General Remarks

We thank Max Maahn and the second referee for the reading the manuscript again and making valuable suggestions. The referee comments are marked in grey and the response in black with indentation.

Specific Reply to Referee #1

I thank the authors for carefully addressing all my questions and comments. The manuscript has significantly improved and I have only a few specific comments and corrections to add before I can recommend it for publication.

We thank the Referee for the positive opinion.

Minor:
P.3, L. 7: "The pulse repetition frequency was 5kHz and one Doppler spectrum was based on the fast Fourier transform of 256 pulses, yielding a Doppler resolution of 0.082ms^-1 (Tab. 1)." This sentence is maybe a bit misleading: You generate one raw Doppler spectrum based on 256 pulses. As you say a few sentences later, the actual spectrum stored and the one which is analyzed in your study is an average over 195 raw spectra. Thus, I suggest to write "raw spectra" in this sentence to make the difference to the final spectra more clear.

We added 'raw' as suggested.

P.3, L.20: Why is the polarization leakage problem even more relevant for spectral LDR? Also later in L. 25: Are you requiring the spectrum in linear units to be a factor of 3 larger than the noise or does it relate to the dB space? Why factor of 3 and not 10? Is that based on previous studies or did you do your own sensitivity study using different thresholds and comparing the total LDR?

Indeed, the initial sentence "Accurate measurements of polarization variables, like the LDR, depend strongly on instrument hardware due to polarization leakage." already sufficiently emphasizes the relevance of polarization leakage. We thus removed the sentence addressing the spectral LDR.
The standard MIRA-35 spectra processing does the noise estimation only for the co channel and stores the spectral reflectivity for co and cross channel if in a bin signal in the co channel is above the noise. Hence for low SNR in the co channel LDR might artificially high in the cross channel. To prevent this we introduced this ad-hoc threshold guided on the factor 5 of Görsdorf et al [2017 JAOT]. However, we found the slightly lower factor of 3 to be sufficient for the rather long averaging time we use.

P.5, L.6: "A minimum is skipped, if the prominence of either of its subpeaks is less than 1dB" In Table 2 it says that "prom." has units of dBZ. Since it is defined as a difference, dB seems to be right.

Corrected, thanks for pointing out.

P.5, L.10 (also P.7, L.4): "Reflectivity factor Z" should be Ze especially if you look at ice clouds.

This point was already raised during the prior review and we inserted a respective section in Appendix A. To make this more clear also in the continuous text we modified the sentence to "Equivalent reflectivity factor Z (the subscript e is omitted for brevity) is calculated by integrating the spectral reflectivity of the whole peak (i.e. from the noise-floor up)".

Section 4.1: I think you should mention in the beginning of this section that this particular case has been extensively analyzed in Kalesse et al.2016 and that the reader can find there even more detailed information.

> Actually Kalesse et al 2016 [ACP] only discuss the 21 February 2014 case. The 02 February case is subject of Kalesse et al 2019 [AMT]. We have added a reference to this publication in the mentioned section.

Typos:
Abstract, L.8: "particle populations Arctic multi-layered" probably an "in" missing?
Abstract, L. 9: Programs' BAECC (?)
P.7, L.17: rule is based on (?)

> Corrected as suggested. Thanks!

Specific Reply to Max Maahn

I thank the authors for revising their manuscript, overall the quality of the paper has been significantly improved and I recommend the paper for publication subject to the following minor modifications:

> We thank the Referee for the positive opinion.

P2L9: Shouldn't this be multi-modal peaks?

> Thanks for pointing out. Corrected as suggested.

P4eq3: I don't understand the meaning of the brackets, isn't application of something like a floor function required to ensure you get the same parent for the left and right child?

> Yes, the brackets represent the floor function. We added a description after the equations.

P4L16: This is not correctly described if you have multiple peaks separated by noise. between '-v_nyq and +v_nyq' should be between the left edge of the left-most peak and the right edge of the right-most peak.

> Thanks for pointing to that misleading description. We omitted the part with v_nyq: "The root node contains all signal of the Doppler spectrum above the noise threshold."

P5L13: "node 0 is similar…' Also this statement is only true when there are not several, noise separated peaks in the Doppler spectrum. Many radar moment estimations (e.g. microasrcl) use the concept of the most significant peak. I.e. if there are two peaks in the radar Doppler spectrum that are separated by noise, the reflectivity corresponds only to the larger peak.

> Thanks for pointing us to this circumstance. We rephrased the passage accordingly:
> "Node 0 contains all components of the Doppler spectrum which are above the noise threshold. In general, this node is similar to the moment estimation commonly used to analyze Doppler spectra (e.g. Carter et al., 1995; Clothiaux et al., 2000; Görsdorf et al., 2015). Only in case of the presence of noise-separated subpeaks within node 0, some moment estimators such as microARSCL apply the moment retrieval to the most significant peak only. The child nodes (1-2) of node 0 […]"

Fig 3: In the text it sounds like the values would be normalized, but the number don't look like. Please add at least labels to avoid confusion.

Done as suggested.

P11L2: I mentioned that before but didn't express myself clearly enough: I still find it confusing that the authors distinguish the peak populations in the text by velocity ('faster-falling particle population', but Figure 8 separates by LDR. I recommend to make the naming consistent.

The selection is a hybrid of LDR, v and Z. The LDR was only used to guide the manual selection of the anchor nodes. The automated grouping algorithm then only relies on velocity and reflectivity. But we acknowledge, that the description is hard to follow and added more interconnections between fast falling and low LDR and slower falling and high LDR respectively.

Minor changes

We removed the reference to Griesche et al. (2019 in preparation), as this publication is not submitted yet.