Manuscript amt-2015-171 – Reply to Reviewer comments

The Authors are grateful to the Reviewer for the time spent analyzing the manuscript, which value we are confident to demonstrate and improve taking advantage of her/his comments/suggestions.
In the following paragraphs we reply item-by-item to the Reviewer comments which are enumerated and copied in blue color.

General comments

1. Comment: The manuscript by Vulpiani et al., describes two convective events observed by the X band radar in Catania, Italy, which is operatively used by the Italian civil protection. The work is well written and clear and surely the measurements shown are interesting.
   However I do not understand which is the final goal of the paper.
   From the methodology point of view there are no improvements with respect to the state of the art. Some methodological aspects are highlighted whereas others are sketchy.
   A balance among the methodological arguments is needed.
   REPLY. As per the title, abstract, introduction and conclusions, this paper is not aimed at introducing innovative methodologies. Rather, it describes/analyzes two very intense convective storms observed in the Mediterranean area through a mobile X-band radar, located in a flooding-prone area, which is run operationally. As for several Mediterranean areas, the Italian peninsula is prone to intense, short –lasting events which, due to the complex orography, often impact small but densely populated catchments characterized by short hydrological response time. The use of radar observations might be crucial for early warning purposes. However, in complex terrain conditions, C-band radar systems might be severely affected by beam blocking. Mobile (or fixed) X-band radar observations might complement them but it is known that attenuation can be detrimental.
   We try to understand whether operational X-band radar can be effectively used in such difficult conditions.
   Furthermore, the documented events are worthy to be described, as stated by the same Reviewer. The events have been characterized by hail as documented by press and human reports (unfortunately we have not hail pads available). Notwithstanding, the polarimetric signature still remains pretty evident. Furthermore, the literature related to X-band observations has never documented, at least to our knowledge, events characterized by such high amount of precipitation in very short time period.
   Summarizing, there are at least two ingredients justifying the work: a) the reported events are very interesting being representative of risky events relatively frequent in Italy (an in other Mediterranean areas); b) it is aimed at testing the quantitative use of X-band radar observations to monitor severe events in complex environmental scenario complementing C band radar measurements.
   Regarding the description of the processing procedures applied, due to the fact that they are not new, we have decided to summarize with more emphasis what we consider the crucial step (e.g., the differential phase processing).
   ACTION: These comments will be reported in the revised version of the manuscript.

2. Comment: From the scientific point of view the microphysical analysis should be further expanded and accompanied by the explanation on how the analysis performed by the Authors, would benefit the future operational activities of civil protection.
   REPLY. As stated before, the goal is to document a couple of severe weather events using an operational radar. The adjective "operational" should not mislead the Reviewer. It is intended

that the system is run in operational mode (scheduled volumetric observations with limited number of PPIs, repeated every 10 minutes, no RHI scan mode, etc.). Also, the applied processing chain is not the one running operationally, despite there are some common algorithms (such as the phase processing), the adopted parameterization may differ.
Providing new recipes and/or suggestions for civil protection activities is an important issues that deserves specific attention, but is totally of the scope of the manuscript. It does not deal with early-warning issues and how to deal with them. Furthermore, for these subjects, AMT journal would not be the more appropriate one.
ACTION: This point will be further clarified in the revised version of the manuscript.

3. Comment: The conclusions are really weak.
After reading the paper I am not enriched by a sort of indication on how to improve the precipitation estimation at X band nor on which are the most critical aspects to take into account to avoid wrong interpretations of radar products. The only conclusion seems to be that the MPZ—R gives a larger error with respect to the use dual pol. Rain estimator alone or in a combined form.
REPLY. In our opinion, the work clearly shows how, despite the known limitations, X-band radar observations can be still used to provide fairly good estimates of rainfall thanks to the extensive use of K_{DP}. The validation carried out with the available rain gauge network shows the relatively good performance, independently by the compared radar algorithm. The Z-R algorithm is considered as reference to outline the benefit coming from dual-polarization.
ACTION: This point will be clarified in the revised version of the manuscript.

4. Comment: After the characterization of the hail bearing storms, which would be the indications given by the Authors to do a step forward in an operational environment as such that described in the manuscript?

REPLY.
The reply provided to comment 2 holds also in this case.

