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Atmospheric Measurement Techniques
Discussions

# *Interactive comment on* "Meso-scale modeling and radiative transfer simulations of a snowfall event over France at microwaves for passive and active modes and evaluation with satellite observations" *by* V. S. Galligani et al.

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(Please accept my apologies for a late comment). Overall I'm satisfied with the basic premise of the paper: forward modeled microwave radiances being compared with passive and active observations, however there are a few fundamental issues that I believe detracts from the paper. The use of the Maxwell Garnett mixing formula is only strictly valid for inclusions having low volume fractions – the physical argument being that the inclusion materials must remain electrically disconnected. Indeed, this results



in increasingly excessive behavior by the application of the dielectric mixing formula as the inclusion volume fractions increase. Petty and Huang (Petty, G. W., and W. Huang, 2010: Microwave backscatter and extinction by soft ice spheres and complex snow aggregates.J. Atmos. Sci., 67, 769–787, and Johnson 2012 (cited in your paper)) identify some issues associated with this approach. The use of the Bruggeman formula is preferred for a more physically realistic approach.

Also I'm concerned with the use of a frequency dependent "softness" parameter. Two comments in particular: (1) There's not an obvious frequency dependence there. Dmax, D, and D0 are all physical parameters, unless Dmax is the maximum diameter of the soft sphere, which would presumably fluctuate with whatever density is chosen. (2) The choice of a frequency dependent density has a number of other important physical consequences, the most important being it's physically unrealistic. For example, by changing the densities, by implication you're going to have different terminal fall velocities for each particle, or risk a discontinuity between presumed fall velocity and total particle volume. Even if you only consider mass-based measures of precipitation (e.g., Ice-water content / Ice-water path), the use of a frequency dependent density introduces a "tuning parameter" which has very little basis in reality.

"The radiative transfer simulations presented so far in Figs. 2 and 3 fail to reproduce the observed scattering signatures because either (1) the amount of frozen particles produced by Meso-NH simulations is underestimated, or (2) there is a misrepresentation of the scattering properties of the frozen phase, more specifically of snow species, 15 in the RT simulations in terms of dielectric properties, effective size, and shape."

One of the issues we ran into with simulating brightness temperatures and radar reflectivities was that the spherical / spheroidal particles suffer from resonances inside the spheroid, which directly impacts the relationship between backscattering ( and asymmetry parameter (used for computing TBs). No matter what mass-density relationships were chosen, we could never get both multi-frequency TBs and radar reflectivities to match to a desired uncertainty. This led to a number of studies from ourselves and within the community to start looking at non-spherical particles (i.e., from DDA) – our initial studies, still ongoing, indicate that non-spherical particles more accurately capture the correct relationship between backscattering and asymmetry, resulting in more consistent TB - Z simulations.

Another comment is the seemingly scattered and ad-hoc nature of the selection for density and choice of particle shapes. The description is not systematic enough in order for a researcher to reproduce your approach. My recommendation is to provide a more clear depiction of what assumptions are present for a given analysis, including but not limited to: dielectric constants and assumed temperature of ice and water, dielectric mixing method used, shape assumptions used, particle size distribution assumptions (parameters of the PSD), how truncation of the PSD tails are handled, cloud liquid water attenuation / emission, surface property assumptions for TB calculations, melt-water generation assumptions in melting-layer regions and how the PSDs are being modified as melting occurs (i.e., how does the ice get converted to rain).

Also, I didn't see any discussion of how the model resolution is scaled to the MHS resolution, was an actual antenna pattern used? A 2-D gaussian? Similar comment regarding the CloudSat comparison – what scales are being compared? All of these items mentioned here are important as the choice of how they are handled can have significant impacts on the computed TBs and reflectivities.

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Interactive comment on Atmos. Meas. Tech. Discuss., 7, 7175, 2014.