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Discussions

Interactive comment on "Hydrometeor classification through statistical clustering of polarimetric radar measurements: a semi-supervised approach" *by* Nikola Besic et al.

Anonymous Referee #4

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General Comments

This paper illustrates a semi-supervised technique for the hydrometeor classification from dual-polarization radar observations. The authors suggest that by introducing a degree of (unsupervised) adaptability by means of K-medoid cluster analysis, the classification can be improved upon standard supervised techniques relying on fuzzy-logic. In general I enjoyed reading this manuscript and I liked the idea proposed. In particular the implementation of the double loop for the cluster analysis with "successive refinements" in combination with Kolmogorov-Smirnov test appears a clever solution for the realization of the semi-supervised method. The technique is in general well described, although some parts need further clarification (see specific comments below). In ad-

dition, the generalization of the technique is affected by few arbitrary assumptions, in particular regarding the choice of the distance measure, which is different for the cluster analysis and for the pixel assignments (there may be a reason, but I did not got it). Another issue in the technique description is related with the choice of the membership functions. These are said to be "appropriately modified and enriched by means of scattering simulations based on double layer T-matrix method". This needs to be discussed with more detail, indicating how the electromagnetic scattering simulations are performed and which hydrometeor classes have been modified and how.

Although the description of the method can be easily improved, my major concern is about the validation, which is actually quite weak. For the C-band classification in particular the discussion is superficial and I do not really understand how a single-polarization product (hail) could be used to validate a multiple-parameter classification (which is expected to provide better information about the cloud microphysics). In addition, only the classification results are presented, preventing a comprehension of the reasons for the very different results using the fuzzy logic and the semi-supervised approach (figure 10). Fig. 12 does not really add a significant contribution to the validation (light vs. rain distinction) and I suggest to drop it. On the other hand, the X-band discussion appears much more meaningful to me and I suggest expanding this part. In particular, the results illustrated in fig. 9 help showing the reliability of the classification (not properly "validation") should be expanded (also showing the radar dual-polarization moments). In particular you may add the same comparison using standard fuzzy logic. If you can show that the classification from two different radars produce more similar results using the semi-supervised approach in comparison with fuzzy logic, this may be a very good result demonstrating the robustness of the approach. This should be discussed more extensively. Could you also do the same comparison between C-band and X-band classifications?

Finally, the use of the 0C level only through deltaH (eq. 4) appears to me somehow "dangerous" for general applications. In fact, this implies the assumption of a standard

temperature profile. It may work well for selected "standard" cases, but I anticipate problems with situations where the real profile is more complex, showing inversions or even multiple freezing levels. In these cases, consideration of the whole temperature profile would be more adequate. More specific comments are listed below.

Specific comments / Technical corrections

P.3, L.28: "role of adhesive". I'm not sure this is proper English wording for this context.

P.5, L.23: "The reason behind the smaller initial set for Monte Lema radar..". Do you mean Plaine Morte here instead of Monte Lema?

Data preparation: the selection criteria, i.e. elevation above 3.5deg and range below 40km, poses the question of how the method could be actually useful for operational implementation, where the larger amount of observations usually comes from lower elevations and ranges up to 150-200km.

P.6, L.15-18: it is not clear to me why you need to arbitrarily define two different transformations, one for the centroids derivations and another one for the assignments.

P.7, L.1: please provide more detail about the "standard operational procedure" for noise correction in the correlation coefficient. Also, specify whether the same general processing of the radar moments (including Kdp estimation) is applied to all the radars considered in this study or there are differences between the Rad4Alp network and the X-band systems.

P.7, L.19: you should explain better the inverse sampling method. This is a fundamental part of the method, for the comparison of the two sample distributions, but it is actually not clear how the reference observations are determined in practice.

P.7, L.30-P.8, L.7: this part (cluster splitting) is one of the original contributions of this work, but it is also not clearly illustrated. I suggest improving this description with additional figure/practical example. In addition, it may be useful to provide some statistics on the percentage of observations which needs to go through the internal loop with

cluster splitting.

P.8, L. 10: correct "focussed" with "focused"

P.9, L. 10-11: where is this shown?

P.9, L.19-26: Pixels assignment: what is the purpose of using two different normalization procedures, one for the centroids (eq. 2), using the standard deviation, and another one for the pixel assignments, using arbitrarily defined ranges for each variable?

P.10, L. 11-14: please provide evidence of this comparative analysis. In particular I'm wondering about the impact of the correlations inherent in the radar observations (e.g. Zdr vs. Zh or Kdp vs. Zh in rain,...). The Mahalanobis distance provides a way to account for the correlations in the dataset and therefore should give a better distance measure. How have you evaluated this and eventually preferred the simple normalization using arbitrary ranges?

P.11, L.4: "they are derived using computationally expensive, fairly sophisticated clustering method". The use of a computationally expensive procedure is not per se a guarantee of better quality. I suggest to drop this statement. The following (human expertise, complementary data) is enough to justify why you take this as reference.

Fig. 8: you should add the RHI plots for all the dual-pol radar moments. Otherwise the interpretation of the classification and the evaluation of differences between the different techniques is not possible.

Appendix A: "As well, a number of other parameters from the original membership functions has been altered to fit the specific purpose these clustering constraints have in the framework of our approach." This is quite obscure; please explain more in detail what has been altered, with proper justification.