Additional comments

5. Comment: *Section 2.1 Operational scenario*: Why do you show the Visibility maps based on DEM and standard atmosphere assumption instead of using the clutter map (Vulpiani et al., 2012) as starting point to produce the same pictures taking into account anomalous propagation effects? However, I recognize that this is not really important for the final goal of the paper.
REPLY. The visibility maps are shown to qualitatively outline the potential impact of the orography on the radar observations focusing on the beam obstruction caused by the nearby Etna volcano. This is useful to show that the analysis carried out on the considered azimuth (5 deg for the 1st event) is not affected by beam shielding (at least for the considered elevations). In other words, the documented shielding is related to signal extinction. In this perspective, a clutter map does not carry this information.
As in Vulpiani et al., (2012) the clutter map is used here as input of the clutter filter algorithm.
ACTION: This point will be clarified in the revised version of the manuscript.

6. Comment: Section 2.2 Processing methodology Some processing steps are really scarcely described with respect to other ones. Differential Reflectivity: For example how do you performed the ZDR calibration? Since the Authors emphasize in the manuscript the value of the operational environment I would expect at least an indication of how ZDR is calibrated operationally (automatic rain identification form vertical profiles?, wet radome effects equally impair H and V channels? How often the operational calibration is performed?). Could you

please provide a scatterplot (as reply to this review is fine) of the corrected and calibrated ZH and ZDR samples in rain for the collected events? Just to have a more robust an evidence of the correct calibration procedure. The vertical profiles provided later on in the main text are representative only of an instant. I suggest providing a statistic (even two numbers of the average and standard deviation of ZDR in rain would be fine)

REPLY. The applied processing chain is not the one running operationally in Italy.

We are dealing with just a couple of weather events, we are surely not so ambitious to demand conclusive results. This work cannot be seen as a validation of operational procedures or a set of recommendations/recipes to the operational community.

More specifically, the manuscript never mentioned the use of any operational calibration procedures. With regards to Z_{DR}, based on the vertical observations collected when the stratiform tail of the storm overpassed the radar site, we have just stated that ***"****Focusing on the differential reflectivity profile below the melting or in the snow region, a negligible calibration bias can be noticed."*. Influence of wet radome attenuation on Z_{DR} calibration has been verified to be negligible by Gorgucci et al. (2013).

Test on the application of the self-consistency principles for the absolute calibration, the estimate of the differential reflectivity bias using the Sun signals have be conducted (Holleman et al., 2009). Regarding the operational chain it has been decided to proceed with the latter.

ACTION: In the revised manuscript the mean and std of Z_{DR}, as retrieved by vertical incidence observations, will be added.

7. Comment: *Specific differential phase estimation:* Could you please add the curve of filtered phi_dp in figure 2 as would come out from Figueras and Ventura, 2013, that you mention in the text for comparison?

   REPLY. There are many K_{DP} estimation algorithm on the market and their actual implementation is not available to researcher (It would be great if Reviewer 1 could suggest us how to obtain the source code necessary to apply the mentioned algorithm: this would preserve the correct implementation of the algorithm avoiding further review iterations related to the same topic). Moreover, the reference was added to testify that the window size adopted to filter Φ_{DP} is objectively much smaller than usual. This is not a declaration of superiority with respect to Figueras and Ventura et al. (2013) or any other algorithm.

   ACTION: To be as much clear as possible, we will modify the text as follows: *"It is worth noting that the length of the adopted moving window (1 km), shorter with respect to that applied in Vulpiani et al. (2012) or that generally used in other Φ_{DP} filtering scheme (e.g. Hubbert and Bringi, 1995, Figueras and Ventura et al., 2013) was necessary to capture small-scale phase gradients (thus avoiding an excessive smoothing of rainfall fields) whereas, at the same time, the iterative scheme enables to smooth noise effects."*

8. Comment: Looking at figure 2 I have two doubts: The original raw phase is not so noisy thus probably the highlight on the iterative phase filtering appear to me unmotivated in the context of the current stage of the manuscript;

   REPLY. The iteration scheme is aimed at keeping under control the expected K_{DP}-standard deviation especially when using short moving window size. This is a general purpose of the algorithm which we believe is worth of mentioning, even if in some circumstances might be unnecessary provided it does not impair the performance. From a computational point of view it does not. Resorting to the capability to easily manipulate matrix by the most common interpreted languages (e.g., IDL, Matlab, Python), the algorithm can be easily implemented and is extremely fast. The increase in the number of iteration *Ni* (here we are talking of a very few iterations, e.g. *Ni*=3) is largely compensated by the reduction in the window size so that the overall effect is that the entire Φ_{DP} volume can be processed in a very few seconds.

