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Interactive Comment

Interactive comment on "Multi-sensor analysis of convective activity in Central Italy during the HyMeX SOP 1.1" *by* N. Roberto et al.

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I find this to be a very well-written and well-developed paper that provides a valuable contribution to the body of work on the relationship between C-band weather radar and 2-D video disdrometer observations and lightning activity. My comments on the paper follow.

Major comments

1. My main concern deals with the verification (or lack thereof) of the radar hydrometeor classification algorithm (HCA). The authors state the algorithm

was "tailored" for C-band and graupel detection, also write (on p. 9258, lines 15 and 16) that mis-detections of graupel by the HCA are possible, and write (on p. 9263, line 18) that the HCA is "properly tuned":

a. How do the authors really know how well the algorithm performs without coincident in-situ verification (or some other means)?

b. Can an error structure for the algorithm be estimated and how does this error structure impact the results of the study?

2. Furthermore (and related to the previous comment), there is a newer radar HCA:

Bechini R., and V. Chandrasekar, 2015: A semisupervised robust hydrometeor classification method for dual-polarization radar applications. *J. Atmos. Oceanic Technol.*, **32**, 22–47.

This HCA includes a class solely for graupel (instead of the class "graupel and small hail" used in the present HCA), which seems to be more appropriate for this study, as well. Why wasn't this HCA used instead of the Lim and Chandrasekar (2005) method used by the authors?

3. Related to the issue of efficacy and verification of a dual-polarization HCA, the following reference related vertically integrated reflectivity to lightning activity:

Mosier, R. M., C. Schumacher, R. E. Orville, and L. D. Carey, 2011: Radar nowcasting of cloud-to-ground lightning over Houston, Texas. *Wea. Forecasting*, **26**, 199–212.

I suggest the authors cite and briefly explain this work in the context of their work. How can the authors justify using an HCA to only vertically integrate graupel when that HCA is not verified? In other words, why wouldn't vertically

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integrated reflectivity be used for this study instead of (or at least compared to) vertically integrated graupel when the vertically integrated graupel quantity may contain significantly more error?

4. I believe the authors should state explicitly where the values of the coefficients ("a" and "b") used in Eq. (2) came from and how can their use be justified with such high (above 50%) NSE values (even if unbiased)?

5. On p. 9251, the authors write that they estimate specific differential phase using the finite difference method (Bringi and Chandrasekar, 2001). There is a newer and better method for this:

Wang, Y., and V. Chandrasekar, 2009: Algorithm for estimation of the specific differential phase. *J. Atmos. Oceanic Technol.*, **26**, 2569–2582.

Why wasn't this (better) method used in this study?

Minor comments

1. The authors include a reference to a seminal paper on this topic in the "References" section,

Carey, L. D. and S. A. Rutledge, 2000: The relationship between precipitation and lightning in tropical island convection: A C-band polarimetric radar study: A C-band polarimetric radar study, *Mon. Weather Rev.*, **128**, 2687–2710

This work in not cited in the body of the paper, however. I believe the authors should cite this work in the text and provide suitable material supporting the citation as this is an important paper in the context of the authors' work here.

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2. I believe the authors should include and explain in context reference to the following paper (especially their Section 2):

Woodard, C. J., L. D. Carey, W. A. Petersen, and W. P. Roeder, 2012: Operational utility of dual-polarization variables in lightning initiation forecasting. *Electronic J. Operational Meteor.*, **13**, 79–102.

3. I also believe the authors should consider including reference to and brief discussion of the content of the following paper:

Preston, A. D., and H. E. Fuelberg, 2015: Improving lightning cessation guidance using polarimetric radar data. *Wea. Forecasting*, **30**, 308–328.

4. The authors seem to use the terms lightning "flashes" and "strokes" interchangeably. I believe the authors should provide a definition for each and then use this definition as a basis for consistent usage throughout the paper. Are the authors using "flash rate density" in their analysis but not using this terminology explicitly?

5. On p. 9246, lines 18 through 22, the authors mention lower lightning detection efficiencies and higher location errors in the Mediterranean region. How does this impact the results of this study?

6. On p. 9247, line 17, the phrase "did not flight" should be changed to "did not fly".

7. On p. 9248, line 4, the authors mention "the entire convective event". How is this, an "event", precisely defined? A convective storm may grow and decay to

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extinction wholly within the observation domain, or an event may skirt the edge of the coverage domain yet still exist beyond the observational coverage area.

8. Also on p. 9248, on line 12, the authors write that a broad population of graupel PSDs was obtained by randomly varying parameters of an exponential PSD. By "random" do the authors imply a uniform distribution from which the samples are taken? Why or why not?

9. Also on p. 9248, on line 17, the authors perform the T-matrix simulations to +5 deg C. Why did the authors choose to simulate above freezing when this level is not mentioned elsewhere in the paper?

10. Can the authors provide a reference to the attenuation method used (p. 9251, line 13)?

11. The authors may need to re-phrase p. 9251, line 15, "Resampling of the polarimetric measurements at 1200m of range resolution (16 range bins)". I find this a bit confusing. Should the phrase read, "Resampling the polarimetric measurements to have 1200m resolution in range (i. e., 16 range bins)"?

12. On p. 9252, line 25, how is the term "bands" defined?

13. On p. 9257, line 27, the word "underestimated" is mis-spelled.

14. In the "Summary and conclusions" section, the authors should discuss and

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explain the variability they showed between cases in reference to their mention of the 15 October case.

15. On p. 9263, line 16, the word "are" should be changed to "were", since the experiment occurred in the past.

16. On p. 9264, line 21, the authors should consider re-phrasing "High performance from statistical scores" to "High performance in terms of statistical and skill scores".

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 9241, 2015.