Replies to reviewer 2 comments/suggestions

At the outset, we would like to thank the reviewer for his constructive suggestions and comments, which we feel improved the manuscript significantly.

1) Authors should either exclude rain erosion from the title or need to include it in the analysis and discussion. Since the analysis of single profiler completely ignores the spatial variability of rain, which is essential considering the topography (Fig 2), I think the data set cannot be used for this ambiguous topic. Also, soil moisture, soil type, vegetation, inclination, wind-driven rain etc. need to be included in order to properly address this issue.

Reply: We agree with the reviewer that the soil erosion is a rather complex problem. It depends on several factors although rain (including high impact weather systems like cyclones) is the most important one among others. The above information is included in the revised manuscript together with relevant references indicated by Reviewer 1.

The title of the manuscript has also been changed to better reflect the material presented in the revised version. As per one of the reviewers' suggestion, the title has been changed to "One year analysis of rain in a tropical volcanic island from UHF wind profiler measurements".

Please note that T. Narayana Rao and O. Bousquet , who actively participated in the revision of the paper, have also been added as co-authors.

2) UHF profilers are not designed for measurements in heavy precipitation or DSD measurements, even though some of those quantities can be retrieved under special conditions. However, the paper needs to address some of those error sources such as reflectivity measurements and rain attenuation, quality of wind measurements during heavy rain etc. The results also need to be discussed remembering those error sources. For instance, all cases discussed in the paper have a vertical extend of 3 km which is caused by attenuation rather than true cloud top heights. The paper needs to include a section about the retrieval steps and qc of profiler measurements. Are the methods discussed at page 3252 used for this analysis - this needs to be clarified.

Reply: It is true that UHF profilers are not designed for precipitation studies. Their receivers are tuned to receive very weak backscattered Bragg scattered echo. Therefore, the receiver often gets saturated during heavy precipitation (because of strong echo). During heavy precipitation (or more generally in convection), the vertical wind is also significant. Both unsaturated backscattered power and vertical wind information is required to obtain accurate DSD and the assumption of small vertical wind as is considered in the present study may not be valid during convection as it may produce considerable error in the retrieved DSD and its bulk parameters. This has been more clearly specified in the revised version of the manuscript.

Nevertheless, we would like to point out that rain attenuation is negligible at L band. The upper limit of 3 km is thus not because of attenuation, but only because DSD retrieval is only possible in rain regime. Solid or mixed phase precipitation are present above 3-3.5 km. Also, the DSD derivation is not possible in the bright band region that can be observed below the freezing level.

We agree with the reviewer that wind measurement is a concern during convection. It may produce some error when the assumption of horizontal homogeneity between beams is not met. All the above information are included and discussed in the revised manuscript.

« The measurement of precipitation by UHF radar is achieved by mapping the Doppler velocity spectrum into diameter space assuming that measured velocity is solely due to the falling of the hydrometeor. Vertical ambient air motion and atmospheric turbulence are thus considered as being negligible and Rayleigh scatter from particles is the essential contribution to the signal. Although the assumption of negligible vertical air motion is valid during the stratiform rain, whose occurrence is predominant, it fails during convection. Kanofsky and Chilson (2008) found that the largest errors in rain-rate estimates are due to unaccounted vertical ambient air motion. Although there is no easy way to retrieve DSD with only UHF profiler information, this error is partly reduced by a spectral average made over 3 radar cycles (~ 15 min). The DSD is derived from the Doppler spectrum at each sampled height by applying a specific relationship between drop diameter and terminal fall speed. »

3. Due to the high spatial and temporal variability of rain, the calibration of radars is only useful for a long time series. It seems that the authors only used a short time period and not the entire year. However, more discussion and clarification about the radar calibration needs to be included. Furthermore, a figure showing scatter plots of all data (profiler vs rain gauge) needs to be included to support the argument that the radar is well calibrated.

Reply: Rain data collected over an entire year are now being used for radar calibration. Also, as suggested by the reviewer, we have presented a scatter plot between rain gauge-R and radar derived-R, using all rain events. The scatter indicates the statistical uncertainty in the calibration of reflectivity/rain rate. Some discussion about the comparison has been added in the revised version:

« In the present study, the calibration is achieved by comparing rain rates obtained with profiler and rain gauge, separately for wet and dry seasons (Figure 3). »

« It can be seen from Figure 3 that *R* is larger during the wet-season than during the dry-season In spite of the complications involved in radar-rain gauges comparisons mentioned above, the correlation between reaches more than 0.7 in both seasons, which is quite good. The spread of the scatter is mostly due to different wind regimes, which will be described in the following section. »

4) Throughout the paper more details about the methods need to be included. For instance, are all analyses based on hourly mean data? What qc is applied to profiler data? How are data handled when the rain gauge measures precipitation while the profiler is not? what is the qc procedure for the rain gauge data?

