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Atmospheric Measurement Techniques
Discussions

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Interactive comment

Interactive comment on "Airborne validation of radiative transfer modelling of ice clouds at millimetre and sub-millimetre wavelengths" *by* Stuart Fox et al.

Anonymous Referee #1

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Review comments for "Airborne validation of radiative transfer modelling of ice clouds at millimeter and sub-millimetre wavelengths" by Fox et al.

This work analyzed the pioneer airborne campaign data from ISMAR, a miniature of the ICI instrument that is planned to be launched to space in the near future, to demonstrate and explore the capabilities and difficulties of sub-mm passive microwave techniques would face. Other in-situ data (e.g., size and habit derived from CIP probes, and lidar retrieved PSD) are used together with a comprehensive radiative transfer model (RTM) called "ARTS" to make every effort to keep the retrieval results cross-instrument, cross-parameter and cross-frequency interconsistent. This approach adds

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Discussion paper

solidity to the results. The authors found that a great variety of ice particle habit could result in similar brightness temperature depression that are sometimes comparable to the observations while sometimes not. Same particle habits may achieve good agreement at certain channel frequency (e.g., 325 GHz) but not higher frequency (e.g., 664 GHz). Although one of the flight only flew through relatively thin cirrus cloud, it actually helped on identifying the detectability threshold of sub-mm technique on cloud ice water path/content, which is informative albeit the fact that RTMs had difficulties on reproducing this case (B895). Furthermore, the authors found out that one particle habit that worked well for mm-wave performs bad for sub-mm, indicating that we need to be very cautious when assuming one universal ice crystal shape across different spectrum regimes when it comes to the data assimilation in order to "truly" benefit the model and weather forecasting.

It is enjoyable reading this manuscript as the English and logic flow are smooth, and the results and methodology are nicely presented with many details and being strict at the same time. There are a few touches I hope the authors can take into consideration before final publication to make the discussion more in depth and open to future explorations at the same time.

(1) Please color the flight legs as a function of height so it's directly visible which heights the cloud information comes from.

(2) Other than to avoid H/V complication, why you particularly interested in only focusing on analyzing and comparing the near-nadir observations? Considering that the orientation information is most prominent at slant views, and considering that the ICI viewing geometry is designed to be conical (?, or am I have a wrong impression, I remember the viewing geometry is somewhat similar to GMI), near-nadir view story might not be so suggestive for the satellite-borne mission. Please do elaborate somewhere in the paper about what conclusions might be changed when we move to slantwise view? What are the potential difficulties that observations and RTM might face with the slantwise view, especially for other microphysical parameters that further perplex

the problem. Would multi-frequency V/H observations potentially helpful on resolving some of the issue, given that ARTS can now qualitatively capture the V/H through the Monte-Carlo method (i.e., not using the single scattering database, but using the 3D radiative transfer).

(3) It's not a very good assumption to assume the particle shape doesn't change over the whole flight leg, neither horizontally nor vertically. Also, it's natural somewhat to me to understand why 338 and 664 GHz cannot be achieved best-match at the same time, simply because they are sensitive to different size/shape of the particles which may likely co-exist in the vertical column at different altitudes. So I think you should discuss these possibilities that may likely happen in the real world and that partially account for the failure to match RTM simulations with observations.

(4) How do you deal with the antenna pattern (i.e., line function) for the ISMAR sensor? That may cause 1-2 K warm bias even if you have a perfect background atmosphere setting.

(5) B895 is really not an idea leg for this study because it's reaching the lower boundary of sub-mm sensitivity, as you also pointed out in your manuscript. So I would rather not put too much effort on match the B895 result – channel noise, imperfect background atmosphere, etc., all these factors can beat down the observed BT difference for this flight.

(6) I still don't get why the "smallPlateAggregate" produces the best match for IWC (Fig. 8), but not for IWP (Fig. 10)? It seems to me that the "problematic SectorSnowFlake" actually produces the best match for the BT difference – IWP relationship as shown in Fig. 10.

(7) In the discussion or conclusion section, please elaborate with a few sentences that whether your approach can be applied to previous campaigns, e.g., OLYMPEX with multi-frequency radar, CIP and CoSMIR? Further back in TC4 campaign, we have CoSSIR that is similar to ISMAR in some sense. If possible, using previous campaign

observations that carried out in different weather regimes might aid greatly on identify the sub-mm capability and RTM caveats/advantages.

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