

The paper was improved even if I regret that it contains too many descriptions of the figures but not enough associated physical interpretations. New elements that the authors have been added (the POSS description in particular) are sometime a bit confusing. Moreover, I have found some other mistakes in the manuscript. I am sorry that a part of my new comments was not in my first review but I think it is important to take them into account. The last figures have been also modified and I think that some errors have appeared... I'm afraid that this paper is not totally ready for publication.

- Page 8, line 145: I am surprised by the value of water density assumed by the authors. I think that 1 g m^{-3} should be changed into 10^6 g m^{-3} , isn't it?
- Page 8, line 150: "Unis" should be changed into "Unit".
- Page 8, line 150-151: This factor just corresponds to the conversion of length units into mm and time units into hours.
- Page 8, line 161, last sentence: "Using DSD estimate" is confusing because it mixes the principle of the POSS radar for DSD estimation (I understood that POSS radar estimate DSD from Doppler spectra) and the POSS calibration technique. Indeed Johnson and Hamilton (2008) explained in their section 4 that $N(D)$ was estimated by inverting their equation (2) (corresponding to your equation (11)) using predetermined weighting functions. They explained also in their section 3 that "The "forward" equation (2) could also be used to calculate $S(f)$ for hypothetical $N(D)$ ".
- Same part: X (in $V(X)$) is not defined.
- Page 9, line 170 : I propose to change « A Doppler » by « The Doppler »
- Page 9, line 185, I don't understand the meaning of the sentence: "Number of DSD was smaller than ..." It means that the authors compare between the number of spectra with a number of channels (I understood that it corresponded to velocity or diameter classes) ? What do you think about this proposition: "Non-atmospheric data were removed from the analysis if the DSD spectrum was concentrated in less than five consecutive channels,..."
- Figure 3: PSN C-band radar is plotted but it is not used and not described in the text. I propose to delete it.
- Page 12, line 236-237: The authors introduced classification methods for rainfall type via the analysis of 3 microphysical characteristics. For the first one, they mentioned a $N0-R$ relationship. But they did not explain how $N0$ or/and R is/are used to classify as stratiform or convective rain.
- Page 12, line 240: $R < 0.5 \text{ mm h}^{-1}$ during 5 minutes is equivalent to 0.04 mm during 5 minutes. I thought it was very low threshold for convective rainfall! So, I checked in Johnson and Hamilton (1988) and I found that they mentioned that the threshold is 0.5 mm per 5 min (that corresponds to 6 mm h^{-1}). The sentence should be therefore modified in the manuscript. Hopefully, the classification method proposed by Bringi et al. (2003) is applied in the present study.
- Page 14, line 279: I think that " $0 < \mu > 5$ " should be replaced by " $0 < \mu < 5$ "
- Figure 4, a bracket is missing in the caption just after " solid blue line"
- Page 20, line 417: the double bracket should be transformed into a simple one
- Section 3.1. and 3.3.1.: I regret that there is too much figures description and not enough associated physical interpretations.
- Figure 12 and 13: I think that "NT" and "DT" should be replaced by "Winter" and "Summer" in the legends as well as in the captions. I am confused also because your previous figure 14(b) in

the first version of the manuscript is the same as the figure 12(c) in the new version of the manuscript whereas the captions are different.

- The reference to Waldvogel (1974) seems to be not used and should be removed of the references list.