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Interactive comment on "Retrieval of an Ice Water Path over the Ocean from ISMAR and MARSS millimeter/submillimeter brightness temperatures" *by* Manfred Brath et al.

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Answers to Anonymous Referee #1

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Reviewer:

Review comments for "retrieval of an ice water path over the ocean from ISMAR and MARSS millimeter/submillimeter brightness temperatures" by Brath et al. This work details a neural-network retrieval algorithm that could retrieve some key hy- drometer quantities at high accuracy from a combination of mm/sub-mm sensors. I especially very much like the sensitivity study using different combination of channels shown in Fig. 4, which quantitatively showcases us the advantage of using combined mm/sub-mm channels. With the nice spread of water vapor channels at 183, 325 and 448 GHz during the flight, I believe a further realistic (and not too ambitious) goal is to actually retrieve the entire water vapor vertical profile, but this is beyond the scope of this study (but giving people very much hope). The writing is not concise, but very clear, detailed and easy to follow. The comparisons against previous works and discussions are comprehensive. The scientific value of this work is high. I fully support the final publication of this work on AMT.

There are five major comments that I'd recommend the authors to address/discuss in the revision:

1) The training database and the neural network was generated from the simulated TB and ICON model simulated atmospheric profiles for the same frontal case for Flight B897. Later on, the retrieved physical quantities are then evaluated against ICON model simulated ones again, the latter of which is treated as the "truth". Under this logic flow, the discrepancies between them are hard to justify whether they can be attributed to (1) the imperfection of ICON model simulation; (2) the imperfection of ARTS model; or (3) the imperfection of the neural network. I understand that SWP/LWP/RWP are very difficult to measure during field campaign, and the only measurable "truth" IWV compares well, which increases the credibility of the retrieval. But authors need to be explicit of the caveat of the whole underlying logic of building-up your retrieval system.

Answer:

The training database and the neural networks were not specifically generated from simulated TB and ICON model simulated atmospheric profiles for the same frontal case for Flight B897. We agree that the text was maybe misleading. We added an additional figure (Fig. 1) in Sect 3.1, where the position and time of each randomly selected profile is shown. We added additional sentences (p. 6 line 10 to p. 7 line 4) , which explain that the database covers a much wider range of atmospheric conditions and that it is not optimized for that specific flight.

We do not consider the ICON as "truth", but this was maybe not emphasized enough. We revised the introduction (p. 3 lines 8-25), which now includes the basic idea of the paper to emphasize the logic flow of the paper. We additionally revised Sect. 5.1 (p. 23 line 32 to p. 26 line 13) now explicitly saying that the ICON model data is not considered as truth and that the comparison is considered as a consistency check. We also address now explicitly the imperfection of the ICON model. Furthermore, we included in Sect. 5.1 (p. 26 line 18 to p. 28 line 23) a more detailed discussion

and analysis of possible reasons for the differences between the retrieval and model including one modified figure (Fig. 7) and an additional figure (Fig. 9).

Reviewer:

2) There is no error analysis in this paper at all. No discussion about the error sources of the retrieved results either. The errors need to be addressed in the revision.

Answer:

We do not agree, that there is no error analysis at all. In Sect. 4 we comprehensively address the combined error due to the neural networks and the noise of the radiometer. But we agree, that it was not stated explicit enough and that the discussion in Sect 5.1 was not detailed enough. We revised the beginning of Sect. 4 (p. 14 line 24 to p. 15 line 2) now explicitly saying, that the addressed error is due to the neural networks and the noise of the radiometer, that modeling errors are excluded in Sect. 4 and that the retrieval errors are likely to be larger when applied to observations. According to that, we adapted the summary of Sect. 4 (p. 22 lines 13-21). Furthermore, see also previous answer, we included in Sect. 5.1 (p. 26 line 18 to p. 28 line 23) a more detailed discussion and analysis of possible reasons for the differences between the retrieval and model arguing why the differences result mainly from model imperfections and not from the neural networks.

Reviewer:

3) Sort of following my major comment#1, one way to show the credibility of your ICON model simulation and the accuracy of ARTS calculation is to show

the simulated TB for each channel on top of Fig. 6 (maybe increase the number of panels but show simulated and observed TB lines one-to-one). In that way, at least we can exclude or conclude the imperfection of neural network model as the major source for some discrepancies.

Answer:

We followed the suggestion and included the simulated brightness temperatures in Fig. 7. We further used the simulated brightness temperatures in the revised analysis and discussion of in Sect. Sect. 5.1 (p. 26 line 18 to p. 28 line 23) .

Reviewer:

4) Are the definitions of ice, snow, liquid and rain, including the size, shape and density are likely to be inconsistent between ICON model and ARTS, correct? What are the drawbacks of these inconsistencies, should they exist?

