

Interactive comment on “Detection of potentially hazardous convective clouds with a dual-polarized C-band radar” by A. Adachi et al.

A. Adachi et al.

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Response to the second referee

We are grateful to the reviewer for the very careful and thorough examination of our manuscript. Detailed responses to the suggestions are given below.

1. Lines 227-227: The definition for VMI (vertical maximum intensity) is not given in the paper. It could be defined at these lines.

Response: We have defined VMI in the revised manuscript as follows.

“The vertical maximum intensity of the rainfall rate, VMI ( $R$ ), may be defined as

$$VMI(R) = \max_{z_1 \leq z \leq z_2} R(z), \quad (R1)$$

where  $z_1$  and  $z_2$  are the lowest and highest altitudes for the analyses. The corresponding altitude,  $z_{\max}(R)$ , may be defined as

$$z_{\max}(R) = \operatorname{argmax}_{z_1 \leq z \leq z_2} (R(z)).” \quad (R2)$$

2. Line 245: It should be mentioned that the differential phase  $\Phi DP$  unlike measured  $\Psi DP$  does not include the backscatter phase shift and the method to remove it.

Response: Please see our # 3 response to the first reviewer. Also, we have mentioned the method to remove the backscatter phase shift at step 2 of our algorithm in the revised manuscript as

“A five-gate running mean was applied to the filtered  $\Psi_{DP}$  in each ray to remove  $\delta_{co}$  and obtain  $\Phi_{DP}$ .

3. lines 276-277: ZDP is insensitive to ice particles not because they are more spherically symmetrical but due to their isotropic orientation (Bringi and Chandrasekar 2001).

Response: We have changed the sentence accordingly.

“The  $Z_{DP}$  is insensitive to ice because ice particles such as randomly oriented hail appear statistically isotropic (Bringi and Chandrasekar 2001).”

4. Line 290: What is the standard error around this rain line? For an ice fraction of 0.1, which is used by the authors as a threshold for “pure” rain for the application of Eq. (7), corresponds to a rather small  $\Delta Z=0.5$  dB. Probably a figure with an example of measurements for the estimation of the rain line Eq. (4) should be given.

Response: We added a scatter plot of the data used to determine the rain line with statistical results including the standard deviation. The standard deviation of  $Z_{DP}$  was 1.0 dB, which corresponds to  $\Delta Z=1.0$  dB. So, we have changed a threshold of ice fraction for pure rain in Eq. (7) from 0.1 to 0.2, and have reprocessed all the data.

5. lines 303-305: Have you used a more accurate attenuation correction algorithm with variable instead of fixed coefficients of the dependence on differential phase like in Kalogiros et al. (2013) , <http://dx.doi.org/10.1109/TGRS.2013.2250979>. There should also be a dependence on elevation angle.

Response: We have not used the SCOP-ME algorithm or other algorithms with variable coefficients. However, the scope of this study is not to retrieve rainfall rate accurately but to detect potentially hazardous clouds, and the comparison with disdrometer shows that our method with fixed coefficients has enough reliability to detect them. On the other hand, the differential phase has a dependency on elevation angle, as the reviewer pointed out. So, we have changed our algorithm to account for that.

6. line 307-309: Have you calculated a simple parametric dependence of ZDR (as well as the rest polarimetric measurements like KDP) on the elevation angle using T-matrix

simulations (e.g. a cosine squared dependence). This would be of practical usage.

Response: We have added a figure that shows the elevation dependency of  $Z_{DR}$ ,  $K_{DP}$  and  $Z_{DP}$  in the revised manuscript.

7. line 321: How was Eq. (8) estimated ? or give a reference.

Response: We have added a reference. (Hitschfeld and Bordan, 1954, J. Meteor.)

8. Section 2.3: The data length in the comparison with the disdrometer is too short (1.5 hour). Probably you should include data from more rain events.

Response: We have added another comparisons with measurements of a different disdrometer in the revised manuscript.

9. line 453: What is the difference of the dashed blue-white line from the blue contour line? The height of VMI(R) is the freezing level (line 459)?

Response: The blue contours show that the altitude where  $Z_{DR}$  takes its maximum value (i.e.,  $z_{max}(Z_{DR})$  in Eq. (R2)) is above the freezing level, and dashed blue-white line shows  $z_{max}(Z_{DR})$  is lower than the freezing level (below the bright-band level, actually). We have modified figure captions accordingly.

10. line 461: VMI(R) is estimated below freezing level but include the bright band. Does Zrain shows a bright band effect?

Response: We have recalculated VMI(R) to exclude bright band in the revised manuscript ( $z_2 < \text{bright band level in Eq. (R1)}$ ), although Zrain dose not show a bright band effect. Indeed, this modification does not change the results very much.

11. lines 491-498: The inclusion of Doppler information as additional information to estimate convergence areas and updrafts would be valuable in the proposed algorithm. The horizontal wind could also be used to estimate horizontal advection in addition to the time delay of heavy rain to reach ground. This could be future work for a more elaborate algorithm.

Response: We have added this point in the concluding remarks. Thank you.

12. Do the authors have any indication from ground observations (like flood occurrence, damages) for the very high estimated rainfall (250 mm h<sup>-1</sup>)?

Response: We have not get any information of damage with this event so far, probably because this event occurred in a rural area (rice paddy fields). In addition, since the cumulus cell was not stationary but was moving at the speed of 10 m s<sup>-1</sup>, the total rainfall accumulation at one location could be much less than 250 mm.

13. lines 532-534: Does this conclusion mean that the proposed algorithm failed in cell B<sub>2</sub>, which indicates that a more advanced algorithm should be used?

Response: No, it does not. We have modified this conclusion as follows.

“In addition, the surface rainfall rate associated with B<sub>2</sub> was not as strong (~60 mm h<sup>-1</sup>) at that time, but the VMI(R) suggests that heavy rainfall (~250mm h<sup>-1</sup>) was occurring in this region, which is also consistent with subsequent observations near the ground (Fig. 6).”

14. lines 596: Does “more extreme events” mean rainfall rate more than the already very high estimated value of 250 mm h<sup>-1</sup>.

Response: We have removed this sentence.

15. line 632: Define “effective” area, i.e. what are the margins of it.

Response: The size of the effective area is 27 mm x 180 mm. We have added the dimension of the area in the revised manuscript.

16. lines 634-644: In a recent paper, Tokay et al. (2013) attributed the overestimation of large drops number to the partially inhomogeneous laser beam in the first version of OTT Parsivel after communication with the company. This is a different explanation than the usage of  $D_e$  for  $D_p$  that the authors propose in the current paper. Have the authors contacted OTT for verification if the company uses indeed  $D_e$ ?

Response: We have sent our results to OTT in 2009 and 2013 by emails via its agency in Japan, but we have received no comments on our results so far. Although the partially inhomogeneous laser beam could be responsible for the overestimation, it seems us that Tokay et al. (2013) and OTT did not prove it enough yet. Indeed, the comparison in the present study shows that the two first-version Parsivels have almost the same statistical characteristics, which may show that the quality of the laser diode is quite good. Since the method in the current paper improves the reliability of OTT measurements very much, we believe that the cause of the overestimation should include the use of  $D_e$  for  $D_p$  (in addition to the partially inhomogeneous laser beam). We thank the reviewer for drawing our attention to that paper and have referred it in the revised manuscript.