

Reviewer 3

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.

This is an interesting analysis of radar spectra to analysis differences in velocities and temperatures in mono and multimodal spectra. The authors have some good ideas, but I have several major comments. I have specific concerns about how the authors categorize modes and assumptions about which modes are liquid or ice without sufficient evidence. I have quite a few comments, but want to be clear that this paper is promising and I look forward to seeing it in its final form.

Thank you!

General/Major Comments:

My most general concern lies with the justification for your methods. You use an existing scipy function with specific parameters and make a brief comment that your method is in agreement with Radenz et al. (2019) and Vogel et al. (2024), but do not demonstrate any comparison to existing methods or justify the importance of having a new method. I can assume your method was chosen because of how efficient functions from scipy can be to run on large datasets, however this should be in your paper. More critically, without a comparison to existing methods shown in this paper, it is hard to justify the use of the method and understand how accurate it may be in detecting secondary and tertiary modes. Specifically towards the end of your paper (lines 568-569), you state there is no way to quantify the error of inaccurate peak detection, however if you presented a statistical comparison with any other preexisting methodology (e.g., Peako-Peaktree as you referenced in developing the method), you can establish more confidence in the method and quantify the differences in errors between methods. It would improve the paper to include a one to one comparison of your method. Having 23 cases, depending on the time periods of each used, may be tedious or overdoing it to run the methods from Peako-Peaktree, but at minimum confirming it on the two shown case studies would build much more confidence in the method and results. Any other method you choose to compare against would significantly improve the strength of your conclusions here.

SciPy's find_peaks function was indeed chosen because it is highly efficient when dealing with large datasets; a statement explaining this has been added to Section 3.1.

With regard to a comparison between different methods for identifying peaks in Doppler reflectivity spectra, thanks for this interesting idea. We feel that an intercomparison of find_peaks with Peako would be interesting, but is beyond the scope of the present study. Such a comparison would likely show similarities between the results from the two methods, but it would also likely show differences in certain regions of cloud. This then raises the question: which results are the 'truth'? There is no unambiguous reference to tell us this.

Peako is based on supervised machine learning; our understanding of this process is that visually inspected and labelled spectra are used in the training phase of this method. In our study, once the peak finding algorithm has been applied in the method presented in our study, visual inspection is used to validate that peaks are being identified where we expect. Ultimately the design and verification of any peak detection method is based on manual inspection, and this verification is already included as part of our study.

Section 3.1.3: I am particularly concerned with the classification of the identified modes as primary and slow. I am equally concerned about not having enough evidence to declare which modes are drizzle or secondary ice. Lines 267-268 state that the primary mode is determined by reflectivity. However in the next sentence, you state the secondary mode is classified by velocity being faster or slower than the primary mode. First, "fast mode" never comes up again. Secondly, knowing there are

tri-modal events in this study, it is confusing and ambiguous how they are being evaluated and compared to the full dataset. A higher reflectivity mode will not always be the fast or slow mode, and a more robust way of naming and categorizing the modes is important for understanding the potential processes being observed. There is no comparison between the reflectivity of each primary, secondary, or tertiary mode (beyond the shown spectrograms of two examples). In the bulk of the analysis, you compare “primary” modes defined by reflectivity to just the “slow mode” which remains ambiguous, despite your assumptions. On a large scale, I am concerned by essentially throwing out the drizzle modes in favour of only showing what you presume to be the primary mode and the ice mode. You make this assumption based on fall speed which concerns me. In the absence of aircraft observations or linear depolarization ratio to make a more confident guess on if a mode is liquid or ice, we need to be able to see each of the three modes to have an understanding of if rime splintering could be occurring in the subset of trimodal events.

Throughout the manuscript, the ‘primary mode/population’ is referring to the population of particles sedimenting from the cloud top. As seen in the spectrograms shown in the manuscript, this primary population typically has a discernibly higher spectral reflectivity than any secondary/tertiary populations which are present. Wugofski and Kumjian (2025) follow the same approach when differentiating between primary and secondary populations. The higher reflectivity mode is therefore not the fast or slow mode, but the primary population.

