Review of “Retrieval of Terahertz Ice Cloud Properties from airborne measurements based on the irregularly shaped Voronoi ice scattering models” by Ming Li et al.

General comments

The paper is about applying the Voronoi model to the retrieval of IWP and r_e using brightness temperature differences between 380, 640, and 874 GHz. Not surprisingly, the authors find the Voronoi model re-produces previous retrievals of IWP and r_e more accurately than the sphere. This aspect is not new in the microwave and sub-millimetre, see for instance, the study by Eriksson et al. (2015), https://amt.copernicus.org/articles/8/1913/2015/amt-8-1913-2015.pdf as to why the authors find the sphere to be an inadequate representation of non-spherical ice scattering in the microwave and sub-mm regions. The important aspect of this paper is that the Voronoi model has been previously applied to simulate solar and infrared observations, and now it is being applied over the Terahertz region to see how well the model performs there. However, as to how skillfully it performs against other ice crystal models is yet to be tested. The authors find very good correlations between the Voronoi-based retrievals and Evan’s Bayesian retrievals using data from the CoSSIR instrument. The paper is relatively well-written and can be followed. The figures are also well represented, and the analysis is quantitative, with no obvious flaws. Further proof-reading is recommended to help improve the flow of the paper. This paper could be significantly improved, which if followed, would make the paper a more important contribution to the remote sensing of ice cloud in the microwave and sub-millimetre regions of the spectrum.

The major recommendations are as follows:

1. It is felt that the authors missed an opportunity to test the veracity of the Voronoi model in the microwave and sub-mm by not comparing their results with another more representative ice crystal scattering model. For instance, why not use the scattering models contained in the ARTS database of single-scattering properties? One model from the ARTS collection of models to try and test against the Voronoi model is the large column aggregate model. This model was shown by Fox (2020) to simulate better than some of the other models, the microwave and sub-millimeter brightness temperature measurements between the frequencies of 183 and 664 GHz. I recommend the authors compare their retrievals and simulations against more realistic ice crystal scattering models such as the ARTS large column aggregate. See, Fox, S. An Evaluation of Radiative Transfer Simulations of Cloudy Scenes from a Numerical Weather Prediction Model at Sub-Millimetre Frequencies Using Airborne Observations. Remote Sens. 2020, 12, 2758. https://doi.org/10.3390/rs12172758.

2. The authors make use of existing retrievals of r_e and IWP to test the Voronoi model but do not make use of the independent measures of IWP and r_e as derived from the in-situ aircraft during TC4. Why is this? Is the in-situ aircraft data not available? Was there no in-situ data co-incident with the radiometric measurements? The problem with comparing with the existing CoSSIR retrievals is that those retrievals are based on differing assumptions of mass, ice crystal shape and PSDs – comparing apples and oranges. It could be said that the CoSSIR ice crystal shape and mass assumptions are just as valid as the Voronoi model, yet they may be entirely different. It would be much better to compare retrievals with in-situ measures if those are available.

3. The authors propose a convoluted and unnecessary method of relating r_e to D_{me}. This is surprising, since in the terahertz region the scattering cross sections are more dependent on mass rather than area. Why use an area-weighted size such as re rather than a mass-weighted size such as D_{me}? The problem with using r_e in the terahertz region is nicely explained in the study by Seiron et al. (2017), see

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017JD026494. In the region of interest, a mass-weighted size would be the more appropriate characteristic size of the PSD to utilize in this paper.

4. No evidence is presented as to how representative the PSDs used in the analysis are for the TC4 cases considered in the paper. The best way to do this is to derive the moments of the assumed PSDs and in-situ PSDs and show how well correlated they are. Of course, if the in-situ PSDs are not available, this cannot be done!

5. Related to 4, is the question of how representative is the ERA5 re-analysis product for a couple of TC4 cases? The temperature, water vapour and ozone profiles are important in the radiative transfer simulations. If the ERA5 re-analysis product is not representative of the actual state of the atmosphere for those few days, this could bias the brightness temperature difference results. The authors should compare some of the ERA5 atmospheric profiles with the aircraft profiles, if the latter are available.

