Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-402-RC2, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "Simultaneous and synergistic profiling of cloud and drizzle properties using ground-based observations" *by* Stephanie P. Rusli et al.

Anonymous Referee #2

Received and published: 30 April 2017

This manuscript describes a retrieval method that provides simultaneous cloud and drizzle retrieval from combined ground-based radar/lidar and microwave radiometer measurements. Since the retrieval presented here is important for understanding cloud and precipitation evolutions, I strongly support the work and admire that the authors made efforts on such a difficult problem. However, I feel that the writing and organization have made the manuscript quite difficult to follow. In particular, I was hoping to provide more specific suggestions how to reorganise the method part, but the unclear and convoluted sentences are just too confusing. I am afraid that the authors really need to rewrite a lot to make sure that the paragraphs have clear and better connections; the sentences/wording are precise and accurate, and more importantly,

many parts sound like "tuning" exercise, when the authors could have provided more convening reasoning.

1) Methodology

There are A LOT of assumptions, retrieval variables and tuning in the proposed retrieval method. Observations are supposed to provide "evidence" of cloud and drizzle profiles to allow us to explore new features, or to test if current assumptions and parameterizations are appropriate in models. If possible, we should let observations speak themselves, rather than forcing all kind of assumptions in the retrieval process. Although the authors mention that these assumptions are based on some other independent observations, it would be good to keep in mind that these assumptions are based on very limited observations, and may not work everywhere. As the authors may have already realised, "adjustment" of these assumptions is needed when this algorithm is applied to different cloud regimes. It would be good to know where the assumption fails, and how this failure affects the overall retrieval. Any limitation does not undermine the value of the proposed work/method.

These assumptions also intrinsically introduce many variables to be retrieved. We need to keep in mind that we only have lidar backscatter, radar reflectivity, and microwave temperature measurements. These are limited observations after all, so we should ask ourselves if these observations really contain sufficient information content to retrieve all the variables proposed in the manuscript. The answer is clearly, a No, and that's why the authors use "tuning" so often in the manuscript. In the end, it will be quite hard to track/ensure that there is no compensating error in the retrieval process. Could the authors comment on this and perhaps have a way to prevent the compensating errors?

The algorithm looks a bit unnecessarily complicated to me. For example, I don't quite understand why is needed to go through all the trouble to "tune" cloud base height. As shown in Figure 2, the authors apply an ad-hoc smoothing in order to get reasonable cloud base height that matches with lidar measurements. Why not using the "observed"

cloud base height instead, and then discuss/understand how sensitive the retrieval will be to the accuracy of the observed cloud base height?

There are also a lot of ad-hoc smoothing bits and thresholds in the proposed method. Rigorous scientific justifications about their choices are needed. For example, why choosing only 1 or 2 radar range gates to classify non-drizzling case. When should we use 1, and when to use 2 gates? Does this really perform better than threshold-based approaches, or they actually agree to a large extent?

Page 5: The justification of constant cloud droplet number concentration (N) with height is a bit misleading, and I feel that the authors are stretching this a bit too far. There is really no sufficient information to infer the vertical profile of N from radar/lidar/microwave measurements. Yes, some could probably use a stronger priori, but the result will not be mainly determined by observations. Saying that a constant N is "adopting the homogeneous mixing case" is just not quite right.

2) Evaluation:

Synthetic data set:

The key point of the manuscript is about cloud/drizzle properties. I am surprised that the authors chose a non-drizzling case in the synthetic data test. Without the presence of drizzle particles, I am less convinced about the performance of the proposed method. I think demonstrating a drizzling case is necessary.

For the current case, I am not sure I understand the results. In section 2, the authors keep emphasizing that they apply a droplet size threshold to separate the cloud and drizzle regime. As a result, they use 13 microns as the separation threshold, meaning that "at any altitude, cloud effective radius has to be smaller than 13 microns and drizzle effective radius cannot be less than 13 microns (page 11)". If that's the case, how come cloud effective radius in Figure 4 clearly exceeds 13 microns for most altitudes? I also don't understand why the majority of radar reflectivity is greater than –20 dBZ for a

non-drizzling cloud (and interestingly, it is opposite for the case from the ACCEPT campaign; see next). Also, the narrow range of cloud droplet number concentration may not be the best case for testing whether the retrieval method is robust.

The ACCEPT campaign:

Could the authors please modify the range of colour bar of radar reflectivity in Figure 6? It is unclear if radar reflectivity is much higher than -30 dBZ or not. If not, it is surprising to see such low radar reflectivity corresponds to drizzle effective radius up to 60 microns. Also, this time series does not include many precipitating profiles. It would be much better to choose another time period that includes a wide range of precipitating conditions.

Could the authors include any independent datasets for evaluations? For example, compare optical depth as shown in Figure 5?

3) Presentation:

Re-organisation of the section 2: I would suggest starting the section with 2.3.2, and making Figure 1 more understandable and stand-alone. The authors need to refer to Figure 1 in a bit more detail to guide readers to understand the overall structure/flow of the retrieval method. It would be nice to construct Figure 1 into a number of main components, provide an overall flow and linkage of all components in the first paragraph, and then synthesize the details in each component.

Some examples that need better connections and wording:

Page 9, Line 18–21: The sentence was talking about z_cb and z_peak, and then the equation below uses z_max and z_min. After reading the line below equation (17), it is unclear how the equation links to z_cb and z_peak. Also, many methods for determining cloud base height from lidar measurements have been proposed and compared; what has presented in this paragraph is a result of the uncertainty in cloud base height determination. Why not mentioning this to justify what has been done here,

instead of presenting them as “the actual cloud base” and “the model cloud base”? More importantly, what is the implication of the need to find the optimal cloud base height? It is not very good news if retrieval needs such precise determination of cloud base height.

Page 9, Line 29: It is unclear what ‘as the maximum number of consecutive range gates around z_cb” means. Do you mean, if z_cb is at range gate # 10, for example, then the maximum number of consecutive range gates is 10? What is the physical justification for this smoothing? Softening certain behaviour to get rid of something does not sound very scientific to me. It would be much more appropriate and convincing if the authors could link this behaviour to some sources of uncertainty/noise for justification.

Page 9, Line 29: Is p_cb the pressure at z_cb? It may be obvious for readers, but all variables should be denoted clearly.

There are also quite a few repetitions. Could the authors please read the manuscript carefully and clean things up?

4) Finally, I feel the manuscript could use a bit more positive attitude/tone - we don’t need to play down other people’s work to justify our work.

Hope it helps.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-402, 2017.