In all of the spectra that were examined visually (including those in the main manuscript, those in the supplement and many more), any secondary population present (secondary in the sense that it is the second population, not that it was necessarily formed by SIP) is falling slower than the primary population, hence the chosen terminology of ‘slow mode’. Trimodal spectra are identified relatively infrequently (1.8% of all spectra compared to 11.7% for bimodal spectra), and inspection of the spectrograms in these regions show that the drizzle mode is the slowest falling of the three populations – see Figure 10 (now Figure 12) in the manuscript and Figures S1, S17 and S23 in the supplement. Concerning the ‘throwing away’ of the drizzle mode – the focus of our paper was investigating multimodal ice populations, and drizzle is therefore of interest in the context of deducing microphysical processes which may be influencing the Doppler spectra (e.g. the availability of liquid particles for SIP), but in the bulk of the analysis we are focussed on the ice phase particles.

With regard to the ‘fast mode’ – you correctly point out that it is not talked about at any other point in the manuscript. A ‘fast mode’ is identified infrequently (on ~2% of spectra), and inspection of the identified fast modes suggests that they are data artefacts on the fringes of the spectra resulting from imperfections in the (analogue) sampling of the in-phase and quadrature components of the radar signal. For this reason, the fast modes are not included in the analysis. This discussion is missing from the original manuscript, so has now been added in Section 3.1.3.

Specifically from differentiating between modes in 6 Jan 2017 case study:

“When a trimodal spectrum with two slower falling particle modes is identified, the faster of these slow modes is shown in Fig. 9c as we assume this mode is associated with ice particles instead of a drizzle mode, which would likely have very low fall speeds” - this is a bold assumption, especially without showing results in Figure 9 from all detected modes. We are not shown clearly the individual characteristics of each mode, and the spectra in figure 10 struggle to clearly show the multimodal region clearly because the x-scale is set to show the speeds of rain. What should be clear are velocities of the multi-modal layer from -3 to 0 ms⁻¹.

Tertiary velocities are now shown in Figure 9 (now Figure 11). Figure 10 (now Figure 12) also been significantly changed, and includes the velocities of the separate populations plotted on the spectrograms. The spectrograms shown have also been updated to focus clearly on the spectra above the melting layer.

Differentiating between liquid modes and ice modes:

Generally, there seems to be an assumption before the analysis that most or all multimodal events within -8 to -3 deg C are specifically SIP and rime splintering. I am not sure enough consideration is being given to identifying what the modes are before making those assumptions. Korolev and Leisner (2021) highlights this assumption towards rime splintering and points out the varied results from different studies, which I take as advice to caution assumptions that rime splintering is dominant in the atmosphere. This assumption appears to guide most classification in the discussed cases, rather than relying on something more objective. I had specific comments and concerns on this. The secondary mode could be drizzle, primary or secondary ice, though there is still a chance the mode is also liquid. Liquid and ice modes can be differentiated with LDR (e.g. Oue et al., 2015; Sinclair et al., enough evidence for confidence on the active process and other processes should be considered, and weighed. I think it is quite likely you are correct that it is occurring, but we need to see more for the degree of confidence here.

This was commented on by another reviewer as well. Rime splintering was intended to be described as one possible explanation for the signatures we observe. More consideration has now been given to other secondary ice mechanisms in the discussion.

Specific comment in line 378: Would you say that the drizzle mode persists in the first case, as you're using the argument that it doesn't in the second case? I struggle to see this in the chosen times for the first case, which worries me about using the justification confidently for the second case.

Figure 10 (now Figure 12) has been updated and now shows one-dimensional spectra from heights above and below the melting layer. We can see that at heights above the melting layer, three peaks are visible in the Doppler spectra. Below the melting layer, two populations are visible, despite the broader and noisier spectra caused by the liquid phase particles. One-dimensional spectra for the second case (Figure 13, now Figure 16) do not show this multimodal signature below the melting layer.

Analysis shown in Figure 15: I think it could be beneficial to repeat this analysis with a standardization of the primary mode MDV to better illustrate the typical separation between primary, secondary, and tertiary modes. You capture a spread here, but I wonder if information is being lost by averaging such a large and variable dataset, which could be masking more detailed insights into the typical velocity separation rather than spread.

We chose not to standardise the primary mode vertical velocities because the resulting distributions and trends were essentially unchanged, with the main difference being a rescaling of the x-axis. We therefore retained the original velocity units, as these provide a more direct physical interpretation of the magnitude of the velocity separation. In response to the reviewer's broader concern that averaging across the dataset may obscure the typical separation, we have instead added quantiles and error bars to Figure 15 (now Figure 18) to better represent the distribution and variability within the dataset.

In general, I have some mild concern for only showing two cases; is there a way to visualize even a snapshot of all 23 effectively? The answer might be no, but it was a recurring concern as I read through the paper because I do not have a way to see more of those cases to understand how they contribute to the averages presented.

