

Reviewer 4

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.

Review of the manuscript “Characterising the occurrence of monomodal and multimodal ice hydrometeor populations and their fall speeds in midlatitude frontal ice clouds using radar Doppler spectra” by Mammatt et al.

The study of Mammatt et al. 2026 addresses the relevant topic of the occurrence of multiple hydrometeor modes in stratiform cloud systems.

I found the manuscript worth reading and the presented approach well implemented. Thus, basically there are no major concerns which would hinder a publication of the study.

Nevertheless, I list below a few minor comments which I would consider helpful for an improved representation of the work.

Frequent usage of ‘evidence’. I kindly ask the authors to review their manuscript with respect to this strong impression. Apparently, the word is inspired by the title of an earlier very relevant study of one of the co-authors of the study. There are some examples, where ‘evidence’ truly does not apply. E.g., line 325, where Fig. 10b is discussed. It might be likely that there is a drizzle mode present. But there’s definitely no evidence of it. Basically, I kindly ask the authors to only use the word evidence, where there’s actually a way of proving their statement. Interpretation is not an evidence.

The word evidence is used here to mean data that supports one of the arguments being presented. It was not intended to imply proof or a conclusive interpretation (there can be weak evidence and strong evidence for an idea). To avoid this impression (which we did not intend) we have reworded the relevant parts of the manuscript.

Section 2.1.1:

- The definition of the range correction is wrong. It has nothing to do with the beam width. It has to do with isotropic expansion of the radiation. The transmitted power P_t scales on a sphere defined by $2\pi R^2$ $P_u = P_t / (2\pi R^2)$

Agreed, this was not well expressed, and has been changed.

- P_u needs to be defined. It should be included in the beginning of the section, together with P_{noise} .
- It should be noted that Z_c is in dB scale, same for P_u and $C_{calibration}$

P_u is now defined with P_{noise} , and it has been made clear that Z_c , P_u and $C_{calibration}$ are all in dB units.

Line 90: “it takes observations of the Doppler spectra every minute”. This should be clarified. Is there a spectrum (of which duration) every minute? Or are the spectra averaged over 1 minute? If the second item applies, I kindly suggest to the authors to elaborate a bit more about temporal smoothing effects. E.g., Radenz et al. 2019 or Vogl et al., 2024 are based on spectra smoothed across 2-3 seconds. Both study contain evidence for the reason why such short averaging is required. Thus, a discussion is needed about the suitability of longer averaging times.

The radar measures a 1-second spectrum which is recorded every minute – no averaging of the spectra takes place. This has been clarified in Section 2.1.

Line 197: What is meant by “effects of a sampling a finite number of independent realisations of the hydrometeor population”?

We are referring to the fact that the weather echo fluctuates randomly, as a result of the time-varying relative positions (and therefore relative phases) of the scatterers (see, for example Keat et al 2016 <http://dx.doi.org/10.1175/JAMC-D-15-0272.1>), and this manifests itself in random fluctuations in the value of sZ in each velocity bin. The precise magnitude of this random error is dependent on the number of pulses collected, the resolution of the Doppler spectrum, and the width of the spectrum itself.

Line 314: which velocity is meant in ‘we see that velocity increases’? Can you specify?

We are referring to the increase in the velocity of the primary population here – this has been clarified.

Line 370: Why couldn’t the slow-falling peak at -0.25 m/s correspond to a liquid layer?

It is possible that this slow-falling peak consists of small (~100 micron) drizzle drops (this has been acknowledged as a possibility), but liquid cloud droplets settle at velocities much slower than this.

Line 375: Figure 13, not Figure 10

Fixed.

Vertical velocity vs. fall velocity. The authors should spend some space in the discussion/conclusion about the differences between vertical velocity and fall velocity. The study makes the impression that both terms are the same for stratiform clouds, but this is by far not evident.

Throughout our analysis, the velocities analysed remain in Doppler velocity space; at no point are these velocities treated as fall speeds/terminal velocities. But you correctly point out that references made to ‘fall velocity’ imply that we are treating this as analogous to ‘vertical velocity’. For consistency, the term ‘fall velocity’ has been changed to ‘vertical velocity’ throughout.

Being a bit more critical with respect to the applied smoothing. It is indeed the case that the smoothing affects the number and location of peaks. E.g., also the MDV might be affected if a peak ‘moves’ due to averaging/smoothing. Providing some considerations about this would complement the discussion section.

To avoid significant blurring of the spectra, our data have an integration time of 1 second. This avoids the peak moving during the averaging window, which as the reviewer indicates is important. We do smooth the resulting spectra in velocity space, but this is different, and does not introduce the artefacts mentioned.

Line 491: The reference to Fig. 3 refers to the study of Westbrook and Illingworth 2009, right? Please specify.

That is correct and has been clarified.

Code publication. It should be aspired to publish the code of the retrieval

Processing of the radar data is individual to each instrument, and is therefore difficult to incorporate into a standardised piece of code. The peak finding method uses a standard Python algorithm, the settings for which are few and described in the manuscript. The spectral data can be made available on request to the authors; this has been added to the data availability statement.

Polarimetry: It would be great if the authors could discuss a bit about the possible advantages of using polarimetry as additional constraint. Polarimetry would, e.g., help to constrain the microphysical properties of the individual fall streaks. Other studies, such as Radenz et al., 2019 or Vogl et al. 2024 apply polarimetric methods, which aids in an improved classification

Unfortunately polarimetric radar data was not available for the dataset analysed here, but some discussion about how it could benefit future similar studies has been added in Section 4.5.

Further points for the discussion: I missed to see some references to basic observational techniques which would indeed help to provide evidence of the presence of certain processes. E.g., the detection of liquid water could be utilized by using lidar techniques or machine learning techniques such as the VOODOO approach (Schimmel et al., 2021)

Reference to these methods has been combined with that added in response to your previous comment. We did start to investigate the lidar in an attempt to identify liquid water, but unfortunately the lidar was fully attenuated within these deep precipitating cloud systems.

Oue et al., 2018, Wugofski et al. 2025 are also studies which use and interpret peak-separation techniques. The authors might consider to include these references into the introduction and/or discussion.

This has been added to the introduction.

Spelling and grammar: I identified several typos and grammatical errors. But I leave the correction of them to the numerous native English-speaking co-authors of the study (preferred) or an AI.

The revised manuscript has been proof-read carefully by the authors, so these typos have hopefully now been corrected.