

Authors' responses to reviewer #1 comments

Title: Hydrometeor classification of quasi-vertical profiles of polarimetric radar measurements using a top-down iterative hierarchical clustering method

Notes to the reviewer:

The authors would like to thank the reviewer for the feedback and constructive comments on our manuscript. Comments have been taken from the provided review and listed below in numbered list format, and are addressed directly afterward in sub-bullets, for example:

1. Reviewer Comments

Authors' Response

“citation from the revised manuscript”

As the reviewer comments were copied from the original review, the line numbers there will refer to the reviewed manuscript. Line numbers in our responses refer to the revised manuscript.

Figures that are in the manuscript are referenced just with the number, e.g. Fig. 1.

Reviewer #1:

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.

We appreciate the Reviewers comments about particle image classification. Whilst image classification may be useful to pursue in the future, there is still significant uncertainty and variation in the quality of these algorithms and the usefulness in this study due to limitations of the observations for large precipitation particles. We have added the following text to p.10, L.1 - 6 to clarify:

“No attempts have been made to classify particle images, for two key reasons. (1) The larger particles, which have the greatest influence on the polarimetric properties, are poorly sampled by the CIPs. (2) A recent study by O'Shea et al (2020, in review at AMTD) suggests existing procedures to classify particle images using the CIP can lead to inaccurate results due to the effects of diffraction when particles are imaged more than a few mm off the focal plane, which is the most common scenario. A thorough assessment of the accuracy of these image classification algorithms, with respect to particle size and probe configuration, is much needed.”

2. 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).

We appreciate the Reviewer's concern about the maximum observable particles in the CIP100 data. We agree that the in-situ measurements are limited in the ability to quantify the concentration of larger particles, which may have an important contribution to the bulk polarimetric properties. We have added the following text p.9, L.13 - 17 to clarify:

“It should be noted that despite using the centre-in method with the CIP data, which increases the effective sample volume for larger particles at the expense of uncertainty in particle size, the ability to measure particles with size > 6 mm is negligible with this configuration. An indication of the potential presence of such large particles can be obtained through a visual inspection of the particle images, but no conclusions can be drawn.”

3. 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?

We thank the Reviewer for the suggestion and acknowledge that the section might be difficult to read. We split Section 5.3 into four subsections.

4. 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)?

We appreciate the Reviewer's concern about the representativeness of the dataset. To clarify our view on the purpose of this study we added a paragraph to p. 3, L. 16 - 19:

“The point of this study is not to create a set of cluster characteristics that could be applied to other datasets. Rather the goal is to demonstrate the viability of this type of data-driven methodology for creating a set of labelled clusters (i.e. hydrometeor classes). As such, the comparison to in situ data and labelling done as part of this study is only shown as an example of how this tool can be used. “

5. 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.

We thank the Reviewer for this recommendation. The samples are available for both the radar and the CIP data on the Centre for Environmental Data Analysis (CEDA) archive. The links to the data can be found in the correspondent references (p.8, L.31 - p.9, L.2).

“In situ data for this study comes from FAAM BAe-146 flights C013, C076, C081, and C082 (FAAM, 2017; FAAM, 2018a; FAAM, 2018b; FAAM, 2018c) and observational data are available on CEDA archive. The dates of the flights with corresponding flight numbers can be found in Table 1.”

The manuscript is updated by adding:

“Radar data selected for this study can be found in the list of mobile X-band radar observations on CEDA archive (Bennett, 2020).”

and

“The original data can be found on CEDA archive (Met Office, 2016).”

to p. 7, L.16 - 17 and p. 8, L. 5 .

The QVP extraction and the clustering code can be made available on request to the authors and this is mentioned in the new version of the manuscript (p. 10, L. 16).

“and the code can be made available on request.”

6. 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.

We appreciate the Reviewer’s feedback on this. Seeing this comment, we agree that more discussion is warranted. As such we have added

“Note that this is a novel application of the QVP data product and the interpretation of QVP polarimetric variables differs from that of PPI or RHI scans due to the averaging used to construct them.”

to p. 3, L. 27 - 29 and

“As QVP polarimetric characteristics differ from polarimetric characteristics of hydrometeors observed by PPI and RHI scans, care must be taken when comparing these results to the literature.”

to p. 12, L. 6 - 8.

We also added a discussion on the influence of errors and noise in the data on the clustering and labeling, where QVP advantages are mentioned one more time (p. 14, L. 30 - p. 15, L. 3):

“Measurement errors may influence the clustering results. As it was shown by Bringi et al. (1990) noise in the observations has a strong impact on k-mean HCA results. Unfortunately, it is impossible to run the same type of analysis conducted by Bringi et al. (1990) for a unsupervised hierarchical clustering algorithm as the added noise might deliver modified hierarchical structure with another optimal number of clusters and direct comparison to the original set of final clusters would be impossible. Here this issue of noise is partially addressed through our use of QVPs. In particular, azimuthal averaging of a QVP reduces the noisiness of the differential phase within the melting layer, Trömel et al. (2013, 2014) and was recommended in Kumjian et al. (2013) to quantify rather small enhancements of Z_{DR} and K_{DP} .”

Detailed comments

7. 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.

We appreciate the Reviewer's suggestion. The sentence:

“Further refinement and development of automatic HC algorithms included the application of fuzzy-logic, machine-learning techniques (such as the identification of clusters representing data-wise similarities) and neural networks (Straka et al., 2000; Liu and Chandrasekar, 2000; Wen et al., 2015; Grazioli et al., 2015; Besic et al., 2016; Wang et al., 2017; and Ribaud et al., 2019).”

