Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-143-RC2, 2020

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 #2

Received and published: 23 June 2020

The paper presents a new approach for unsupervised hydrometeor classification using polarimetric radar data and temperature. The suggested procedure uses a hierarchical clustering methodology to determine a number of data clusters that can be objectively distinguished from the multiparameter observations. These clusters can be associated with certain classes of hydrometeors using either in situ observations or general physical considerations. It is important that the study utilizes the quasi-vertical profiles (QVPs) of polarimetric radar variables which capture the vertical structure of the cloud and its temporal evolution with high vertical resolution and statistical accuracy. An-

other positive aspect of this investigation is the utilization of the airborne microphysical probes to label the identified multiparameter data clusters to characterize hydrometeors with different microphysical properties. The paper is well written and is worth to be published after several issues with the analysis and interpretation of the results are addressed. Here are my comments and recommendations (1) The authors do not consider the impact of the measurement errors (biases and statistical errors) on the outcome of their classification. There is little doubt that if the measurements are too noisy then the number of objectively distinguished clusters of data is reduced. The potential biases of ZDR and KDP due to miscalibration of ZDR, differential attenuation, and backscatter differential phase in the melting layer (ML) are not mentioned in the manuscript. The evidence of such biases is indicated by negative values of mean ZDR and KDP for several classes. I strongly recommend at least some discussion about the effects of the measurements errors on the classification results. (2) I do not agree with microphysical labeling of several identified clusters. For example, the cluster f_cl1 is labeled as upper part of ML. However, it is obvious from Table A1 and Figs. 6 and 8 that the corresponding signature is observed at negative temperature above the ML and is likely associated with heavily aggregated or rimed snow. Its melting often produces the sagging of the ML as demonstrated in Fig. 6. Cluster f_cl7 has highest KDP which is a manifestation of the rapid growth of ice via vapor deposition and onset of aggregation in the dendritic growth layer (DGL) centered at -15°C. However, the authors label f_cl9 as DGL although the corresponding temperature is higher than -10°C. DGL is not a hydrometeor class but a layer where dendrites or hexagonal plates typically grow. Depending on the height of the top of the cloud, DGL can be characterized either by a combination of high KDP and low ZDR or low KDP and high ZDR (Griffin et al. 2018, JAMC, pp. 31 – 50). This important characterization is completely missed in the manuscript as well as the reference to the very pertinent Griffin et al. (2018) article. Cluster f_cl12 is labeled as "ambiguous small ice / drizzle". Small ice is very unlikely because it would completely melt at T = 3°C. (3) The onset of melting is determined by a zero value of the wet bulb temperature rather than regular temperature and

I suggest using the wet bulb temperature for classification. (4) For a future studies I would recommend using vertical gradient of Z and the height of the top of the cloud as additional classification variables. Vertical gradient of Z better characterizes the aggregation / riming process than the absolute value of Z. It can be seen from the results of classification presented in the manuscript that the cluster f_cl11 is correctly recognized as pristine ice with low Z and KDP and high ZDR and is identified during time periods when the height of the cloud top was law. Significant aggregation is unlikely in this situation due to low number concentration of ice particles and small difference in their terminal velocities. (5) I notice that several literature references mentioned in a body of the manuscript (Kumjian 2012, Hampton 2019, 2020, Murphy 2018) are missing in the reference list.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-143, 2020.