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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An Improved Hydrometeor Detection Method for Millimeter-Wavelength Cloud Radar" *by* Jinming Ge et al.

Anonymous Referee #3

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In my view this paper is well-written, straightforward and makes a solid contribution to detection of atmospheric returns in radar receiver power outputs. Relative to the ARM algorithm (and perhaps the CloudSAT algorithm too?) it decreases the number of false negatives while importantly keeping the number of made up cloud detections (i.e., false positives) low in number as it must. I recommend its publication in Atmospheric Chemistry and Physics. I would like to see one addition to the paper which I outline below and then I have a few minor comments and clarifications that follow.

Additional piece to the paper:

On Line 343 the following sentence occurs: "This thin cirrus, however, is well-captured by our cloud mask method (Fig. 6b)." This is a subjective statement and I do not think

that a comparison of Fig. 6b to Fig. 6e supports it. Comparison of Fig. 6b to Fig. 6c shows improvement of the new algorithm relative to the ARM one, thereby lending support to the value of this new algorithm, but the thin cirrus appears to be much better detected by the lidar than the radar with application of either the proposed or ARM algorithms. Similarly, on Lines 378-380 the following sentence occurs: "This is because hydrometeors in the upper of troposphere are usually with smaller size and cause weak SNR values that will be effectively detected by the noise reduction scheme." The paper does demonstrate that the new algorithm does a better job detecting thin cirrus than the ARM algorithm but the paper does not demonstrate that thin cirrus "will be effectively detected by the noise reduction scheme." To address this weakness in the paper the authors should remove all subjective words from the paper, like "good", "well-captured", "remarkable" and replace them with comparative statistics. Morever, the authors state that they have mapped lidar and radar data to the same time height grid (see Lines 335-337. They should use this mapping to provide the percentage detected in Fig. 6b relative to Fig. 6e. Moreover, in a figure similar to Figure 7, the authors should illustrate results of the number of all lidar cloud detections also detected by the radar (but not necessarily vice versa as the goal is to determine how good the new algorithm is at mapping all lidar detected clouds) as a function of height. If this site had a lot of thin cirrus during either January or July 201, this will be a good test of the new algorithm applied to radar data. With these new results in the paper it will be interesting to see if the authors' claims on Lines 391-394 will hold up.

Minor details:

1) Line 141: On this line a reference is made to "the noise power". Is this noise power just individual values of Pn from the top 30 range gates without being averaged? Please make it perfectly clear the source of the data for the non-Gaussian curve in Figure. 1a.

2) Lines 149-150: "SNRs for clear skies closely follow a Gaussian distribution"

Lines 151-152: "SNR for the noise does not exactly obey the Gaussian distribution"

For clear sky the SNRs represent noise, right? If so, these two phrases seem to contradict each other. Minimally, I do not understand what the authors are trying to say here.

3) Line 176: I am not sure what "of each five successive profiles" means. Does this mean that the 150 range gate powers from the top 30 of five consecutive profiles are used to compute S_o and Sigma_o? Do these five profiles move with the 5 by 5 processing window that is used to create the results for this paper?

4) Line 275: "Note that a larger": Should "larger" be "smaller" here?

5) Line 297: "detection method works quite good": Remove the words "quite good" and quantify what you mean.

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