We agree entirely with the reviewer that this is important information, and the supplement contains details for each of the cases analysed, including plots of reflectivity, the peak finding results and two example spectrograms per case. We believe this is a more effective way of visualising all the cases involved in the analysis than trying to squeeze this into the main text. The material in the Supplement (and indeed its existence) was poorly signposted in the original manuscript, so this has been made clearer.

Minor Comments:

General minor comment: For the 23 cases, you consider both warm, cold, and occluded front events. Is there any merit to splitting this into two separate datasets and examining differences? Examining them as a subset is hopefully not too arduous, and you may have already examined this and decided there is no difference, but it would be useful to know.

This is an interesting suggestion. The difficulty in investigating this question arises from the fact that synoptic-scale low pressure systems are rarely as organised as the conceptual Norwegian/Shapiro-Keyser models of low pressure systems. As indicated in Table 1, the synoptic situations associated with the cases analysed are often complex, with multiple different fronts occurring in quick succession, and it would therefore be difficult to segregate the features with belong to each front. However we take this as an interesting avenue to consider for a future study.

Line 27: You could include additional references beyond the 2026 paper, because it is known and studied by a wealth of publications that accurate representation of these PSDs remains a challenge. Additional citations and examples would strengthen this.

We agree – these have been added.

Line 54: “to infer microphysical processes” treads a little close to suggesting we observe processes. I’d use a little caution and suggest a rephrase, because the observations from a radar are observations of properties of the clouds/precipitation rather than of a confirmed process.

Noted; the wording has been changed to say ‘to suggest microphysical processes which may be active and result in differing velocities’.

Line 57: You cite the review article Field et al. here, but don’t include any of the more foundational papers associated with this. Taking a look at the older papers in the citations in the first paragraph of Field et al. where this claim is restated would strengthen this paper.

This paragraph has been added to, including some older references.

Line 5, Line 83: I want to point out a study that also quantifies the temperatures associated with multimodal spectra, Wugofski and Kumjian (<https://amt.copernicus.org/articles/18/6569/2025/>) which seems to complement some of your findings. The study is not specific to frontal clouds, but has some similar findings (see Figure 8).

Thank you, and indeed we are aware of this study which we have been exploring another facet of in another piece of work. Whilst the Wugofski and Kumjian study provided valuable results, their distribution of multimodal spectra is based on moment-level observations (i.e. enhanced spectrum width) as a proxy for multimodality, rather than direct detection of multimodal spectra, but we agree that this study should be acknowledged and so this has been added (Section 4.4 and elsewhere).

Lines 138-149: It is unclear and needs to be clarified if this threshold is based on a single case. You state the turbulent contribution is low here, and then apply the calculated turbulence threshold. Is this recalculated for each case, or is the same threshold used in all cases? Is this checked in the cases that are not shown as case studies but still used for the overall calculations to determine the distribution of multimodality with temperature?

The same threshold is used in all cases, because our spectral resolution remains the same throughout the dataset. The turbulent contribution was calculated and examined dynamically for all cases when choosing the threshold, since this depends on the measured velocity fluctuations and the horizontal wind speed. This has been clarified in the updated manuscript.

Line 170: I am somewhat concerned at the infrequent availability of temperature data (hourly) when attempting to address the temperatures associated with radar spectra with 1 minute frequency. Are there any periods affected with large changes in temperature between hourly data points? Are these potentially skewing any results?

Our cases are fronts associated with extratropical cyclones, and do not have fine scale temperature fluctuations. Mittermaier and Illingworth (2003, <https://doi.org/10.1256%2Fqj.02.19>) compared the height of the 0°C isotherm from a mesoscale modelled against the height of the bright band for several frontal cases. They found that the models were able to capture the height of the bright band and the shape of the frontal surface with very good accuracy. Comparison of the heights of the bright band and modelled 0°C isotherm for our own cases showed similarly good agreement, which gave us confidence that large temperature differences between the modelled temperature and true atmospheric temperature are unlikely, and are not introducing large uncertainties in our results. The coherence of our statistics as a function of temperature is also evidence that there is not a large random error in these data. This discussion has been added to Section 2.2.

