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Atmospheric Measurement Techniques Discussions
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Interactive comment on "peakTree: A framework for structure-preserving radar Doppler spectra analysis" *by* Martin Radenz et al.

Anonymous Referee #1

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The authors present an algorithm for identification and detection of multiple peaks in radar Doppler spectra. This peak identification is an important step in Doppler spectral analysis in order to separate and trace different particle populations. The authors demonstrate their new algorithm with a case study of an Arctic mixed-phase cloud.

In general, I think this new algorithm is an important novel tool which will help to analyze Doppler spectra. I especially like that the authors tried to develop an algorithm which uses as few as possible a priori assumptions about the number of possible sub- or secondary peaks. In that way, it is a very flexible tool which allows the user to apply the filtering and analysis needed for her/his specific application. I also appreciate that the authors make their code publicly available which is unfortunately not the case for many previously published algorithms. I like the general structure of the manuscript

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and I think the content fits nicely into AMT. However, the text, some descriptions, and the case study analysis part need some major revisions (see comments below).

General comments:

A much more careful proof-reading by all authors is needed regarding the English, punctuation, typos, and sentence structure. I will list a few examples in the specific comments but not all. This should be one of the main duties of the co-authors rather than the reviewers.

Algorithm description (Section 3): I recognize that the authors put a lot of effort in illustrating and explaining their new algorithm. However, I have to admit that I still got confused in some parts and would like to suggest a few improvements: In your example spectrum (Fig. 1) you show a spectrum with several sub-peaks but without an additional noise separated peak. I think such a more general example would be much better to illustrate the method. This would also better connect to your mixed-phase cases where one often finds the narrow, noise separated liquid peak next to the broader ice/snow peak with sometimes additional sub-peaks for example caused by riming. In such a diagram, I would also like to see all terms which are used in the text to be included. I was for example very much confused by all the node termination: root node, parent node, child node, leaf node, etc. Please make this easier for the reader to follow or to quickly figure out what is what.

I have my biggest problems with the second part of the case study (Section 4, P8, L5 and following). The description itself is very lengthy, descriptive, and contains a lot of speculations but very few clear conclusions. The fall streak analysis is also done very poorly by manually following streaks of maximum radar reflectivity. In that way, you are always tracking the largest particles which dominate the reflectivity signal. There are many datasets (Cloudnet sites, ARM database) where you can do such an analysis in a proper way and at the same time take full use of your new mode identification: Simply take the horizontal wind profile and the mean Doppler velocity of your nodes and you can reconstruct the fall streaks of your individual nodes. The current fall streak analysis you present appears to me not very convincing. Also the application of the Hogan 2006 Z-T-IWC retrieval to the different nodes is not very sound. As you mention, Hogan et al., 2006 derived the Z-T-IWC for a large set of aircraft measured PSDs. I find it very questionable to apply such a relation to your different nodes, which you identified in order to separate (!) different particle populations and their different properties. You could have used (or even self-derived) Z-IWC retrievals for more column or needle shaped particles (for your mode with larger LDR at lower levels) and a Z-IWC retrieval for aggregates or plates for the first mode. In that way you would have demonstrated some convincing added value of your peak separating approach. I suggest to either shorten/remove some of these parts or extend it (better datasets, other cases, more appropriate Z-IWC relations).

I am also missing some discussion in your manuscript about how to best decompose Doppler spectra. Several studies in your reference list used for example Gaussian fitting or fuzzy logic while in your approach you basically cut the spectrum at the minima. I understand that your focus in this work is in the peak identification logic but I would welcome some discussion on this topic as well since it appears to me to be closely connected.

Specific comments:

Abstract, L. 2: “Cloud radar observations contain information on multiple particle species, when there are distinct peaks in the Doppler spectrum”. This is not always true. Turbulence can cause multi-modal spectra even though only one population of particles is present.

Abstract, L. 3: “Complex multi-peaked situations are not captured by established algorithms”. Not clear to me what you mean here. What means “complex”? What is “not captured”? What should be captured and for what? Be more specific.

P2-3, Dataset description: It appears to me that the dataset is not really ideal for

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demonstrating the algorithm for Doppler spectra analysis. 10s averaging will remove a large number of interesting microphysical features and also the horizontal wind influence due to pointing uncertainties can cause many artefacts. I understand that you probably want to use data of recent campaigns to acknowledge these projects and their funding but from a scientific point of view it appears to me that there are several datasets (e.g., ARM datasets from the Arctic) which provide much better quality for such a demonstration.

Figure 1: Why does the spectrum have these "tails" to the sides (lowest/fastes velocities). It looks like a broadening effect due to the long temporal averaging and/or swinging of the beam with the ship motion.

P3, L12: "with signal above the noise level": Please provide the exact threshold when you consider the signal to be above the noise.

P3, L20: I can't find v_left/right/add in Fig. 1. Are they not relevant for the algorithm? As I mentioned in my general comments, it would be good to show an example which contains a noise-separated peak. Here, you only describe it but in such an example you could easily explain all terms used.

