Response to Referee 2:

Referee 2:
This manuscript is well written and organized. The authors present a technique to use vertically-pointing dual-polarization data to automatically isolate the precipitation signal in the data. This single-polarization precipitation data is the analyzed to obtain information about the precipitation overhead, including an estimation of riming. This paper is a good contribution to radar meteorology and should be published in Atmospheric Measurement Techniques. There is room for improvement and several details that should be clarified. These are detailed in the attached document.

Authors:
Thank You for reviewing our manuscript. Based on Your comments and suggestions, we have provided additional details of our methodology and modified the presentation of some results for clarity. Point-by-point replies and the corresponding changes in the revised version of our manuscript are listed below.

Referee comments in the attached document:

• line 15: I assume RMF is rime mass fraction. It should be defined.
Following the Referee's recommendation, RMF is now explicitly defined in the abstract of the revised manuscript in line 15.

• line 69: birdbath?
Yes, thank you. Corrected in line 69.

• line 115: The Blackman window should be better defined. I refer the authors to Harris 1978 (F. J. Harris, "On the use of windows for harmonic analysis with the discrete Fourier transform," in Proceedings of the IEEE, vol. 66, no. 1, pp. 51-83, Jan. 1978, doi: 10.1109/PROC.1978.10837.) for clarification. Are you using the Blackman window of Harris figure 22 or figure 23? Both of these windows are excellent windows, but they are not the same.
Thank you for pointing us to this excellent overview of window functions used in signal processing by F. J. Harris. After comparing the numerical values of the coefficients for the window function used in our signal processor with the different flavors of Blackman windows described by Harris 1978, we find that we use Harris' 3-term Blackman-Harris window that achieves the minimum sidelobe level of -67 dB (shown in his Fig. 24).
Based on these findings, we have specified the flavor of Blackman window in the revised manuscript in line 117f, following Harris 1978, and also included a reference to this window function in his paper.

• line 116: I suggest deleting this sentence as it doesn't add to the discussion.
We followed the Referee's suggestion and deleted this sentences in the revised manuscript.

• line 128: corresponding?
Yes, corrected in line 128.

• line 174: SciPy is a large package. Please be more specific and cite the routine or routines that you use.
We use the ndimage.grey_closing() routine in SciPy. As requested by the Referee, the name of the grayscale closing function is now mentioned in the text in line 183.

• line 175: smooth?
The sentence was rewritten in line 184f to better express the idea that grayscale closing was chosen to essentially fill small holes, or deep minima, in the radar data.

• line 175: high?
As noted for the comment above, this sentence was rewritten in line 184f to better express the idea that grayscale closing was chosen to essentially fill small holes, or deep minima, in the radar data.

• line 225: Figure 4 would be better if it had C1, C2, and B1 labels on the lines.

Following the Referee's recommendation, we have now included the 3 different threshold identifiers as labels in Fig. 4 and also list them in the caption.

• line 228: Is the interpolation done in linear power space or log (dB) space?
The interpolation is done in dB space.
We have included this information in line 238 in the revised manuscript.

• line 234: The weather signal has been isolated very nicely. Now you analyze the part of the spectra that compose the weather signals. Do you use data from one polarization or are data from both polarizations combined? If combined, how are they combined?
We use only the data in the H polarization channel for the analysis. The V channel does not add new insights to the meteorological interpretation.
To better explain the final interpolation step for isolating the weather signal for subsequent quantitative analysis, the corresponding paragraph was rewritten in the revised manuscript (see line 236ff). In the revised manuscript, we also remind the reader in line 243f that only the filtered and interpolated data in the H polarization channel are used as the weather signal for analysis.

• line 235: weather?
Yes. Corrected in line 245.

• line 272: precipitation?
Yes. Corrected in line 285.

• line 289: I suggest replacing "a suitable" with "the UniDip". You have established this as the technique you are using for you proceesing. There may be other suitable methods but here the issue is what you are doing for the processing.
As the Referee states, being more specific here is probably better. So, we replaced 'a suitable' with 'the UniDip' in line 302 of the revised manuscript as suggested.

