Manuscript amtd-8-9241-2015 "Multi-sensor analysis of convective activity in Central Italy during the HyMeX SOP 1.1"
By N. Roberto et al.

Reply to Reviewer (E. Ruzanski) comments

The Authors thank Dr Ruzanski for spending his time reading the manuscript and for having appreciated our work, whose drafting will be improved by taking into account his comments.
In the following paragraphs, we reply item-by-item to the Reviewer's comments, which are enumerated and copied in blue color.

Major comments:

1 **Comment:**
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, lines15 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?

Reply
It is very difficult to provide a direct verification of HCA: a direct verification would require coincident measurements with aircrafts equipped with specific probes. Most of the works published on classification do not include such verification (a relevant exception being Lim and Chandrasekar, 2005). In this work, the HCA is not a new one, but it is based on existing fuzzy-logic HCAs (Liu and Chandrasekar, 2000; Lim and Chandrasekar, 2005) modified for C-band and using a membership function optimized for graupel identification. The particular membership functions relative to graupel have been modified based on T-matrix simulation outputs (Section 3.1.2). In order to evaluate the performance of the optimized HCA for graupel identification, we compared it with the non-optimized scheme in Fig. 5 and in Table 4. The results show that the new membership functions used for this work for the identification of graupel hydrometeors lead to graupel mass estimation better correlated (higher R^2) to the LINET strokes than the "original" membership functions. However, regarding the possibility to directly estimate an error, this should be done using coincident in situ measurements of graupel that were not available for the flights scheduled for the SOP1.1 in CI (Ferretti et al., 2014). In this work, we limited to consider an improvement to an operational algorithm, which actually provides a better agreement with strokes detection (see Table 4).

2. **Comment**
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?

Reply

Of course, we are aware of the HCA of Bechini and Chandrasekar, 2015. It is an important step toward the improvement of HCAs (although also this HCA lacks in direct validation), since it exploits spatial information that is not considered by the conventional HCSs. Concerning the goals of our study, we think this new scheme could be useful to mitigate the occurrence of non-homogeneous pixels, but its skills in discriminating graupel from small hail need to be tested thoroughly. In this study, we have used an operational HCA scheme in order to compare our improvements applied to a consolidated product. Likely future studies we will be carried out we will make use of newer hydrometeor classification algorithms.

3. **Comment**

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 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?

Reply

Thanks for your suggestion in relation to this reference. As demonstrated by Moiser et al. 2011), it is difficult to determine a unique threshold to detect graupel or *vertically integrated ice (VII),* since the thresholds on reflectivity depend on different parameters such as the distance from radar or the height of isothermal layer. Using MBFs (the one for reflectivity is shown in the *Figure I* below) presents two advantages: i). reflectivity threshold is modulated by a proper function of probability ii) besides reflectivity the HCA is based on other polarimetric measurements (such as K_{dp}, Z_{dr}, ϕ_{dp}) which properly modulated (as described in table 2) improve the detection of graupel.

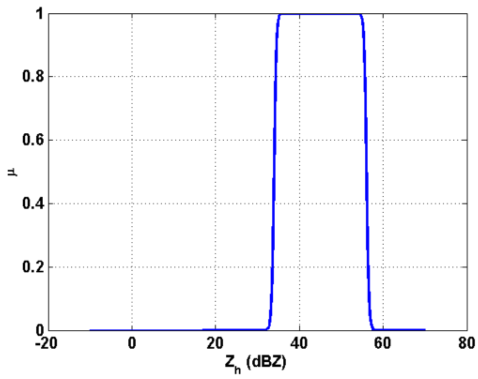

Figure I MBF for reflectivity factor at C-band.

Action

The reference suggested will be inserted in the introduction of the revised manuscript in relation to the topic of graupel identification. In particular the following sentence will be added

"*Moiser et al. (2011) used the radar-derived parameter vertically integrated ice (VII) to forecast CG lightning. They found that any increase of VII over 0.42 kg m^2 is shown to represent a sufficient amount of precipitation mass for cloud electrification.*"

4. **Comment**

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)?

Reply

The coefficients "a" e "b" were derived from T-matrix simulations of the IWC for graupel and the radar reflectivity factor for the two shapes of graupel (conical and spheroidal) as listed in Table 1. The results of the T-matrix simulations are shown in *Figure II*. The scatter plots were fitted with exponential curves (Eq 2) and the derived coefficients "a" e "b" are provided in the manuscript (lines 22-23 page 9250).

High values of NSE are associated to the high variability of the population of simulation used. In fact, T-matrix simulation input parameters were set varying randomly over a wide range of values (see Table 1). This means that heterogeneous inputs were created with a significant impact on the dispersion of simulated parameters and consequently high NSE values.

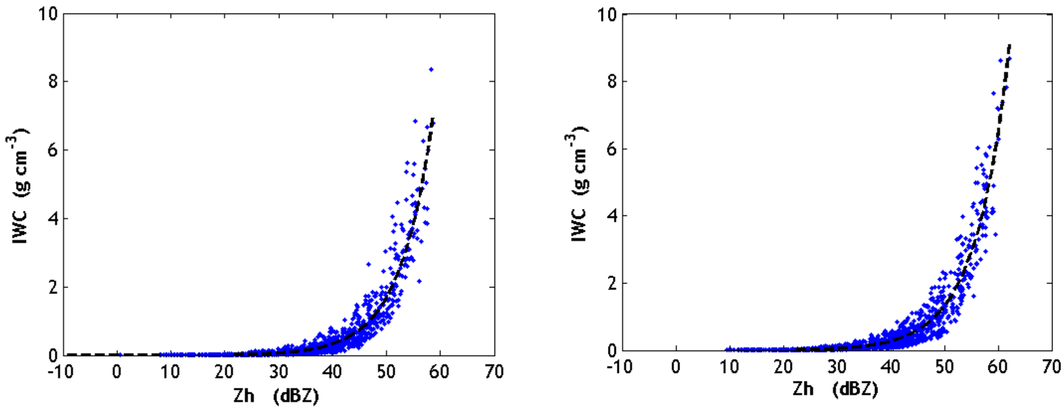

Figure II Scatter plots for T-matrix graupel simulation for spheroidal (left panel) and conical (right panel). Exponential fit is shown in black dashed curves.

