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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.

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Thanks for the kind words about our manuscript.

Below we will try to answer your questions and what changes of the manuscript we will implement. Your comments are in italic, with our answers below.

As the retrieval database is in the background of many of the questions, we start by mentioning that the efforts so far have focused on the core algorithm and the retrieval database discussed has been produced has to be considered an initial working basis. Future studies will be required to expand the database.

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.

It is correct that the surface term in Eq. 13 and the channel selection to some extent do the same thing. However, Eq. 13 models the uncertainty in a simplistic manner. The main limitation is that the correlation between channels is ignored. An error in assumed surface emissivity will cause an error that is correlated between channels, but presently we have no knowledge at all on this correlation at sub-mm wavelengths. There should be correlation, but it is likely not one between all channels. See further answer to question 4b.

In any case, if found unnecessary the channel selection can be "deactivated" by setting the optical thickness thresholds to some very high value.

We will add comments to make these aspects clearer.

Yes, there is a high risk that there will be discontinuities in the retrieval at e.g. coasts. They will to a large extent dependent on the quality of the final retrieval database. It could be mentioned that many microwave products are ocean-only, due to the problem of modelling land emissivities, but we wanted to at least keep the door open for producing results over land.

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%?

To our best knowledge, there is no manner to set a required number. The general rule is simple: the more the better! This means that the final retrieval database shall

represent as many cases as possible, and to make this easier we have included the possibility to use a prior weights (a_i).

At the end of the outlook section, we make a comment about that machine learning seems to decrease the demand on database size, and that to apply BMCI for combined MWI and ICI retrievals are probably out of the question.

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?

Nothing like this is built into the algorithm. It will depend on sensible choices of the configuration parameters. The recommendation is, of course, to use broad ranges for inclusion and that tests should be performed when the final retrieval database is at hand.

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 matching 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?

There is no automatic method to set the required number of matches. The number shall be set as high as possible considering the size of the final retrieval database.

We don't see a problem if there is a wide a range of probabilities for the matching database cases. On the contrary, if the range is narrow then you have likely not sampled a sufficient broad part of the a priori distribution.

The rules for increasing variances are important, but require quite some space to describe. For this reason, we decided to only refer to Rydberg (2018) regarding this part.

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 con-

sider using a long-tailed probability distribution, which would be justified by systematic errors, such as various modelling errors?

BMCI is more flexible here than OEM (1D-VAR), but BMCI stills assumes that all uncertainties that are covered by S_o approximately follow Gaussian statistics. So on that side the answer is no. On the other hand, in the generation of the database you are not limited to Gaussian assumptions. (We will add text to clarify this) This means that "outlier cases" can be included in the database.

Above we discussed modelling of surface emissivity. In the generation of the database the emissivity can be varied, and if the knowledge is poor the surface emissivity should be given a broad distribution (that does not need to be Gaussian). This is relatively straightforward. The problem is that the correlation between frequencies of emissivity variations also has an impact, and here reasonable assumptions are very hard to formulate before we actually have the ICI data.

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

Will be done.

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?

We briefly discussed that in the Outlook section. We will add some text to Sec 4.1 to make this more clear.

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.

We see Sec. 4.2 as a general discussion. Details of generation of the database used are found in Sec 4.3.1 (that will be somewhat expanded).

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)?

A smaller dataset, compared to the retrieval database, is used in Fig. 7. We will make this clear.

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 explanation in the short figure caption is not enough.

A description of how DOF is calculated will be added.

10. Section 4.3.2: 448.0+-1.4 GHz is an upper troposphere water vapour 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)?

It is correct that the 448 GHz transition is stronger than the 183 and 325 GHz ones, and has the potential to provide information at higher altitudes than the two later transitions. Figure 1 supports this. However, the present NEDT values for the 448 GHz front-end are relatively high. We have not performed a dedicated test, but we suspect that the assumed NEDT gives too noisy data for obtaining information from the two innermost 448 GHz channels, for single-footprint retrievals. (NEDT is also high for 325 GHz, and likely 183 GHz dominates the IWV information content in Fig 8.) We will make a small rewording, and leaves the details to a dedicated study on ICI's performance at clear-sky conditions.

The DOF for low IWP and IWV is 4. We will redo the figure with another 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?

There is just one single retrieval database. We will clarify how the database is used. Yes, an IWP threshold is applied in Fig. 9. Thanks for spotting this. We will add this information.

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.

We will follow this advice.

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.

In fact, we have been working on this topic, and the first data and results just appeared in AMTD. So we will gladly write a few words about this.

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.

Yes, totally correct. The present text does not make this totally clear. We will rewrite.

Best regards,

Patrick and co-authors

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