Is now modified to p.2, L. 17 - 20:

“Further refinement and development of automatic HC algorithms included the application of fuzzy-logic (Straka et al., 2000; Liu and Chandrasekar, 2000), machine-learning techniques (such as the identification of clusters representing data-wise similarities) (Wen et al., 2015; Grazioli et al., 2015; Besic et al., 2016; and Ribaud et al., 2019) and neural networks (Wang et al., 2017).”

8. 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.

We thank the Reviewer for this correction. The variable Z_H is referred to as

“horizontal reflectivity factor”

now. The corresponding adjustment is made in the text (p. 2, L. 23).

9. 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 also includes the backscattering contribution. See for example Otto and Russchenberg (2011); Grazioli et al. (2014b).

We have adjusted the variable introduction. The Φ_{DP} is referred now as

“differential phase shift on propagation”

(p.2, L. 24).

10. 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?

We appreciate the reviewer suggestion. The references are added to the text on the p.2, L. 22 and p.3, L. 21 - 22.

11. 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?

We thank the Reviewer for stressing the importance of the underlying PCA assumptions. We have adjusted the text adding description of the data preprocessing, required for the application of PCA. The manuscript p.10, L. 19 - 21 now includes:

” The non-parametric transformation based on the quantile function maps the data to a uniform distribution. This standardization helps to deal with outliers and satisfy PCA data assumptions.”

12. 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.

The text:

” The WG index was chosen as best performing according to comparison studies (Niemelä et al., 2018 and Hämäläinen et al., 2017). The BIC is best for the calculation of the posterior probability of a clustering.”

was added to the manuscript (p. 6, L. 8 - 10)

13. 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.

We thank the Reviewer for the recommendation. The manuscript is adapted by adding:

” The WG-index is a measure of compactness based on the distances between the points and the barycenters of all clusters.”

to p.6, L. 21 - 22 and

“The BIC is an estimate of a function of the posterior probability of a clustering being true, under a certain Bayesian setup, so that a higher BIC in (2) means that a clustering is considered to be more likely to be the optimal clustering.”

to p.6, L. 28 - 29.

14. 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).

The reviewer is correct, the modification is a non-standard 2.4m antenna. We have changed the text accordingly (p. 7, L. 11 - 12) to make this clear.

“The NXPol is a mobile Meteor 50DX (Leonardo Germany GmbH) X-band, dual-polarization, Doppler weather radar with a 2.4 m diameter antenna.”

15. P7, L84: Kdp is a derived quantity. Could you please mention which estimation method was used?

We thank the Reviewer for the question. We have added

“Here K_{dp} is calculated as the linear gradient of differential phase shift, where the phase shift has been filtered to remove non-meteorological targets ($\rho_{HV} > 0.85$) and progressively smoothed using decreasing length averaging windows”

to the text on the p.7, L. 21 - 23.

16. P7: how are ZH and ZDR corrected for signal attenuation?

Both ZH and ZDR are corrected for attenuation using a single factor linear correction based on the differential phase shift on propagation in liquid precipitation. We have added the following to the text to make this clear

“and Z_H and Z_{DR} are corrected for attenuation using a linear correction (Bringi et al. 1990)”
(p.7, L. 23 - 24).

17. P7, L96-98: in more complex terrains (Alps for example), I believe that the errors of such an approach may be larger.

We agree that in more complex terrain an alternative approach may be required for temperature rather than using interpolated model output.

18. P8, L31: for the hydrometeor sizes of interest for X-band weather radars, isn't 6350 μ m still small?

See our response to the major aspect comment 1 above.

19. 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, Kdp for instance, treated differently?

See our response to the 11-th detailed comment.

20. 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.

The data used for clustering is shown in Table 1 and we explain that the clustering is only applicable to this dataset in our discussions. We have added an extra

reference to this table on p.19 L. 3 to aid the reader in considering this at an appropriate point.

21. P12, L38: I suspect that part of the variation of Kdp among clusters is due to the fact that it is a variable with an extremely skewed typical distribution.

Thank you for this comment we added it to the discussion p. 13, L. 5 - 6:

” In part this variation may be due to the fact that this variable has an extremely skewed distribution.”

22. 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)

We thank the reviewer for these helpful suggestions of additional references. We have cited them where appropriate (p. 14, L. 10 - 11):

“These characteristics were also recognised as a signature of dendritic crystals in Bechini et al. (2013).”

and p.3, L. 20, L. 22; p. 4, L. 30; p. 7, L. 25.

23. 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.

It is corrected by adding (p. 18, L. 10 - 11)

“in top-down hierarchical”

to the sentence

“This represents the first attempt, in top-down hierarchical clustering of weather radar data, to identify the number of clusters based solely on the embedded data characteristics.”

24. 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.

We thank the reviewer for this suggestion. We have tried to adjust this figure to improve it further but unfortunately thicker lines make all the plots unreadable.

References:

O'Shea, S., Crosier, J., Dorsey, J., Gallagher, L., Schledewitz, W., Bower, K., Schlenczek, O., Borrmann, S., Cotton, R., Westbrook, C., and Ulanowski, Z.: Characterising optical array particle imaging probes: implications for small ice crystal observations, *Atmos. Meas. Tech. Discuss.*, <https://doi.org/10.5194/amt-2020-265>, in review, 2020.

Niemelä, M., Ayrämo, S. and Kärkkäinen, T.: Comparison of Cluster Validation Indices with Missing Data. ESANN 2018 proceedings, European Symposium on Artificial Neural Networks, Computational Intelligence and Machine Learning. Bruges (Belgium), 25-27 April 2018, i6doc.com publ., ISBN 978-287587047-6. Available from <http://www.i6doc.com/en/>

Hämäläinen, J., Jauhiainen, S., Kärkkäinen, T.: Comparison of Internal Clustering Validation Indices for Prototype-Based Clustering. Algorithms. 10. 105. 10.3390/a10030105, 2017.