6. The authors need to provide images of the Voronoi model with increasing ice crystal size, such that it can be seen by readers how the model aggregation varies with size. What are also required in the revised paper are the Voronoi model's mass– and area–dimension power laws. These power law relations will go some way to explaining the single-scattering results and sensitivities of the Voronoi model to IWP and the characteristic size of the PSD in the brightness temperature difference sensitivity analysis. The fractal dimensions of mass and area of the Voronoi model are important in these respects.

7. Apart from plotting the retrieved quantities, a further measure of how well the Voronoi model represents the measured brightness temperatures at the three channels is to plot the residuals (i.e. brightness temperature differences between the forward model and measurements) as a function of time for all three channels.

The minor comments are listed as follows with page numbers:

1. Introduction line 34. Since the authors discuss 20-30% of the global cloud mass, would it not be better to cite more updated studies that more directly measure the ice mass such as studies using CloudSat global retrievals of ice mass? As well as mm-wave retrievals of ice mass?

2. Line 36. As the paper is discussing Terahertz frequencies, another important property of large ice crystals that contribute to the radiative properties of ice cloud is their orientation.

3. List of citations on line 47. Fox (2020) should be added to this list?

4. Line 51. The description of Fox et al. (2019) needs to be more accurate, the study also used sub-mm frequencies up to 664 GHz, and in Fox (2020). The works of Fox (2019,2020) includes the Terahertz region, and not just the microwave.

5. Line 79, again ice crystal orientation is also an important consideration here.

6. Line 97. Another numerical method that could be included in this list is the Boundary Element Method, which has recently been applied to very complex ice crystals by Kleanthous et al. (2022): Antigoni Kleanthous, Timo Betcke, David P. Hewett, Paul Escapil-Inchauspé, Carlos Jerez-Hanckes, Anthony J. Baran, Accelerated Calderón preconditioning for Maxwell transmission problems, Journal of Computational Physics, Volume 458, 2022, 111099, ISSN 0021-9991, https://doi.org/10.1016/j.jcp.2022.111099. A further paper here could be Mano

(2000), who applied BEM to hexagonal ice columns. "Exact solution of electromagnetic scattering by a three-dimensional hexagonal ice column obtained with the boundary-element method," Appl. Opt. **39**, 5541-5546.

7. Line 98. This GOA acronym has not been defined - should it be GOM?

8. The discussion beginning on line 108. Another ICS model worthy of note in this context is the ensemble model of cirrus ice crystals developed by Baran and Labonnote (2007). The ensemble model attempts to be more representative of the evolution of the ice crystal aggregation process as a function of increasing size, see Baran, A.J. and Labonnote, L.-C. (2007), A self-consistent scattering model for cirrus. I: The solar region. Q.J.R. Meteorol. Soc., 133: 1899-1912.) https://doi.org/10.1002/qj.164), and Baran et al. 2014 (Baran, A.J., Cotton, R., Furtado, K., Havemann, S., Labonnote, L.-C., Marenco, F., Smith, A. and Thelen, J.-C. (2014), A self-consistent scattering model for cirrus. II: The high and low frequencies. Q.J.R. Meteorol. Soc., 140: 1039-1057. https://doi.org/10.1002/qj.2193).

9. Typo on line 117 Mo.,del -> Model

10. Line 118. The word effectiveness is sufficient, using the word "superiority" is inappropriate here because it has not been proven relative to all other models that are now available.

11. Line 122. ICI is not correct here, the instrument is ISMAR (International Sub-Millimeter wave Airborne Radiometer) described in Fox et al., 2017. ISMAR was jointly funded by the Met Office and ESA – not ICI.

12. Section 2.1. Which refractive indices are being used to compute the SSPs? The refractive indices in the microwave and sub-millimeter are temperature dependent - is this dependence considered in the simulations that follow? If not, which temperature has been assumed in the calculation of the SSPs? How have you justified the selection of this temperature?

13. Section 2.3. Is the cloud between the boundaries assumed to be homogeneous? If so, please state this.

14. Equation 2, line 186. In the denominator, this is why you need to provide the model's mass–dimension relationship.

15. Equation 4, line 205. In the denominator, this is why you need to provide the model's area–dimension relationship.

16. Equation 7, there is a missing wavelength dependence in the denominator for the scattering cross section.

17. Equations 9 -12, how accurate are the parametric fits as a function of D_e?

18. Figure 9, this figure might be better plotted as a PDF of the retrievals, and statistically measure how different the distributions are from the reference PDFs using some statistical measure.