Section 3.1.1, Lines 193-195, and Table 2: I think the choice to not include a parameter for distance between peaks needs to be addressed further, similar to the choices for smoothing (figure 5) and prominence (figure 6). While not part of the detection, another related parameter that may be useful to retain is skewness. It can be informative and previous work (e.g., Luke and Kollias 2021, though other work by both applies here) characterizes the skewness of radar spectra and relates it to the presence of drizzle. You often assume the presence of a drizzle mode, so generally incorporating skewness into the analysis may help address other comments I have regarding determining what each mode is made up of.

Evaluation of the distance parameter has been added in a similar manner to the evaluation of prominence and smoothing. Skewness has been added into the discussion regarding both cases, and we find that regions of high skew are consistent with regions where multimodal spectra are identified using the peak finding algorithm. With regard to ‘often’ assuming the presence of a drizzle mode – only when trimodal spectra are identified do we assume that drizzle is likely to be present. As mentioned in response to a previous comment, this occurs infrequently in 1.8% of spectra. As the focus of the paper was identifying multimodal ice populations, drizzle is discussed in the context of the microphysical processes which may be invoked by its presence, not in the context of identifying how frequently it occurs. The first case study was chosen to demonstrate that trimodal events are caused by microphysical processes and are not artefacts of the peak finding method, and that each peak can therefore be assigned to a particle population.

Lines 241-243: This sentence contradicts the previous and suggests that mixed phase processes are not active at temperatures colder than 0 deg C. I would recommend more precise language.

The wording here has been changed to say ‘we are interested in processes active in ice and mixed phase regions of cloud, and therefore apply a temperature threshold to neglect spectra at temperatures warmer than 0°C’.

Line 272: You say the proportion of ambiguous cases is small; it would be advantageous to quantify how many were ambiguous. Was it one of the 23 events? Was it a certain number of minutes or hours across the entire dataset? As a reader, I want to know and be shown by the author that the impact is limited rather than just be told.

Providing a quantitative estimate of the number of ambiguous cases is difficult, as it would require individually inspecting the identified primary and secondary velocities against their spectrograms. However, we have confidence that only infrequent, isolated spectra are being affected by this, and that this misclassification is certainly not impacting whole cloud systems/days. This explanation has been added to Section 3.1.3.

Line 426: You state that multimodal spectra below -20 deg C are rare, but you do not quantify their occurrence in your study. For example, Wugofski and Kumjian (2025) found about 5% of cases across a three year period to be colder than about -20 deg C. A similar statistic from your work would make this claim stronger. Later in line 568, you state your analysis is the first time someone has quantitatively estimated the distribution of multimodal spectra with temperature; the analysis is

different but does quantify multimodal layers/events by temperature so steering away from “first” might be best.

The distribution in Figure 14 shows that <1% of spectra between temperatures of -20°C to -50°C are multimodal. We have calculated the percentage of all multimodal spectra which occur at temperatures between -20°C to -50°C and find it to be very low at 0.64%. This has been added to the discussion at this point.

Wugofski and Kumjian did not directly detect multimodal spectra, however references to this being the first multimodal spectra distribution with temperature have been removed throughout to avoid confusion.

Paragraph from line 526-535: This discussion of the cold modes relates to the above minor comment, a sizable proportion of the events in Wugofski and Kumjian (2025) occurred between -8 to -18 deg C. There may be additional insights therein, however the authors partitioned temperature in discrete categories so the ranges are not identical to yours. I also noted in line 572, your 40% of multimodal spectra from -3 to -8 aligns pretty well with Figure 8 from that paper as well.

It is not immediately clear why the proportion of multimodal events between -8 and -18°C in Wugofski and Kumjian is different to our results, but may be partly a result of the way temperatures are partitioned. Direct comparison of these results is beyond the scope of the present study, but is being investigated as part of current work by the authors.

Lines 552-553: Are there already papers examining the fall speeds of early aggregates published? Dunnavan (2021) initially comes to mind, and references therein may be able to answer or provide insights into this.

Whilst there have been several recent papers (which are now referenced in the manuscript at this point) investigating aggregate fall speeds in the context of improving parameterisation, answers to the specific questions posed here are still unclear.

Table 1: Consider if estimating depth “from examination of radar images” (I assume visually?) is justified or using an alternate threshold to quantify cloud top is easily available or able to be calculated. Also consider that “within 5 degrees C” is precise enough to describe these events.

As this is just for a basic overview of the cloud for each day, we believe this is sufficient. Cloud top is not uniform in any of the cases, and for this reason the precision stated is appropriate. Reflectivity and peak finding results with temperature contours for each day can be found in the supplement for a better overview of each case. The supplement is now signposted here.

