Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-301-RC1, 2016


Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Retrieval of the raindrop size distribution from polarimetric radar data using double-moment normalisation" *by* Timothy H. Raupach and Alexis Berne

**Anonymous Referee #1**

Received and published: 22 December 2016

Title: Retrieval of the raindrop size distribution from polarimetric radar data using double-moment normalization. Author(s): Timothy H. Raupach and Alexis Berne MS No.: amt-2016-301

In this paper, the authors present a new technique to estimate the raindrop size distribution and its parameters directly from polarimetric radar measurements. As already highlighted in the quick report, the present work can be of particular interest for the radar meteorologist scientific community. The logic flow of the conducted analysis is well exposed. The main revision points refer to the presentation of the results. The tables are very useful, while the figures are sometimes a bit small and it is difficult to easily distinguish dots, lines (i.e. in Figure 2 is difficult to distinguish the retrieved



and measured rain rate time series). I suggest to increase the figure size where it is possible. As general comment, at least in one out of the three datasets (it is not well explained if the HyMeX dataset is provided by Parsivel first or second generation), the authors use the Parsivel first generation data. Has been demonstrated the better performance of Parsivel second generation (Parsivel2) with respect to the first generation (Tokay et al., 2014 ,JTECH). Even if the authors, using the Raupach and Berne 2016a,b approach to correct the Parsivel data, this could affect the goodness of the results.

Considering these general and the following specific comments, I recommend the publication of the paper on the Atmospheric Measurement Techniques after the authors address the revision.

Specific comments.

- The simulation of the radar variable from disdrometric measurements to test the efficiency of the proposed technique with respect to the common used technique is particularly appreciated and useful. In the Section 5, they describe the difference between Rayleigh and Mie scattering region as function of the raindrop size at X-band frequency. They put a threshold at ZH=35 dBZ to discriminate the two regions by using the HyMeX Parsivel data only. What about the other two datasets? If they apply the say procedure, do they obtain the same threshold? There may be a climatic dependence on this threshold (i.e. the same reflectivity can be obtained by different DSD with a higher (lower) number of smaller (larger) drops respectively, which fall in the Rayleigh or Mie scattering region).

- From Figure 1 in linear scale, it is almost impossible to individuate ZH=35 dBZ (3.16e+03 mm6m-3). I suggest to change the linear to dBZ scale.

- Figures 2-4. I suggest to increase the size of the plots. Even the dots size (especially for Figure 3) can be slightly decreased to a better interpretation of the figures.

- Page 12. This is probably the most confusing part of the paper for the results interpretation. I clearly understand the summarize in only one figure the big amount of results is not easy, but some point arise reading this part. It could be useful for the reader, that the authors recall in the text the explanation of the Figure 5 and Table A1 and A2, which they give in the captions as well as the indicators used (relative bias, IQR of relative bias, correlation coefficient and slope of fit). They also show in Figure 5 the difference in performance between the double-moment technique and SCOPE-ME highlighting the cases where a method outperforms the other. On the other side, Table A1 reports also the absolute values of the considered indicators. I suggest to add this information at least in the Table A2. It is important to show which technique gives the best results, but it is equal (or more) important to know how far is the estimation from the measurement parameters.

- Page 8-line 13 and page 10-line 15: the authors say that they simulate the radar variables "for the MXPol stacked PPI incidence angles" and "for an elevation angle of 4°". Is the radar incidence angle really a input parameter in the T-matrix code? I retain that the incidence angle does not infer the simulation of the radar variables from disdrometer data. Please clarify this point.

- Page 13. Table 3 summarizes the performance difference combining all the Parsivel data and the four axis ratios used. The authors are combining data collected from "different" instruments (same physical base but different version). It could have more sense combining the data collected from the same instruments. Moreover, as they report in lines 5-8, the different axis ratio gives different results (with the Thurai function, the double-moment outperforms the SCOPE-ME, while the opposite is true when the Brandes function is used, etc.). Averaging over the axis ratios, there may be a sort of compensation in the results. My opinion is that could be more interesting just to show the difference for each axis ratio but averaging over the three regions (much better if the considered data are collected by the same instrument as already said). This could give an indication about a climatological dependence of the results.



- Figure 8 and Table A3: the results show that when the double-moment technique is applied to the radar data, the improvements with respect to the SCOPE-ME are not so evident as much as when the technique is applied to the radar variable as simulated from disdrometers. Can the authors tell something about this?

- Page 17-lines 2-4: please explain better the two sentences.

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