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Atmospheric Measurement Techniques Discussions

Interactive comment on "Towards an operational Ice Cloud Imager (ICI) retrieval product" *by* Patrick Eriksson et al.

Anonymous Referee #1

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General Comments:

This aptly titled manuscript describes the considerable progress towards a "day one" retrieval algorithm for the Ice Cloud Imager on the next generation of EUMETSAT polar orbiters. As such it is highly relevant for publication in AMT. The algorithm described is an marked advance over other operational satellite retrieval algorithms in that it does not assume Gaussian statistics and retrieves five quantiles (e.g. median, 5%, and 95%) of the posterior probability distributions. The introduction provides a brief history of microwave remote sensing in general and a comprehensive summary of previous work in sub-millemetre remote sensing of ice hydrometeors. The limitations of the current algorithm and future work required are frankly and appropriately described, especially in section 5. The manuscript is well organized and the presentation is clear

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(though some grammar needs to be polished).

As is necessary in this situation, the manuscript is somewhat of a summary of a detailed technical report. The performance test section, however, does need to provide information about the numerical experiments that produced the results described. Since the manuscript describes an existing algorithm (even though preliminary), it is not appropriate for a reviewer to tell the authors to make different choices about aspects of the algorithm. Instead the reviewer can help to make the presentation clear and complete, and also make suggestions which might influence future algorithm changes.

Specific Comments:

1. Section 3.4.4: Why are channels masked out if they have any surface contribution? Equation 13 shows the Bayesian uncertainty is increased for channels with surface contribution, and in a Bayesian context that approach is all that should be needed to deal with the uncertainty of surface emissivity. Won't the varying number of channels included in the retrievals cause spurious (if not statistically significant) discontinuities in the retrievals between pixels with close brightness temperatures? Perhaps some more justifications or caveats are needed in this discussion.

2. Section 3.5.1: The quantile retrieval approach requires a substantial number of database cases that match the observations so that the posterior probabilities are high enough. How is this assured? How can we trust the retrieved quantiles, especially at 5% and 95%?

3. Section 3.5.3: Could you mention how the database extraction filtering does not exclude cases which would contribute significantly to the integral and thus bias the result?

4. The end of section 3.5.3 discusses the important issue of very few or no database cases matching an observation.

4a) Do you have a non-arbitrary method for determining when there are too few match-

ing database cases or too wide a range of probabilities for the matching database cases? What specifically are the criteria for increasing the variances in S_0?

4b) This problem is exacerbated by the Gaussian pdf assumption for the probability of the difference between an observation and the database simulation. Could you consider using a long-tailed probability distribution, which would be justified by systematic errors, such as various modeling errors?

5. Section 4.1 (Remapping of data) needs to summarize the numerical experiment performed in addition to discussing the results.

6. Section 4.1: Why is the incidence angle relevant for remapping errors for a homogeneous scene? The brightness temperatures simulated in the database can use the correct zenith angle for each channel, right?

7. Section 4.2 (Generation of retrieval database): It would be useful to include some details about the method for generating the retrieval database used in the experiments in this article.

8. Section 4.3.1 (Test retrieval database): How many CloudSat profiles were used? Do the CloudSat profiles correspond to the same 15S to 15N region and the same time (August 2015)?

9. Section 4.3.2 (Degrees of freedom): Again, there needs to be some explanation of the method used here. How is DOF calculated? The bit of explation in the short figure caption is not enough.

10. Section 4.3.2: 448.0+-1.4 GHz is an upper troposphere water vapor channel in the Tropics and is considerably more sensitive than the 183 and 325 GHz channels, so one doesn't want to give the impression that the three channels for each water vapor absorption line are equivalent. Is the DOF for low IWP and IWV 3 or 4 (I'm having trouble telling from the colour scale)?

11. Section 4.3.3 (Overall performance): A description of the method needed. Do the

two regions/seasons (tropical and mid-latitude) use different retrieval databases? Is there a minimum retrieved IWP for including cases in the Zm and Dm retrieval performance graphs?

12. The brief section 4.3.4 (Test inversions) should be omitted. It references inversion tests with ISMAR data, but without any validation or results, and thus is not very meaningful.

13. Section 5 (Outlook): Do you have ideas for how to include particle orientation in the algorithm? If not too speculative, your ideas would be interesting in this section.

14. Section 5: Another important extension to mention is including a wider range of particle size distribution variations. Presumably the variations in Dm vs IWC curves between single beams is larger than between published climatologies. Also the width of ice particle size distributions is important for relating CloudSat radar reflectivity to IWC for the prior probabilities. This issue might lead to a significant underestimate in the retrieval errors.

Technical Corrections:

Author contributions: I think "PE and BE" should be "PE and BR".

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