presented by von Terzi et al is discussed later in Section 4.4, but we agree that it is also relevant to the discussion at this point so reference to this study has been added.

Figure 15: Why is the temperature range for panels a-c going from 0...-50°C but for the lower panels d-f it is focussed on 0...-20°C. I suggest to also plot in addition to your median curve some statistical measures of the width of your distributions (for example quantiles).

As shown in Figure 14, the fraction of multimodal spectra at temperatures colder than -20°C is very low (in the region of 1%), and therefore the sampling statistics at these cold temperatures are poor.

Quantiles are a good suggestion, thank you; the 25th and 75th quantiles have been added.

L. 514: “Our results instead show evidence that changes in the primary population typically govern this decrease in MDV.” I am not sure I see this “evidence” in your results. How can you be sure that a decrease of velocity in your primary mode is due changes in the particles (maybe dendritic extensions growing on aggregates falling from above) but not by for example an upward air motion? Such an upward air motion could produce exactly the same mean Doppler signature. I think the only way to finally answer this question is an independent measure of vertical air motion such as a radar wind profiler (for example as done in Radenz et al. AMT, 2018. <https://doi.org/10.5194/amt-11-5925-2018>). Unless we don’t have this information, I think such conclusions are scientifically not sound.

In one individual spectrogram, we agree that it would be possible for upward air motion to produce the observed decrease in the primary mode velocity and consequently in the MDV. However, the histograms in Figures 15 and 16 show results from all case studies analysed, comprising many hours of data, which suggests that this velocity decrease is a consistent feature. We find it difficult to understand why upward air motion would consistently occur at this specific temperature. In theory there could be a consistent latent heat driven increase in buoyancy within the warm conveyor belt at this level, but the fact that this increase in vertical velocity “stops” rather sharply immediately above the dendritic growth zone seems counterintuitive, and would imply divergence at this level. Although this argument around latent heat driven motion is interesting, and we do mention it, we feel the evidence for it is limited. We support the reviewer’s comment that independent estimates of vertical air velocity add further rigour, and are a useful pathway for future studies.

L. 520-525: I have a general problem with the common interpretation that the presence of a secondary mode at -5...-7°C and super-cooled liquid water means “automatically” that we have evidence for rime splintering process. What we often see is that a new spectral mode is appearing at -7°C and that it is quickly accelerating and spectral LDR indicates that this mode is composed of columns and needles (as also aircraft in-situ data confirm). But in my opinion, the radar observations cannot tell anything about the original process (probably SIP) that generates the small new ice particles. Why does it have to be rime splintering? Why can’t also collisional fragmentation (or other SIP) cause the little ice pieces which grow then at -7°C efficiently into needles? It would produce exactly the same spectral signature, right? To me it would be scientifically sound to argue for rime splintering if we could proof that the secondary mode at -7°C appears ONLY in presence of rimed particles and a super-cooled liquid mode in the spectra. But from my own analysis of many Doppler spectra I can assure you, you see this secondary mode (around -8...-5°C) also in cases when the primary mode is not exceeding 1.5 m/s and if there is also clearly no liquid mode. How can we explain this? I think that we should be all very careful that we don’t just “see” processes which we expect but rather carefully proof that it can most likely be only one specific process! Currently, I am not convinced by the “proof” we have. Considering the latest lab findings of nearly non-existence of rime splintering I am even more sceptical about our “common” interpretation.

Our previous study was based on aircraft observations. From the case study analysed, two segments with a temperature of -5°C were examined in detail. One had liquid water present and vast concentrations of needle crystals were present, whereas in the other there was no liquid water and ice crystal concentrations which were consistent with primary nucleation. From this we suggested that ice-ice collisions could reasonably be ruled out as the main source of secondary particles since it is not sensitive to the presence of LWC. There was also little evidence for drizzle of a size at which droplet fragmentation is efficient, from which we suggested that droplet fragmentation was also not the main source of secondary particles. This has been made clearer in the manuscript at this point. But in general, we agree that new small ice particles produced by any mechanism at temperatures around -7°C would grow into needles by vapour diffusion. Further discussion regarding this has been added.