Figure 1a: I suggest to remove the "units" of the spectral reflectivity (dBZ) and rather use arbitrary units [a. u.] or [dB]. If you would plot the spectrum in linear units, you could write (mmˆ6/mˆ3)/(m/s). In that way, the integral over the full spectrum would result in the usual linear units of Ze. However, the integral over a log spectrum will neither result in mmˆ6/mˆ3 nor dBZ. The radar experienced readers will certainly understand what you mean but it's simply not correct in a strict scientific sense.

Figure 1b: It is not clear to me how I can read the skewness from the triangle, please explain. The caption is also missing the description of what is meant with "spec Z cx" or the line "decoupling".

P3, L32-33: "Only the part of the Doppler spectrum above the threshold defined by the spectral reflectivity minimum that separated the peaks are used". This is a problematic aspect of your approach which I think should be discussed much more and maybe even changed. Let's consider only Ze: For Node 0 you integrate the full spectrum starting at the noise level. Already for Node 1, you integrate only starting from your first threshold (-34 dB). I don't understand why you are not integrating again from the noise level? I would expect that when I sum up all the identified sub-peaks (Node4+3+2; I exclude Node 1 since it is basically 3+4), the resulting Ze should be identical to Node 0. But if I understand correctly, this is not the case for your algorithm, or? From a microphysical point of view, I guess one would like to have moment estimates of the full sub-peak and not only the "peak head" which sticks out of the remaining spectrum.

P4, L3-4: Where do I find Node 5 and 6 in Fig. 1?

P6, Title Section 4: Replace "ice crystal habits" with "ice crystal populations". The spectra indicate that you have two populations of particles with different fall velocities. This could be related to different habits but you could also have two populations with different fall speed and similar habits (e.g. due to onset of riming).

P6, L9: "humidity profile": Actually you only show profiles of air temperature and dew point. The humidity information is contained in them but why not plotting relative humidity directly?

P7, L2-3: "previous studies used the simple criterion of low reflectivity and vertical velocity close to 0ms$\hat{}-1$ to identify regions of a cloud, where the presence of liquid is likely" I think this description is not very precise: In fact, the peak is thought to be due to liquid if it is a very narrow peak since the PSD of super-cooled droplets can be assumed to be rather narrow. In the way you describe it, any peak with low Ze and v close to 0 m/s might be interpreted as liquid. How reliable are those thresholds (especially the Ze threshold)? Are your values different from the studies cited? Are the thresholds used within those studies all the same or different?

Figure 3f: Why is there no color for N=2? A second node would be the most likely scenario for a liquid water and an ice peak, or?

P8, L1+6: Be consistent whether you use the minus sign when indicating the Doppler velocity or not.

P8, L1: The low LDR indicates plate-like particles, right? But then they are oblate and not prolate (like columns). At P12, L11 you denote them as oblate. . .

P9, L24: "indicating no change in particle habit": Well, if the particle habit changes for example from plates to dendrites, I would also not expect a big change in LDR. I think the conclusion that habit does not change only because LDR is rather constant is not true in general.

P9: In addition to my principal problems with your fall streak analysis (see general comments): Why don't you show range spectrograms for your different fall streaks?

P12, L4: Another important advantage of your method to microARSCL is that you provide the code for the community. For further development of Doppler spectra analysis, this is absolutely key!

P13, L7: Why are v_left/right relevant for the moment estimation. They don't appear in any formulas. How are they actually determined? Maybe a certain threshold for the spectrum above the noise level?

Style and Typos:

Abstract, L. 3: Add comma after "In this study" and before "that". These are very typical punctuation mistakes which I found very often throughout the manuscript. I will not list them all but ask all authors to do a more careful reading.

P1, L15: Better: "Cloud radars are frequently used. . ." P1, L17: Add comma after "In general" P2, L.4: Add comma "formed ice, and" P2, L7: "In a further step," P2, L7-9: Confusing and very complicated sentence. Please re-structure and/or split in two. P2, L10: Prior approached should be approaches P2, L12: Remove comma after “available” P2, L14: “In this study,” P2, L16: “algorithm IS easily applicable” P2, L24: More a question to the editors but are citations of manuscripts in preparation appropriate? P3, L20, L31: “In a first step,” “In the next step,” P3, L23: Add comma after v_add P3, L22: Better “All minima found” P7, L1: Remove comma before “that” P7, L2 and P9, L3: “can not” vs “cannot” use consistently P7, L7: “during the whole case study” better “during the entire event” P7, L7/L8: “The top/second one” is a bit slang-like, better “The uppermost layer” P7, L7: “single moments of the full spectrum”: I think the “single” is redundant here P7, L9-10: “Together with the lidar backscatter indicating a liquid cloud base at 750m between” awkward sentence, please rephrase. P9, L4-7: Very long and complicated sentence. Split in two and rephrase. Also, the sentence is very speculative. P9, L1: add comma before “which” P12, L6: application OF this new P12, L6: In a second step, P12, L13: Within this liquid layer,

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