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Atmospheric Measurement Techniques Discussions

Interactive comment on "Using passive and active microwave observations to constrain ice particle models" *by* Robin Ekelund et al.

Anonymous Referee #2

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The authors provide radiative transfer (RT) simulations at microwave and submillitre frequencies and demonstrate both the sensitivity to microphysical assumptions (ice/snow particle model and the particle size distribution (PSD)) and compare RT simulations to Global Precipitation Measurement Microwave Imager (GMI) observations in the tropics. The authors also present simulations relevant for the upcoming Ice Cloud Imager (ICI) sensor.

The authors tackle a very complicated topic given the considerable variability of cloud microphysical composition and its subsequent effect on upwelling microwave brightness temperatures. They adopt a combined radar (CloudSat) and radiometer (GMI) framework to constrain microphysics and show that this strategy is preferred to methods that do not leverage radar observations. While this strategy has been adopted in

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Discussion paper

previous studies and is not itself unique, the current study uses state-of-the-art particle model scattering databases and numerous PSD parametrisations to move the scientific needle forward on this topic. Simulations at ICI frequencies are also a novel concept that provide beneficial insight for eventual retrieval development for this instrument. While no particular ice model is shown to offer optimal results when RT simulations are compared to observations, this study provides useful guidance on PSD and ice model combinations that can be used in future retrieval research. The authors also readily acknowledge that no particular ice model can realistically be applied as a universal answer, but illustrate important sensitivity studies that can be used to propel further research on this topic.

The manuscript is well-organized and written in an easy-to-follow manner. Figure quality is excellent. The manuscript can be published largely intact, but I suggest a few minor methodological description improvements and other random suggestions that will hopefully allow the authors to further refine the manuscript. I do not see any obvious or fatal scientific flaws regarding the study design and interpretation of the results. My only comment that could be considered as something more than a minor issue regards other possible options to provide further quantitative analyses that might be interesting to the community. I do not classify the suggestions as mandatory, but hopefully will spur the authors to find further creative ways to tabulate their comparisons to observations. Specific comments are outlined below.

Title: Since 664 GHz is considered in this study, maybe consider adding submillimetre to the title to better advertise the ICI applications? This suggestion is purely semantics, but this study extends beyond the wavelengths typically associated with microwave radiometry. Some remote sensing specialists designate submillimetre wavelengths as a distinct category occupying the space between microwave and infrared, while others may consider ICI-like frequencies as part of the microwave spectrum. Another option is to specifically include sensors like GPM and ICI in the title. This suggestion is not mandatory but is something the authors should consider to better advertise the novel

ICI-related content.

Line 44: Should MLS, SMILES, and Odin-SMR acronyms be explicitly written?

Lines 58-59: A reference or references might be beneficial to prove that soft spheroid models produce good results at single frequencies.

Lines 276-278: A CloudSat-GMI coincident dataset exists, but as the authors mention, tropical coincidences are limited. Would it be worth highlighting how few coincident observations exist compared to higher latitudes? The Rysman et al. (2018) manuscript gives a quantitative analysis of global CloudSat-GMI coincidences for snowing observations (see Figure 2). At the very least, the Rysman et al. (2018) manuscript could be referenced to illustrate this point without the authors calculating their own statistics.

Rysman, J.-F.; Panegrossi, G.; Sanò, P.; Marra, A.C.; Dietrich, S.; Milani, L.; Kulie, M.S. SLALOM: An All-Surface Snow Water Path Retrieval Algorithm for the GPM Microwave Imager. Remote Sens. 2018, 10, 1278.

Line 312: Please provide more information regarding gaseous and cloud liquid water absorption methodology used in the RT simulations. This information will allow other investigators to better replicate the study. Studies have also indicated RT variability using standard water vapour continuum and cloud liquid water absorption models (e.g., Turner et al 2009, Kneifel et al. 2014 and others), so knowing what absorption models were used is essential information.

D. D. Turner, M. P. Cadeddu, U. Lohnert, S. Crewell and A. M. Vogelmann, "Modifications to the Water Vapor Continuum in the Microwave Suggested by Ground-Based 150-GHz Observations," in IEEE Transactions on Geoscience and Remote Sensing, vol. 47, no. 10, pp. 3326-3337, Oct. 2009. doi: 10.1109/TGRS.2009.2022262

Kneifel, S., S. Redl, E. Orlandi, U. Löhnert, M.P. Cadeddu, D.D. Turner, and M. Chen, 2014: Absorption Properties of Supercooled Liquid Water between 31 and 225 GHz: Evaluation of Absorption Models Using Ground-Based Observations. J. Appl. Meteor.

Climatol., 53, 1028–1045, https://doi.org/10.1175/JAMC-D-13-0214.1

Line 333: Very minor wording suggestion. Change “D14 puts emphasis at smaller particles” to “D14 emphasizes smaller particles”.

Line 429: The authors use the subjective term “good” to describe GMI and RT simulation comparisons. Can a more quantitative or less subjective term be used here to describe the comparisons?

Line 453-454: I agree that ERA-Interim water vapour content is probably the likely culprit to cause clear sky biases. Clear sky RT results could also be influenced by the water vapour absorption model, but I’m not sure if that would cause the TB offset. What is the exact bias value under clear sky conditions? This would be useful information to convey. At lower microwave frequencies, ocean emissivity models can also be responsible for clear sky biases on the order of a few K. But surface effects are probably limited at some of these submillimetre channels, especially near water vapour absorption features.

Line 533: Capitalize Pacific Ocean?

Line 540: Change second sentence to “The biggest issues occur under clear-sky conditions, like due to [. . .]”

Lines 540-544: Regarding the deep convective cores with the lowest TB values, is it possible that graupel or hail aloft is not properly considered in the RT simulations? Would higher density particles be more appropriate under such conditions? This suggestion might increase the population of simulated extremely low TB values that already exceed the population of GMI TB values below about 170 K (Figure 10). But this suggestion might reduce the relative peak just below 150 K (Figure 10 bottom panels) by shifting the TB distribution to lower values and make the simulated TB distribution shaped more similarly to GMI observations. This is admittedly semi-educated speculation, but it might be another issue to highlight in the discussion section.

Related to the previous point, can the authors provide further useful analyses by partitioning their TB analyses using CloudSat-specific properties? The authors surmise, probably correctly, that certain TB regimes are related to deep convection, etc. CloudSat properties allow those assumptions to be ascertained without ambiguity and provide extremely valuable context. If the authors feel this type of analysis is beyond the current scope of the study, I would appreciate some justification. At a minimum, I encourage the authors to include language in the discussion section on possible ways to analyze the observations more deeply in follow-on studies. I envision multiple ways that CloudSat could inform the ICI simulations to better define TB simulation uncertainties for specific meteorological conditions based on cloud properties and related ambient conditions.

Lines 567-573: Passive infrared information would only provide cloud-top microphysical information, correct? The sensitivity to smaller particles would increase and further constrain the microphysical properties, but that information would only pertain to the cloud-top environment. Would the IR information advantage be related to the fact that if cloud-top microphysical properties could be better constrained, then microphysical evolution at each ensuing level below the cloud top is also better constrained?

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