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Atmospheric Measurement Techniques Discussions

Interactive comment on "Hydrometeor classification of quasi-vertical profiles of polarimetric radar measurements using a top-down iterative hierarchical clustering method" *by* Maryna Lukach et al.

Anonymous Referee #1

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Overview

The manuscript presents an approach to hydrometeor classification based on clustering. It contributes therefore to the relatively new family of algorithms and approaches that are built on collected data rather than on simulations (Grazioli et al., 2014a; Bechini and Chandrasekar, 2015; Besic et al., 2016, 2018)

The topic is certainly of interest for the readership of this journal, and I see two main contributions (with respect to previous research) of this manuscript:

- A better conceived usage of clustering, in a smartly designed algorithm.
- The method is designed for QVP data, which are becoming very interesting in terms of microphysical interpretation and noise removal.

I have several comments and suggestions for the authors, to be answered before final acceptance. I recommend overall a major revision.

Major aspects

1. I found the weakest aspect of this manuscript to be how the particle imagers have been used to provide a physical interpretation of the content of the clusters. This is still largely done by visual inspection, while classification techniques have been developed in the past years to automatically classify hydrometeors from various imagers.

   Additionally, I am a bit concerned that the maximum size of particles observable by the airborne instruments is small with respect to the sizes of interest for weather radars. The hydrometeor type of the largest particles is especially important because they often dominate the Z_H or Z_{DR} radar signatures (while there are less concerns on K_{dp} in this sense).

   Finally, Section 5.3 is quite hard to follow because of the large use of interpretation and because the clusters are still named by their "anonymous label" (f_c14...), which makes the narration very dense. Could it be split into smaller sections?

2. There seem to be only a few precipitation events contributing to the dataset. As the authors acknowledge in the conclusions, the classification will then be representative only of this dataset. Is it possible to extend the dataset of this research, and try to achieve a hydrometeor classification that is representative of all the weather types that can be expected (given your radar and your geographical location, of course)?

3. I strongly recommend to make available (on github or other platform) the codes and some sample of data.This aspect is becoming crucial in modern research, and it will give significant visibility to this work.

4. The authors should better underline, in my opinion, the difference (a difference that affects also the interpretation of polarimetric variables) between hydrometeor classification applied to radar scans, and hydrometeor classification applied to QVPs. I think it is a key novelty of the manuscript.

Detailed comments

I refer here to the line numbers of the .PDF version of the manuscript, although I suspect that the numbers are partially cut by the left margin of the pages.

1. P2, L41-43: I would recommend the additional effort to specify which of the cited works are based on fuzzy logic, which are based on clustering, and which ones are based on neural networks.

2. P2, L46: A detail. I believe that Z_H should be indicated as "horizontal reflectivity factor" (and the authors may provide the units as [dBZ]). The expression "logarithmic reflectivity factor" sounds to me not common in the radar meteorology jargon.

3. P2, L47: I believe that Φ_{DP} should be the "differential phase shift on propagation", to differentiate it from the "total differential phase shift" (Ψ_{dp}) which includes also the backscattering contribution. See for example Otto and Russchenberg (2011); Grazioli et al. (2014b).

4. P2/3: To complete your review of the state of the art of hydrometeor classification, I recommend also Besic et al. (2018) which tackles the problem of hydrometeor mixtures, and the very smart spatial approach to classification through clustering proposed by Bechini and Chandrasekar (2015). The latter at least partially considers the temporal dimension.

5. P4, L89: the usage of PCA is quite common and always tempting. However, which are the underline assumptions on the input data when we use PCA on them?
6. P5, L51: why those two evaluation scores have been chosen among the many available in the literature? In my experience, this is really a key point and often the answer is very different according to different evaluation scores.
7. P5: I would recommend to provide a few “qualitative” words about the indexes used. For example, if I interpret well, WG is an index that values compactness and separation. It could be stated. A visual aid, even with simple 2D data, would help the readers who are less familiar with those techniques.
8. P6, L78: a “modified” Meteor 50DX. The authors may want to specify the modification (i believe for this radar is a bigger antenna dish, but I may be wrong).
9. P7, L84: K_{dp} is a derived quantity. Could you please mention which estimation method was used?
10. P7: how are Z_H and Z_{DR} corrected for signal attenuation?
11. P7, L96-98: in more complex terrains (Alps for example), I believe that the errors of such an approach may be larger.
12. P8, L31: for the hydrometeor sizes of interest for X-band weather radars, isn't 6350μm still small?
13. P9, L68: About data standardization. Is it a simple standardization based on mean and standard deviation before to apply PCA, do I interpret it correctly? Are very skewed variables, K_{dp} for instance, treated differently?
14. P10: it is not clear to me, maybe I missed it, how much data is used as input of the clustering. Is it representative of various seasons? How is it chosen? The

clusters will then be representative of this dataset, so it is important to clarify this point.

15. P12, L38: I suspect that part of the variation of K_{dp} among clusters is due to the fact that it is a variable with an extremely skewed typical distribution.
16. P14/15: I could recommend, if they can help the discussion and the interpretation, the following researches dedicated to ice-phase microphysics: Bechini et al. (2013); Grazioli et al. (2015); Kennedy and Rutledge (2011)
17. P16, L82/83: considering past papers on the topic, I found slightly overstated to claim this research to be the first tackling the issue of the number of clusters as they appear in the data. The authors may consider to specify more in detail the novel aspects of their approach here.
18. Figure 7: a very nice way to display the clusters, although it is a bit hard to see the different types of lines, corresponding to the different days.

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