Figure 2 (and in general): Most figures showing radar moment data over time show the entire 12 km depth and 24 h time span, which makes it challenging for readers to easily see the features of interest. Reconsider how you’re visualizing these to ensure readers can easily identify the important features. Additionally, I would recommend confirming if this color scale is colorblind friendly. Related, do you disclose what hours of each event you use in the study? Are all 24 hours considered, including periods that do not contain frontal clouds? If so, I would consider adding information regarding that.

This is a good point. Plots for the two case studies shown have been “zoomed in” to make features of interest easier to see.

The colour scale used in Figure 2 (and the majority of the other figures in this paper) is the Batlow colour map, which is designed to be black-and-white/colourblind friendly (one of the authors is colourblind and has confirmed this is clear!).

All 24 hours of each case study are included in the analysis. A statement explaining this has been included in Section 2.1.

Figures 5-6: Both figures have useful information that is challenging to read in the current format of these figures. The black line overlaying the grey could be dotted or dashed to remedy this. In figure 6, you indicate that small changes have little effect. It is challenging to see the effects it does have when the panels are significantly smaller than in figure 5, which leads to the magenta dots covering the peaks more so than highlighting them. Additionally for both of them it would be advantageous to include a reminder of the units (dB for Pr) in the figure or caption.

We have changed the linestyle of the black line (now dash-dotted), which has improved visibility of the grey line underneath; the grey line has also been made slightly darker. The magenta dots in Figure 6 have been made semi-transparent so that the peaks can be seen underneath. A reminder of units has been added to the figure captions.

Figures 9 and 12: This relates to my major comment above. I think it's critical to, instead of just showing "slow mode velocities" clearly label the modes as "primary, secondary, tertiary" and show all three. For the second case with fewer tri-modal occurrences, this might not be needed. Additionally as stated above, narrowing the figures to specifically show the time periods of interest would make results much easier to interpret. An additional suggestion would be to plot the difference between the velocities of the primary/secondary mode, as when viewing the figure I had to attempt to do this in my head to understand the separation of the modes.

Tertiary velocities are now plotted for the first case study; as you say, there are very few trimodal spectra in the second case so tertiary velocities have not been plotted here. The difference between the primary and secondary velocities have been added to the discussion for both cases.

Figures 10 and 13: This is addressed above, however I strongly would reconsider taking daily/24 h averages of radar spectra. For example, 0-4 UTC 6 Jan 2017 being averaged with the period from 12-24 UTC does not sound meaningful. Reconsider if you'd like to add additional figures showing hourly (or 15-30 minute periods) of averaged spectra limited to the period of interest instead. The benefit of spectral data is in those fine details that are lost in such a large temporal average. You point this out specifically in the second case study, which should indicate that this specific panel of your figure is not capturing something clearly for your audience. This could be much more meaningful with a smaller window of averaging, perhaps 1 hour (or 15-30 minutes).

We have taken your advice and these average spectrograms have been removed in favour of showing specific examples, which are more useful to the discussion.

Figure 14: It is not clear why (b) is needed, as this is easily visible and well shown in panel (a) already. Panel (b) is included to provide a clearer and cleaner summary of the overall statistics shown in panel (a).

Figure 15: It is initially unclear if this is an analysis from all cases or a single case - I assume it's aggregated from all cases, however that should be clearly stated. Additionally, reformatting panel (f) would make the variations easier to see. Moving or modifying the legend to accommodate a better x-scale could help.

This is analysis from all case studies, and this has been explained more clearly at the beginning of Section 4.4. Panel (f) has also been reformatted – the legend now simply refers to the other panels ((a), (b) etc) so that a higher resolution x-scale can be used.

Figure 16: This figure could benefit from adding a panel similar to 15(f), particularly because the changes in median are small. Visualizing those medians across a small x-scale would help tell the story more so than just reading the accompanying paragraphs.

A panel similar to 15(f) has been added, we agree that this makes comparison between the medians simpler.

Typographical errors:

Line 357: Figure 2 is referenced instead of figure 12. [This reference to Figure 2 is correct, as we are referring to where the turbulent contribution to spectral width is estimated in Section 2.1.2.](#)

Line 365: Check the velocity units. [Fixed.](#)

Line 374: Figure 10 is referenced instead of figure 13. [Fixed.](#)

Line 518: I assume 20 deg C should be 20%? [Fixed.](#)

Line 539: I believe “then” should be “them”. [Fixed.](#)