Response to Anonymous Referee #3

- 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.

Response: We thank the reviewer for his/her constructive comments and suggestions on this manuscript, which are very helpful for us to improve our paper. Our responses to the reviewer's comments/suggestions are given below.

-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.

Response: We thank the reviewer for the insightful comments very much. We compare the radar cloud mask results derived from our method and ARM algorithm with the MPL detected features in January and July, 2014 when both radar and lidar observations are available. We calculated the percentage of the increased detections

identified by both our mothed and MPL observations in the total increased detections only found by our mothed as shown in Figure 1. We can see that most part of the increased detection from our method is also detected as features by MPL. The percentage drops to a minimum of 70% at about 9 km, where the total increased cloud range bins are only about 110 and there are 35 range bins that are identified by our method are not observed by MPL. Considering all the increased detections by our method, 98.6% of them are confirmed by MPL as features. We replaced the subjective words with comparative statistics in the revised manuscript.

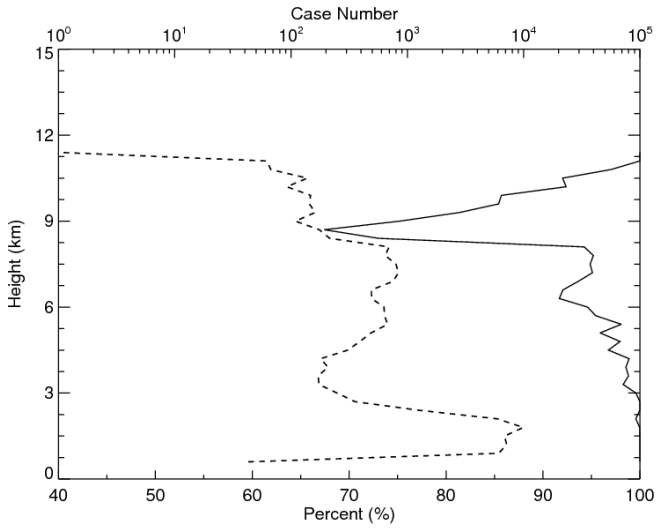

Figure 1. The solid line is the percentage of increased detections seen by both KAZR with our method and MPL as compared with the total increased detections. The dot line is the number of increased detections in each level.

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.

Response: We have added a reference to the noise power. The power distribution is derived from individual values of Pn at the top 30 range gates. The statement is clarified.

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.

Response: Yes, for clear sky the SNR values represent noise. The second phrase is used to explain the reason why the mean value of the SNR is not zero. We have modified this part in the revised manuscript to avoid confusions.

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?

Response: Yes, it means the total 150 range gate powers from the top 30 of given five consecutive profiles and these five profiles move with the 5 by 5 spatial filter.

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

Response: We thank the reviewer for the careful review. Yes, it should be smaller here. “larger” is replaced with “smaller”.

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

Response: “quite good” is removed. We have modified the statement here.