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Atmospheric Measurement Techniques Discussions

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Interactive comment on "How to estimate total differential attenuation due to hydrometeors with ground-based multi-frequency radars?" *by* Frédéric Tridon et al.

Anonymous Referee #2

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General comments:

The authors present a handy method to identify regions where hydrometeors can be assumed as Rayleigh scatterers at Ka and W bands. This Rayleigh region is useful, because it allows the retrieval of the path-integrated differential attenuation between Ka and W bands. Then, the authors elaborate how the derived differential attenuation can be used to estimate the liquid water path. The method presented is independent of a threshold reflectivity as was used in previous studies and has potential applications in multi-frequency radar observations.

The authors well illustrate the background of this study and cite relevant literature. This

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Discussion paper

manuscript has some interesting aspects which deserve publication. However, more clarifications are still needed. Please see my comments below.

Major comments:

1) The title confuses me. I believe the community has already been aware of the answer to this question, namely, matching reflectivities at the cloud top facilitates the retrieval of total differential attenuation. After reading this manuscript, I feel that the most innovative part is the presented method for identifying the Rayleigh region in clouds and its applications. I suggest the authors modify the title based on their main contribution.

2) Although the gaseous attenuation has been noted in Section 2, it is necessary to elaborate its impact in a subsection. I also have two questions. 1) the gaseous attenuation has been corrected in the BAECC case, have you done the calibration for the TRIPEx case? 2) How would the performance of this method be affected without correcting the gaseous attenuation? I believe it will at least modify the DFR profile.

3) I think the method in general works well. The edges of the non-Rayleigh areas in Figure 5b 7b are more or less smooth, which is reasonable and expected. However, they are rather noisy in Figure 10 b (many spikes). Those spikes can be troublesome in applications and indicate the technical limitations of this method. But such information seems missing in the manuscript. In particular, there should be one section describing the conditions that this method is applicable. At least, the scenario of rain can be problematic.

Minor comments:

1) I suggest the use of DWR instead of DFR, since the DWR is more widely used in the community.

2) Equation 2. Could you please specify the meaning of equivalent water content and its unit?

3) L155. It is better to specify why not water droplets.

4) L178 and L190. The cloud top can be covered by a layer of liquid. The dielectric constants of liquid water are different at Ka and W bands, then the observed DFR is different even for Rayleigh-scattering liquid drops. Given the method is mainly applied to cloud top where a layer of liquid is commonly observed, how did you exclude the existence of this liquid layer? Maybe it is not easy to recognize this liquid layer without using lidar measurements, then how the DFR would be affected without the information about the hydrometeor phase? I expect it to be relatively small, have you quantified it?

5) Figure 2. It takes a lot effort for me to match the block in the flow chart to the explanation in the text. It will be more readable if you could number each block in the flow chart and order the explanation by serial numbers.

6) Figure 3. It helps the interpretation if you could also present the reflectivity profiles at Ka and W band

7) L207. Why the Savitzky-Golay filter is used? It works much better than other filters?

8) Figure 4b. Why there is a spike of DFR up to 9 km at around 4:20? Strong liquid attenuation? This spike seems resulting in the misclassification of non-Rayleigh region in Figure 5b. Could you please elaborate the reason and hint the readers the limitation of this method?

9) L256. Why the opposite is expected? Because of the melting?

10) Figure 5. The layout of (c) should be improved to match with (a) and (b).

11) L275. Although the agreement looks good, I am curious how much attenuation can be attributed to ice attenuation. It seems to me that the Ka-band reflectivity and DFR(Ka,W) are not that small.

12) Figure 6. It is hard for me to recognize the periods. Given the interrupts by rain, I suggest the authors mark the same short period by one color range.

13) L280. Radome attenuation should also affect the ΔPIA.

14) Figure 7 & 8. This case well demonstrates the dependence of liquid attenuation on the temperature. There is no liquid cloud below 4 km, therefore the liquid layer should be detected by the ceilometer. It would strengthen the conclusion if the cloud base detected by the ceilometer is marked in Figure 7(b).

15) L358. Why this temperature region is expected? Have you checked the lidar data? Marking the liquid layer in the plot will be more convincing.

16) L375. Are you assuming the temperature of -10 deg?

17) L394. What is the maximum measurable LWP for MWR? At around 2:30, the agreement seems rather good although the LWP is large.

18) L408. ‘negligible particle growth’. odd statement. What matters is the particle size instead of its growth.

Typos: 1) L80: ‘non-perfect’ 2) L116: ‘higher frequencies’ 3) L140: ‘W-band’ is missing 4) L170: ‘generally’ 5) L234: ‘by Dias Neto et al.’ 6) L295: not ‘curve’ in Fig.6 7) L329: ‘(Kalesse et al., 2016)’

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