L. 536-539: Let's assume the dendritic structures grow on particle falling into the dendritic growth layer from above. The very efficient depositional growth would release latent heat which could cause (together with the larger cross sectional area of the aggregate+dendritic extensions) a slight updraft and hence a reduction of MDV of the main mode. To your second point: The number of collisions and subsequent fragmentation of ice particles which could produce a secondary mode depends strongly on the concentration and relative velocity distribution in the primary mode. Those are two quantities which we cannot very well derive from the radar observations. I think the only way to better understand these features is combining the radar observations with detailed model simulations. I am also puzzled by the fact that sometimes the second mode at -15°C appears and sometimes it does not. If you argue for riming playing a role (you refer to Fig. 15, is actually not rather Fig. 16?) I think you have to show that the velocity differences you see are statistically significant and not just a result of different samples. As you have the width of your distributions you should be able to apply suitable significance tests.

Whilst it should not be ruled out, our reasoning for why we believe an upward air motion is not causing these features is explained in response to your comment on L514. With regard to velocity differences between populations, the benefit of analysing many cases is the high volume of samples for each population. Figure 15 of the manuscript (now Figure 18) has been updated with the standard error of the distribution (calculated as the width of the interquartile range / square of the number of samples) for each temperature bin shown by the black error bars. As a result of this large number of samples, the error bars are very small; this gives confidence that the distributions are significantly different.

You are correct that reference here should be made to Fig 16 (now Fig 19) instead of Fig 15 – this has been fixed.

L. 541ff.: "When we examine the velocity of the primary population in multimodal spectra it is systematically higher than it is in monomodal spectra, which suggests that the processes involved in the formation of the multimodal population are also conducive to increasing the fall velocity of the primary population." Can you guide the reader where one can see this? If I compare Fig. 15c and d at -15°C , I find roughly MDV of 0.9 m/s for monomodal primary velocity and 1.0 m/s for multi-modal primary velocities. Given the width of your distribution I am very sure that this difference is not significant. In general, I like that we formulate and discuss also more unconventional hypothesis in our publications but here I think too many indications speak against it. For rime-splintering one needs strongly rimed particles (graupel-like) and larger super-cooled drizzle drops. Both of them are extremely rarely observed at -15°C and colder. A slight variation of the MDV of the primary mode around 1 m/s cannot only be explained by riming. Simply a slightly different composition of the aggregates by different monomer types or sizes might cause such a variation. But even if your observation is correct and the primary mode velocity of multi-modals spectra is larger, I think a simple alternative explanation can be given: Let's assume the new slow mode is initiated indeed by collision-fragmentation (maybe collisions of aggregates with dendritic extensions). The break-off of those dendritic extensions would reduce the cross sectional area of the aggregates and could cause an increase in their terminal velocity. Maybe the collisions even lead to larger aggregates (could be

evaluated with DWR spectra) with slightly larger fall velocities. As long as our MDV are below 1.5 m/s many different particle properties can cause variation in terminal velocity. Riming is certainly only one of them.

In panel (f) of Figure 15 (now Figure 18), we show the medians of the distributions of the different populations. Here it can be seen that the median of the primary velocity (it is important to remember here that these are not the MDVs of the populations, but the velocity at which the peak was identified) in multimodal regimes is greater than the median of the primary velocity in monomodal regimes. Whilst there is overlap between the IQRs of the populations, the medians are noticeably different.

Thank you for the alternative explanation – it has been added to the discussion.

L. 549-556: I completely agree! I think one way to approach the problem further (in addition to lab studies, which are not trivial to do with snowflakes) is to use detailed modern modelling approaches. This includes aggregation and riming models but also tools such as Lagrangian super-particle models (e.g., Brdar and Seifert, JAMES, 2018) which include secondary ice processes, habit dependent growth, and where the history of particles and their interactions can be traced. Together with extensive scattering databases and radar forward operators (e.g., von Terzi et al., GMD, 2026) we have great tools to link microphysics and radar observables.

Thank you for pointing these studies out. This section has been expanded slightly to acknowledge that super-particle models are one way of developing greater understanding of processes active in the DGL.

L. 591 ff.: In this respect the recent publication by Pfeifer et al., 2025 <https://doi.org/10.1038/s43247-025-02953-3> should be added.

Reference to Pfeifer et al has been added in the introduction instead of at the end of the manuscript.

Typos:

1. L. 253: “the the”
2. L. 518: “up to -20°C of spectra”?

Both fixed.