Reply: All the above information has been added in the revised version. The instantaneous profiler data with 5 min. resolution have been used in the present study. Details of QC (for both profiler and gauge) have also been included. For instance, following Rao et al. (2008), we identified the rain echo from reflectivity and Doppler velocity profiles. The problems in estimating wind and DSD during heavy rain are also detailed in Sections 2.2 and 2.3.

5. Sections 2.3 and 3.1 are confusing (see also commend #3). Why is the calibration done at 600 m profiler level, while the comparison is done at 800 m. Second it is not clear what data are used for the calibration. If the same data set is used, then this would not make sense at all. Based on Fig. 5, is seems that there is a clear pattern of rain followed by no rain, the way the red bar are plotted looks strange (width of the bars and the white space) - The authors should check their code. Statistical values of median, spread etc between the profiler and rain gauge should be provided. In general what is the purpose of the statistics? Why are rain characteristics not analyzed based on the title. I understand the need to show at the profiler data are correct, but I doubt that rain gauge measurements (point measurements) are sufficient to be compared to vertical profiles. I would suggest that authors do a long-term calibration of profiler using rain gauge data and then discuss rain gauge, disdrometer, and profiler measurements in terms of rain characteristics as promised in the title.

Reply: As mentioned previously, rain data collected over an entire year are now being used for radar calibration The faulty figure has been replaced with a new one that more clearly illustrates the comparison between rain gauge and radar-derived estimates.

6. What is the purpose of the case studies? what kind of events are those? Again since the measurements only reach up to a height of 3 km, are those low clouds, are the measurements limited to attenuation etc.? The measurements themselves look weird and noisy, e.g., on Fig. 8-2a at 1700 reflectivity is around 0 dBZ and fall velocity > 5 m/s, 3b 1530 wind max of 30 m/s at 3.5 km. So, before the authors start calculating DSD parameters the quality of the data has to be excellent, so far, that is not given by looking at the plots presented:

Reply: We have tried to improve the manuscript by categorizing rainfall events based on weather patterns. Four types were identified and one case study associated with each category was investigated in more details. Also, in the previous version, Bragg scattered echoes were not filtered, which resulted in the strange patterns pointed out by the reviewer. In the revised version, we first identify rain echoes, following Rao et al. (2008), by using reflectivity and Doppler velocity profiles and apply the DSD retrieval algorithm only to precipitation data. As mentioned before, rain attenuation is negligible at L band.

7. Secs. 3.2.2-3.2.4 are completely useless unless the authors show that their data are excellent quality (which needs to be done with another instrument than rain gauge) and that the derived parameter are trustworthy. Why don't the authors show the difference in DSD between the profiler and a disdrometer? Again, what is the purpose of those sections - the figures need to be interpreted and not just shortly discussed. It seems that the authors just show that they can calculate stuff with interpreting results, I.e., vertical structures of rain and how that relates to erosion.

Reply: As per reviewers' suggestion, we have removed sections 3.2.2 & 3.2.3.

8) What are the conclusions of this study?

Reply: The paper aims to study the rainfall characteristics and soil erosion by rain associated with the different weather patterns that affect the La Reunion Island. The conclusions in the revised version are also oriented in this direction.

Minor Comments:

1) Introduction: Previous studies should not just be listed in the introduction, but results from those studies presented in the introduction should be discussed and how those results relate to the analysis done by Réchou et al. What are the objectives of the Réchou analysis?

Reply: As per reviewers' suggestion, the introduction has been modified and the objectives are more clearly spelled out.

2)P. 3251: line 13 What region is the Chen & Chen study for?

Reply: Taiwan. It is now mentioned in the text.

Line 28 What time of the year is the jet stream close to the island?

Reply: During winter (June to August). This has been specified in the revised version

3) P. 3252, 2nd par: what types of profilers were used? What does "good agreement" mean? What technique is used in this study?

Reply: The SAM model was applied to 915 MHz profiler observations in central Florida. The sans air motion (SAM) model uses only the Rayleigh scattering portion of the Doppler velocity spectrum to estimate, the spectral broadening, the ambient vertical air motion, rain rate, mass weighted mean diameter, and the raindrop size distribution from 300 m to just under the melting level at 4 km. There is a good agreement between SAM-model-retrieved rain rate (and mass-weighted mean diameter) at an altitude of 300 m and simultaneous surface disdrometer-derived rain rate (mass weighted mean diameter).