Action

The scatter plots in *Figure II* will be added in the revised manuscript in order to clarify the derivation of coefficient in Eq. 2.

5. **Comment**

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?

Reply

In relation to Wang and Chandrasekar (2009) method we did not use it mainly because

a) The software is not freely available to us.
b) Its implementation needs ρ_{hv} measurements which were not available in our dataset (spatial variability of polarimetric measurements is used in HCA).

Finally, note that Bechini and Chandrasekar (2015) does not use Wang and Chandrasekar (2009) and explains why at page 26.

Minor comments

1. **Comment**

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.

Reply

Thank you for your reporting.

Action:

The citation Carey and Rutledge (2000) will be added in the text, in particular in the introduction of the revised manuscript.

2. **Comment**

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.

Reply

Agreed.

Action

We will add the following part at the end of section 2 *"The use of DP radar observation joint to an ad hoc scanning strategy provide a unique ability to improve ice mass quantification via detection of hydrometeor type such as graupel. This important ability of DP radar and its consequences for operational forecasting are highlighted by Woodard el al (2012) in the determination of the precursor microphysical conditions relevant to lightning production and thus first flash forecasting* "

3. **Comment**

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.

Reply

Agreed.

Action

The reference suggested will be included in the revised manuscript.

4.**Comment**

The authors seem to use the terms lightning "flashes" and "strokes" interchangeably. I believe the authors should provide a definition for each and the 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?

Reply

Agreed. Some clarifications are needed. No, we did not use “flash rate density” but always the number of strokes occurred in 5 minutes and in 120Km of ray from radar. In the light of comments provided by the Reviewer #2 will be introduced a scatter plot between graupel and flashes.

Action

The term “flash” will be left in the manuscript where referred to cited works that use “flash”. Where we refer to our findings, which are obtained from LINET measurements, we will use the terms “strokes”. It will be also clarified the use of strokes shown in Figure 7 in comparing to flash obtained from model output (Formenton et al., 2013) .

5. **Comment**

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?

Reply

The location error does not impact our findings, in fact the number of strokes used in this work are those registered in a wide area (of 120 km of ray from radar), thus the exact position of the strokes is not relevant as far as they occurred in that area. The lower detection efficiency might have an impact on the number of IC strokes detected. This aspect is thoroughly discussed at Pag. 9260, Line 8-18.

6. **Comment**

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

Reply

Right.

Action

The revised manuscript will be corrected accordingly.

7. **Comment**

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 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.

Reply

The term “event” in this context were a wrong exception, for this will be removed.

Action

The terms “event” will be removed in the revised manuscript and the sentence will be changed as follow. “*Polar 55C radar measurements were employed in this study to characterize the ice mass of* *graupel when convective cells occurred* ~~*during the entire convective event*~~ *within 120 km of the radar*".

8. **Comment**

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?

Reply

Yes, we refer to uniform distribution from which the samples are taken because our objective is to create a population of PSD with heterogeneous characteristics as wide as possible. Using this kind of populations we can obtain a good variability of parameters related to different graupel characteristics.

9. **Comment**

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?

Reply

We are sorry, this is a mistake. In fact in early simulations were performed in the range of temperature [-15 +5]°C that was late modified in [-15 0]°C in order to be in agreement with radar measurements but we left the wrong range in the text.

Action

The right range will be insert in the revised manuscript.

10. **Comment**

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

Reply

The method used and the reference were specified in p. 9250 lines 1-3.

11. **Comment**

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)"?

Reply

Agreed.

Action

The phrase in the revised manuscript will be rewritten as suggested.

12. **Comment**

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

Reply

In this frame with"bands" we were referred to frequencies.

Action

We will correct "bands" with "*frequencies*" in the revised text.

13. **Comment**
On p. 9257, line 27, the word "underestimated" is mis-spelled.
Reply
Right
Action
It will be corrected.

14. **Comment**
In the "Summary and conclusions" section, the authors should discuss and explain the variability they showed between cases in reference to their mention of the 15 October case.
Reply
Thanks to report this observation. The section will be modified accordingly.
Action.
The paragraph in "Summary and conclusion" section (page 9263 lines 25-27 and page 9264 lines 1-5) will be modified as follow.
"*Among the eleven convective precipitation events selected during SOP1.1, three important case studies were ~~selected~~ analysed. For these cases, linear relations with different slopes between the total mass of graupel and number of LINET strokes were found. Noticeably, the linear relation found for the case study on 15 October exhibits ~~a linear relation between total mass of graupel and number of LINET strokes, with~~ a high coefficient of determination (R^2 = 0.856) and a slope in agreement with model results. The variability of the slopes found for different cases can be related to the characteristics of convection, such as the ice mass and the different strength of updraft, although the radar measurement geometry might also play a significant role. Further research will be carried out to investigate on these aspects.*"

15. **Comment**
On p. 9263, line 16, the word "are" should be changed to "were", since the experiment occurred in the past.
Reply
Right.
Action:
The manuscript will be corrected accordingly.

16. **Comment**
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"
Reply
Right.
Action:
The manuscript will be corrected accordingly.