Finally, it is worth mentioning that, the concept of iterative Φ_{DP} filtering was introduced by Hubbert and Bringi (1995) to deal with backscatter differential phase.
ACTION: This point will be clarified in the revised version of the manuscript.

9. Comment: The initial part of the reconstructed phase seems to me to underestimate the raw phase. Do you have any explanation? There has been any adaptation in the phase reconstruction algorithm from C band (in Vulpiani et al) to X band?
REPLY. The (minimal) differences rely on the a-posteriori estimated offset, which is not an outcome of the algorithm. The estimated Φ_{DP} is obtained by integrating K_{DP}, which is immune to the offset being retrieved as range derivative. Generally speaking, we do not exclude that the offset, roughly estimated by comparing the raw and the filtered Φ_{DP}, may be slightly biased. Indeed, it has been obtained looking for the minimum difference between the raw and filtered Φ_{DP} in the very first available range path. Enhancing the estimated offset by a very few degrees (just for explanatory reasons) would not change the overall impression that the algorithm is able to catch about 100 deg of phase shift in about 12 km. More importantly, the a-posteriori estimated offset does not affect in anyway the processing, i.e., attenuation and rainfall retrieval.
The algorithm proposed in Vulpiani et al. (2012) has not been adapted for X-band, the choice of a shorter window size is the only difference.
ACTION: To clarify this point, the original statement *"The estimated offset has been added to the filtered Φ_{DP} for easier comparison"* will be modified with *"The offset of the raw Φ_{DP}, which is not an outcome of the processing algorithm, nor it influence the applied processing (i.e., attenuation correction and rainfall estimation), has been roughly evaluated by the a-posteriori comparison with the filtered Φ_{DP} for easier interpretation of the results."*

10. Comment: Attenuation. Could you please add some comments on the gamma attenuation coefficients? How they differ from those used in the reference literature that you cited? What does it means DSD collected in Italy? Are DSD collected all over the Italian country? If DSD collection refers to a specific site, do you think that this can have an impact on the choice of the coefficient that you used in the paper for the Sicily events? Just to put units to gamma, could you please specify the units on the attenuation (A) in the A=gamma*Kdp relationships? Page 8, line 15. Is it gamma_H=0.29 and gamma_DP=0.048?
REPLY. Based on DSD measurements collected in California, Matrosov et al. (2005) found $\gamma_h = 0.25$ $\gamma_{dp} = 0.033$. The γ_h value adopted in this work are derived from 4-year DSD measurements collected in Rome (central Tyrrhenian coastline of Italy). According to the updated Köppen-Geiger climate classification (Peel et al., 2007, Rubel et al., 2010), the central-southern Tyrrhenian coastline of Italy is climatologically similar to Sicily. We believe that, in absence of any other in situ DSD observations, the adopted algorithm parameterization can be fairly justified.
This point will be specified in the revised version of the manuscript.
ACTION: The units of $\gamma_{h,dp}$ (dB deg^{-1}) will be inserted in the revised paper.

11. Comment: Eq. 2 What "K" stands for? R and R_C have the same mening? The same equation is not fully clear to me. Is R_C calculated for each (x,y,h) available coordinate. How do you produce the final precipitation map R_C(x,y)? Is there any additional threshold or criteria, with respect to those of the clutter removal and quality check to filter out unrealistic values of Z(x,y,h) and KDP(x,y,h)? Please more details are needed on this methodological aspect of the paper.
REPLY. The subscript *"K"* refers to the use K_{DP} for rainfall estimation. As stated after eq. (2) at line 25-26, i.e., **"**where R_Z and R_K are the rainfall estimates obtained applying specific power laws to the lowest beam map of Z and K_{DP}, respectively". This means that starting from the polar volume of $Z(r, \theta,\varphi)$ and $K_{DP}(r, \theta,\varphi)$ the lowest (unshielded) elevation is considered to get

the corresponding near-surface maps (2-D) Z(r, θ_L,φ) (where θ_L stands for the lowest elevation). The rainfall fields obtained separately using Z(r, θ_L,φ) and K_{DP}(r, θ_L,φ) are then weighted as in eq. (1).
ACTION: It will be clarified in the revised manuscript.