4) P. 3254, quantify "strong precipitation", " most intense precipitation events"; topographic map would be helpful, maybe overlaid with the mean precip map.

Reply: Topography and rainfall maps (Figures 1 and 2) have been included in the revised manuscript.

5) Fig. 1: explain red dots, green trees. Where is North?

Reply: Green trees represent forest area, whereas towns are shown with red dots. This has been explained in the manuscript.

6)P. 3255: List of equations should come before they are discussed, e.g., after 1st par.Discussion about error sources should be included. For instance, what is the accuracy of the DSD parameter depending on the accuracy of reflectivity and fall speed?

Reply: The above information is now included in the revised version of the manuscript. Kirankumar et al. (2008) quantified that the error in rain rate could be as large as 50% for a vertical wind velocity of 1 m/s. This information is included in the revised version of the manuscript

7)P. 3256, line 23: vertical velocity is negligible compared to raindrop fall speed. Is this assumption true for tropical convection and strong orographic forcing such as over the island? These issues need to be discussed?

Reply: The assumption of negligible vertical velocity may not be valid during strong convection. Nevertheless, the occurrence (not rain fraction) of stratiform rain is predominant (~60-70%) in tropics. The above assumption is valid in such rain events.
Furthermore, the integration of 15 minutes (3 radar cycles) should be enough (average on three cycles of 5 minutes) to mitigate the impact of the vertical winds.

8) P. 3257, lines 1-4, what are those values based on?

Reply: The approach proposed by Rao et al. (2008) is now used in the revised version of the paper. We followed the same approach, but used slightly different reflectivity threshold, which were selected from rain gauge measurements (i.e., non-zero rain to zero-rain below this threshold)

9) P. 3258, 1st par: The discussion in this par is completely hypothetical without any justification? Authors claim that the differences between radar and rain gauge are purely due to atmospheric processes but never discussed in terms of instrument differences. For instance, if radar-based rain rate is lower than there is evaporation -why should there be strong evaporation in the tropics during a strong rain event when the gauge measures 15 mm/h while the profiler measures 4 mm/h? Fig. 5 should be discussed more scientifically and in more detail.

Reply: Figure 5 is removed from the revised manuscript. Instead, a scatter plot with all data is included. Nevertheless, as suggested by the reviewer, the problems in such comparisons (sampling mismatch, errors in the retrieval, geophysical processes, etc.) are also discussed.

10)P. 3258, line 15. Explain typical differences?

Reply: They are explained later in the text. Moreover, we have included mean surface rain rates (as measured by rain gauge) also in the figure to highlight the differences in rain rate.

11) Fig. 6, why is the rainfall rate max between 1.2-1.6 km? What Z-R relationship was used?
Reply: R is estimated directly from DSD (equation 7), not through Z-R relation.

12) P. 3259, 1par. Clarify this paragraph.
Done

13) P. 3259, line 16: text mentioned wind shear but only wind is shown in fig 8 and further discussed. Line18, clarify "such a vertical structure"
Reply: The wind shear is estimated from winds given in figure 8. It is discussed qualitatively here and that is why it is not given as a separate figure.

14)P. 3262, line 1: vertical structure of rain rates and velocities have not been validated in this study. What is the basis of this conclusion?
Reply: We agree with the reviewer that the vertical structure of rain rate and velocities have not been validated. Only the rain rate at 450-600 m was evaluated. We, therefore, modified this sentence in the revised version.

15) P. 3262, line22, the maximum occurs between 1.2-1.6 km, however, the authors do not show that it is the same level of the mean trade wind inversion, therefore, this is not a conclusion of this analysis.
Yes we are agree that we don't show in this study the mean trade wind inversion. Nevertheless, in La Réunion, Lesouef (2010) and Lesouef et al.(2011), by dynamical analysis of UHF radar data (U close to zero) found that trade wind inversion occurred are around 3000m from April to October, and between 2000m to 4000m from November to March due to deep convection. By analysing the refractive index, they show that the trade inversion is around 2500 m during the austral summer and between 2400 and 3000m the rest of the year.

16) P. 3262, line 25: What is a weak and a strong DSD? This statement needs clarification. Also results need to be put in prospective of other DSD measurements in tropical convection.
Reply: As per one of the reviewers' suggestion, this part of the manuscript (i.e., sections 3.2.2 and 3.2.3) has been removed.