

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.

Anonymous Referee #1

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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 hydrometer 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

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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.

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.

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.

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?

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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.

In addition, some minor comments need to be clarified: 1) In the title, would “snow water path” be more accurate than “ice water path”?

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?

3) 100um threshold used to separate snow and ice particles are more or less too arbitrary. 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.

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