• Fig. 6: As a reader I find this hard to compare with Fig. 5a since the y scales are different. My recommendation would be to the y scale of Fig 6 for Figure 5a. This would remove a lot of "white" space from 5a and allow direct comparison.
The original idea was to have the same y-axis scales for Figs. 1, 3, and 5a, i.e. for all postprocessing steps applied to isolate the weather signal. But, as Referee 2 notes, it makes probably more sense to instead have consistent y-axes for Figs. 5a and 6 (and 7) to better compare the analysis results with the isolated weather signal.
Therefore, we followed the recommendation of the Referee and modified the y axis of Fig. 5a, which is now consistent with Fig. 6.

• line 313: The authors have all the data and processing. Rather guessing about the cause of the multimodality could the original and intermediate data be examined to determine the cause? This multimodality could be an artifact of the processing, or it could be caused by some physical process that might tell more about the meteorology.
For these very weak Doppler powers (compared to the main precipitation signal at lower altitudes), a definitive statement is difficult. This ambiguity in interpreting our analysis results at very low Doppler powers was originally implied by the use of the qualifier 'probable'. This conclusion is also consistent with finding generally higher uncertainties for the calculated quantitative characteristics of the spectral modes at higher altitudes in our uncertainty assessment in line 362f. Nonetheless, a detailed comparison of radar output, postprocessed data, and quantitative characteristics, as recommended by the Referee, indicates that this multimodality is indeed an artifact of the postprocessing, and of the interpolation routine in particular.
In the revised manuscript, we have addressed the Referee's comment by rephrasing the corresponding text passage accordingly (see line 325ff).

• line 316: Is there are reference to atmospheric turbulence or are you assuming atmospheric turbulence is the cause?
In this specific case, we assume atmospheric turbulence is the cause, because we are looking at the initial more convective phase of a snowstorm. The analysis of birdbath scan measurements before and after

the birdbath data analyzed in the manuscript also suggests variable vertical air movements over a short time scale with various degrees of spectral broadening, characteristic of turbulence.
Since such a clear interpretation is not evident only from the one example of birdbath data and analysis presented in the manuscript, we do not include our specific interpretation as turbulence in the revised manuscript (deleted reference to turbulence in line 331).

• line 328: Is BMA bimodal amplitude? It needs a definition.
Yes. BMA is now defined in line 342 of the revised manuscript.

• line 330: relatively?
Yes. Corrected in line 345.

• Fig. 8, 10-12: This would be much easier to understand of the color scale were at the left or right of figure a so the plots were all the same size with the same y axis scales. This comment applies to Figures 10-12 also.
Based on the Referee's comment, the vertical scales of the subplots in Fig. 8, as well as in Figs. 10, 11, 12 are now aligned in the revised manuscript.

• line 422: analysis?
Yes. Corrected in line 437.

• line 564: Figure 12 appears to me to be affected by strong vertical motions, not atmospheric turbulence. Turbulence affects the width while vertical motions changes the mean fall velocity of the precipitation.
Fig. 12a alone does not show clearly that turbulence also plays a role here, because some parts of the fringes of the weather signal are cut off by the postprocessing algorithm. This loss at the fringes is the cost of rejecting most of the clutter and background noise here to isolate a relatively 'pure' weather signal. We also state explicitly in the discussion of Fig. 12 the strong impact of atmospheric air movements (causing the striking zig zag shape of the Doppler spectra profile, see line 535ff of the revised manuscript).
To not suggest that the characteristic shape of the Doppler spectra profile in Fig. 12 is mostly due to turbulence and not due to vertical air motions we have replaced 'turbulence' by 'vertical air motions' in line 580 in the revised manuscript. And for completeness, we have added a sentence about the loss of the fringes of the weather signal due to the postprocessing in the discussion of Fig. 12 in line 519ff.