

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 #2

Received and published: 24 September 2017

This study investigates high frequency microwave and sub-mm based retrievals of a various quantities like snow water path, rain water path, and integrated water vapor using a neural network (NN) retrieval methodology. My overall impression of this manuscript is that is extremely well written, thorough, and advances the state of retrieval science in the sub-mm portion of the electromagnetic spectrum. The authors included relevant and necessary discussion sections that elucidate major sources of uncertainty. I was admittedly thinking of possible major concerns, but authors inevitably preemptively addressed these concerns with thorough discussion sections within the manuscript. I truly appreciate these efforts by the authors. I suggest minor revisions based on the comments below, mostly related to NN methodology and applicability to

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eventual global retrievals.

Page 6, Lines 4-5: “Because the atmospheric profiles were from the same season and the same region as the measurements, these profiles are expected to sufficiently cover the situations encountered during the measurement flight.” The NN training dataset serves as arguably the most important component of this retrieval methodology. Populating the training dataset with a sufficient number of representative profiles is absolutely crucial for success. My question: this study analyzes a case study, comparing it to airborne observations, so the NN training dataset is probably sufficient and representative. But this particular region is susceptible to cold air outbreaks and synoptic scale weather systems that ultimately drive the weather and associated cloud formations. I assume the three simulated days chosen have representative synoptic conditions of this region, including a representative cold air outbreak scenario? This seems especially important for sample field campaign airborne retrievals presented later in the manuscript. Is a NN feasible for global retrievals? How do you sufficiently populate a database for global retrieval applications? These final two questions are relevant if the authors want to utilize this methodology for eventual ICI retrievals. Perhaps this question might be best addressed in the Summary section.

Page 6 Lines 14-15: “No explicit spectral response function was used to simulate the the ISMAR and MARSS channels; instead, we conducted monochromatic radiative transfer simulations for the center frequencies of the two side bands of each channel and obtained their average”. This seems like a reasonable approach. Can the authors supply any sample uncertainties for this methodological approach? I assume uncertainties may increase under highly scattering conditions for sub-mm frequencies with weighting functions low enough in the atmosphere to be prone to ice scattering.

Page 6, Near line 25: FASTEM discussion regarding surface emissivity – can simple examples be provided that illustrate the lack of surface sensitivity for a few representative sub-mm channels? This type of analysis could be appropriate as a supplement, or at least provide references if this type of work has been published beforehand.

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Page 6, last paragraph: Suggest adding unit information parenthetically. Current sentence seems awkward with comma devoted to unit information. For example, “cloud ice water (converted to kg m⁻³)”. Or just include the units parenthetically without “converted to” wording. Also verify AMT publication standards regarding unit display. For example, kg/m³ versus kg m⁻³.

Page 7, Cloud ice: I am curious why a different Hong et al (2009) particle rendition is not chosen for cloud ice scattering simulations? I understand soft spheres may be fine for very small ice particles when the size ratio is sufficiently small, but do some sub-mm channels violate this small size ratio restriction and necessitate using DDA databases instead of soft spheres?

Page 12, Lines 19-25: I appreciate the authors being frank with possible downsides of using neural networks. Not having used NN before, is computational burden also excessive? It seems like the combined computational burden of (a) needing a large sample of numerical model results to populate the training dataset and (b) adopting an ensemble NN approach make this exercise fairly computationally intensive. This approach seems defensible and justifiable for the current application of illustrating retrieval efficacy for various parameters using combined microwave and sub-mm channels. But will a NN approach be untenable for real-time retrievals from space borne sensors (for instance, when ICI and MWI are eventually launched)?

Page 22, lines 5-6: Any specific reason why a 3.5 minute running average was chosen versus a different time averaging duration?

Section 5.1: So the NN training datasets from all 3 numerical simulations were used for the retrievals shown in this section? Or was the training dataset from the 18 March simulation results applied exclusively to this case? This is not a major issue for the results presented in this particular study, but my main question is how a similar NN retrieval methodology can be applied to a global dataset. Would the training dataset require daily simulations to provide a robust training dataset for global retrievals? Or

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would a handful of simulations that temporally and strategically sampled various seasons suffice?

Summary section: As mentioned, ice scattering simulations are rare for sub-mm frequencies. I would add more emphasis in this section (another sentence or two) to encourage the community to produce more ice model scattering datasets at frequencies exceeding 200 GHz. This seems like a necessary research step to improve ice and snow column retrievals at sub-mm frequencies.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-167, 2017.

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