Answer:

Yes, the definitions of ice, snow, liquid and rain, in terms of size, shape and density are inconsistent between ICON model and ARTS. But this is not problematic, because the function of the ICON model for the database is simply to deliver physically realistically profiles, which span the range of conditions that may be encountered. For this case, it is not needed to be consistent with the ICON model. If we would be interested in the microphysics of the ICON model than consistency would be needed. We added similar sentences to Sect. 3.2 (p.8 lines 27-31).

Reviewer:

5) The "no-offset" conclusion draw from Fig. 3 is more or less biases when SIWP/LWP/RWP is very small, as there's a tight linear line across the upper-domain that "balances" the broad "under-estimation" domain. That means for small values that ISMAR+MARSS are not sensitive to, this retrieval approach tends to generate "bi-model" solutions that not behave symmetrically. Rather than arguing that the "offset is zero", I'd suggest you consider drop trusting the small SWP/LWP/RWP values, and mark your thresholds explicitly on Fig. 7. In this way, readers won't bother thinking why ICON simulated LWP "oscillates" against retrieved ones when the flight enters and leaves the fronts, as the retrieved LWP are so low that the retrieved value itself is not very meaningful.

Answer:

We agree. We added in Fig. 8 and in the newly added Fig. 9 marks, which indicate the range, where the retrieval is considered as offset free according to Sect. 4. We added some words and a sentence calling the specific behavior bimodal (p. 15 lines 23-24).

Reviewer:

In addition, some minor comments need to be clarified:

1) In the title, would "snow water path" be more accurate than "ice water path"?

Answer:

We agree, it would be more accurate to use the term *"snow water path"* instead of *"ice water path"*, but within the literature the term "ice water path" is more familiar. Furthermore, as mentions in Sect. 4, within the literature the term *"ice water path"* is frequently used for total amount of frozen hydrometeors, which mainly consist in our study of snow. Therefore, we think, it is better to use the more familiar term in the title and distinguish in the text.

Reviewer:

2) For the viewing geometry of ISMAR and MARSS, what does "nadir angle" mean? Do they share the same viewing geometry with GMI/CoSSIR? How do you deal with different foot-print size and beam filling effect of different channels in ARTS?

Answer:

We revised Sect. 2 explaining the viewing geometry of ISMAR (p. 4 lines 10-17) and MARSS (p. 4 line 21) thoroughly. The nadir +50 degree view of ISMAR is designed to give a close match in incidence angle to conically-scanning imagers such as ICI, GMI and CoSSIR as used during the Tropical Composition, Cloud and Climate Coupling (TC4) experiment in 2007 (Evans et al., 2012).

We neglect possible effects due to different footprint sizes and beam-filling as the footprint of MARSS and ISMAR are much smaller than a grid cell from the ICON model. The footprint size at ground level is pretty much the same for all the ISMAR channels and are in the order of 700 m for a flight altitude of 10 km. The footprint sizes of MARSS are in the order of $1,500$ m for the 89.0 GHz and the 157.05 GHz channels and in the

order of $1,000$ m for the 183.31 GHz channels. The footprints of ISMAR and MARSS are much smaller than footprints from similar satellite instruments. We added a similar statement to Sect. 3.2 (p.7 lines 17-20).

Reviewer:

3) 100um threshold used to separate snow and ice particles are more or less too arbi- trary. I'm not familiar with the set-up with ICON model. As the microphysics schemes is not explicitly presented in the current manuscript (e.g., two-moment or one moment? How many hydrometer species? Do you allow super-cooled water to present? What's the ice/snow density? etc.), I'm not sure if the definition of SWP is consistent of what's been defined in the ICON model simulations.

Answer:

The 100 µmthreshold is not set arbitrary but emerges from the used size distributions. We agree that this point was not stated clearly. We added a few sentences in the introduction (p. 2 line 4) and in Sect. 3.2 (p. 9 lines 20-21).

The ICON runs used a 1-moment microphysics scheme with four distinct hydrometeor types namely liquid cloud water, cloud ice, rain and snow, which is also mentioned in the revised text (p. 8 lines 13-14) , now. Super cooled water exist in the model runs. The microphysics used in the paper differs in terms of size, shape, and density from the ICON internal but in the basic definition they are the same.

References

KF Evans, JR Wang, O'C Starr, G Heymsfield, L Li, L Tian, RP Lawson, AJ Heymsfield, A Bansemer, et al. Ice hydrometeor profile retrieval algorithm for high-frequency microwave radiometers: application to the cossir instrument during tc4. *Atmospheric Measurement Techniques*, 5(9):2277–2306, 2012.