Comment: Hydrometeor Classification. How the altitude of the melting layer is obtained? In the classification scheme I noticed you use the cross correlation coefficient RHV. Do you compensate it for SNR depressions? I noticed after in the manuscript that you specify this point. I suggest to mention the RHV compensation in the methodology section. How did you calculate the constant SNR constant (C) to compute the SNR? Have you assumed SNR_H= SNR_V in Horizontal and Vertical polarization?
REPLY. The altitude of the freezing layer is roughly estimated from the observations at vertical incidence which are in a relatively good agreement with the available radio soundings.
Regarding the compensation for low SNR, the correlation coefficient is computed taking into account the noise power at both channels

$$\rho_{hv} = \frac{R_0^{HV}}{\sqrt{|R_0^H - P_N^H| \cdot |R_0^V - P_N^V|}}$$

where R_0^{HV} is cross correlation between horizontal and vertical channel at lag 0, R_0^H (R_0^V) is the clutter corrected power from the horizontal (vertical) channel, P_N^H (P_N^V) is the noise power from the horizontal (vertical) channel.
The noise power (i.e., P_N^H and P_N^V) are estimated every 10 minutes using the Selex-Gematronik GDRX ® utility called "*Zero check*" which average over samples and gates, taken at 60 deg of antenna elevation, to produce a noise power average.
ACTION: As suggested by the Reviewer, the aforementioned procedure will be described in the revised version of the manuscript.

12. Comment: Figure 3. Could you please put a marker to highlight the radar position? This would facilitate the reader while she/he is trying connecting the main text to the figure. In figure 3 I noticed that the peak depression of RHV is mismatched with respect to the maximum peak shown by Z. Can you please add more comments on this aspect in the main text?
REPLY. As outlined by some authors (Zrnic et al., 1994) the peak of reflectivity shows up where the melting process creates the largest drops, whereas the minimum of ρ_{HV} occurs where the precipitation medium is heterogeneous at the most. Consequently, the mentioned mismatch is expected. Moreover, it is consistent with Doppler velocity profile of Fig. 3 (Baldini and Gorgucci, 2006).

ACTION: this effect will be described in the revised manuscript.

13. Comment: Pag 10, lines 5---15. The radome attenuation is really evident from the VMIs at16:00 and 17:00 UTC. From what I understand, the radome effects are not taken into account for the operational radar in Catania. Could you please at least spend some words for indicating a possible future strategy to way out from the wet radome issue in the light of an operational framework?
REPLY. We agree with the Reviewer comment. Indeed, at 16:00 UTC the effect of radome attenuation is quite clear. However, we would like to stress that it does not affect K_{DP} which is the main parameter used for rainfall estimation. Regarding the operational correction of such effect, we planned to test a couple of techniques: the first is based on an external, nearby measurement of rain (After Bechini et al. 2010) in order estimate wet radome attenuation, or self-consistency as in Gorgucci et al. (2013). According to last paper, the two approaches are

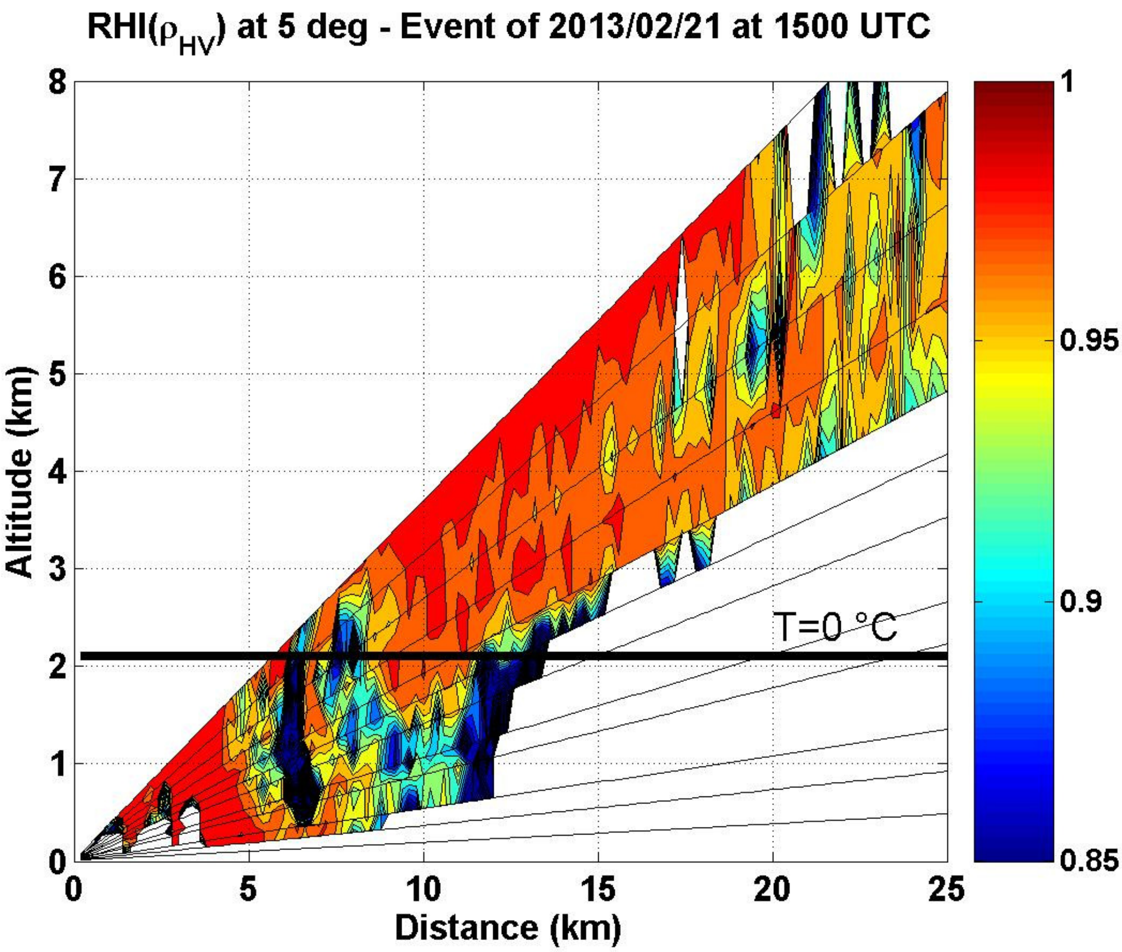
equivalent. For an operational implementation, the redundancy of these two methods could be exploited.

