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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

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The authors present a novel method to describe multiple peaks in the radar Doppler spectrum. Personally, I thought about this for a couple of years and I am very excited to see that there is finally a good idea and progress. In particular, I like that there is no need for a somewhat arbitrary distinction between noise separated and sub-peaks anymore. I recommend the manuscript to be published, but I think the manuscript can be strongly improved by addressing the following points:

Major comments

Impact of 10s averaging: 10s is quite a lot for spectral cloud radar applications. Most cloud radar data set I'm aware of (e.g. ARM) use temporal resolutions in the order of 1-3 s. What is the impact of this on the method? I would expect that the spectrum is bumpier when averaging less and that maybe a different sub-peak threshold needs to

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be used? At the same time, sub-peaks might get smoothed out during averaging. And do I assume correctly that the authors hope that the ship motion cancels out within 10 s?

Using Doppler spectrum above the separation threshold for moment estimation: I think I understand why the authors decided to estimate the moments this way. However, I'm afraid that this method will also lead to biases, in particular for the higher moments. The authors could do a quick sensitivity study and quantify the change of the moments by using a normal monomodal peak and cutting of the tails at different spectral reflectivities.

Grouping: The authors should discuss why they chose the threshold used for the grouping (50s, 150 m, d<0.9, 0.4 and 0.9 normalization factors) and the impact of changing these thresholds on the results.

Application of grouping: I would strongly recommend adding a few sentences on how the data set was grouped exactly. I do not understand a couple of processing steps: Did the authors manually select anchor nodes for both nodes separately? What criteria were used? Were the criteria for liquid nodes used in Fig 4 used as a starting point for one node? What about other nodes then the two shown ones? Can they be grouped, too?

Language: The paper needs a lot of work to improve the English. German grammatical structure shows through in numerous places. Word selection and punctuation can also be improved upon. Given that publishing includes language editing for Copernicus, I do not list language-related issues.

Minor comments

P1L19: I would recommend adding a short discussion about the difference between peaks that are separated by noise and peaks which are not (eg see fig 13 of Williams et al 2018)

P2L2 “which likely causes significant errors“: Are the authors sure? I would argue that for most empirical retrievals the climatology of multi-peak situations is (unknowingly) included into the retrieval so that there are no biases. Also applies to P9L30.

P2L23: I don’t think the authors can cite papers in preparation

P2L29: vertical-stare -> vertically pointing

P2L30: This part could be shortened using a table with the radar specifications.

P3L12: LDR -> LDR spectrum?

P3L28f “the prominence of one of its subpeaks is less than 1 dB” and “height of the peak above”: I assume the authors talk about the maximum of the subpeak?

P3L32 “Doppler spectrum above the threshold”: In the appendix, the authors mention this does not apply to Ze?

P4 Fig1a: According to P3L19f, node 0 is the full radar Doppler spectrum

P4 Fig1b: An explanation of how skewness is actually displayed in the figure is missing.

P4L1: node 0 -> node 1?

P5 Table1: Z is not defined yet. Also, I strongly recommend to use Ze (equivalent radar reflectivity factor) instead of Z (radar reflectivity factor) because Z is typically defined with 10*log10(SUM(N*Dˆ6)) which applies only to Rayleigh scattering of liquid drops (see eg. ‘Radar for Meteorologists’ by R. Rinehart.

P5L1: I would recommend indicating that ‘indices’ refers to the nodes not the bin in the Doppler spectrum.

P6 Fig2: Are Ze and v normalized in this plot?

P6L14: ‘giving hints’ please specify

P7 Fig3: I would recommend adding the fallstreaks also to this figure because it is

interesting that the lower end of the first one can be only seen after applying the grouping.

P7L3 “to identify regions of a cloud, where the presence of liquid is likely”: I would say these thresholds are rather to identify regions where drops are the dominating particle type. Liquid is likely also present in other cloud regions.

P7L9 “periods of liquid” add ‘likely’ or ‘possible’

P7L9f: The liquid extends from 750 to 1000 m?

P7L13 “The faster-falling particle population” and title Fig 5: was velocity or LDR used or grouping? Also, I would recommend naming the nodes consistently.

P7L15 “generated ice”: the authors should mention before that they assume the second peak to be ice

P8 Fig 4: Because it is described in the section before, I would recommend to clearly indicate that the grouping is not used in this figure.

P8 Fig 4: I would recommend indicating the ceilometer cloud base by e.g. a black in this and other plots

P8 Fig 4: How does LDR look? It should have a sufficient SNR at least for the lower layer.

P9L3 “We cannot fully rule out that ice multiplication was triggered. . .”: the authors should think about removing the following discussion because it is speculation and not of importance for this study.

P9L14ff: It is a little challenging to follow which population the authors discuss.

P10 Fig 5: How many anchors were manually selected here?

P10 Fig 6: P10 Fig 6: Given that IWC scales with Ze, I don’t see a benefit of this figure.

P12L4: To my knowledge, microARSCL uses actually sub. I would recommend to focus

stronger on the greater flexibility by overcoming the separation in noise separated and sub peaks.

Appendix A: I would recommend adding a definition for spectral reflectivity.

P13L19: MIRA or Mira-35?

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