ACTION: This comment will be reported in the revised version of the manuscript.

14. Comment: Pag 10 lines 20-25. Do you have some physical arguments to explain why Kdp does not show two double columns of the same intensity? The "first reflectivity nucleus" ranges between 5 and 6 km in terms of radial distance from the radar. In that range distance RHV>=0.95 and the peak of Kdp does not match with that of ZH. In the main text the Authors state that for the first reflectivity nucleus, "RHV is mostly below 0.95" which is in contrast with what one can extract by the visual inspection of figure 6. Thus, I would suggest to better support their conclusion on the presence of rain and hail coexistence in the "first reflectivity nucleus". In general, from figure 6 I note that there is a spatial mismatch from ZH, RHV and KDP that makes me confused when thinking to the Author's conclusions. Figure 7 confirms the mismatch of the radar variables noted in figure 6.

    REPLY. It is not clear to us why K_{DP} should have the same intensity in the two consecutive cells.

    Regarding the correlation coefficient, we confirm what has been stated in the manuscript, i.e. the likely presence of rain/hail mixture. Indeed, as it can be seen by the figure reported below, ρ_{HV} drops to about 0.9 (light blue) starting from about 0.5 km A.S.L.



15. Comment: Pag 11, line 15. Why you show 3h rain accumulation [15-18 UTC] in figure 8 while you are calculating the error scores in table 1 on hourly basis for the whole event? In figure 8, I would show the rain accumulated in the same interval which is used for the values in table1.Just

next to figure 8, I would insert the rain map from rain gauges that were available at that time for operational purposes. Please specify the time interval of accumulation. Additionally, is it not clear if the error scores in table 1 are calculated using only one rain gauge in Catania or all the available gauges nearby.

REPLY. Most of the precipitation event occurred in such time interval, the aim of the Figure is to show the imprint of the reconstructed rain field while the results are computed on hourly basis. Because, most the precipitation was observed over the sea, the gauge –derived map would not have the same explicative impact. All the available rain gauges have been used (about 20 gauges registering precipitation are distributed mostly along the coastline).

ACTION: This argument will explicated on the revised manuscript.

16. Comment: Pag 12, line 5. Lightnings are in the lower right panel of figure 9 and not in the left one as has been written in the main text.

    REPLY. Right.

    ACTION: The manuscript will be corrected accordingly.

17. Comment: Pag 14, line 18. The Authors say that the radome effects are negligible for the events that they analyzed. This statement is not consistent with what is shown in figure 5, lower panels.

    REPLY. Right. The effect cannot be generally neglected. However, because of the extemporaneous occurrence and the extensive use of K_{DP}, the influence on the rainfall estimation is really negligible.

    ACTION: The manuscript will be modified as follows: "*However, intense precipitation rarely occurred above the radar, keeping generally negligible the influence of radome attenuation on the rainfall estimation which has taken benefit by the extensive use of* K_{DP}"

References

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